Prime Broker-Level Comovement in Hedge Fund Returns: Information or Contagion?

Ji-Woong Chung
Korea University

Byoung Uk Kang
Hong Kong Polytechnic University

We document strong comovement in the returns of hedge funds sharing the same prime broker. This comovement is driven neither by funds in the same family nor in the same style, and it is distinct from market-wide and local comovement. The common information hypothesis attributes this phenomenon to the prime broker providing valuable information to its hedge fund clients. The prime broker-level contagion hypothesis attributes the comovement to the prime broker spreading funding liquidity shocks across its hedge fund clients. We find strong evidence supporting the common information hypothesis, but limited evidence in favor of the prime broker-level contagion hypothesis. (*JEL* G11, G14, G23, G24)

Received September 6, 2014; accepted January 7, 2016 by Editor Philip Strahan.

We document strong comovement in the returns of hedge funds serviced by the same prime broker (PB). This PB-level comovement in hedge fund returns is driven neither by funds in the same family nor by those in the same style, and it remains significant after removing the effect of common risk factors. PB-level comovement is also distinct from market-wide comovement and local comovement in hedge fund returns. Our finding has an important implication

We thank George Aragon, Stephen Brown, Darwin Choi, Tarun Chordia, Timothy Chue, Alexander Eisele, Robert Faff, Bruce Grundy, David Hirshleifer, Kewei Hou, Po-Hsuan Hsu, Lei Jiang, Marcin Kacperczyk, Bing Liang, Rose Liao, Alexander Ljungqvist, Igor Makarov, Spencer Martin, James Ohlson, Grace Pownall, Tarun Ramadorai, Sergei Sarkissian, Katherine Schipper, Christopher Schwarz, Clemens Sialm, Wei-Ling Song, Philip Strahan (the editor), René Stulz, Avanidhar Subrahmanyam, Zheng Sun, Chang Hwan Sung, Cristian Tiu, Michael Weisbach, Harold Zhang, Lu Zheng, Jean-Pierre Zigrand, two anonymous referees, and seminar and conference participants at the City University of Hong Kong, HKUST, Hong Kong Polytechnic University, KAIST, Korea University, London School of Economics, Monash University, Seoul National University, SKKU, UNIST, University of Hong Kong, University of New South Wales, University of Melbourne, University of Technology Sydney, Yonsei University, the 2014 AsianFA Annual Conference, the 2014 China International Conference in Finance, the 2014 European Finance Association Annual Meeting, and the 9th Annual Conference on Asia-Pacific Financial Markets for useful comments and suggestions. Yuanhang Qiu provided excellent research assistance. Any errors are our own. We acknowledge funding support from Korea University (the Asian Institute of Corporate Governance and the Institute for Business Research and Education) and Hong Kong Polytechnic University (grant no. G-UB25). Supplementary data can be found on *The Review of Financial Studies* web site. Send correspondence to Byoung Uk Kang, School of Accounting and Finance, Hong Kong Polytechnic University, Hung Hom, Kowloon, Hong Kong; telephone: +852 2766 7085. E-mail: byoung.kang@polyu.edu.hk.

doi:10.1093/rfs/hhw052 Advance Access publication June 21, 2016

for hedge fund investment: the benefit of diversifying across hedge funds may be limited if some funds in a portfolio are serviced by the same PB, which is likely to be the case given the large number of hedge funds versus the much smaller number of PBs.

We offer two potential, nonmutually exclusive explanations for this phenomenon. First, the *common information* hypothesis posits that the PB provides valuable information to its hedge fund clients, inducing comovement in the clients' returns as they trade on the information. PBs, as investment banks, might obtain alpha-generating information from their in-house research or, controversially, from their investment banking or lending activities, as suggested by the popular press and several academic studies (Massa and Rehman 2008; Bodnaruk, Massa, and Simonov 2009; Jegadeesh and Tang 2010; Goldie 2011; Kedia and Zhou 2014).[1] While we cannot directly observe how individual PBs come to possess such information in the first place, their economic incentives to pass it on to hedge fund clients are clear: hedge funds generate substantial revenue for investment banks because of high turnover in their portfolio and the prime brokerage fees associated with taking leveraged and short positions.[2]

Second, the *PB-level contagion* hypothesis posits that the PB transmits funding liquidity shocks across its hedge fund clients when its financial health deteriorates. The financial distress of a PB could translate into increased margin requirements for its hedge funds clients as the PB curtails its lending (Klaus and Rzepkowski 2009a; Boyson, Stahel, and Stulz 2010). Since most hedge funds rely on short-term financing from their PBs to pursue leveraged investment strategies, increased margins could force hedge funds to close out some of their positions at unfavorable cost, leading to (downside) comovement in hedge fund returns. An extreme example of the impact of a PB's distress on its hedge fund clients can be found in Lehman Brothers' bankruptcy, where many of its hedge fund clients were brought down when it failed in September 2008. Aragon and Strahan (2012) show that Lehman's hedge fund clients failed more than did other funds in 2008.[3]

Of course, we are mindful of the possibility that PB-level comovement is not due to the PB inducing it, but instead is due to the PB simply choosing to service "similar" funds that comove in their returns (or, equivalently, due to similar funds choosing to use the same PB). As noted by Aragon and Strahan

[1] Alpha-generating ideas might also be obtained from their prime brokerage activities as PBs routinely observe the trading and holdings of their hedge fund clients.

[2] According to a 2005 estimate, more than one in every eight dollars of investment bank revenue comes from hedge fund clients (Lynn 2005). Also, Wall Street collects \$33 million a year in trading commissions from the average hedge fund and \$16 million from the average mutual fund (Onaran 2007). Given this, "Wall Street research departments are rapidly organizing themselves to serve their best-paying customers: hedge funds" (Schack 2003).

[3] Yet another story—which we discuss further in Online Appendix Section A.2—is that there exist some sort of PB-specific valuation mechanisms for calculating the fund's net asset value (NAV), and these, by acting as a PB-specific component in hedge fund returns, could drive comovement at the PB level.

(2012), hedge funds choosing the same PB might be similar to one another along some important unobserved dimension. If this unobserved dimension is correlated with return comovement, it will lead to a commonality in the returns of hedge funds using the same PB. We call this possibility for similar hedge funds to select into the same PB (or vice versa) the self-selection hypothesis.

We measure the PB-level comovement of a hedge fund by the time-series sensitivity (beta) of its returns to the returns of an index of hedge funds using the same PB. When estimating PB-level comovement, we control for comovement with both the overall sample funds and funds in the corresponding style category (we call them "market-wide" and style-level comovement, respectively). The latter is particularly important provided that some PBs specialize in servicing funds in certain investment styles. As an additional robustness test, we use a subsample of hedge funds located in the United States and show that the PB-level comovement is different from the local comovement in hedge fund returns documented by Sialm, Sun, and Zheng (2014). Throughout, our results do not materially change when we orthogonalize the returns of each hedge fund against the Fung and Hsieh (2004) seven factors and use the orthogonalized returns in place of original returns.

What drives this phenomenon? We first turn to and address the self-selection hypothesis. To this end, we identify a subsample of hedge funds that experience an exogenous change in their PBs because of PB mergers. Under the self-selection hypothesis, these hedge funds should continue to comove with (and only with) the corresponding premerger PB group regardless of the merger. We find that this is not the case: before the merger, these funds exhibit strong comovement with other funds using the same PB, whereas they exhibit insignificant comovement with funds serviced by the merger-partner PB. After the merger, however, their comovement with the merger-partner PB group increases significantly, while their comovement with the premerger PB group does not significantly change. These results argue against the self-selection hypothesis.

We then examine how PBs induce the comovement we observe, by assessing the relative claims of the common information versus PB-level contagion hypotheses. Our main test concerns the relation between PB-level comovement and fund performance. Under the common information hypothesis, PB-level comovement arises because of hedge funds deriving some of their investment ideas from the information passed on from their PB. This information should be ex ante highly profitable in order for hedge funds to (at least partially) deviate from their existing proprietary trading models. On average, therefore, a higher PB-level comovement should be associated with better fund performance, ceteris paribus. In contrast, under the PB-level contagion hypothesis, PB-level comovement is the consequence of hedge funds being hit by a common funding shock, namely, increased margins or margin calls by their PBs. To the extent that such a shock to their funding liquidity has any detrimental impact on their performance, therefore, the PB-level contagion hypothesis suggests a negative relation between PB-level comovement and hedge fund performance. We form portfolios of hedge funds based on their PB-level comovement and examine the subsequent performance of these portfolios. Consistent with the common information hypothesis, we find that PB-level comovement is positively related to hedge fund performance. For example, over a one-year holding period, the highest comovement quintile outperforms the lowest by 2.79% per year, after adjusting for differences in their risks. The return difference between the two portfolios is statistically and economically significant. We also examine the relation between PB-level comovement and fund performance, using multivariate regressions. After controlling for other fund characteristics, we confirm the positive relation between a fund's PB-level comovement and its subsequent performance in the multivariate regression setting.

There are other predictions of the PB-level contagion hypothesis that are not supported by the data. First, under the PB-level contagion hypothesis, PB-level comovement should be greater for downside moves than for upside moves. By allowing different time-series betas for downside versus upside moves when measuring PB-level comovement, however, we find only weak evidence of asymmetry in PB-level comovement. PB-level comovement is evident for both downside and upside moves,[4] and the difference between downside and upside comovement is small and often statistically insignificant. When significant, it is because upside comovement is greater than downside comovement.

Second, PB-level comovement is also related to several PB- and fund-specific characteristics in a way consistent with the common information hypothesis. We find that PB-level comovement is stronger for funds with more established PB ties and relationships, such as older funds and fund families, and for PBs with better economies of scale in information production and provision, such as PBs serving a larger number of hedge fund clients. We also find that PB-level comovement is stronger for funds that face less regulatory oversight, such as offshore funds and funds headquartered outside the United States. To the extent that PB-level comovement is due to information that is not so innocuous, these results are consistent with reputation and litigation concerns curbing passing or trading on such information.

Some support does emerge for the PB-level contagion hypothesis, however, when we focus on significant crisis episodes in our sample period, such as the fall of 1998 (Long-Term Capital Management blowup), the summer of 2007 (the Quant crisis), and the fall of 2008 (the financial meltdown). Using a rolling window to estimate PB-level comovement each month, we observe that the level of comovement tends to increase when the estimation window overlaps with the crisis periods. The on-average positive relation between the comovement and fund performance also becomes much attenuated, and

[4] Note that the null hypothesis here is zero, not normal. Further analysis reveals that downside comovement, although greater than zero, does not exceed that expected from a multivariate normal distribution.

even turns negative for leveraged funds, when the comovement arises over the crisis windows, whereas the positive relation further strengthens when the comovement arises over the noncrisis windows. This evidence illustrates that, although not overwhelming, the contagion effect cannot be completely ruled out. Rather, it paints a more nuanced picture of how PBs induce the comovement we observe: in normal times (and on average), the information channel dominates, but in times of stress, the contagion channel also plays a role. The results suggest that the role played by the contagion channel over the crisis windows is at least big enough to counterbalance the otherwise dominating role played by the information channel.

Our paper relates to two recent strands of literature. First is the emerging literature that documents excessive comovement in hedge fund returns. Boyson, Stahel, and Stulz (2010) and Dudley and Nimalendran (2011) use hedge fund index data and find strong evidence of return comovement *across* hedge fund styles (i.e., market-wide comovement). Using individual hedge fund data, Sialm, Sun, and Zheng (2014) find additional comovement—over and above the market-wide comovement—among hedge funds located in the same metropolitan statistical area (MSA) (i.e., local comovement). Consistent with the notion of contagion, these authors find the corresponding comovement mainly, if not entirely, from a lower quintile of the return distribution and show that its magnitude increases upon or following large adverse shocks to measures of hedge fund funding liquidity. Note that the comovement we observe is similar to the local comovement of Sialm, Sun, and Zheng (2014), in that it is *localized* among certain groups of hedge funds. Under the PB-level contagion hypothesis, we attribute this to PBs, and Sialm, Sun, and Zheng (2014) to local funds of funds (FoFs), spreading liquidity shocks among the corresponding groups of hedge funds. Nevertheless, this contagion channel, although playing a role during discrete times of stress, does not seem to be the main driver of the comovement we observe.

