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

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Abstract

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 little evidence in favor of the prime broker-level contagion hypothesis.

1 Introduction

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. The PB-level comovement is also distinct from the market-wide comovement and local comovement in hedge fund returns. Our finding has an important implication 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 explanations for this phenomenon. First, the *common information* hypothesis posits that the PB provides valuable information to its hedge fund clients, inducing comovement in their returns as they trade on it. 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

[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, while $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).

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 from 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 other funds did 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 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 (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.[4] 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

[3]Yet another story—which we discuss further in Section 8.2—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.

[4]In Section 8.5, we consider the possibility that our results are due simply to some imperfection in our style control.

by Sialm, Sun, and Zheng (2014). Throughout, our results do not change materially 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 change significantly. 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, which results from a common funding shock, should be positively associated with the liquidation probabilities of hedge funds. Aragon and Strahan (2012), who use the Lehman bankruptcy as an instrument for a common funding shock to Lehman's hedge fund clients, also show that funding shocks to hedge funds increase the likelihood of fund liquidation. Contrary to this prediction, however, we find that higher PB-level comovement lowers, rather than increases, the fund liquidation rate: Using the cessation of reporting to a database as a sign of liquidation, we find a 3.25-percentage-point difference in the liquidation rate between the highest- and lowest-comovement quintile portfolios (19.26% and 22.52%, respectively) two years after portfolio formation. Second, 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 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.

PB-level comovement is also related to several PB- and fund-specific characteristics in a way that is 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 that are 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.

Our paper relates to several recent strands of literature. First is the emerging literature that documents excessive comovement in hedge fund returns. Boyson et al. (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 et al. (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 et al. (2014), in that it is *localized* among certain groups of hedge funds—which we attribute under the PB-level contagion hypothesis to PBs, while Sialm et al. (2014) to local funds of funds (FoFs), spreading liquidity shocks among them. Nevertheless, our PB-level contagion hypothesis for the comovement we observe does not seem to be consistent with the data.

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 non-audited funds do. 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.[5] 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. While 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 a number of hedge fund styles.

Finally, and more broadly, our results speak to current debates about the extent to which institutional investors trade on information from their investment bank connections. Several recent studies (and popular press) suggest that they do "rampantly" (Massa and Rehman 2008; Bodnaruk et al. 2009; Jegadeesh and Tang 2010; Ivashina and Sun 2011; Kedia and Zhou 2014). For example, Massa and Rehman (2008) find that mutual funds tend to invest in the stocks of firms borrowing from their affiliated bank and perform better as a result. Bodnaruk et al. (2009) find evidence that advisory banks increase their holdings of target shares, either directly or through affiliated institutional investors, prior to mergers. Similar evidence of profitable trading is also found by Jegadeesh and Tang (2010), who examine the high-frequency trading of (outside) institutional investors that trade through brokerage arms

[5] In a placebo test, we include an index of hedge funds using the same auditor (instead of the same PB). We find no similar comovement at the auditor level. See Section 3.1 for details.

of advisory banks. In stark contrast, however, Griffin, Shu, and Topaloglu (2012) find no support for any of these activities, despite examining a broader set of possible connections and a higher-frequency data set than much of the literature. Note that Griffin et al. (2012) use broker-level trading data (aggregated across all clients), while we focus on a subset of brokerage house clients (i.e., hedge funds), which are likely recipients if brokerage houses are indeed passing information to their best-paying clients. It is possible, therefore, that hedge funds are rewarded with inside information, whereas the average brokerage house client is not. Of course, we do not claim to have detailed data or an empirical strategy to distinguish whether PB-level comovement is driven by inside information or information generated from innocuous in-house research. At the very least, our results call for further investigation on this issue, focusing on hedge fund trading.

2 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, which leaves 10,014 unique funds. We also filter out observations before 1994, which yields 10,011 unique funds. To control for backfill bias, we further exclude the first 18 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 24 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, which reduces our sample to 4,548 funds: The returns of FoFs and individual hedge funds can be correlated simply because the former invest 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.[6] Because TASS does not maintain historical information on PBs, we utilize all 18 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. 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").[7] 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 59 unique

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

[7] 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.

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 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 65-month period.[8] 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 Section 8.3, we show that our main results do not change greatly 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 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.

Table 1 about here

There are 49 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

[8] Many of these changes are due to investment bank failures and mergers in 2008. The corresponding number for any 65-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.

23 per year, ranging from a low of 16 in 1994 to a high of 29 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.

From 2001 onward, Morgan Stanley has the largest number of hedge fund clients, with an average of 288 funds per year. Before 2001, Bear Sterns had the largest clientele, with an average of 128 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.[9]

Panel B of Table 1 presents the distributional characteristics of hedge fund styles per PB. TASS groups hedge funds into 11 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, multi-strategy, and options strategy. As shown in the table, the PBs in the sample are fairly well diversified across styles: The average (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.[10] Nevertheless, there are also a few PBs in the sample that have only one or two styles. Given that funds in the same style tend to comove, it will be important to control for the style effect when studying the PB-level comovement in hedge fund returns.

[9]Names appearing twice with different (cleaned) ID before and after mergers are listed only once.

[10]There are fewer than 15 options strategy funds in the sample.

3 PB and the Comovement in Hedge Fund Returns

3.1 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.[11] We then estimate a series of fund-level time-series regressions with the following general structure:

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

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, i.e., the "market" index. All returns are in excess of monthly T-bill rates. As discussed above, R_t^{STY} is included in the regression to control for style effects, while R_t^{MKT} is included to control for overall market-wide comovement in hedge fund returns. To avoid mechanical correlations, when calculating the return on each index, we exclude the return of the corresponding fund. Also excluded from each index are funds in the same family as the corresponding fund. 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).[12]

To supplement our analysis, we also estimate a parallel series of regressions to Equation (1) by replacing raw returns with the returns filtered against commonly known risk factors.

