Opponent's Report on Dissertation Thesis

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Address the following questions in your report, please:

a) Can you recognize an original contribution of the author?
   YES
b) Is the thesis based on relevant references?
   YES
c) Is the thesis defendable at your home institution or another respected institution where you gave lectures?
   YES
d) Do the results of the thesis allow their publication in a respected economic journal?
   YES
e) Are there any additional major comments on what should be improved?
   NO
f) What is your overall assessment of the thesis?
   (a) I recommend the thesis for defense without substantial changes

(Note: The report should be at least 2 pages long.)

I choose to provide my comments chapter by chapter.

Chapter 2: Is research on hedge fund performance published selec- tively? A quantitative survey

1) I think it is useful to distinguish between publication selection bias caused by re- searchers and journals desiring to publish statistically significant results, and the selection bias generated by hedge funds selectively choosing which results to report. Presumably, they will choose to report the results that make the hedge funds look especially successful. This type of selection bias will not be a function of the standard error and thus will not be identified, or corrected, via the inclusion of a standard error variable

Thank you for your comment. We agree that the two biases you point out are concep- tually different and they will be reflected in the data in different ways. The methods of detecting publication selection bias allow us to quantify an adjust for the former bias caused by researchers and journals desiring to publish statistically significant results because this bias likely introduces an association between the magnitude of the estimated performance coeffi- cient and its standard error. In contrast the latter bias generated by hedge funds selectively choosing which results to report to the commercial databases is of different nature and it must be treated differently. We argue that the direction of this latter bias is not obvious. Unsuccessful hedge funds may choose not to report their performance to the commercial databases because their poor results are not likely to attract new investors. This would make the hedge funds look especially successful, as you might argue. Nevertheless, very successful hedge funds may also skip reporting their performance to commercial databases because they might be closed for new capital and therefore have no incentives to advertise their success.

We agree that the latter bias should not be a function of the standard error and thus will not be identified, or corrected, via the inclusion of a standard error variable. To quantify the effect of this bias we need to rely on the differences in the performance coefficients reported in primary studies that do and do not correct for these biases in their primary analysis. For example, the backfilling bias may be addressed by discarding initial months of performance data in the commercial databases. Furthermore, the survivorship bias may be addressed by using data from databases that do not purge the data on inactive funds or by using performance data for the funds of funds rather than individual hedge funds. When collecting our data set from the primary studies we code dummy variables that indicate whether or not a given estimate is obtained using one of these adjustment techniques. We are then able to use the differences between these estimates to evaluate the impact of a given bias.

We observe that while the publication selection bias does not significantly affect the inferences about the overall performance of hedge funds, the impact of the backfilling bias is indeed substantial. The difference between these two empirical results further underscores the importance of distinguishing between these two biases.

REED: I am satisfied with the response, but there should be some discussion of this in the thesis.

2) On page 35, Yang states: "*In the last two columns of Panel A of Table 2.2 we report our weighted least squares estimates of Equation 2.2. In the fifth column, we weigh the observations by the inverse of their standard error (WLS). This approach gives less weight to less precise estimates, which helps to adjust for potential heteroskedasticity in our ob- servations. The sixth column shows our results from estimation when the observations are weighted by the inverse of the number of estimates reported in a given study (wNOBS).*" There is an inconsistency here. Standard WLS uses "inverse variance" weights. So weight- ing by the inverse of the SE squared. Practically, this means dividing all the observations by the SE. I thought this is what Yang meant, but then she said Column 6 weighted by number of estimates. To be consistent with the WLS estimates, the

Column 6 estimates should divide by the square root of the number of estimates. However, it would be better to use the appropriate language and say that Columns 5 and 6 weight by the inverse of the squared SE and the inverse of the number of estimates per study, respectively.

Thanks for drawing our attention to the point. Indeed, the formulation you refer to is imprecise. We have changed the expression about the issue in the text and notes:

*"In the fifth column, we weigh the observations by the inverse of their **squared** standard error (WLS)."*

REED: I am satisfied with the response.

Chapter 3: Where Have All the Alphas Gone? A Meta-Analysis of Hedge Fund Performance

1) The Abstract is not related to the content of the chapter. The abstract is about banking regulation, but this chapter is about hedge fund performance.

Thank you for pointing out this issue. You are quite right. By mistake, we included the wrong abstract when compiling the .pdf file of this dissertation thesis. We have now included the correct version of the abstract, which we also copy below.

"We conduct a systematic meta-analysis of the factors influencing hedge fund performance estimates published between 2001 and 2021. Using a sample of 1,019 in- tercept terms from regressions of hedge fund returns on risk factors (the "alphas") collected from 74 studies, we document a strong downward trend in reported alphas that persists even after controlling for heterogeneity in hedge fund characteristics and research design choices in the underlying studies. Our best-practice estimates of current performance are not reliably different from zero for all common hedge fund strategies. In addition, we provide an estimate of the sizeable impact of management and performance fees charged by hedge funds. We also document how reported performance estimates vary with hedge fund and study characteris- tics. Our results suggest that while hedge funds have generated positive value for investors in the past, on average, they no longer do so."