The second related strand of literature concerns hedge fund intermediaries. Focusing on hedge fund auditors, Liang (2003) finds that audited funds have better data quality than do nonaudited funds. Bollen and Pool (2008, 2009) suggest that auditing helps deter (at least temporarily) forms of return manipulation, although Cassar and Gerakos (2011) find that more reputable auditors and administrators are not associated with lower levels of return smoothing. More closely related to our paper are Klaus and Rzepkowski (2009a) and Goldie (2011), who study the role of PBs in hedge fund performance. Klaus and Rzepkowski (2009a) find that an increase in PBs' distress, captured by changes in credit default swap (CDS) spread and in (the negative of) distance-to-default, is associated with a significant decline in hedge fund performance, the result that motivates our PB-level contagion hypothesis. Though their result implies the existence of a PB-specific component in hedge fund returns, they do not examine comovement therein. Moreover, we find that these PB distress variables do not completely explain the PB-level comovement we document. Meanwhile, Goldie (2011) exemplifies the information provision role of PBs, as envisaged in our common information hypothesis, in the context of merger arbitrage hedge funds. Specifically, he finds that hedge funds are more likely to invest in merger deals where their PBs also work as advisors and that hedge funds outperform naive portfolios of merger arbitrage investment *only* when their PBs are advisors in the deals. Our analysis using hedge funds overall and various style subsamples suggests that the information-provision role of PBs is not confined to merger arbitrage funds but is more of a universal phenomenon across hedge fund styles.

1. Data and Descriptive Statistics

Our main source for hedge fund data is the Lipper TASS database, which includes a history of monthly hedge fund returns, as well as a series of fund characteristics. As of July 2012, TASS contains a total of 18,418 live and graveyard funds. Following the literature, we filter out funds that report quarterly (not monthly), funds that report returns denominated in currencies other than U.S. dollars, funds that report returns before (not after) fees, and funds with unknown styles, leaving us with 10,014 unique funds. We also filter out observations before 1994, yielding 10,011 unique funds. To control for backfill bias, we further exclude the first eighteen months of returns for each fund, yielding 8,839 unique funds. We then filter out 2,350 funds because they do not have at least twenty-four return observations. Throughout our empirical analysis, we take care not to attribute any mechanical correlations to PB-level comovement. As a first step, we filter out FoFs, reducing our sample to 4,548 funds: the returns of FoFs and individual hedge funds can be correlated simply because the former invests in the latter. We also ensure that the comovement that we document is not due to funds in the same family. To this end, we drop funds that do not provide a management company in TASS, leading to a sample of 4,498 unique funds. Finally, we follow Aggarwal and Jorion (2010) and correct for master-feeder duplicates, resulting in a sample of 3,837 unique funds.

TASS also contains header information on PBs, as well as other service providers: the live folder contains the current PB; the graveyard folder contains the PB as of the last reporting date.[5] Because TASS does not maintain historical information on PBs, we utilize all eighteen different downloads of the TASS database available to us to match the most accurate PB information with each fund in each month. We have downloads of the database in 2007 (March 5), 2009 (May 6, July 28, and October 2), and 2010 (July 26) and multiple downloads in 2011 and 2012 (until July 27). Starting with the 2007 download, we carry forward the PB information from the most recently available download and update the PB information as each new download becomes available.

[5] For example, for a fund that exited the database in 1998, the PB information is current as of 1998.

In addition, we also use the PB information in the 2007 download (or a later download in which the fund first appears) to match with return observations before the first download date. As a result, for our sample of 3,837 funds from January 1994 to June 2012, we identify 419 unique PBs by their "CompanyID."

However, we notice that TASS sometimes assigns more than one CompanyID to a PB when different funds input (slightly) different names for the same PB (e.g., "Morgan Stanley & Co Inc." and "Morgan Stanley & Co. International").[6] For the purpose of our analysis, we manually clean the data so that each investment bank (including its subsidiaries) is given one ID. In addition, when PBs merge during our sample period, we follow Corwin and Schultz (2005) and Bao and Edmans (2011) and give a separate ID for the acquirer before and after the merger. After the cleaning procedure, the details of which can be found in the Appendix, we have 217 unique PBs by their cleaned ID. Finally, as we discuss below, we include in our sample only PBs that service at least five hedge funds, leading to a final sample of fifty-nine unique PBs. These PBs service about 70% of the hedge funds in the sample (2,635 funds), but we use all 3,837 funds to control for the market-wide and style-level comovement in hedge fund returns.

Using the first download to identify the PB for the period before the first download date may create an error in estimating PB-level comovement, depending on (1) how frequently funds change their PBs, and (2) how far backward we go from the first download date (for a live fund) or the last reporting date (for a graveyard fund). Among the 1,867 sample funds that exist in both our first and last downloads of the database (with PB information), however, we find that only 6.86% of them (i.e., 128 funds) have changed their PBs over the sixty-five-month period.[7] Moreover, on average (median), the PB information in the first download is carried backward only as far as 58 (46) months before the first download date or the last reporting date. In Online Appendix Section A.3, we show that our main results do not greatly change when we drop the PB information matched for the period before the first download date. After all, any imperfection in our matching of the PB information here will only bias against finding PB-level comovement.

Table 1 presents summary statistics for the funds and PBs in our sample at the beginning, middle, and end of the sample period. Panel A provides the total number of funds and PBs in the sample, as well as the distribution of the number of funds serviced by a PB. The total number of funds varies over time: at the beginning of the sample period, there are 378 funds, while at the middle

[6] This data issue is also noted by Aragon and Strahan (2012). We also notice that when a fund inputs more than one name (e.g., "Bear Sterns & Citigroup") in the name field, TASS assigns a new CompanyID for this input even though each PB may already have a CompanyID.

[7] Many of these changes are due to investment bank failures and mergers in 2008. The corresponding number for any sixty-five-month period before our first download date is likely to be smaller. Here, we do not double count acquirers changing their (cleaned) ID before and after mergers.

Table 1
Summary statistics

A

Year	Number of funds	Number of PBs	Number of funds per PB Mean	Median	Max.	Min.
1994	378.00	16.00	15.19	10.50	57.00	5.00
2003	1,740.00	27.00	50.89	19.00	280.00	5.00
2012	1,374.00	20.00	47.30	21.50	184.00	5.00
Ave.	1,490.79	22.53	45.20	19.55	228.95	5.00

B

Year	Number of styles	Number of styles per PB Mean	Median	Max.	Min.
1994	10.00	4.06	4.00	9.00	1.00
2003	11.00	5.89	5.00	10.00	1.00
2012	11.00	6.10	6.00	10.00	1.00
Ave.	10.79	5.69	5.16	10.26	1.63

Panel A provides the total number of funds and PBs in the sample, as well as the distribution of the number of funds per PB for 1994, 2003, and 2012 (until June). Panel B reports the distribution of the total number of styles in each PB for the same years. The last row of each panel reports time-series averages of the corresponding yearly statistics across the entire sample years.

of the sample period, there are 1,740 funds. The number of hedge funds drops to 1,374 in 2012, due mainly to the financial crisis in 2008.

There are forty-nine PBs in 1994. However, more than half of the PBs service one or two sample funds. In fact, the number of PBs that have at least five hedge fund clients averages about twenty-three per year, ranging from a low of sixteen in 1994 to a high of twenty-nine in 2005. The average (median) PB services about 45 (20) hedge funds per year, on average, with a low of 15 (11) in 1994 to a high of 65 (36) in 2008. Since we exclude PBs with fewer than five hedge fund clients from the sample, the smallest PBs, by design, include at least five funds.

Before 2001, Bear Sterns had the largest clientele, with an average of 128 funds per year. From 2001 onward, Morgan Stanley has the largest number of hedge fund clients, with an average of 288 funds per year. The top-ten PBs based on the number of sample funds serviced during the entire sample period are Morgan Stanley, Goldman Sachs, Bear Stearns, UBS, JP Morgan, Bank of America, Deutsche Bank, Citigroup, Credit Suisse, and Merrill Lynch, in descending order.[8]

Panel B of Table 1 presents the distributional characteristics of hedge fund styles per PB. TASS groups hedge funds into eleven style categories: convertible arbitrage, dedicated short bias, emerging markets, equity market neutral, event driven, fixed income arbitrage, global macro, long/short equity hedge, managed futures, multistrategy, and options strategy. As shown in the table, the PBs in the sample are fairly well diversified across styles: the average

8 Names appearing twice with different (cleaned) IDs before and after mergers are only listed once.

(median) PB covers about 5.7 (5.2) styles per year, on average. Again, Bear Sterns has the largest number of styles in the early years of the sample, but from 1998 onward, an increasingly larger number of top PBs cover all 10 or 11 hedge fund styles.[9] Nevertheless, a few PBs in the sample have only one or two styles. Given that funds in the same style tend to comove, it is important to control for the style effect when studying the PB-level comovement in hedge fund returns.

2. PB and the Comovement in Hedge Fund Returns

2.1 The PB-level comovement in hedge fund returns

We begin our analysis by examining the degree of comovement of a fund with other funds serviced by the same PB. For each fund in each month of our sample, we construct a "PB index" by equally weighting the returns of all sample funds that share at least one PB with the fund.[10] We then estimate a series of fund-level time-series regressions with the following general structure:

$$R_{i,t} = \alpha_i + \beta_i^{PB} R_t^{PB} + \beta_i^{STY} R_t^{STY} + \beta_i^{MKT} R_t^{MKT} + \varepsilon_{i,t}, \tag{1}$$

where $R_{i,t}$ is the monthly return of a particular fund, R_t^{PB} is the monthly return of the fund's corresponding PB index, R_t^{STY} is the monthly return of the fund's corresponding style index, and R_t^{MKT} is the monthly return of all hedge funds in the sample, that is, the "market" index. All returns are in excess of monthly Treasury-bill rates. As discussed above, R_t^{STY} is included in the regression to control for style effects, and R_t^{MKT} is included to control for overall market-wide comovement in hedge fund returns. To avoid mechanical correlations, we exclude the return of the corresponding fund when calculating the return on each index. Funds in the same family as the corresponding fund are also excluded from each index. This is to eliminate any confounding effects caused by funds in the same family, which are likely to exhibit a high return correlation (Elton, Gruber, and Green 2007).[11]

To supplement our analysis, we also estimate a series of regressions parallel to Equation (1) by replacing raw returns with the returns filtered against commonly known risk factors. Specifically, we first regress the excess return of each fund on the seven factors of Fung and Hsieh (2004), which include an equity market factor, a size spread factor, a bond market factor, a credit spread factor,

9 The sample has fewer than fifteen options strategy funds.

10 As also pointed out by Pirinsky and Wang (2006), equal weighting allows us to better address the question of how a particular fund comoves with other funds using the same PB, especially for PBs with relatively few funds and PBs for which a small number of large funds dominate the clientele. In Online Appendix Section A.4, we also replicate our tests by using a value-weighted index, and the results, although weaker, are qualitatively similar.

11 Alternatively, we could include the return of the fund's corresponding family index in the regression. By design, however, this approach excludes single-fund families (or funds whose other family members do not report to TASS) from the sample.

and trend-following factors for bonds, currencies, and commodities.[12] Filtered return is measured as the sum of the intercept and the residual. We then construct the PB, style, and market indices in the same way as above, except that we use filtered returns instead of excess returns. Using the filtered returns should further reduce the possibility that we attribute correlations due to commonly known factors in hedge fund returns to PB-level comovement.

We estimate PB betas, β^{PB}, for each fund that allows at least a twenty-four-month estimation period. Cross-sectional averages of the estimated betas and their t-statistics are presented in panel A of Table 2, along with the average adjusted R^2. The first set of columns contains the results obtained using raw returns, and the next set of columns contains the results obtained using filtered returns. For comparison, we also report the results without the PB index below the corresponding results with the PB index.

We observe that PB betas are significantly positive in all specifications considered (see the first, second, fifth, and sixth rows of panel A). PB betas also exhibit strong economic significance: average betas with respect to the PB index are between 0.35 and 0.60 over the various models. The first two rows of panel A also indicate that in the presence of the PB index, the significance of the market index is substantially weaker: average market betas are 0.35 using raw returns and 0.29 using filtered returns, while the corresponding numbers for PB betas are 0.60 and 0.53, respectively.

The fifth and sixth rows of panel A show that the comovement results in the first two rows are not driven by comovement with funds from the same style category. Style betas are, as expected, strongly significant (style comovement appears weaker after filtering). Although the introduction of the style index reduces the magnitude and significance of PB betas, PB betas still remain highly economically and statistically significant: average PB betas are 0.36 using raw returns and 0.35 using filtered returns.

Replacing the market index with an index of funds that do not share a PB with the corresponding fund does not much change the magnitude and significance of the PB index (not reported). In contrast, average betas with respect to this "other PB" index are only 0.02 using raw returns and 0.06 using filtered returns in the presence of the PB and style indices (the corresponding t-statistics are insignificant). This is not surprising, however, considering a high correlation between the other PB index and the market index.