[11] As also pointed out by Pirinsky and Wang (2006), equal weighting allows us to address better the question of how a particular fund comoves with other funds using the same PB, especially for PBs with relatively few funds and PBs in which a small number of large funds dominate the clientele. In Section 8.4, we also replicate our tests by using a value-weighted index and the results, although weaker, are qualitatively similar.

[12] 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.

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, and trend-following factors for bonds, currencies, and commodities.[13] 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 to PB-level comovement correlations due to commonly known factors in hedge fund returns.

We estimate PB betas, β^{PB}, for each fund that allows at least a 24-month estimation period. Cross-sectional averages of the estimated betas and their t-statistics are presented in Panel A of Table 2. The first set of columns contains the results obtained using raw returns, while the next set of columns contains the results obtained using filtered returns.

Table 2 about here

We observe that PB betas are significantly positive in all specifications considered. 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 next two rows of Panel A show that the comovement results 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.

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

Adding in the regression an index of funds that do not share a PB with the corresponding fund does not change much 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. Below, we use a similar index but constructed using a much narrower set of funds, i.e., an index of funds serviced by one particular PB that does not service the corresponding fund (e.g., those denoted by RPB or PB_2 below).

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 regressions need to be independent; otherwise, the resulting 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, in Panel C, panel regressions with standard errors clustered by both month and fund (Petersen 2009).[14] Our results show up robustly across different methods, suggesting that they are not driven by cross-sectional or time-series dependence in regression errors. 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.

To further ensure that we do not falsely declare a significant effect,[15] we run two distinct

[14]Similar to Sialm et al. (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 we cannot use funds using multiple PBs in this setting. More importantly, this approach is subject 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 et al. 2010).

[15]For example, Van der Laan and Rose (2010) point out: "[F]or large enough sample sizes, every study—

"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 not to 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 remain largely unchanged from those reported in Table 2. These results reassure that our results in Table 2 are unlikely "false positive," providing further support for the existent of PB-level comovement in hedge fund returns.

Table 3 about here

As an additional robustness test of PB-level comovement (see Section 8 for other additional tests), we follow Sialm et al. (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 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

including ones in which the null hypothesis of no effect is true—will declare a statistically significant effect."

Moskowitz 1999, 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 7, we examine in detail the cross-sectional determinants of PB-level comovement in terms of both fund and PB characteristics.

3.2 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.[16]

We construct our sample of PB switching funds as follows. First, we identify funds that change their PBs (according to cleaned IDs). Then, we verify each change with a list of major PB mergers (see the Appendix for its construction) and exclude nonmerger-related switches. To avoid contamination, we further eliminate changes that are 25 months or less apart from each other. In order 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 18 monthly observations in each of the 24-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 6 different

[16] We also consider using a subset of funds that switch their PBs for nonmerger-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., 43 after imposing a similar set of requirements as those listed below).

PB mergers.

We organize our analysis around the following specification:

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

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.[17] 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.[18] 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 that (2) while PB_1 betas do not change much, PB_2 betas increase significantly after the merger. We test these predictions by interacting an indicator variable A, which equals one if the observation is after the merger and zero otherwise, with the PB indices, that is,

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

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 A versus Panel B or C of Table 2). Panel A of Table 4 reports the results when fund fixed effects are included in

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

[18] $R_t^{PB_1}$ before the merger corresponds to R_t^{PB} in Equation (1); after the merger, it is a weighted average of $R_t^{PB_1}$ and $R_t^{PB_2}$ that corresponds to R_t^{PB}.

the regression and standard errors are clustered by month; Panel B reports the results when standard errors are clustered by both month and fund.

Table 4 about here

We observe first 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; and 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 : $\beta_A^{PB_2} > 0$). 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 (much robust) increase in PB_2 betas to rule out the self-selection hypothesis.[19]

[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.

4 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 four separate sets of analyses in this and the following three sections. First, we begin by studying the relation between PB betas and hedge fund performance. As discussed in the introduction, the common information hypothesis posits that hedge funds are fed privileged information by the investment banks from which they purchase prime brokerage services and PB-level comovement arises because funds derive some of their investment ideas from this common source. To the extent that the information has any value, therefore, the common information hypothesis suggests that PB betas are positively associated with hedge fund performance, ceteris paribus. In contrast, the PB-level contagion hypothesis posits that PB-level comovement arises because of hedge funds being hit by a common funding shock, that is, increased margins or margin calls by the PBs. To the extent that such a shock to hedge fund funding liquidity has any detrimental impact on hedge fund performance (e.g., through forced, costly liquidation of potentially profitable positions), the PB-level contagion hypothesis predicts a negative relation between PB betas and hedge fund performance. To probe the relation between PB betas and hedge fund performance, we use a portfolio sorting approach in Section 4.1 and a multivariate regression approach in Section 4.2. For the purpose of differentiating information versus contagion, our analyses in this and the next three sections will be based primarily 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 change greatly when PB betas estimated using raw returns are used instead.

4.1 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 24 months.[20] 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, Jiang, Tang, and Yang 2013), we also allow longer holding periods, i.e., 3, 6, 12, and 24 months. In any case, following Titman and Tiu (2011), the portfolio is revised 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, including 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, Ingersoll, Spiegel, and Welch (2007) manipulation-proof performance measure, given by

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

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-

[20] To avoid look-ahead bias, we run the initial filtering regressions here within each 24-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.

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, the spreads increase marginally 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.[21] The distribution is constructed by a 5,000-times repetition of essentially our sample portfolio analysis, except that we sort funds randomly rather than 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).