REED: I am satisfied with the response.

2) Note the similarity between the BMA and OLS estimates in Table 3.3. This is not a coincidence. The BMA estimates are weighted averages of OLS estimates.

Thank you for pointing out this relationship. I absolutely agree that the similarity between the results based on the BMA and the OLS is by no means a coincidence. As you argue and as we also point out in the research design section of this chapter, the BMA results essentially are the weighted averages of OLS estimates under various regression specifications that differ in the inclusion of

different combinations of control variables. Hence, I agree that it is not at all surprising that the two results are similar. Nevertheless, I still believe that despite this similarity, the OLS result is incrementally informative relative to the BMA-based result, which justifies reporting it in Table 3.3. First, while the BMA results are based on a multitude of various regression models, the OLS results are based on a single regression model. Thus, our BMA estimation can also be interpreted as a way of identifying the set of explanatory variables that are relevant to the OLS estimation. Second, while we consider the BMA results as our baseline findings and we rely on them in drawing our conclusions, we also report the OLS result as a robustness check. These auxiliary results are based on a regression estimation approach that most readers are likely familiar with and are readily comparable to results reported in prior studies. Reporting both sets of results also gives the readers an opportunity to choose from the estimates based on the frequentist approach or the Bayesian approach, whichever they prefer. We have newly included in the updated version of this dissertation thesis the quote below that explains this relationship between the two sets of results.

> *"To check the robustness of the BMA results, we also report in the right panel of Table 3.3 the ordinary least squares (OLS) estimates, which are based on a single regression model including a set of the most relevant variables identified by BMA. The frequentist OLS approach is better comparable to prior study results and thus may enhance the understanding of the effect of these relevant factors."*

Correspondingly, we have updated the way we comment on the OLS results presented in Table 3.3, for example, see the quote below.

> *"In the OLS regression, all of the nine variables included in the BMA model with the best fit are also significant at a better than 5% level."*

REED: I am satisfied with the response.

3) I would suggest that Yang report I-squared. In particular, it would be interesting to note what percent of total variation can be explained by the included regressors. Alternatively, if she continues to base her main estimates on OLS estimation, she could report R-squared.

Thank you for pointing out this issue to us. We have considered reporting some measures of heterogeneity of the data we use for our meta-analysis, e.g., I-squared. We recognize that in some settings reporting information on the percentage of variation across studies that is due to heterogeneity rather than chance may have important implications for the interpretation of the results. Nevertheless, after some consideration, we have agreed with my co-authors not to report I-squared in this study. There are several reasons that led us to this decision. First, we argue that the relevance of heterogeneity measures varies across fields of study and the nature of how the underlying measures are typically estimated in the primary studies. We also believe that in economics sample heterogeneity is commonly considered substantial without explicitly reporting estimates of the heterogeneity measures. Second, there is no universally accepted consensus about the relevant cut of levels for the I-squared measure in different fields. According to Cochrane Review Guidelines, an I-squared above 0.75 is considerable heterogeneity in medicine, and according to Vivalt (2020), an I-squared of 0.9 is common in economics meta-analyses. This limits the usefulness of the measure

in informing researchers about the underlying data characteristics and the consequences these characteristics should have for research design choices. Third, we also observe the controversy in the prior method- ological literature about the problematic precision, with which these measures are typically investigated. Fourth, since the purpose of our study is to explain the heterogeneity in the hedge fund performance estimates reported in the primary studies, estimating I-squared is not the first-order consideration for our analysis. Given these limitations, we have doubts about whether I-squared can reasonably be considered a reliable measure of the inconsistency of studies' results and how informative would reporting the estimate be for the readers.

In contrast, in response to your suggestion, we have newly included the R-squared in Table 3.3 for the OLS regression as a measure of the proportion of total variation that can be

explained by the included regressors. Correspondingly, we have updated the main body text of that chapter that refers to that newly included measure, e.g., see the quote below.

" Thus, our results identify nine key characteristics that are essential for explaining the heterogeneity in reported alphas. And the R-squared in the OLS regression indicates that these nine variables explain 23% of the variability."

REED: I still think I-squared should be reported in the text.

Chapter 4: The Impact of Regulatory Change on Hedge Fund Perfor- mance

1) better to say a "smaller drop" instead of a "less drop".

Thank you for pointing this out. I have changed the wording, as you suggested.

*"H2: EU Hedge funds domiciled in countries with strict regulations before the AIFMD would experience a **less drop** than the hedge funds domiciled in countries with lax regulations."*

REED: I am satisfied with the response.

2) On page 131, second and third lines from the top: Shouldn't "experienced a negatively significant influence from 0.08 to 0.15" be "0.03 to 0.15?