Note that the way we obtain t-statistics so far is based on a variant of the Fama and MacBeth (1973) procedure in which we conduct fund-by-fund time-series regressions first and then average the coefficients across funds. This approach is frequently used in the literature and, in particular, by Coughenour and Saad (2004) and Pirinsky and Wang (2006), in a context similar to ours. Importantly, however, for this approach to be reliable, the residuals across

12 These seven factors have been shown by Fung and Hsieh (2004) to have considerable explanatory power on hedge fund returns.

Table 2
PB-level comovement

	Raw returns				Filtered returns			
	β^{PB}	β^{STY}	β^{MKT}	Adj. R^2	β^{PB}	β^{STY}	β^{MKT}	Adj. R^2
A. Fama–MacBeth by fund								
Estimate	0.60		0.35	28.08	0.53		0.29	11.23
t-stat	22.63		13.84		23.17		13.20	
Estimate			0.91	22.91			0.74	8.07
t-stat			43.38				37.75	
Estimate	0.36	0.59	−0.03	33.44	0.35	0.45	0.03	15.19
t-stat	13.59	20.73	−0.96		14.68	16.21	1.02	
Estimate		0.63	0.28	31.32		0.44	0.36	13.58
t-stat		23.48	9.62			16.86	12.69	
B. Fund fixed effects and SE clustered by month								
Estimate	0.56		0.42	14.22	0.46		0.44	6.50
t-stat	16.79		9.07		14.61		7.51	
Estimate			0.93	11.97			0.81	5.19
t-stat			18.96				11.36	
Estimate	0.39	0.52	0.05	18.41	0.38	0.38	0.13	7.94
t-stat	4.68	3.22	0.42		8.43	4.41	1.81	
Estimate		0.57	0.36	17.34		0.43	0.39	7.07
t-stat		3.34	1.88			4.57	4.66	
C. SE clustered by month and fund								
Estimate	0.55		0.42	13.87	0.45		0.44	5.30
t-stat	12.25		7.45		10.80		7.14	
Estimate			0.93	11.64			0.81	4.02
t-stat			17.48				11.11	
Estimate	0.39	0.52	0.05	18.06	0.37	0.38	0.13	6.74
t-stat	4.46	3.15	0.39		7.35	4.19	1.67	
Estimate		0.57	0.36	16.99		0.43	0.38	5.90
t-stat		3.27	1.84			4.33	4.37	

This table reports the results of a number of different regressions of monthly hedge fund returns on a PB index, the style index, and the market index. The first four columns contain the results using raw excess returns; the next four columns contain the results using filtered returns. We obtain filtered returns by regressing the excess return of each fund on the seven factors of Fung and Hsieh (2004) and then adding the intercept and the residual. With or without the filtering, the PB index is constructed as the equally weighted return of all funds using the fund's corresponding PB; the style index is the equally weighted return of the fund's corresponding style, according to the TASS classification; and the market index is the equal-weighted return of all funds in the sample. The fund itself and its family funds, if any, are excluded from each index. Panel A reports the results from (a variant of) Fama and MacBeth (1973) regressions in which we conduct fund-by-fund regressions and average the coefficients across funds; panel B reports the results from panel regressions with fund fixed effects and standard errors clustered by month; and panel C reports the results from panel regressions with standard errors clustered by both month and fund. The number of fund-month observations used for each regression is 201,685. Adjusted R^2s are in percentages.

regressions need to be independent; otherwise, the resultant t-statistics can be substantially overstated, as shown in Gow, Ormazabal, and Taylor (2010). To check whether our results are driven by cross-sectional correlation in residuals, we conduct panel regressions with fund dummies and standard errors clustered by month in panel B of Table 2 and panel regressions with standard errors clustered by both month and fund in panel C of Table 2 (Petersen 2009).[13] Our

[13] Similar to Sialm, Sun, and Zheng (2014), we also cluster betas by PB, while maintaining the variant of the Fama–MacBeth procedure. Although doing this does not change our results much, it reduces our sample size slightly since in this setting we cannot use funds using multiple PBs. More importantly, this approach is subject

results robustly show up across different methods, suggesting that the results are not driven by cross-sectional or time-series dependence in regression errors.[14] Nevertheless, the much-reduced t-statistics after correcting for cross-sectional dependence in our data prompt us to continue to do so in all our subsequent analyses.[15]

To further ensure that we do not falsely declare a significant effect,[16] we run two distinct "placebo"-type regressions, in which we include alternate indices of sample funds that are expected *not* to comove with the corresponding fund, to see if they indeed exhibit no effect. First, we include an index of sample funds serviced by a PB that is randomly selected every month from among those that do *not* service the corresponding fund. This "random PB" index is similar in spirit to the other PB index above, but serves as a better placebo index in our baseline regression as it does not suffer from multicollinearity with the market index. Second, we make use of information on hedge fund auditor—another important type of hedge fund service provider in the literature (e.g., Liang 2003; Bollen and Pool 2008, 2009; Cassar and Gerakos 2011)—and include an index of sample funds that share an auditor with the corresponding fund. Unlike PBs, auditors have little incentive to pass on private information to hedge fund clients and play no role in funding liquidity provision; hence, the effect is expected to not be observed, under either the information or the contagion channel.

The results summarized in Table 3 confirm that the presence of a significant effect occurs only where one is expected to occur: betas with respect to the random PB and auditor indices (denoted by β^{RPB} and β^{AD}, respectively) are close to zero and insignificant, with or without the PB index in the regression, whereas the magnitude and significance of the PB beta largely remain unchanged from those reported in Table 2. These results reassure us that our results in Table 2 are unlikely "false positive" and provide further support for the existence of PB-level comovement in hedge fund returns.

As an additional robustness test of PB-level comovement (see Online Appendix Section A for other additional tests), we follow Sialm, Sun, and Zheng (2014) and use a subset of sample funds whose management firms are located in the United States. The motivation is to include an index of local

to the same criticism as the Newey–West corrected Fama–MacBeth procedure because it applies clustering to *coefficients*, while the dependence is in the underlying *data* (Gow, Ormazabal, and Taylor 2010).

14 In an unreported work, we also include year dummies in the regression and obtain similar results.

15 Some observations about the adjusted R^2 are worthy of note: First, the adjusted R^2 tends to be higher when using raw returns than using filtered returns, which is due to the effect of common risk factors in raw returns that is removed from filtered returns. Second, the adjusted R^2 tends to be higher in panel A than in panels B and C. This is because fund-by-fund time-series regressions, unlike panel regressions, allow each fund to load differently on a given index (so that the model fit can be maximized at the fund level), and we report the average of these fund-level R^2s in panel A.

16 For example, Van der Laan and Rose (2010) point out: "[F]or large enough sample sizes, every study—including ones in which the null hypothesis of no effect is true—will declare a statistically significant effect."

Table 3
PB-level comovement: Placebo tests

	Raw returns						Filtered returns					
	β^{PB}	β^{RPB}	β^{AD}	β^{STY}	β^{MKT}	Adj. R^2	β^{PB}	β^{RPB}	β^{AD}	β^{STY}	β^{MKT}	Adj. R^2
A. Fund fixed effects and SE clustered by month												
Estimate	0.39	0.01		0.52	0.04	18.41	0.38	0.02		0.38	0.11	7.94
t-stat	4.75	0.82		3.22	0.32		8.61	1.48		4.42	1.54	
Estimate		0.03		0.57	0.33	17.35		0.03		0.43	0.36	7.08
t-stat		1.06		3.34	1.84			1.60		4.58	4.15	
Estimate	0.41		0.03	0.50	0.00	19.23	0.39		0.02	0.36	0.10	7.90
t-stat	4.56		0.95	3.15	0.05		8.39		1.16	4.30	1.69	
Estimate			0.04	0.55	0.33	18.12			0.02	0.41	0.37	6.99
t-stat			0.89	3.25	1.91				1.02	4.43	5.27	
B. SE clustered by month and fund												
Estimate	0.39	0.01		0.52	0.04	18.06	0.37	0.02		0.38	0.11	6.75
t-stat	4.51	0.77		3.15	0.30		7.46	1.39		4.20	1.44	
Estimate		0.02		0.57	0.33	17.00		0.03		0.43	0.36	5.91
t-stat		1.02		3.27	1.79			1.53		4.33	3.92	
Estimate	0.41		0.03	0.50	0.00	18.91	0.39		0.02	0.36	0.10	6.94
t-stat	4.33		0.95	3.08	0.04		7.33		1.12	4.07	1.48	
Estimate			0.04	0.55	0.32	17.82			0.02	0.41	0.37	6.04
t-stat			0.89	3.18	1.85				0.99	4.19	4.82	

This table reports the results of falsification tests on our baseline results reported in Table 2 by employing two alternate placebo indices. The first six columns contain the results using raw excess returns; the next six columns contain the results using filtered returns. Panel A reports the results from panel regressions with fund fixed effects and standard errors clustered by month; panel B reports the results from panel regressions with standard errors clustered by both month and fund. The first four rows of each panel report the results when we include in the regression an index of sample funds serviced by a PB that is randomly selected every month from among those that do not service the corresponding fund. The next four rows report the results when we include an index of sample funds that share an auditor with the corresponding fund. The fund itself and its family funds, if any, are excluded from each index. The number of fund-month observations used for each regression in the first (next) four rows of each panel is 201,685 (178,498). Adjusted R^2s are in percentages.

hedge funds in the regression so that we can evaluate the relative importance of PB-level comovement compared with other effects well documented in the literature (e.g., Coval and Moskowitz, 2001; Hong, Kubik, and Stein 2005). The (unreported) results show that local betas remain mostly significant after controlling for the style and market indices. More importantly, while adding the PB index in the regression does not much reduce the magnitude and significance of the local index (and vice versa), PB betas are about two times larger than local betas using raw returns and more than two and a half times larger using filtered returns. We note, however, that PB-level comovement appears relatively less pronounced in U.S. funds than that in Table 2. In Section 5, we examine in detail the cross-sectional determinants of PB-level comovement in terms of both fund and PB characteristics.

2.2 PB merger and changes of PB-level comovement

So far, we have found that returns of hedge funds serviced by the same PB exhibit a strong degree of comovement. In this subsection, we study the change in PB-level comovement for a subset of hedge funds that involuntarily switch their PBs because of PB mergers. The empirical analysis on this switching sample provides a more rigorous control for fund characteristics potentially correlated with PB-level comovement and hence allows us to address the self-selection hypothesis.[17]

We construct our sample of PB switching funds as follows. First, we identify funds that change their PBs (according to cleaned IDs). Next, we verify each change with a list of major PB mergers (see the Appendix for its construction) and exclude non-merger-related switches. To avoid contamination, we further eliminate changes that are twenty-five months or fewer apart from each other. To obtain a clean measure of changes in the PB affiliation, we restrict the analysis to funds serviced by a single PB. After requiring at least eighteen monthly observations in each of the twenty-four-month estimation windows before and after switch (i.e., $[-25,-2]$ and $[+2,+25]$), our final sample of PB switching funds consists of 260 funds covering six different PB mergers.

We organize our analysis around the following specification:

$$R_{i,t} = \alpha_i + \beta^{PB_1} R_t^{PB_1} + \beta^{PB_2} R_t^{PB_2} + \Gamma' \mathbf{Controls}_t + \varepsilon_{i,t}, \tag{2}$$

where PB_1 denotes the fund's corresponding PB, PB_2 denotes PB_1's merger partner, and $\mathbf{Controls}_t$ denotes a vector containing the style and market indices.[18] Note that funds included in the PB_1 index share the same PB with the corresponding fund both before and after the merger, whereas funds included in the PB_2 index do so only after the merger, but not before. Our key predictions here are that (1) consistent with Table 2, PB_1 betas are significantly positive before the merger, but PB_2 betas are not, and, more importantly, (2) while PB_1 betas do not change much, PB_2 betas significantly increase after the merger. We test these predictions by interacting an indicator variable $Post$, which equals one if the observation is after the merger and zero otherwise, with the PB indices, that is,

$$\begin{aligned} R_{i,t} = {} & \alpha_i + \beta^{PB_1} R_t^{PB_1} + \beta^{PB_1}_{Post}\, Post \cdot R_t^{PB_1} + \beta^{PB_2} R_t^{PB_2} + \beta^{PB_2}_{Post}\, Post \cdot R_t^{PB_2} \\ & + Post + \Gamma' \mathbf{Controls}_t + \varepsilon_{i,t}. \end{aligned} \tag{3}$$

Note that finding a significant change in PB_2 betas will in itself amount to a rejection of the self-selection hypothesis: the self-selection hypothesis posits that funds' PB selection is driven by some unobserved fund characteristics, which at the same time give rise to comovement among funds that share such characteristics. Hence, under this hypothesis, an exogenous change in funds' PB affiliation should not affect PB-level comovement.