Table 5 about here

4.2 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 that are known to affect fund performance. Similar to the empirical design of Titman and Tiu (2011) and Sun et al. (2012), we estimate the following regression:

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

[21] To test for the difference between Sharpe ratios, one may alternatively use the Jobson and Korkie (1981) test, corrected by Memmel (2003). Following the advice of Ledoit and Wolf (2008), however, 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 non-normal.

where $\text{Performance}_{i,t+1:t+12}$ is either average monthly excess return, Fung and Hsieh (2004) seven-factor adjusted monthly alpha, Sharpe ratio, 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.

The $\mathbf{Controls}_{i,t}$ 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).[22] 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

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

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.

Panel A of Table 6 reports the results when month fixed effects are included in the regressions while standard errors are clustered by fund; Panel B 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 Panel A, 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, an increase of 0.64% per year in the manipulation-proof performance measure MPPM_3, and an increase of 0.65% per year in the manipulation-proof performance measure MPPM_4. These relationships are statistically as well as economically significant. As shown in Panel B of Table 6, the results are robust to how we correct for time-series and cross-sectional dependence in the data.

Table 6 about here

5 PB-Level Comovement and Fund Failure

In this section, we examine how PB betas are related to hedge fund liquidation. As confirmed by Aragon and Strahan (2012), funding shocks to hedge funds increase the likelihood of fund liquidation. Thus, if PB-level comovement indeed arises because of hedge funds being adversely affected by a common funding shock, as postulated by the PB-level contagion hypothesis, we would expect to see a positive relation between PB betas and the liquidation probabilities of hedge funds. If PB-level comovement is information-driven, however, there is no a priori reason to expect a positive relation between PB betas and liquidate rate. Rather, we would expect a negative relation that may be driven by better subsequent performance associated with high PB betas.[23]

To probe this relation, we sort funds, every month, into five quintiles, as before, according to their PB beta measured over the previous 24 months. We then look at the percentage of funds in each quintile that are liquidated over the next n months, for $n \in \{1, 3, 6, 12, 24\}$. Since liquidation in itself is not directly observable in TASS, we follow Aragon and Strahan (2012) and Patton, Ramadorai, and Streatfield (forthcoming) and define liquidation by whether a fund stops reporting to a database. While it is possible that funds stop reporting to TASS for reasons other than liquidation, Getmansky, Lo, and Mei (2004) argue that over 90% of funds that cease reporting to the database are plausibly liquidated funds.

Table 7 summarizes the time-series averages of these liquidation rates for each quintile, as well as the differences between high- and low-PB-beta quintiles. The corresponding t-statistics are adjusted for autocorrelation (i.e., cross-correlation between starting dates) and heteroscedasticity. The results show that the liquidation probabilities for quintile 1 range from 0.99% to 22.52%, while the corresponding figures for quintile 5 are 0.89% and 19.26%. The difference between these probabilities ranges from 0.11% to as high as 3.25%, and is strongly statistically significant for all four horizons beyond one month. Again, the results do not change much when based on PB betas estimated using excess raw returns. Clearly, the evidence presented here finds little support for a positive relation between PB betas and hedge fund liquidation.

Table 7 about here

The negative relation that we find instead between PB betas and liquidation rates, while arguing against the contagion hypothesis in its own right, also serves to reinforce our performance results in the previous section. This is because low-PB-beta funds are more likely to

[23]If PBs that provide profitable investment ideas also provide effective risk management aid to their hedge fund clients, this negative relation between PB betas and fund liquidation may go beyond what can be explained by better performance.

exit from the database than high-PB-beta funds, and average "delisting" return for existing funds tends to be lower than average return for funds that remain in the database (see Ackermann, McEnally, and Ravenscraft 1999; Hodder, Jackwerth, and Kolokolova forthcoming; Agarwal, Fos, and Jiang 2013; Aiken, Clifford, and Ellis 2013). Since delisting return is unaccounted for in our analysis, the true performance spreads between high- and low-PB-beta funds could be even greater than those indicated in Table 5.[24]

6 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 (see Boyson et al. 2010; Dudley and Nimalendran 2011; Sialm et al. 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 an 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 on the 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:

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

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. When $x_d \neq x_u$, $\mathbf{Controls}_t$ also includes $R_t^{PB} \cdot I\left(x_d \leq R_t^{PB} < x_u\right)$. The downside (upside) PB beta, β^{PB_d} (β^{PB_u}), measures fund i's sensitivity to other funds

[24] Titman and Tiu (2011) assume, in their main analyses, that the returns of the exiting funds are -100% in the month following the last reporting month. Teo (2011) also considers, in a robustness check, a return of -10% for the month after a fund exits the database. We do not make such an assumption in our performance tests, but doing so would only make our performance results stronger, given what we find in this section.

sharing a same PB, when the latter experiences downturns (upturns) in performance. Since $I\left(R_t^{PB} < x_d\right) + I\left(R_t^{PB} \geq x_u\right) + I\left(x_d \leq R_t^{PB} < x_u\right) = 1$, 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 where $x_d = x_u = 0$. In our analysis, x_d and x_u for each fund are set to be the 50th and 50th, 25th and 75th, or 10th and 90th percentiles for the corresponding PB index.

Table 8 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.

Table 8 about here

For comparison, we also similarly split the style and market indices. Interestingly, the (unreported) results indicate strong asymmetry in style beta with greater downside than upside comovement. Although weaker, we also find the same form of asymmetry in market betas using filtered returns. These results are consistent with Klaus and Rzepkowski (2009b) and Boyson et al. (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 PB beta now becomes much greater than the downside beta, whether we use raw returns or filtered returns.

7 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 down credit lines to its hedge fund clients in reverse order of their importance in terms of revenues they generate for the PB.[25]

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). Again, we do not have a plausible contagion-based story for these cross-sectional patterns in

[25] 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.

PB-level comovement. If anything, the PB-level contagion hypothesis would 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 reestimate Equation (1) for these subsamples of funds and report the results in Table 9. 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. As before, our discussion will be based on the results obtained using filtered returns, as the inferences using raw returns are similar.

Table 9 about here

The results, summarized in Table 9, are largely consistent with our predictions above under the common information hypothesis. Specifically, the PB beta is greater for older funds, funds that use leverage,[26] 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;[27] past-two-year Fung and Hsieh (2004) alpha instead of past-two-year average return; and an indicator variable for

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

[27]Brown, Goetzmann, Liang, and Schwarz (2008) also use these additional leverage variables.

whether the fund is headquartered outside the United States (NonUS_i) instead of offshore indicator.[28] As shown in Panel B of Table 9, 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 would be 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.[29]

8 Additional Tests

In this section, we report some additional robustness checks of our main results presented in Table 2. First, we consider including the PB distress variables examined by Klaus and Rzepkowski (2009a) in the initial filtering regressions. Next, we conduct the analysis on style subsamples to see whether our results are confined to a particular style of hedge funds.

[28] 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.

[29] 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.

We then analyze whether our results change when we drop the PB information matched for the period before the first download date. Further, we consider using value-weighted PB, style, and market indices. Finally, we examine whether our results are driven by possible imperfections in our style control.

8.1 PB Distress Variables

Klaus and Rzepkowski (2009a) show that hedge fund returns load significantly on the changes in CDS spread and distance-to-default of the PB associated with the corresponding fund. To see whether these PB distress variables, as PB-specific components in hedge fund returns, give rise to the comovement that we document, we add these and other PB distress variables considered by Klaus and Rzepkowski (2009a) in the initial filtering regressions and rerun the analysis using the filtered returns obtained that way. PB distress variables included in the filtering regressions are: Change in the 5-year CDS spread, change in (the negative of) the distance-to-default, and change in the option-implied volatility, as well as the one-month lagged terms of these variables (in addition to the Fung and Hsieh (2004) seven factors).[30] The results are reported in the first two rows of Panels A and B of Table 10 and are not substantially different from the results reported in Table 2.

Table 10 about here

8.2 Style Subsamples

Using a sample of merger arbitrage hedge funds, Goldie (2011) finds that funds profitably concentrate their investments in merger deals where their PBs act as advisors. While this finding exemplifies how PBs could play an information-provision role for their hedge fund clients and may imply the existence of PB-level comovement among merger arbitrageurs, we do not expect that our full-sample results are due entirely to this particular style of funds.

[30] As Klaus and Rzepkowski (2009a) also note, much of the data required to compute these variables is unavailable before 2004. In an unreported work, we also add PB stock returns and the results are similar.

To check this, we conduct subsample analyses similar to those in Table 9, but based on styles. The results are summarized in Table 11 and show that PB-level comovement does indeed arise for event-driven style (to which merger arbitrage funds belong), but it is by no means confined to this style category only: At least five styles out of nine (emerging markets, event driven, long/short equity, managed futures, and multi-strategy) robustly exhibit the comovement, with emerging markets and long/short equity styles clearly standing out in terms of magnitude.[31] Importantly, the fact that long/short equity funds exhibit the strongest PB-level comovement alleviates concerns of any PB-specific valuation mechanisms (for calculating the fund's NAV) driving our results, because these funds hold mostly liquid, equity securities, which are traded and priced on exchanges (Jorion and Schwarz 2014).[32]

Table 11 about here

8.3 Observations Before the First Download Date

Although funds do not change PBs often (see Section 2), using the PB information contained in the March 2007 download (or a later download in which the fund first appears) to match with all return observations before the download date may create noise in the data. To check the impact of this noise, we repeat the analysis after discarding the PB information matched with return observations that precede the download date (or the last reporting date, if the fund is already "dead" in the first download containing it) (1) by more than 36 months (as in Chung and Teo 2012) or (2) at all (as in Aragon and Strahan 2012). Note that the latter may be too conservative a choice because it means that we discard the PB information of all graveyard funds that cease reporting to TASS before the first download date, although the information in the months leading up to the cessation can be accurate; similarly, it means that we also do not assign the PB information to all live funds for the period before the first

[31] These five styles represent over 80% of all sample funds. We exclude dedicated short bias and options strategy due to an insufficient number of funds.

[32] Meanwhile, emerging markets funds showing strong PB-level comovement echoes our earlier result on the offshore versus onshore subsamples (see Table 9) because emerging markets style is dominated mostly by offshore funds unlike other style categories in TASS (Aragon, Liang, and Park 2014).

download date, although PB information in the months leading up to the download date can be accurate. As a result, we drop 154,226 (76.5%) of the observations by the latter choice, and 71,418 (35.4%) by the former. The results are presented in the third to sixth rows of each panel of Table 10 and are similar to, if not stronger than, those reported in Table 2.

8.4 Value-Weighted Indices

Following Pirinsky and Wang (2006), we use equal weighting when computing the PB index and other independent variables in Equation (1). This is because equal weighting can better address the question of how a fund comoves with others sharing the same PB, as it does not emphasize or deemphasize the fund's comovement with other funds based on the size of the latter as long as they share a PB with the fund. In addition, the frequent occurrence of discontinuities in the historical series of AUM, as noted by Fung and Hsieh (2004), means that value-weighted indices discard funds that do not report AUM in the previous month (on average, about 25% of the funds included in the equally-weighted PB index are excluded from the value-weighted PB index, for this reason). Nevertheless, the results obtained using value-weighted indices, reported in the seventh and eighth rows of each panel of Table 10, although weaker, are qualitatively similar to the results obtained using equal-weighted indices.