We estimate Equation (3) as panel regressions, as before, given the strong evidence of cross-sectional dependence in our data (see, e.g., t-statistics in panel

---

[17] We also consider using a subset of funds that switch their PBs for non-merger-related reasons (e.g., voluntary switch). Such switches, however, are less ideal for addressing the self-selection hypothesis because they could be driven by the change in fund characteristics. This test also suffers from an insufficient number of sample funds (i.e., forty-three after imposing a similar set of requirements as those listed below).

[18] Consistent with Equation (1), the fund itself and its family funds, if any, are not used when calculating the return on each index.

Table 4
PB merger and changes of PB-level comovement

	Raw returns					Filtered returns				
	β^{PB_1}	$\beta^{PB_1}_{Post}$	β^{PB_2}	$\beta^{PB_2}_{Post}$	Adj. R^2	β^{PB_1}	$\beta^{PB_1}_{Post}$	β^{PB_2}	$\beta^{PB_2}_{Post}$	Adj. R^2
A. Fund fixed effects and SE clustered by month										
Estimate	0.40	0.08			21.31	0.43	−0.07			7.18
t-stat	3.30	1.64				5.47	−1.01			
Estimate			−0.04	0.28	20.22			−0.05	0.23	6.34
t-stat			−0.30	3.15				−0.53	2.22	
Estimate	0.46	−0.07	−0.07	0.20	21.44	0.45	−0.17	−0.05	0.20	7.30
t-stat	4.54	−1.12	−0.83	2.65		5.92	−2.01	−0.91	2.46	
B. SE clustered by month and fund										
Estimate	0.40	0.07			21.37	0.43	−0.07			6.86
t-stat	2.90	1.04				4.45	−1.01			
Estimate			−0.03	0.27	20.25			−0.04	0.22	5.97
t-stat			−0.21	2.58				−0.36	2.06	
Estimate	0.46	−0.07	−0.07	0.20	21.49	0.45	−0.16	−0.04	0.18	6.97
t-stat	3.77	−0.91	−0.65	2.16		4.67	−1.79	−0.57	2.32	

We identify a sample of 260 funds that experience an exogenous change in their PBs due to PB mergers. For each fund in the sample, we use observations in twenty-four-month windows before and after the PB merger (i.e., [−25, −2] and [+2, +25]) and estimate a series of regressions with the following general structure:

$$R_{i,t} = \alpha_i + \beta^{PB_1} R_t^{PB_1} + \beta^{PB_1}_{Post}\, Post \cdot R_t^{PB_1} + \beta^{PB_2} R_t^{PB_2} + \beta^{PB_2}_{Post}\, Post \cdot R_t^{PB_2} + Post + \Gamma' \mathbf{Controls}_t + \varepsilon_{i,t},$$

where PB_1 denotes the fund's corresponding PB; PB_2 denotes PB_1's merger partner; $Post$ is an indicator variable that equals one if the observation is after the merger and zero otherwise; and $\mathbf{Controls}_t$ denotes a vector containing the style and market indices. The fund itself and its family funds, if any, are not used when calculating the return on each index. The first five columns present the results using raw excess returns; the next five columns present the results using filtered returns. Panel A reports the results from panel regressions with fund fixed effects and standard errors clustered by month; panel B reports the results from panel regressions with standard errors clustered by both month and fund. The number of fund-month observations used for each regression is 11,821. Adjusted R^2s are in percentages.

A versus panel B or C of Table 2). Panel A of Table 4 reports the results when fund fixed effects are included in the regression and standard errors are clustered by month; panel B reports the results when standard errors are clustered by both month and fund.

We first observe that before the PB merger, the returns of all funds from our sample exhibit very strong sensitivity to the returns of funds serviced by the same PB: PB_1 betas are between 0.40 and 0.46 using raw returns and between 0.43 and 0.45 using filtered returns, after controlling for the style and market indices. This result is consistent with the evidence that we document earlier using the full sample. Strikingly, before the PB merger, the funds exhibit no sensitivity to the returns of funds serviced by the PB's merger partner: PB_2 betas are moderately negative in all specifications considered and never statistically different from zero.

After the PB merger, however, the sensitivity of the funds to the PB_2 index increases economically and statistically significantly: the increase in PB_2 betas ranges from 0.20 to 0.28 using raw returns and from 0.18 to 0.23 using filtered returns; the t-statistics invariably reject, at a 0.05 or more stringent significance level, the null hypothesis of no change in favor of the alternative represented by our second prediction above (i.e., $H_a : \beta_{Post}^{PB_2} > 0$). Meanwhile, the results on the change in PB_1 betas are mostly insignificant but are less consistent across specifications: if anything, the significant decline in PB_1 betas in one specification will only corroborate the (robust) increase in PB_2 betas to rule out the self-selection hypothesis.[19]

In a related analysis, we look at hedge fund holdings around PB mergers to see if the degree of overlap between hedge funds' portfolios also increases after the merger of their PBs. The results from this analysis not only complement our main results here but also provide support for one of our hypotheses for the PB-level comovement in hedge fund returns. We provide further discussion of this analysis in Online Appendix Section B.[20]

3. PB-Level Comovement and Fund Performance

Given the evidence thus far, we now turn to investigate the mechanism by which PBs generate comovement in the returns of their hedge fund clients. To this end, we assess the relative claims of the common information versus PB-level contagion hypotheses, by undertaking three separate sets of analyses in this and the following two sections. First, we begin by studying the relation between PB betas and hedge fund performance, using a portfolio sorting approach in Section 3.1 and a multivariate regression approach in Section 3.2. For the purpose of differentiating information versus contagion, our analyses in this and the next two sections are primarily based on PB betas estimated using filtered returns, given that the beta component attributable to common factors is less likely to be informative in this regard. As will be shown, however, the results do not greatly change when PB betas estimated using raw returns are used instead.

3.1 Portfolio analyses

To gauge the relative performance of funds with different PB betas, for every month, we sort funds into five (quintile) portfolios according to their PB beta measured over the previous twenty-four months.[21] We then take the equal-weighted average return of the funds in each quintile portfolio for the subsequent month. Since it may take a while for some informed positions to fully reap the benefits of private information (Agarwal et al. 2013), we also allow longer holding periods, that is, 3, 6, 12, and 24 months. In any case,

---

19 Note that our evidence here by no means rules out the possible existence of fund characteristics that dictate the fund's PB selection. We also do not claim that fund characteristics play no role in inducing return comovement among funds that share them. Our evidence in this subsection only suggests that these two sets of fund characteristics, if existing, are unlikely to overlap.

20 We thank a referee for suggesting this additional analysis.

21 To avoid look-ahead bias, we run the initial filtering regressions here within each twenty-four-month window. For later purposes, we note that the standard deviation of PB betas measured in this way equals 2.61. After winsorizing the extreme 1%, as we do in our panel regressions below, the standard deviation becomes 2.15.

following Titman and Tiu (2011), we revise the portfolio in each month, so that for the three-month holding period, for example, one-third of the portfolio is revised in each month. The portfolios run from December 1997 to June 2012.

We consider various performance measures for each portfolio and include the Fung and Hsieh (2004) seven-factor adjusted alpha and the corresponding information ratio (defined as a fund's alpha divided by its residual standard deviation), as well as the raw excess return and the Sharpe ratio. In addition, because hedge funds can smooth and manipulate their returns in other ways, we also consider the Goetzmann et al. (2007) manipulation-proof performance measure, given by

$$\mathrm{MPPM}_{\rho}=\frac{1}{(1-\rho)\Delta t}\ln\left(\frac{1}{T}\sum_{t=1}^{T}[(1+r_t)/(1+r_{ft})]^{1-\rho}\right), \tag{4}$$

where T is the length of the time series on which the measure is evaluated, Δt is the frequency of the time series (i.e., 1/12 when annualizing our portfolio returns), r_t is a hedge fund's rate of return for month t, r_{ft} is the risk-free rate at month t, and ρ is a coefficient that indicates the degree to which risk in the fund's returns is penalized. Following the literature, we calculate this measure for $\rho \in \{3,4\}$.

The results, summarized in Table 5, reveal that high-PB-beta funds outperform low-PB-beta funds for all five holding horizons considered. The return spreads between quintiles 5 and 1 range from 2.12% to 2.59% per annum and are statistically significant, at least at the 10% level. After adjusting for the factors from the Fung and Hsieh (2004) model, we find that the spreads marginally increase to 2.52% to 2.79% per annum with t-statistics all greater than 2. The Sharpe ratio, information ratio, and manipulation-proof performance measures are also higher for the portfolio consisting of high-PB-beta hedge funds than for the portfolio consisting of low-PB-beta hedge funds. The statistical significance of the differences between Sharpe ratios, information ratios, and manipulation-proof performance measures is tested, as in Titman and Tiu (2011), based on the distribution of these differences simulated under the null of no difference.[22] The distribution is constructed by a 5,000-times repetition of essentially our sample portfolio analysis, except that we randomly sort funds rather than sorting based on their PB beta. Consistent with the results above, the p-values suggest that the differences between Sharpe ratios, information ratios, and manipulation-proof performance measures of quintiles 5 and 1 are mostly highly statistically significant (except for the Sharpe ratio and information ratio for 1- and 3-month holding horizons).

[22] To test for the difference between Sharpe ratios, one may alternatively use the Jobson and Korkie (1981) test, corrected by Memmel (2003). However, following the advice of Ledoit and Wolf (2008), we choose not to use the Jobson–Korkie test because it is valid only when data are i.i.d. normal, and hedge fund returns are often highly serially correlated and nonnormal.

Table 5
Portfolio performance based on PB betas

	Excess return (% per month)					Alpha (% per month)				
	1m	3m	6m	12m	24m	1m	3m	6m	12m	24m
Q1 (low)	0.42	0.41	0.40	0.38	0.38	0.29	0.28	0.27	0.26	0.26
Q2	0.31	0.33	0.33	0.34	0.32	0.24	0.26	0.26	0.26	0.24
Q3	0.37	0.37	0.36	0.36	0.36	0.30	0.29	0.28	0.28	0.28
Q4	0.41	0.42	0.42	0.41	0.42	0.32	0.33	0.33	0.31	0.32
Q5 (high)	0.61	0.58	0.59	0.60	0.59	0.52	0.49	0.48	0.49	0.48
Q5-Q1	0.20	0.18	0.18	0.22	0.21	0.23	0.21	0.21	0.23	0.23
t-stat	2.02	1.85	2.01	2.85	2.96	2.26	2.01	2.18	2.83	2.79
	Sharpe ratio					Information ratio				
	1m	3m	6m	12m	24m	1m	3m	6m	12m	24m
Q1 (low)	0.18	0.17	0.17	0.16	0.16	0.24	0.23	0.23	0.22	0.21
Q2	0.20	0.22	0.22	0.22	0.20	0.25	0.30	0.31	0.30	0.27
Q3	0.26	0.26	0.25	0.25	0.25	0.35	0.35	0.33	0.34	0.32
Q4	0.24	0.24	0.24	0.23	0.24	0.30	0.31	0.31	0.30	0.31
Q5 (high)	0.22	0.21	0.21	0.22	0.22	0.30	0.28	0.29	0.31	0.30
Q5-Q1	0.04	0.04	0.04	0.06	0.06	0.06	0.05	0.06	0.09	0.09
p-value	0.13	0.04	0.00	0.00	0.00	0.21	0.09	0.01	0.00	0.00
	$MPPM_3$ (% per month)					$MPPM_4$ (% per month)				
	1m	3m	6m	12m	24m	1m	3m	6m	12m	24m
Q1 (low)	0.12	0.11	0.11	0.09	0.08	0.09	0.08	0.08	0.06	0.05
Q2	0.06	0.08	0.09	0.09	0.07	0.05	0.07	0.08	0.08	0.06
Q3	0.13	0.12	0.11	0.12	0.12	0.12	0.11	0.10	0.11	0.11
Q4	0.15	0.16	0.16	0.15	0.16	0.14	0.15	0.15	0.13	0.14
Q5 (high)	0.28	0.25	0.26	0.28	0.27	0.24	0.21	0.22	0.24	0.24
Q5-Q1	0.16	0.14	0.15	0.19	0.19	0.15	0.13	0.14	0.18	0.18
p-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

We sort funds into quintiles based on their PB betas measured over the previous twenty-four months. PB betas are estimated via Equation (1) using filtered returns for funds that allow at least an eighteen-month estimation period within each twenty-four-month window. Portfolios are rebalanced every month and held for 1, 3, 6, 12, or 24 months. For the three-month holding period, for example, one-third of the portfolio is revised in each month. The top-left (middle-left) panel reports the monthly excess returns (Sharpe ratio) of these portfolios; the top-right (middle-right) panel reports the Fung and Hsieh (2004) seven-factor adjusted monthly alphas (the corresponding information ratios); and the bottom-left (bottom-right) panel reports the manipulation-proof measures with $\rho=3$ (4). The t-statistics are derived from Newey–West standard errors with three lags. The p-values are derived from 5,000 bootstrap simulations under the null of no difference between the corresponding performance measures for the low- and high-PB-beta portfolios.