8.5 Style Controls

The TASS style classification, based on which our style index is constructed, may have some limitations. First, since the TASS classification is based on the self-reported styles of funds, it may be subject to errors and managerial manipulation. For example, if a fund classifies itself as one style while it is better described by another, our results may be due simply to the style effect as the style index fails to control for it. To address this possibility, we follow Pirinsky and Wang (2006) and include in the regression an additional style index (denoted by the superscript STY_b) that best describes the corresponding fund over the previous 24-

month period, as measured by the R^2, from among the remaining 9 or 10 styles.[33] By using a rolling window of 24 months to compute the R^2, we also allow for the possibility that hedge funds switch styles over time (i.e., style drift). The results are presented in the ninth and tenth rows of each panel of Table 10, and show that adding this "best-fit" style index in the regression does not change much the magnitude and significance of the PB beta.

Second, the TASS style classification defines some styles in a way that is too broad, so it may lump together funds that can differ in their true underlying (sub)styles (Sun et al. 2012).[34] In this case, a fund in a broadly defined style category may load heavily on the PB index but not on the style index, if its PB specializes in servicing the substyle to which the fund belongs. Given that there are no data on substyle classification, this possibility is essentially the self-selection hypothesis, with substyles being unobserved characteristics that drive both funds' PB selection and comovement among those that share such characteristics. To see whether our results are due to this (unobserved) substyle effect, we include in the regression a substyle index (denoted by the superscript STY_s), constructed using a subset of funds constituting the style index that best resemble the corresponding fund, as measured by the return correlation over the previous 24-month period (top decile).[35] By construction, this additional index controls for anything (observed or unobserved) that gives rise to return correlation but the TASS classification is too broad to capture. The results are presented in the last two rows of each panel of Table 10, and show that adding the substyle index in the regression does not change much the magnitude and significance of the PB beta.

[33]In an unreported work, we also use correlations, instead of R^2, as a criterion for selecting this additional style index and the results are similar.

[34]For example, Cassar and Gerakos (2011) note that the event-driven category in TASS and HFR covers at least three distinct style categories in CISDM.

[35]In an unreported work, we also construct a substyle index by matching on some observed characteristics such as style, size, and average past returns (as in Aragon and Strahan 2012) and obtain similar results.

9 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, while the second attributes hedge fund comovement to common adverse shocks to their funding liquidity.

We are unable to find much support for the contagion view on the PB-level comovement in hedge fund returns, since this comovement does not lead to poor fund performance nor increase the likelihood of fund failure. PB-level comovement on the downside is also no stronger than it is on the upside.

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 that are 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 call for further investigation of the sort conducted by Griffin et al. (2012), but focusing on hedge fund trading.

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 where 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,[36] we comb through Capital IQ, Factiva, company website, 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 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 acquiring or selling of those that are 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.

[36]Note that we implement this 5-fund cutoff every month in Section 2, 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 2, but here from 10,014 funds obtained after an initial set of filters. Thus, PBMERGER covers a larger set of PBs than our main sample.

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TABLE 1: **Summary Statistics**

Panel 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	1740.00	27.00	50.89	19.00	280.00	5.00
2012	1374.00	20.00	47.30	21.50	184.00	5.00
Ave.	1490.79	22.53	45.20	19.55	228.95	5.00

Panel 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 of the table 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 of the table 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.

TABLE 2: **PB-Level Comovement**

	Raw Returns			Filtered Returns		
	β^{PB}	β^{STY}	β^{MKT}	β^{PB}	β^{STY}	β^{MKT}
Panel A: Fama–MacBeth by Fund						
Estimate	0.60		0.35	0.53		0.29
t-stat	22.63		13.84	23.16		13.20
Estimate	0.36	0.59	-0.03	0.35	0.45	0.03
t-stat	13.59	20.73	-0.96	14.68	16.21	1.02
Panel B: Fund Fixed Effects and S.E. Clustered by Month						
Estimate	0.56		0.42	0.46		0.44
t-stat	16.79		9.07	14.61		7.51
Estimate	0.39	0.52	0.05	0.38	0.38	0.13
t-stat	4.68	3.22	0.42	8.43	4.41	1.81
Panel C: S.E. Clustered by Month and Fund						
Estimate	0.55		0.42	0.45		0.44
t-stat	12.25		7.45	10.80		7.14
Estimate	0.39	0.52	0.05	0.37	0.38	0.13
t-stat	4.46	3.15	0.39	7.35	4.19	1.67

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 three columns contain the results using raw excess returns; the next three columns contain the results using filtered returns. We obtain filtered return 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 of the table 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.

TABLE 3: **PB-Level Comovement: Placebo Tests**

	Raw Returns					Filtered Returns				
	β^{PB}	β^{RPB}	β^{AD}	β^{STY}	β^{MKT}	β^{PB}	β^{RPB}	β^{AD}	β^{STY}	β^{MKT}
Panel A: Fund Fixed Effects and S.E. Clustered by Month										
Estimate	0.39	0.01		0.52	0.04	0.38	0.02		0.38	0.11
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		0.03		0.43	0.36
t-stat		1.06		3.34	1.84		1.60		4.58	4.15
Estimate	0.41		0.03	0.50	0.00	0.39		0.02	0.36	0.10
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			0.02	0.41	0.37
t-stat			0.89	3.25	1.91			1.02	4.43	5.27
Panel B: S.E. Clustered by Month and Fund										
Estimate	0.39	0.01		0.52	0.04	0.37	0.02		0.38	0.11
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		0.03		0.43	0.36
t-stat		1.02		3.27	1.79		1.53		4.33	3.92
Estimate	0.41		0.03	0.50	0.00	0.39		0.02	0.36	0.10
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			0.02	0.41	0.37
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 five columns contain the results using raw excess returns; the next five 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.