3.2 Multivariate regression analyses

In this subsection, we further extend our analysis using multivariate regressions. Unlike the portfolio approach, this approach allows us to simultaneously control for fund characteristics known to affect fund performance. Similar to the empirical design of Titman and Tiu (2011) and Sun, Wang, and Zheng (2012), we estimate the following regression:

$$\text{Performance}_{i,t+1:t+12} = b_0 + b_1\,\beta^{PB}_{i,t-23:t} + \mathbf{b}_2'\,\mathbf{Controls}_{i,t} + \varepsilon_{i,t}, \tag{5}$$

where $\text{Performance}_{i,t+1:t+12}$ is the average monthly excess return, the Fung and Hsieh (2004) seven-factor adjusted monthly alpha, the Sharpe ratio, the information ratio, or the two manipulation-proof performance measures of fund i estimated on the year after month t, and $\beta^{PB}_{i,t-23:t}$ is the PB beta of fund i

calculated using the past two years of the fund's history, as in the previous subsection.

Controls$_{i,t}$ contains the following variables: the standard deviation of monthly excess returns of fund i calculated using the past two years of history ($\text{Vol}_{i,t-23:t}$), redemption notice period, measured in units of 30 days ($\text{RedemptionNotice}_i$), lockup period ($\text{Lockup}_i$), management fee ($\text{MgmtFee}_i$), incentive fee ($\text{IncentiveFee}_i$), the log of the fund's age at month t ($\log(\text{Age}_{i,t})$), the log of assets under management (AUM) at month t ($\log(\text{AUM}_{i,t})$), monthly money flows, as a percentage of AUM, averaged over the past two years ($\text{Flow}_{i,t-23:t}$), monthly excess return averaged over the past two years ($R_{i,t-23:t}$), the log of one plus minimum investment ($\log(1+\text{MinInvestment}_i)$), indicator variables for whether personal capital is committed (PersonalCapital_i), whether there is a high water mark provision (HighWaterMark_i), whether the fund uses leverage (Leveraged_i), and whether the fund is offshore (Offshore_i), and, finally, style dummies. This list includes almost all of the variables used by prior studies to control for individual fund idiosyncrasies when examining hedge fund performance.

As above, we are mindful of the following considerations when drawing statistical inferences from the panel data: First, given that the dependent variable in Equation (5) is, by design, correlated over time, we must correct for the fund effect. The Fama and MacBeth (1973) procedure cannot adequately address this form of dependence (with or without an adjustment).[23] Second, since hedge fund performance may be correlated across funds at a given point in time, we also need to correct for the time effect. To accomplish these goals, we follow the advice of Petersen (2009) and adopt the following two approaches: First, we address the time effect parametrically by including time dummies, while clustering standard errors by fund. Alternatively, we cluster standard errors by both fund and time. Since our regressions use data of fund-month observations, the number of clusters in each dimension should be sufficient.

Table 6 reports the results when month fixed effects are included in the regressions, while standard errors are clustered by fund; panel A of Online Appendix Table OA.4 reports the results when standard errors are clustered by both month and fund. Consistent with the common information hypothesis, we find a significant positive relationship between PB betas and fund performance, even after controlling for other fund characteristics. The results in Table 6, for example, imply that, ceteris paribus, a one-standard-deviation increase in the PB beta is associated with an increase of 0.68% in the annualized excess return in the subsequent year, an increase of 0.89% in the annualized alpha, a 0.01 increase in the Sharpe ratio, a 0.04 increase in the information ratio, and an increase of 0.65% per year in the manipulation-proof performance measures MPPM_3 and MPPM_4. These

[23] For example, Gow, Ormazabal, and Taylor (2010) find no evidence that the Newey–West adjusted Fama–MacBeth method corrects for time-series dependence.

Table 6
Panel regressions of hedge fund performance on PB betas

	Dependent variable: Performance$_{t+1:t+12}$					
	Ex. ret. (% p.m.)	Alpha (% p.m.)	SR	IR	MPPM$_3$ (% p.m.)	MPPM$_4$ (% p.m.)
$\beta^{PB}_{t-23:t}$	0.03	0.03	0.01	0.02	0.03	0.03
	(3.02)	(3.57)	(3.00)	(3.37)	(2.54)	(2.36)
Vol$_{t-23:t}$ (% p.m.)	0.07	0.03	−0.02	−0.05	−0.07	−0.11
	(6.33)	(2.94)	(−6.84)	(−7.05)	(−5.23)	(−7.69)
RedemptionNotice	0.06	0.05	0.06	0.13	0.06	0.06
	(2.89)	(2.11)	(3.39)	(3.00)	(2.60)	(2.56)
Lockup	0.00	0.01	0.00	0.01	0.00	0.00
	(1.88)	(2.70)	(1.88)	(1.74)	(0.56)	(0.26)
MgmtFee (%)	0.08	0.03	0.01	0.01	0.04	0.03
	(2.29)	(0.78)	(0.68)	(0.48)	(0.87)	(0.66)
IncentiveFee (%)	0.00	0.01	0.00	0.01	0.00	0.00
	(0.06)	(1.27)	(0.68)	(1.39)	(−0.71)	(−0.81)
log(Age$_t$)	−0.02	−0.05	0.01	0.02	−0.03	−0.04
	(−0.58)	(−1.43)	(0.80)	(0.45)	(−0.87)	(−0.92)
log(AUM$_t$)	−0.02	−0.02	0.00	0.00	−0.01	−0.01
	(−2.14)	(−1.51)	(0.25)	(0.08)	(−0.81)	(−0.48)
Flow$_{t-23:t}$ (%)	0.00	0.00	0.00	0.00	0.00	0.00
	(0.67)	(0.40)	(0.03)	(−0.05)	(0.24)	(0.13)
$R_{t-23:t}$ (% p.m.)	−0.15	−0.02	−0.01	0.04	−0.13	−0.13
	(−6.64)	(−0.75)	(−1.81)	(3.17)	(−4.98)	(−4.55)
log(1+MinInvestment)	0.03	0.03	0.02	0.04	0.02	0.02
	(1.94)	(1.43)	(2.94)	(2.74)	(1.72)	(1.64)
PersonalCapital	0.03	0.00	−0.02	−0.07	0.04	0.03
	(1.05)	(0.04)	(−1.15)	(−1.65)	(1.01)	(0.91)
HighWaterMark	0.12	0.04	−0.01	−0.05	0.15	0.16
	(3.20)	(0.98)	(−0.24)	(−0.95)	(3.65)	(3.60)
Leveraged	0.05	0.05	0.04	0.08	0.03	0.03
	(1.62)	(1.26)	(2.60)	(2.20)	(0.89)	(0.74)
Offshore	−0.05	−0.01	0.00	0.00	−0.04	−0.04
	(−1.38)	(−0.37)	(−0.03)	(0.09)	(−1.03)	(−0.94)
Adjusted R^2 (%)	19.74	4.25	22.96	9.08	20.47	21.50
Observations	97,249	97,249	97,249	97,249	97,249	97,249

This table reports the panel regression results for hedge fund performance on PB beta. Performance measures considered include average excess return (Ex. ret.), Fung and Hsieh (2004) alpha, Sharpe ratio (SR), information ratio (IR), and the two manipulation-proof performance measures (MPPM$_3$ and MPPM$_4$), estimated over the twelve-month period after PB betas are calculated. PB betas are calculated as in Table 5. The table reports the results when month fixed effects are included in the regressions, while standard errors are clustered by fund; panel A of Online Appendix Table OA.4 reports the results when standard errors are clustered by both month and fund. In any case, the regressions include style dummies, along with other control variables specified in the table. The extreme 1% of all variables are winsorized. The t-statistics are reported in parentheses.

relationships are statistically and economically significant. As shown in panel A of Online Appendix Table OA.4, the results are robust to how we correct for time-series and cross-sectional dependence in the data.[24]

4. Is There Asymmetry in PB-Level Comovement?

Perhaps the most quintessential feature of hedge fund contagion, whether it is market-wide or local, may be asymmetric correlation among hedge funds

[24] See panel B of Online Appendix Table OA.4 for the results when we include month fixed effects in the regressions in panel A.

(see Boyson, Stahel, and Stulz 2010; Dudley and Nimalendran 2011; Sialm, Sun, and Zheng 2014). In our context, this means much greater PB betas for downside moves, especially for extreme downside moves, than for upside moves. As we argue above, to the extent that the PB-level contagion hypothesis is true, we should expect such asymmetry in PB betas. In this section, we probe this prediction of the PB-level contagion hypothesis, by examining PB betas conditional on the downside and upside.

To operationalize our computation of downside and upside PB betas using individual fund data and in the presence of other controls, we estimate the following regression:

$$R_{i,t}=\alpha_i+\beta^{PB_d}R_t^{PB}\cdot I\left(R_t^{PB}<x_d\right)+\beta^{PB_i}R_t^{PB}\cdot I\left(x_d\leq R_t^{PB}<x_u\right)$$
$$+\beta^{PB_u}R_t^{PB}\cdot I\left(R_t^{PB}\geq x_u\right)+\Gamma'\mathbf{Controls}_t+\varepsilon_{i,t}, \qquad (6)$$

where $I(\cdot)$ is an indicator variable, $x_d\leq x_u$, and $\mathbf{Controls}_t$ denotes a vector containing the style and market indices. The downside (upside) PB beta, β^{PB_d} (β^{PB_u}), measures fund i's sensitivity to other funds sharing a same PB, when the latter experiences downturns (upturns) in performance. Since $I\left(R_t^{PB}<x_d\right)+I\left(x_d\leq R_t^{PB}<x_u\right)+I\left(R_t^{PB}\geq x_u\right)=1$, Equation (1) is a special case of Equation (6), where fund i's downside and upside PB betas are identical. A similar specification has also been used, for example, by Lo (2001) for the case in which $x_d=x_u=0$. In our analysis, x_d and x_u for each fund are set to the 50th and 50th, 25th and 75th, or 10th and 90th percentiles for the corresponding PB index.

Table 7 presents estimation results for Equation (6), with appropriate adjustments for cross-sectional and time-series dependence in the data. The results indicate that the PB-level comovement we document is not confined to a particular direction but is evident both on the downside and on the upside. The differences between downside and upside PB betas are small and mostly statistically insignificant. While there is some weak evidence for asymmetry when we use filtered returns, the indicated direction is such that upside comovement is stronger than downside comovement, which is opposite to what we would expect from the PB-level contagion hypothesis.

One may contend that higher than normal downside comovement is suggestive of the contagion effect.[25] To check if downside comovement, although no stronger than upside comovement, is nevertheless higher than normal, we test the null hypothesis of $\beta^{PB_d}-\beta^{PB_i}=0$ (and of $\beta^{PB_u}-\beta^{PB_i}=0$, for completeness). The idea is to use the intermediate PB beta, β^{PB_i}, to capture the PB beta in normal times, with which to compare the downside PB beta. Or, more formally, we use the intermediate PB beta (as well as the upside PB beta) as a benchmark when testing if the downside PB beta is in excess of that expected

[25] We thank a referee for suggesting this point.

Table 7
Downside and upside PB betas

	Raw returns							Filtered returns						
	β^{PB_d}	β^{PB_i}	β^{PB_u}	$\beta^{PB_d} - \beta^{PB_u}$	$\beta^{PB_d} - \beta^{PB_i}$	$\beta^{PB_u} - \beta^{PB_i}$	Adj. R^2	β^{PB_d}	β^{PB_i}	β^{PB_u}	$\beta^{PB_d} - \beta^{PB_u}$	$\beta^{PB_d} - \beta^{PB_i}$	$\beta^{PB_u} - \beta^{PB_i}$	Adj. R^2
A. Fund fixed effects and SE clustered by month														
(50, 50)	0.42		0.37	0.04			18.41	0.33		0.40	−0.07			7.95
t-stat	3.79		5.83	0.80				5.33		9.08	−1.46			
(25, 75)	0.42	0.40	0.37	0.05	0.02	−0.03	18.41	0.32	0.44	0.40	−0.08	−0.12	−0.04	7.95
t-stat	3.87	4.80	5.69	0.92	0.37	−0.77		5.29	6.87	9.13	−1.64	−2.09	−0.90	
(10, 90)	0.42	0.39	0.37	0.05	0.03	−0.02	18.41	0.30	0.42	0.39	−0.09	−0.12	−0.03	7.96
t-stat	3.86	5.02	5.53	0.95	0.69	−1.00		4.69	9.56	8.13	−1.75	−2.42	−0.85	
B. SE clustered by month and fund														
(50, 50)	0.41		0.38	0.03			18.06	0.32		0.39	−0.07			6.75
t-stat	3.70		5.26	0.62				5.29		7.77	−1.65			
(25, 75)	0.42	0.39	0.37	0.04	0.03	−0.02	18.06	0.33	0.40	0.39	−0.07	−0.07	0.00	6.75
t-stat	3.78	4.49	5.19	0.84	0.46	−0.51		5.37	5.57	7.79	−1.56	−1.35	−0.09	
(10, 90)	0.42	0.39	0.37	0.05	0.02	−0.02	18.06	0.30	0.40	0.38	−0.08	−0.10	−0.02	6.76
t-stat	3.75	4.72	5.10	0.89	0.62	−0.93		4.43	8.02	7.21	−1.72	−2.09	−0.55	

This table reports the results of a number of different regressions with the following general structure:

$$R_{i,t} = \alpha_i + \beta^{PB_d} R_t^{PB} \cdot I(R_t^{PB} < x_d) + \beta^{PB_i} R_t^{PB} \cdot I(x_d \leq R_t^{PB} < x_u) + \beta^{PB_u} R_t^{PB} \cdot I(R_t^{PB} \geq x_u) + \Gamma' \mathbf{Controls}_t + \varepsilon_{i,t},$$

where R_t^{PB} denotes the fund's corresponding PB index, $I(\cdot)$ is an indicator variable, and $\mathbf{Controls}_t$ denotes a vector containing the style and market indices. The first seven columns contain the results using raw excess returns; the next seven columns contain the results using filtered returns. Panel A reports the results from panel regressions with fund fixed effects and standard errors clustered by month; panel B reports the results from panel regressions with standard errors clustered by both month and fund. The first two rows of each panel contain the results where x_d and x_u are set to be the 50th percentile of the corresponding PB index; the next two rows contain the results where x_d and x_u are set to be the 25th and 75th percentiles of the corresponding PB index, respectively; the bottom two rows contain the results where x_d and x_u are set to be the 10th and 90th percentiles of the corresponding PB index, respectively. The number of fund-month observations used for each regression is 201,685. Adjusted R^2s are in percentages.

from a multivariate normal distribution. Since we have $\beta^{PB_d} = \beta^{PB_i} = \beta^{PB_u}$ under multivariate normality, the downside PB beta, although no greater than the upside PB beta, can still indicate a deviation from normality in the form of excess downside comovement, if the downside PB beta is indeed greater than the intermediate PB beta.[26] The results also reported in Table 7, however, reveal little evidence that the downside PB beta is any greater than the intermediate PB beta. If anything, the downside PB beta is often significantly smaller than the intermediate PB beta, when we use filtered returns.

For comparison, we also similarly split the style and market indices. Interestingly, the (unreported) results indicate strong asymmetry in style beta with greater downside comovement than upside comovement. Although insignificant, we also find the same direction of asymmetry in market betas using filtered returns. These results are consistent with Klaus and Rzepkowski (2009b)

[26] An alternative approach is to compare the estimated downside beta with the downside beta implied from a multivariate normal distribution—based on the parameters calibrated to the data. This approach, however, is not as tractable in our case, as we are using individual fund data, as in the previous studies that use a bivariate series of hedge fund style indices (e.g., Boyson, Stahel, and Stulz 2010; Dudley and Nimalendran 2011).

and Boyson, Stahel, and Stulz (2010), who document contagion effects within and across styles, respectively. Nevertheless, allowing asymmetry in the other betas does not help find support for the PB-level contagion hypothesis. If anything, the upside and intermediate PB betas now become much greater than the downside beta, whether we use raw returns or filtered returns.

5. Determinants of PB-Level Comovement

To further assess the relative claims of the common information versus PB-level contagion hypotheses, we examine the degree of PB-level comovement across a variety of fund and PB characteristics. For our purpose, we focus on a set of fund and PB characteristics that are expected to be correlated with PB-level comovement in a certain direction, either under the common information hypothesis or under the PB-level contagion hypothesis, but not both. For example, if a PB shares privileged information to reward hedge fund clients for past business or in exchange for future fees, we would expect stronger PB-level comovement for funds with more established relationships with the PB (such as older funds), funds that generate higher prime brokerage fees (such as funds that use leverage and short selling), and funds that are likely to survive longer to continue generating fees for the PB (such as better performing funds). Stronger comovement for funds with these characteristics could not be easily explained under the PB-level contagion hypothesis (except for leverage; see below). If anything, the PB-level contagion hypothesis would rather predict weaker comovement for such funds, as they may be affected to a lesser degree by the financial distress of the PB, if the PB, upon a negative shock, cuts credit lines to its hedge fund clients in reverse order of their importance in terms of the revenues they generate for the PB.[27]

In addition, if such sharing of information occurs, at least in part, in a way that violates the law, we would expect weaker PB-level comovement for funds that face tighter regulatory oversight (such as onshore funds), to the extent that the regulation has any teeth to restrain passing or trading on illegal information. Finally, if there are economies of scale in information production and provision, we would expect stronger PB-level comovement for funds serviced by larger PBs (such as PBs that have a larger number of hedge fund clients).[28] Again, we do not have a plausible contagion-based story for these cross-sectional patterns in PB-level comovement. If anything, the PB-level contagion hypothesis would

[27] In a similar vein, we would expect, under the information hypothesis, stronger PB-level comovement for the PB's in-house funds (i.e., funds operated by their PBs), whereas the opposite is expected under the contagion hypothesis. Unfortunately, we identify only a few in-house funds in our sample, perhaps because large investment banks with internal hedge fund business might have other channels to market their funds than reporting to TASS.

[28] A larger number of hedge fund clients not only facilitates information production by enabling economies of scale in conducting research but also may facilitate information acquisition (1) via prime brokerage activities (see footnote 1) by granting access to a greater set of information sources, as well as (2) via investment banking and lending activities as larger PBs are more likely to have affiliated investment banking and lending arms.

Table 8
Determinants of PB betas

	Subsample sorted by									
	Age_{t-1}	Family age_{t-1}	Leveraged	Avg leverage	Max leverage	$R_{t-24:t-1}$	$\alpha_{t-24:t-1}$	Offshore	NonUS	PB size_{t-1}
A. Fund fixed effects and SE clustered by month										
High	0.44	0.43	0.40	0.42	0.45	0.53	0.54	0.41	0.50	0.77
t-stat	8.28	8.12	8.52	8.83	8.95	8.52	7.85	8.23	7.81	16.19
Low	0.32	0.32	0.30	0.34	0.30	0.31	0.35	0.34	0.32	0.29
t-stat	7.19	7.15	6.44	7.16	6.76	7.36	8.64	7.70	8.13	7.00
High-Low	0.12	0.11	0.11	0.08	0.15	0.22	0.18	0.07	0.18	0.48
t-stat	3.52	3.00	3.59	2.43	4.61	5.52	3.74	2.21	4.21	13.52
B. SE clustered by month and fund										
High	0.43	0.42	0.39	0.41	0.44	0.52	0.53	0.40	0.49	0.76
t-stat	6.92	6.82	7.10	6.15	6.78	7.46	6.90	6.59	6.15	11.49
Low	0.32	0.32	0.30	0.33	0.29	0.31	0.35	0.33	0.32	0.29
t-stat	6.22	6.03	5.11	6.12	5.59	6.15	7.16	6.21	6.74	5.90
High-Low	0.11	0.10	0.10	0.08	0.15	0.21	0.17	0.07	0.17	0.48
t-stat	2.06	1.77	1.73	1.29	2.52	3.75	2.59	1.26	2.50	7.58

This table reports the PB betas estimated via Equation (1) using filtered returns for the various subsamples listed in the second row. For each month, funds are classified into high and low categories based on the median of the corresponding variable measured at the end of the previous month. We also report the difference between the PB betas for the high and low groups and the corresponding t-statistics. Age_{t-1} denotes fund age; FamilyAge_{t-1} denotes family age (defined as the average of the age of each fund belonging to the fund family); Leveraged denotes an indicator variable for whether the fund uses leverage; AvgLeverage denotes average leverage; MaxLeverage denotes maximum leverage; $R_{t-24:t-1}$ denotes past two-year average return; $\alpha_{t-24:t-1}$ denotes past two-year Fung and Hsieh (2004) alpha; Offshore denotes an indicator variable for whether the fund is offshore; NonUS denotes an indicator variable for whether the fund is headquartered outside the United States; and PB Size_{t-1} denotes the number of hedge fund clients serviced by the fund's PBs. Panel A reports the results from panel regressions with fund fixed effects and standard errors clustered by month; panel B reports the results from panel regressions with standard errors clustered by both month and fund.

predict weaker PB-level comovement for larger PBs, if larger PBs provide more stable credit lines to their hedge fund clients.

To test these predictions, we first create subsamples based on the aforementioned characteristic variables. For each month, we divide the funds in Table 2 into *high* and *low* categories based on the median of the variables measured at the end of the previous month. For example, if a fund's age is greater than (less than or equal to) the median age, we classify it as a *high* (*low*) age fund. We then re-estimate Equation (1) for these subsamples of funds and report the results in Table 8. For brevity, we report only the PB betas. We also report the difference between the PB betas for the high and low groups and the corresponding t-statistics.[29] As before, our discussion is based on the results obtained using filtered returns, as the inferences using raw returns are similar.

The results, summarized in Table 8, are largely consistent with our predictions above under the common information hypothesis. Specifically, the PB beta

29 An advantage of this approach is that we can see how widespread the phenomenon is across different cross-sections of the data—in addition to whether its cross-sectional variation occurs in a hypothesized manner (as in, e.g., Massa and Rehman 2008; Agarwal, Daniel, and Naik 2011). Nevertheless, the (unreported) results show that the inferences about the latter (i.e., the cross-sectional variation) are similar when we simply run a multivariate regression of a fund's PB beta on the aforementioned characteristic variables, together with style dummies.

is greater for older funds, funds that use leverage,[30] funds with higher past two-year average return, offshore funds, and funds serviced by PBs with a larger number of hedge fund clients. Although PB-level comovement is by no means restricted to these groups of funds, the difference in PB betas between these (high) and other (low) groups is all economically large and statistically significant. The results are similar when we use alternative measures for some characteristics, that is, when we use family age (defined as the average of the age of each fund belonging to the fund family) instead of fund age, the average leverage (AvgLeverage_i) or the maximum leverage (MaxLeverage_i) instead of leverage indicator,[31] past two-year Fung and Hsieh (2004) alpha instead of past two-year average return, and an indicator variable for whether the fund is headquartered outside the United States (NonUS_i) instead of offshore indicator.[32] As shown in panel B of Table 8, the results are also largely unchanged when we correct for time-series and cross-sectional dependence in the data in a different way.

It is worth noting that stronger comovement for funds that use leverage, in itself, is consistent also with the PB-level contagion hypothesis. This is because, under the PB-level contagion hypothesis, the damaging effect of increased margins would be greater (hence greater comovement) for funds that employ higher leverage, especially when their holdings are illiquid, difficult-to-trade assets. In this light, we further test the difference between the PB betas for the high- and low-leverage groups, conditioning on the fund's asset illiquidity, proxied by lockup period, redemption notice period, and serial correlation in fund returns. Contrary to the contagion-based prediction that the difference is more pronounced for the high asset illiquidity group than for the low asset illiquidity group, our (unreported) 2×2 double sorting results find no such difference in differences.[33]

30 Unlike leverage, TASS does not provide information on whether, or the extent to which, funds use short selling.

31 Brown et al. (2008) also use these additional leverage variables.

32 Offshore versus onshore have more to do with jurisdiction than location. This analysis is motivated by Griffin, Hirschey, and Kelly (2011), who argue that the United States has one of the lowest levels of preannouncement leakage and that insider trading is much more prevalent in emerging and some small, developed market countries.

33 There are other characteristics that allow both the information and contagion hypotheses to yield the same prediction (and hence are not discussed in the main text). For example, funds using multiple PBs (versus a single PB) are expected to exhibit weaker PB-level comovement under the information hypothesis because they may not be as valuable of customers to each PB as they would be with one PB (Goldie 2011), and because it may be harder for each PB to learn about the fund's entire portfolio and hence investment ideas (Teo 2011). Weaker comovement for funds using multiple PBs can also be expected under the contagion hypothesis because relying on multiple PBs may reduce the impact of funding shock from a PB (Klaus and Rzepkowski 2009a). Although the sample size prevents a meaningful test of this prediction, we do find weaker PB-level comovement (in magnitude) for funds using multiple PBs.