TABLE 4: **PB Merger and Changes of PB-Level Comovement**

	Raw Returns				Filtered Returns			
	β^{PB_1}	$\beta_A^{PB_1}$	β^{PB_2}	$\beta_A^{PB_2}$	β^{PB_1}	$\beta_A^{PB_1}$	β^{PB_2}	$\beta_A^{PB_2}$
Panel A: Fund Fixed Effects and S.E. Clustered by Month								
Estimate	0.40	0.08			0.43	-0.07		
t-stat	3.30	1.64			5.47	-1.01		
Estimate			-0.04	0.28			-0.05	0.23
t-stat			-0.30	3.15			-0.53	2.22
Estimate	0.46	-0.07	-0.07	0.20	0.45	-0.17	-0.05	0.20
t-stat	4.54	-1.12	-0.83	2.65	5.92	-2.01	-0.91	2.46
Panel B: S.E. Clustered by Month and Fund								
Estimate	0.40	0.07			0.43	-0.07		
t-stat	2.90	1.04			4.45	-1.01		
Estimate			-0.03	0.27			-0.04	0.22
t-stat			-0.21	2.58			-0.36	2.06
Estimate	0.46	-0.07	-0.07	0.20	0.45	-0.16	-0.04	0.18
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 24-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:

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

where PB_1 denotes the fund's corresponding PB; PB_2 denotes PB_1's merger partner; A is an indicator variable, which 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 four columns present the results using raw excess returns; the next four 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.

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 24 months. PB betas are estimated via Equation (1) using filtered returns for funds that allow at least 18-month estimation period within each 24-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); 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.

TABLE 6: **Panel Regressions of Hedge Fund Performance on PB Betas**

Panel A: Month Fixed Effects and Standard Errors Clustered by Fund

	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	0.02	0.03
	(3.00)	(3.56)	(3.00)	(3.37)	(2.52)	(2.35)
Vol$_{t-23:t}$ (% p.m.)	0.07	0.03	-0.02	-0.05	-0.07	-0.11
	(6.32)	(2.93)	(-6.85)	(-7.06)	(-5.24)	(-7.70)
RedemptionNotice	0.06	0.05	0.06	0.13	0.06	0.06
	(2.88)	(2.11)	(3.39)	(3.00)	(2.59)	(2.55)
Lockup	0.00	0.01	0.00	0.01	0.00	0.00
	(1.87)	(2.70)	(1.88)	(1.75)	(0.55)	(0.25)
MgmtFee (%)	0.07	0.03	0.01	0.02	0.04	0.03
	(2.22)	(0.80)	(0.68)	(0.51)	(0.83)	(0.62)
IncentiveFee (%)	0.00	0.01	0.00	0.01	0.00	0.00
	(0.12)	(1.25)	(0.67)	(1.37)	(-0.67)	(-0.76)
log(Age$_t$)	-0.02	-0.05	0.01	0.02	-0.04	-0.04
	(-0.67)	(-1.47)	(0.79)	(0.44)	(-0.95)	(-0.99)
log(AUM$_t$)	-0.02	-0.02	0.00	0.00	-0.01	-0.01
	(-2.13)	(-1.46)	(0.26)	(0.10)	(-0.80)	(-0.47)
Flow$_{t-23:t}$ (%)	0.00	0.00	0.00	0.00	0.00	0.00
	(0.32)	(0.20)	(-0.06)	(-0.09)	(-0.06)	(-0.15)
$R_{t-23:t}$ (% p.m.)	-0.15	-0.02	-0.01	0.04	-0.13	-0.13
	(-6.59)	(-0.72)	(-1.77)	(3.19)	(-4.93)	(-4.50)
log(1 + MinInvestment)	0.03	0.02	0.02	0.04	0.02	0.02
	(1.95)	(1.41)	(2.93)	(2.72)	(1.73)	(1.65)
PersonalCapital	0.03	0.00	-0.02	-0.06	0.04	0.03
	(1.04)	(0.04)	(-1.15)	(-1.65)	(1.00)	(0.90)
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
	(1.59)	(1.28)	(2.60)	(2.21)	(0.87)	(0.72)
Offshore	-0.05	-0.01	0.00	0.00	-0.04	-0.04
	(-1.41)	(-0.38)	(-0.04)	(0.09)	(-1.06)	(-0.96)

(continued)