6. PB-Level Comovement and Its Performance Implication over Crisis versus Noncrisis Periods

One concern is that our use of a fund's entire time series in the previous analyses might mask the effect of the contagion channel, if the contagion channel becomes active only in some subperiods. In this section, we check this possibility by focusing on crisis periods, where the contagion channel is expected to be most active, so as to maximize the power to get at the contagion effect.[34] Our sample period includes three significant crisis episodes: the fall of 1998 (Long-Term Capital Management crisis), the summer of 2007 (Quant crisis), and the fall of 2008 (financial crisis). Following Sadka (2010), we define September–November 1998, August–October 2007, and September–November 2008 as the crisis months.[35]

First, we check whether the degree of PB-level comovement varies over time in a way suggestive of the contagion effect. In Figure 1, we use a twenty-four-month rolling window to estimate a fund's PB beta each month and plot the cross-sectional average of the PB betas over the sample period (i.e., $\beta^{PB}_{t-23:t}$ against t). The thicker line indicates when the corresponding twenty-four-month estimation window overlaps with the crisis months (we call such a window a crisis window). By inspection, we see that the level of comovement tends to increase when the comovement is estimated over the crisis windows, that is, when the contagion channel is most likely playing a role in generating the comovement. Note that this plot captures the *aggregate* effect of the information and contagion channels over time; hence, any increase in PB-level comovement over the crisis windows can be attributed to the contagion effect, to the extent that the information effect is stable over time.

Next, turning to the *relative* strength of the information versus contagion channels over the crisis windows, we check whether the relation between PB-level comovement and fund performance reverses for the comovement that arises over the crisis windows. In Table 9, we repeat the regressions in Table 6, by allowing for a different coefficient on PB beta for the crisis and noncrisis windows via

$$\text{Performance}_{i,t+1:t+12} = b_0 + b_{1c}\,\beta^{PB}_{i,t-23:t}\cdot \text{Crisis}_{t-23:t} + b_{1n}\,\beta^{PB}_{i,t-23:t}\cdot \text{NonCrisis}_{t-23:t} + \mathbf{b}_2'\,\mathbf{Controls}_{i,t} + \varepsilon_{i,t}, \tag{7}$$

where $\text{Crisis}_{t-23:t}$ equals one if the corresponding twenty-four-month window overlaps with the crisis months and zero otherwise; $\text{NonCrisis}_{t-23:t}$ equals one if the corresponding twenty-four-month window does not overlap with

[34] In unreported work, we focus on times of stress suggested by the worst (i.e., decile) realizations of the PB distress variables considered by Klaus and Rzepkowski (2009a) to get at the contagion effect, but without much success.

[35] We thank the editor and a referee for suggesting the analyses in this section.

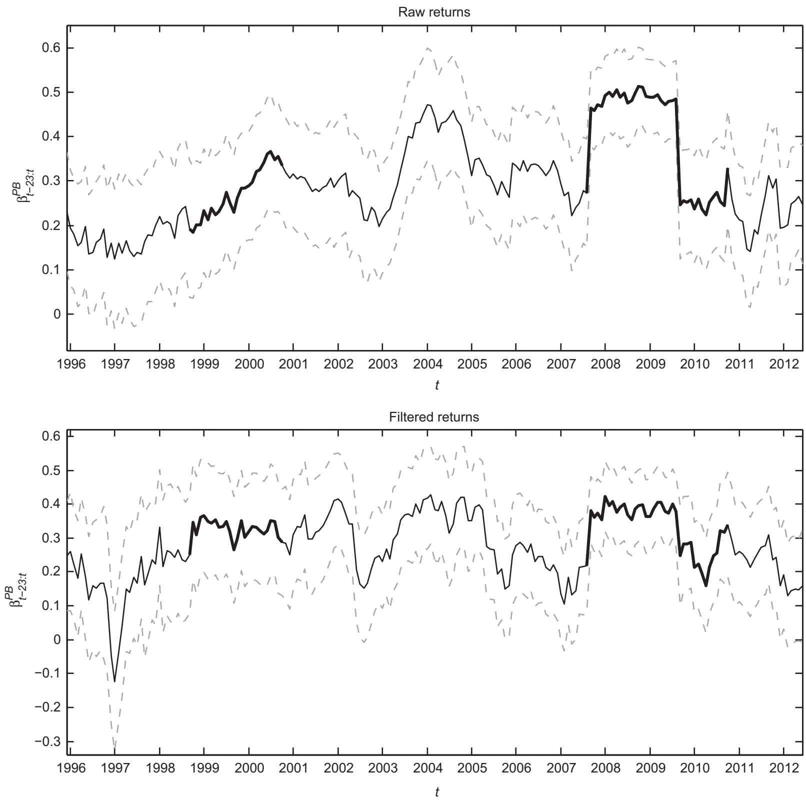

Figure 1
PB-level comovement over time
This figure plots the cross-sectional average (solid line) and corresponding 95% confidence interval (dotted lines) of the PB betas estimated every month t using a twenty-four-month estimation window (from month $t-23$ to month t) for funds that allow at least an eighteen-month estimation period. The thicker line indicates when the corresponding twenty-four-month estimation window overlaps with the following crisis periods: September–November 1998 (Long-Term Capital Management crisis), August–October 2007 (Quant crisis), and September–November 2008 (financial crisis). The top panel estimates the PB betas using raw excess returns and the bottom panel using filtered returns. The PB betas used here correspond to those used in Tables 5, 6, 9, and 10.

the crisis months and zero otherwise.[36] The results reveal that the on-average positive relation between the comovement and fund performance becomes much attenuated (although not quite reversed) for the comovement that arises over the crisis windows, consistent with the contagion channel playing a (relatively) greater role than on average in inducing the comovement. In contrast, when it arises over the noncrisis windows, the comovement exhibits

[36] As in Table 6, Tables 9 and 10 (see below) report the results when month fixed effects are included in the regressions, while standard errors are clustered by fund. The corresponding results when standard errors are clustered by both month and fund are reported in panel A of Online Appendix Tables OA.5 and OA.6. See panel B of Online Appendix Tables OA.5 and OA.6 for the results when we include month fixed effects in the regressions in panel A.

Table 9
Panel regressions of hedge fund performance on PB betas: Crisis versus noncrisis windows

	Dependent variable: $\text{Performance}_{t+1:t+12}$					
	Ex. ret. (% p.m.)	Alpha (% p.m.)	SR	IR	MPPM_3 (% p.m.)	MPPM_4 (% p.m.)
$\beta^{PB}_{t-23:t} \cdot \text{Crisis}_{t-23:t}$	−0.01	0.01	0.00	0.01	−0.02	−0.03
	(−0.84)	(0.39)	(0.01)	(1.23)	(−1.14)	(−1.17)
$\beta^{PB}_{t-23:t} \cdot \text{NonCrisis}_{t-23:t}$	0.04	0.05	0.01	0.02	0.05	0.05
	(4.50)	(4.18)	(3.78)	(3.28)	(4.55)	(4.54)
$\text{Vol}_{t-23:t}$ (% p.m.)	0.07	0.03	−0.02	−0.05	−0.07	−0.11
	(6.53)	(3.00)	(−6.76)	(−6.98)	(−5.16)	(−7.63)
RedemptionNotice	0.06	0.05	0.06	0.13	0.06	0.06
	(2.87)	(2.10)	(3.38)	(3.00)	(2.58)	(2.54)
Lockup	0.00	0.01	0.00	0.01	0.00	0.00
	(1.93)	(2.72)	(1.89)	(1.75)	(0.61)	(0.30)
MgmtFee (%)	0.08	0.03	0.01	0.01	0.04	0.03
	(2.32)	(0.79)	(0.69)	(0.48)	(0.90)	(0.69)
IncentiveFee (%)	0.00	0.01	0.00	0.01	0.00	0.00
	(0.01)	(1.24)	(0.67)	(1.39)	(−0.76)	(−0.86)
$\log(\text{Age}_t)$	−0.02	−0.05	0.01	0.02	−0.04	−0.04
	(−0.66)	(−1.49)	(0.78)	(0.44)	(−0.96)	(−1.00)
$\log(\text{AUM}_t)$	−0.02	−0.02	0.00	0.00	−0.01	−0.01
	(−2.12)	(−1.49)	(0.25)	(0.08)	(−0.79)	(−0.46)
$\text{Flow}_{t-23:t}$ (%)	0.00	0.00	0.00	0.00	0.00	0.00
	(0.63)	(0.39)	(0.01)	(−0.05)	(0.21)	(0.09)
$R_{t-23:t}$ (% p.m.)	−0.15	−0.02	−0.01	0.04	−0.14	−0.14
	(−6.81)	(−0.84)	(−1.90)	(3.12)	(−5.16)	(−4.72)
log(1+MinInvestment)	0.03	0.03	0.02	0.04	0.02	0.02
	(2.03)	(1.47)	(2.98)	(2.76)	(1.82)	(1.73)
PersonalCapital	0.03	0.00	−0.02	−0.06	0.04	0.04
	(1.08)	(0.05)	(−1.14)	(−1.65)	(1.04)	(0.94)
HighWaterMark	0.12	0.04	−0.01	−0.05	0.15	0.15
	(3.14)	(0.94)	(−0.25)	(−0.96)	(3.57)	(3.53)
Leveraged	0.05	0.05	0.04	0.08	0.03	0.03
	(1.62)	(1.26)	(2.60)	(2.19)	(0.89)	(0.74)
Offshore	−0.05	−0.01	0.00	0.00	−0.04	−0.04
	(−1.39)	(−0.38)	(−0.04)	(0.09)	(−1.04)	(−0.95)
Adjusted R^2 (%)	19.86	4.28	22.98	9.08	20.60	21.63
Observations	97,249	97,249	97,249	97,249	97,249	97,249

This table re-estimates the regressions in Table 6 by allowing a different coefficient on PB beta, depending on whether the twenty-four-month window over which PB beta is estimated overlaps with the following crisis periods: September–November 1998 (Long-Term Capital Management crisis), August–October 2007 (Quant crisis), and September–November 2008 (financial crisis). $\text{Crisis}_{t-23:t}$ equals one if the twenty-four-month window overlaps with the crisis periods and zero otherwise; $\text{NonCrisis}_{t-23:t}$ equals one if the twenty-four-month window does not overlap with the crisis periods and zero otherwise.

a strong positive relationship with performance, one even stronger, both in magnitude and significance, than that indicated in Table 6.

In unreported work, we also repeat the analysis in Table 5 in the same way as above; that is, we form portfolios of hedge funds based on their PB beta estimated over the crisis windows and examine the subsequent performance. The results closely echo those in Table 9: the differences in performance metrics between quintiles 5 and 1 are reduced, mostly to insignificance, when sorted by PB beta estimated over the crisis windows, whereas the differences are even larger than those reported in Table 5 when sorted by PB beta estimated over the noncrisis windows. In addition, the portfolios sorted by PB beta estimated over

Table 10
Panel regressions of hedge fund performance on PB betas: Crisis versus noncrisis windows

	Dependent variable: $\text{Performance}_{t+1:t+12}$					
	Ex. ret. (% p.m.)	Alpha (% p.m.)	SR	IR	MPPM_3 (% p.m.)	MPPM_4 (% p.m.)
$\beta^{PB}_{t-23:t} \cdot \text{Crisis}_{t-23:t} \cdot \text{Leveraged}$	−0.04	0.00	0.00	0.01	−0.05	−0.06
	(−1.78)	(0.15)	(−0.80)	(0.58)	(−2.22)	(−2.23)
$\beta^{PB}_{t-23:t} \cdot \text{Crisis}_{t-23:t} \cdot \text{UnLeveraged}$	0.05	0.02	0.01	0.03	0.06	0.07
	(1.48)	(0.55)	(1.49)	(1.50)	(1.59)	(1.57)
$\beta^{PB}_{t-23:t} \cdot \text{NonCrisis}_{t-23:t} \cdot \text{Leveraged}$	0.05	0.06	0.01	0.02	0.05	0.05
	(4.24)	(4.17)	(3.38)	(2.79)	(4.04)	(3.97)
$\beta^{PB}_{t-23:t} \cdot \text{NonCrisis}_{t-23:t} \cdot \text{UnLeveraged}$	0.03	0.02	0.01	0.02	0.04	0.04
	(1.69)	(0.99)	(1.72)	(1.74)	(2.22)	(2.32)
Adjusted R^2 (%)	19.99	4.35	23.03	9.11	20.73	21.76
Observations	97,249	97,249	97,249	97,249	97,249	97,249

This table re-estimates the regressions in Table 6 by allowing a different coefficient on PB beta, depending on whether the fund uses leverage, as well as whether the twenty-four-month window over which PB beta is estimated overlaps with the following crisis periods: September–November 1998 (Long-Term Capital Management crisis), August–October 2007 (Quant crisis), and September–November 2008 (financial crisis). $\text{Crisis}_{t-23:t}$ equals one if the twenty-four-month window overlaps with the crisis periods and zero otherwise; $\text{NonCrisis}_{t-23:t}$ equals one if the twenty-four-month window does not overlap with the crisis periods and zero otherwise. The reported regressions include the same set of control variables as in Table 6, although they are not reported here for brevity.

the noncrisis windows yield a much clearer monotonic pattern in performance measures, compared with Table 5, confirming the dominance of the information channel in generating the comovement over the noncrisis windows.

Finally, we check whether the impact of focusing on the crisis windows is more pronounced for leveraged funds via

$$\begin{aligned}\text{Performance}_{i,t+1:t+12} = b_0 &+ b_{1cl}\,\beta^{PB}_{i,t-23:t} \cdot \text{Crisis}_{t-23:t} \cdot \text{Leveraged}_i \\ &+ b_{1cu}\,\beta^{PB}_{i,t-23:t} \cdot \text{Crisis}_{t-23:t} \cdot \text{UnLeveraged}_i \\ &+ b_{1nl}\,\beta^{PB}_{i,t-23:t} \cdot \text{NonCrisis}_{t-23:t} \cdot \text{Leveraged}_i \\ &+ b_{1nu}\,\beta^{PB}_{i,t-23:t} \cdot \text{NonCrisis}_{t-23:t} \cdot \text{UnLeveraged}_i \\ &+ \mathbf{b}_2' \,\mathbf{Controls}_{i,t} + \varepsilon_{i,t}.\end{aligned} \tag{8}$$

The idea is that if the contagion channel plays any role at all in generating PB-level comovement, it will be greater for leveraged funds, as the impact of funding shocks from PBs would be minimal on unleveraged funds. Indeed, the results, summarized in Table 10, show that a negative coefficient on PB beta shows up only for leveraged funds over the crisis windows and this time with some statistical significance.[37]

[37] As discussed above, a fund's use of leverage not only magnifies the impact of funding shocks but also contributes to PB revenue, for which the fund may be rewarded with information. Thus, both the information and contagion channels are expected to be stronger for leveraged funds than for unleveraged funds. Indeed, the results in Table 10 show that a positive coefficient on PB beta shows up most robustly for leveraged funds when the beta is estimated over the noncrisis windows.

In summary, the degree of PB-level comovement tends to rise when the contagion channel is likely to be most active, and the comovement that arises then also exhibits a performance implication consistent with an increased role played by the contagion channel. This evidence illustrates that, although not overwhelming, the contagion effect cannot be completely ruled out. Rather, it paints a more nuanced picture of how PBs induce the comovement we observe: in normal times (and on average), the information channel dominates, but in times of stress, the contagion channel also plays a role. The above results suggest that the role played by the contagion channel over the crisis windows is big enough to counterbalance the otherwise dominating role played by the information channel.

7. Conclusion

We find a strong degree of comovement in the returns of hedge funds serviced by the same PB. This PB-level comovement is different from the well-documented market-wide and style-level comovement in hedge fund returns. We consider two main explanations of the result, namely, information and contagion. The first story attributes the comovement of hedge funds serviced by the same PB to privileged information distributed at the PB level, and the second attributes hedge fund comovement to common adverse shocks to their funding liquidity.

The contagion view of the PB-level comovement in hedge fund returns is found to be most valid for the comovement that arises in times of crisis. This view, however, is unable to explain the comovement that arises in general over the sample period, since this comovement does not lead to poor fund performance, and it is not stronger on the downside than on the upside. Indeed, it seems to take some significant crises for such evidence to emerge in support of the contagion view: conditioning on times of distress less extreme than crisis does not seem enough to get at the contagion effect.

In general, our results are more consistent with the information view on comovement. We find that PB-level comovement is associated with better subsequent performance. PB-level comovement is also stronger for funds with more established PB ties and relationships, such as older funds and fund families, and for PBs with better economies of scale in information production and provision, such as PBs serving a larger number of hedge fund clients. Consistent with the possibility that PB-level comovement might be capturing potentially illegal information sharing, we also find that PB-level comovement is stronger for funds that face less regulatory oversight, such as offshore funds and funds headquartered outside the United States.

Of course, we have no way to distinguish the exact nature or source of information that may be driving PB-level comovement. At the very least, our results provide directions for future research of the sort conducted by Griffin, Shu, and Topaloglu (2012): our results highlight the possibility that hedge funds may be rewarded with inside information, whereas the average brokerage house client is not, suggesting where to focus future investigations. In addition, the fact that our results are based on hedge fund returns—generated not only from long-equity positions but also from short or derivative positions—calls for more attention to connected trading outside the long-equity stock universe.

Appendix

Our data cleaning procedure on PBs' CompanyIDs boils down to constructing two tables, PBLINK and PBMERGER, which are available from the authors upon request.

- **PBLINK** provides links between CompanyIDs in TASS and a new set of IDs, constructed in such a way that each investment bank, including its subsidiaries, is given one ID. Subsidiaries are identified either by names or by our extensive search using multiple sources as described below. Some small subsidiaries might have gone unnoticed if they are not active in prime brokerage *and* operate under names that are completely unrelated to their parents. In the case in which a subsidiary is acquired (sold) by an investment bank during our sample period, we knowingly assign a different ID to the subsidiary; PBMERGER will later adjust the subsidiary's ID such that the subsidiary has the same ID with the parent bank after the acquisition, but not before (before the sale, but not after).
- **PBMERGER** contains the details of major PB mergers between January 1994 and June 2012. For each of the PBs that have serviced at least five hedge funds during our sample period,[38] we comb through Capital IQ, Factiva, the company Web site, and other public sources to identify mergers and acquisitions, as well as announcement and effective dates. Some mergers are acquisitions of a subsidiary rather than of the entire bank. After the effective date, we assign one new ID for both the acquirer and target (and a separate new ID for the seller, if any), as in Corwin and Schultz (2005) and Bao and Edmans (2011). Since we do not want the acquisition or sale of those not active in prime broking to change the PB's ID frequently, we exclude mergers if they are with companies that have serviced fewer than five hedge funds or do not appear in TASS as a PB.

References

Agarwal, V., N. D. Daniel, and N. Y. Naik. 2011. Do hedge funds manage their reported returns? *Review of Financial Studies* 24:3281–320.

Agarwal, V., W. Jiang, Y. Tang, and B. Yang. 2013. Uncovering hedge fund skill from the portfolio holdings they hide. *Journal of Finance* 68:739–83.

Aggarwal, R. K., and P. Jorion. 2010. The performance of emerging hedge funds and managers. *Journal of Financial Economics* 96:238–56.

Aragon, G. O., and P. E. Strahan. 2012. Hedge funds as liquidity providers: Evidence from the Lehman bankruptcy. *Journal of Financial Economics* 103:570–87.

Bao, J., and A. Edmans. 2011. Do investment banks matter for M&A returns? *Review of Financial Studies* 24:2286–315.

Bodnaruk, A., M. Massa, and A. Simonov. 2009. Investment banks as insiders and the market for corporate control. *Review of Financial Studies* 22:4989–5026.

[38] Note that we implement this five-fund cutoff every month in Section 1 but here for the entire sample period. Also, we require PBs to have at least five funds from a final sample of 3,837 funds in Section 1 but here from 10,014 funds obtained after an initial set of filters. Thus, PBMERGER covers a larger set of PBs than does our main sample.

Bollen, N. P. B., and V. K. Pool. 2008. Conditional return smoothing in the hedge fund industry. *Journal of Financial and Quantitative Analysis* 43:267–98.

———. 2009. Do hedge fund managers misreport returns? Evidence from the pooled distribution. *Journal of Finance* 64: 2257–88.

Boyson, N. M., C. W. Stahel, and R. M. Stulz. 2010. Hedge fund contagion and liquidity shocks. *Journal of Finance* 65:1789–816.

Brown, S., W. Goetzmann, B. Liang, and C. Schwarz. 2008. Mandatory disclosure and operational risk: Evidence from hedge fund registration. *Journal of Finance* 63:2785–815.

Cassar, G., and J. Gerakos. 2011. Hedge funds: Pricing controls and the smoothing of self-reported returns. *Review of Financial Studies* 24:1698–734.

Corwin, S. A., and P. Schultz. 2005. The role of IPO underwriting syndicates: Pricing, information production, and underwriter competition. *Journal of Finance* 60:443–86.

Coughenour, J. F., and M. M. Saad. 2004. Common market makers and commonality in liquidity. *Journal of Financial Economics* 73:37–69.

Coval, J. D., and T. J. Moskowitz. 1999. Home bias at home: Local equity preference in domestic portfolios. *Journal of Finance* 54:2045–73.

———. 2001. The geography of investment: Informed trading and asset prices. *Journal of Political Economy* 109:811–41.

Dudley, E., and M. Nimalendran. 2011. Margins and hedge fund contagion. *Journal of Financial Quantitative Analysis* 46:1227–57.

Elton, E. J., M. J. Gruber, and T. C. Green. 2007. The impact of mutual fund family membership on investor risk. *Journal of Financial Quantitative Analysis* 42:257–77.

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

Fung, W., and D. A. Hsieh. 2004. Hedge fund benchmarks: A risk-based approach. *Financial Analysts Journal* 60:65–80.

Goetzmann, W., J. Ingersoll, M. Spiegel, and I. Welch. 2007. Portfolio performance manipulation and manipulation-proof performance measures. *Review of Financial Studies* 20:1503–46.

Goldie, B. A. 2011. Prime brokers and hedge fund investment decisions. Working Paper, Pennsylvania State University.

Gow, I. D., G. Ormazabal, and D. J. Taylor. 2010. Correcting for cross-sectional and time-series dependence in accounting research. *Accounting Review* 85:483–512.

Griffin, J. M., N. H. Hirschey, and P. J. Kelly. 2011. How important is the financial media in global markets? *Review of Financial Studies* 24:3941–92.

Griffin, J. M., T. Shu, and S. Topaloglu. 2012. Examining the dark side of financial markets: Do institutions trade on information from investment bank connections? *Review of Financial Studies* 25:2155–88.

Hong, H., J. D. Kubik, and J. C. Stein. 2005. Thy neighbor's portfolio: Word-of-mouth effects in the holdings and trades of money managers. *Journal of Finance* 60:2801–24.

Jegadeesh, N., and Y. Tang. 2010. Institutional trades around takeover announcements: Skill vs. inside information. Working Paper, Emory University.

Jobson, J. D., and B. M. Korkie. 1981. Performance hypothesis testing with the Sharpe and Treynor measures. *Journal of Finance* 36:889–908.

Kedia, S., and X. Zhou. 2014. Informed trading around acquisitions: Evidence from corporate bonds. *Journal of Financial Markets* 18:182–205.

Klaus, B., and B. Rzepkowski. 2009a. Hedge funds and prime brokers: The role of funding risk. Working Paper, European Central Bank.

———. 2009b. Risk spillover among hedge funds: The role of redemptions and fund failures. Working Paper, European Central Bank.

Ledoit, O., and M. Wolf. 2008. Robust performance hypothesis testing with the Sharpe ratio. *Journal of Empirical Finance* 15:850–9.

Liang, B. 2003. The accuracy of hedge fund returns. *Journal of Portfolio Management* 29:111–22.

Lo, A. W. 2001. Risk management for hedge funds: Introduction and overview. *Financial Analysts Journal* 57:16–33.

Lynn, M. Investment banks are too dependent on hedge funds. *Bloomberg News*, March 21, 2005.

Massa, M., and Z. Rehman. 2008. Information flows within financial conglomerates: Evidence from the banks-mutual funds relation. *Journal of Financial Economics* 89:288–306.

Memmel, C. 2003. Performance hypothesis testing with the Sharpe ratio. *Finance Letters* 1:21–23.

Onaran, Y. Hey big spender, analysts on call. *International Herald Tribune*, March 6, 2007.

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.

Sadka, R. 2010. Liquidity risk and the cross-section of hedge-fund returns. *Journal of Financial Economics* 98:54–71.

Schack, J. 2003. Unsettled on Wall Street. *Institutional Investor* 28:63–67.

Sialm, C., Z. Sun, and L. Zheng. 2014. Home bias and local contagion: Evidence from funds of hedge funds. Working Paper, University of Texas at Austin.

Sun, Z., A. Wang, and L. Zheng. 2012. The road less traveled: Strategy distinctiveness and hedge fund performance. *Review of Financial Studies* 25:96–143.

Teo, M. 2011. The liquidity risk of liquid hedge funds. *Journal of Financial Economics* 100:24–44.

Titman, S., and C. Tiu. 2011. Do the best hedge funds hedge? *Review of Financial Studies* 24:123–68.

Van der Laan, M., and S. Rose. 2010. Statistics ready for a revolution: Next generation of statisticians must build tools for massive data sets. *Amstat News* 399:38–39.