TABLE 6 (Continued): **Regressions of Hedge Fund Performance on PB Betas**

Panel B: Standard Errors Clustered by Month and Fund

	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	0.00	0.02	0.02	0.02
	(2.51)	(3.47)	(2.42)	(3.19)	(2.00)	(1.85)
Vol$_{t-23:t}$ (% p.m.)	0.08	0.03	-0.01	-0.04	-0.05	-0.09
	(4.84)	(2.14)	(-3.37)	(-5.29)	(-2.49)	(-4.19)
RedemptionNotice	0.06	0.04	0.07	0.13	0.07	0.08
	(2.72)	(1.88)	(3.37)	(2.97)	(2.86)	(2.90)
Lockup	0.00	0.01	0.00	0.01	0.00	0.00
	(1.42)	(2.64)	(1.53)	(1.71)	(0.33)	(0.08)
MgmtFee (%)	0.07	0.03	0.01	0.01	0.03	0.02
	(1.86)	(0.79)	(0.42)	(0.34)	(0.60)	(0.40)
IncentiveFee (%)	0.00	0.01	0.00	0.01	0.00	0.00
	(0.41)	(1.58)	(0.85)	(1.60)	(-0.40)	(-0.50)
log(Age$_t$)	-0.10	-0.09	-0.02	-0.02	-0.10	-0.10
	(-2.16)	(-1.88)	(-1.06)	(-0.58)	(-1.87)	(-1.78)
log(AUM$_t$)	-0.04	-0.02	0.00	0.00	-0.03	-0.03
	(-2.98)	(-1.20)	(-0.71)	(-0.02)	(-1.79)	(-1.49)
Flow$_{t-23:t}$ (%)	0.01	0.00	0.00	0.00	0.01	0.01
	(1.66)	(0.44)	(1.27)	(0.37)	(1.32)	(1.21)
$R_{t-23:t}$ (% p.m.)	-0.22	-0.06	-0.04	0.00	-0.23	-0.23
	(-4.57)	(-1.80)	(-3.66)	(-0.18)	(-4.40)	(-4.32)
log(1 + MinInvestment)	0.04	0.03	0.02	0.04	0.04	0.04
	(2.30)	(1.34)	(3.23)	(2.82)	(2.19)	(2.14)
PersonalCapital	0.06	0.01	-0.01	-0.05	0.05	0.05
	(1.58)	(0.31)	(-0.57)	(-1.30)	(1.30)	(1.15)
HighWaterMark	0.05	0.04	-0.03	-0.08	0.09	0.09
	(0.96)	(0.69)	(-1.41)	(-1.51)	(1.55)	(1.58)
Leveraged	0.07	0.06	0.04	0.09	0.05	0.04
	(1.83)	(1.37)	(2.79)	(2.30)	(1.17)	(1.02)
Offshore	-0.09	-0.03	-0.02	-0.02	-0.07	-0.07
	(-2.46)	(-0.81)	(-0.83)	(-0.35)	(-1.71)	(-1.52)

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 12-month period after PB betas are calculated. PB betas are calculated as in Table 5. Panel A of the table reports the results when month fixed effects are included in the regressions while standard errors are clustered by fund; Panel B 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.

TABLE 7: **Liquidation Probabilities by PB Betas**

	Raw Returns					Filtered Returns				
	1m	3m	6m	12m	24m	1m	3m	6m	12m	24m
Q1 (low)	1.08	3.30	6.39	12.26	22.80	0.99	3.02	6.08	12.10	22.52
Q2	1.00	3.09	6.45	12.41	22.52	1.01	3.13	6.35	12.40	21.99
Q3	0.90	2.86	5.94	11.92	21.60	0.91	2.87	5.89	11.48	21.24
Q4	0.87	2.52	5.10	10.44	19.59	0.93	2.73	5.42	10.48	20.08
Q5 (high)	0.87	2.57	5.15	10.05	18.57	0.89	2.58	5.30	10.64	19.26
Q5-Q1	-0.22	-0.72	-1.24	-2.21	-4.23	-0.11	-0.44	-0.78	-1.46	-3.25
t-stat	-2.63	-3.53	-3.84	-4.23	-5.84	-1.33	-2.34	-2.55	-2.82	-4.56

We sort funds into quintiles based on their PB betas measured over the previous 24 months. PB betas are estimated via Equation (1) for funds that allow at least 18-month estimation period within each 24-month window. Portfolios are sorted every month and held for 1, 3, 6, 12, or 24 months. The table reports the time-series averages of the liquidation rate, in percentages, for each quintile portfolio. The table also reports the difference between quintiles 5 and 1, and the corresponding t-statistics. The t-statistics are derived from Newey–West standard errors with three lags.

TABLE 8: **Downside and Upside PB betas**

	Raw Returns			Filtered Returns		
	β^{PB_d}	β^{PB_u}	Diff.	β^{PB_d}	β^{PB_u}	Diff.
Panel A: Fund Fixed Effects and S.E. Clustered by Month						
(50, 50)	0.42	0.37	0.04	0.33	0.40	-0.07
t-stat	3.79	5.83	0.80	5.33	9.08	-1.46
(25, 75)	0.42	0.37	0.05	0.32	0.40	-0.08
t-stat	3.87	5.69	0.92	5.29	9.13	-1.64
(10, 90)	0.42	0.37	0.05	0.30	0.39	-0.09
t-stat	3.86	5.53	0.95	4.69	8.13	-1.75
Panel B: S.E. Clustered by Month and Fund						
(50, 50)	0.41	0.38	0.03	0.32	0.39	-0.07
t-stat	3.70	5.26	0.62	5.29	7.77	-1.65
(25, 75)	0.42	0.37	0.04	0.33	0.39	-0.07
t-stat	3.78	5.19	0.84	5.37	7.79	-1.56
(10, 90)	0.42	0.37	0.05	0.30	0.38	-0.08
t-stat	3.75	5.10	0.89	4.43	7.21	-1.72

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

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

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. When $x_d \neq x_u$, $\mathbf{Controls}_t$ also includes $R_t^{PB} \cdot I\left(x_d \leq R_t^{PB} < x_u\right)$. The first three columns contain the results using raw excess returns; the next three 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.

TABLE 9: **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}
Panel A: Fund Fixed Effects and S.E. Clustered by Month										
High	0.44	0.44	0.40	0.42	0.45	0.53	0.54	0.41	0.50	0.77
t-stat	8.28	8.16	8.52	8.83	8.92	8.51	7.88	8.23	7.81	16.05
Low	0.32	0.32	0.30	0.34	0.30	0.31	0.35	0.34	0.32	0.29
t-stat	7.20	7.11	6.44	7.16	6.76	7.36	8.67	7.70	8.13	6.94
High-Low	0.12	0.11	0.11	0.08	0.15	0.22	0.18	0.07	0.18	0.48
t-stat	3.55	3.15	3.59	2.43	4.56	5.52	3.74	2.21	4.21	13.31
Panel B: S.E. 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	6.93	6.85	7.10	6.15	6.75	7.45	6.94	6.59	6.15	11.43
Low	0.32	0.32	0.30	0.33	0.29	0.31	0.36	0.33	0.32	0.29
t-stat	6.23	6.01	5.11	6.12	5.60	6.16	7.16	6.21	6.74	5.87
High-Low	0.11	0.10	0.10	0.08	0.15	0.21	0.17	0.07	0.17	0.47
t-stat	2.09	1.85	1.73	1.29	2.49	3.75	2.61	1.26	2.50	7.51

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. 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.

TABLE 10: **PB-Level Comovement: Robustness Tests**

	Raw Returns					Filtered Returns				
	β^{PB}	β^{STY}	β^{STY_b}	β^{STY_s}	β^{MKT}	β^{PB}	β^{STY}	β^{STY_b}	β^{STY_s}	β^{MKT}
Panel A: Fund Fixed Effects and S.E. Clustered by Month										
PB Distress						0.39	0.71			-0.11
t-stat						13.67	32.92			-4.05
36 months	0.43	0.40			0.15	0.41	0.26			0.21
t-stat	4.68	2.70			1.22	8.61	3.28			2.74
0 month	0.56	0.29			0.12	0.54	0.18			0.21
t-stat	6.05	2.39			1.19	10.33	2.59			2.95
Val. Weight	0.18	0.76			0.04	0.20	0.53			0.10
t-stat	7.49	12.09			0.84	9.10	8.99			2.90
Best-Fit	0.42	0.51	0.14		-0.03	0.41	0.40	0.23		-0.07
t-stat	4.86	2.93	4.28		-0.23	8.74	4.25	5.05		-0.84
Substyle	0.43	0.26		0.16	0.09	0.40	0.13		0.22	0.14
t-stat	3.86	9.24		1.33	0.94	6.03	5.45		2.08	2.25
Panel B: S.E. Clustered by Month and Fund										
PB Distress						0.39	0.71			-0.12
t-stat						8.78	17.84			-2.37
36 months	0.43	0.40			0.14	0.41	0.26			0.20
t-stat	4.63	2.64			1.13	8.14	3.14			2.47
0 month	0.57	0.29			0.11	0.55	0.17			0.19
t-stat	5.61	2.30			1.01	7.87	2.37			2.36
Val. Weight	0.18	0.76			0.04	0.19	0.53			0.10
t-stat	6.06	11.22			0.77	7.19	8.26			2.73
Best-Fit	0.42	0.51	0.14		-0.03	0.40	0.40	0.23		-0.07
t-stat	4.56	2.87	3.86		-0.22	7.42	4.03	4.91		-0.75
Substyle	0.43	0.27		0.16	0.09	0.40	0.14		0.21	0.14
t-stat	3.71	7.79		1.31	0.86	5.49	4.31		1.90	1.96

This table reports the robustness of our results reported in Table 2 to various variations on our baseline specification. 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 include PB distress variables considered by Klaus and Rzepkowski (2009a) in the initial filtering regressions, in addition to the Fung and Hsieh (2004) seven factors. The third and fourth (fifth and sixth) rows discard the PB information matched with return observations that precede the date as of which the PB information is current by more than 36 months (at all). The seventh and eighth rows use value weighting when computing the PB index and other independent variables in Equation (1). The ninth and tenth rows include an additional style index that best describes the corresponding fund out of the remaining 9 or 10 styles over the previous 24-month period in terms of the R^2. The last two rows include a substyle index, constructed using a subset of funds constituting the style index that best resemble the corresponding fund over the previous 24-month period (top decile) in terms of the return correlation.

TABLE 11: **Style Subsamples**

	Subsample restricted to								
	Convertible Arbitrage	Emerging Markets	Equity Market Neutral	Event Driven	Fixed Income Arbitrage	Global Macro	Long/Short Equity	Managed Futures	Multi-Strategy
Panel A: Fund Fixed Effects and S.E. Clustered by Month									
Raw Ret.	0.11	0.36	0.14	0.15	0.05	0.11	0.46	0.16	0.16
t-stat	2.54	4.89	3.15	6.66	1.83	1.74	2.84	4.51	5.65
Filtered Ret.	0.12	0.38	0.02	0.08	0.04	0.16	0.46	0.14	0.15
t-stat	2.62	7.63	0.52	3.61	1.14	3.84	7.16	3.39	4.48
Panel B: S.E. Clustered by Month and Fund									
Raw Ret.	0.10	0.35	0.14	0.15	0.04	0.11	0.46	0.16	0.16
t-stat	1.25	3.21	1.86	3.67	1.05	1.27	2.75	2.60	3.14
Filtered Ret.	0.11	0.34	0.02	0.08	0.02	0.15	0.46	0.12	0.15
t-stat	1.34	4.17	0.34	2.78	0.71	2.42	6.08	1.96	3.30

This table reports the PB betas estimated via Equation (1) for various style subsamples listed in the second row. 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 in each panel contain the results using raw excess returns; the next two rows contain the results using filtered returns. The number of sample funds used as the dependent variable is: 109 (convertible arbitrage), 237 (emerging markets), 170 (equity market neutral), 273 (event driven), 78 (fixed income arbitrage), 110 (global macro), 1168 (long/short equity), 237 (managed futures), and 183 (multi-strategy). Dedicated short bias and option strategy are dropped due to an insufficient number of funds.