Thank you for pointing this out. Indeed, we overlooked this misleading formulation in the text. In response to your comment we have changed the text to the following:

*"Our results, obtained from excess returns, three-factor model, and four-factor model as the measurements of hedge fund performance show that within the EU, the managers of larger funds and nonUCITS funds that are subject to AIFMD experienced a negatively significant influence from **0.03 to 0.15** compared with managers of funds with exemption while the one-factor model and FH model do not convey such significant result to support H1."*

REED: I am satisfied with the response.

3) On page 131, seven and eight lines from the bottom: Shouldn't "the EU funds without exemption experienced a drop varying from 0.17 to 0.29" be "0.06 to 0.29".

Thank you for pointing this out. Indeed, we overlooked this misleading formulation in the text. In response to your comment we have changed the text to the following:

*"It shows, based on the alphas from risk factor models, the EU funds without exemption experienced a drop varying from **0.06 to 0.29** in performance after the rule compared with the corresponding NonEU funds of other areas."*

REED: I am satisfied with the response.

4) It has been a while since I have done PSM, but I think it is generally considered good practice to report a figure that shows the degree of overlap between the predicted probabilities of "treatment" between the treatment and control groups

Thank you for the comment. I agree that it is indeed advisable to include such a figure. I added Figure 5.2 to show the distributions of propensity scores of the treated and control groups before and after matching. The figure clearly shows that the PSM increased the similarity of the characteristics of the treatment and control groups. Provided that my PSM- based results are qualitatively similar to the main results based on the DDD, this indicates that the empirical findings I present in this chapter are unlikely to be driven by fundamental differences between the hedge funds in my treatment group and those in the control group.
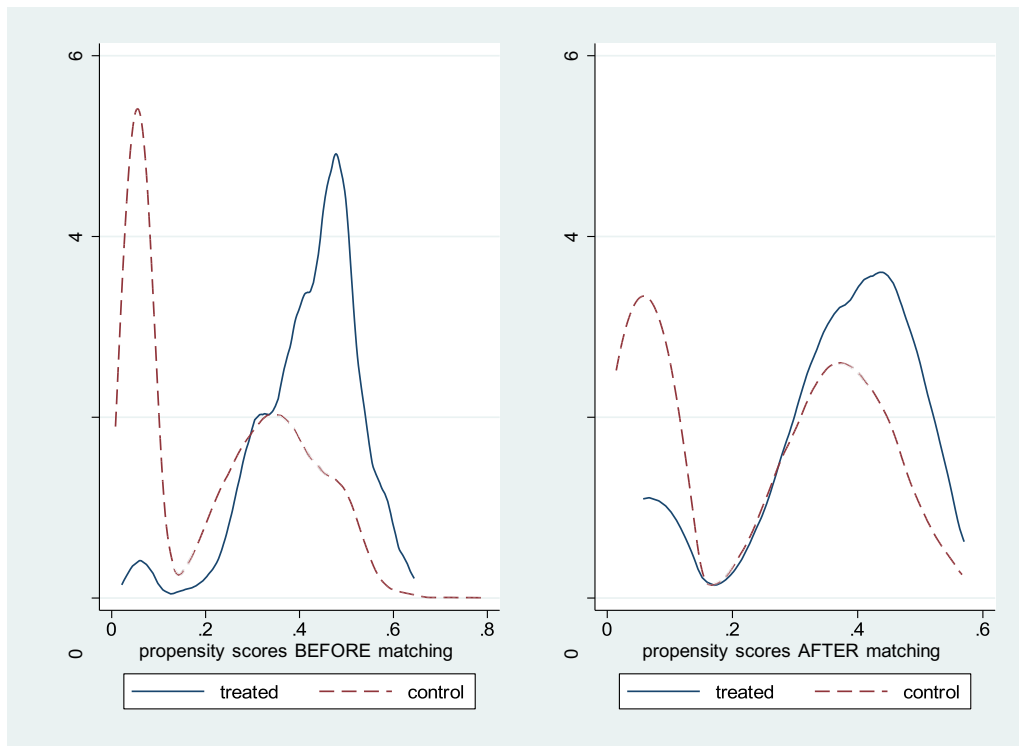
REED: I am satisfied with the response.

5) It would be nice to provide more details about the kind of matching that was done. Is it the nearest neighbor? With replacement? Without replacement? If with replacement, was any allowance made for correcting standard errors due to using the same controls?

Thank you for the good point. I have added the following explanation to provide more details on how I applied the PSM methodology in my research setting.

"First, we exactly match the date and investment strategy to make sure the significant factors, including the time period and strategy, do not influence the funds' performance. Then, we use the funds' characteristics to obtain their propensity score to match the treated observations with control variables using the nearest-neighbor method without replacement. To ensure the quality of matches, we set a caliper of 1%."

REED: I am satisfied with the response.

6) In the interests of open science, Yang might think of making the data and code publicly available so that the results of the chapter are push-button replicable.

We agree that to facilitate future research in this area and to ensure replicability of the published results it is desirable to make the data and code publicly available. My coauthors routinely do so for the meta-analyses they publish. Nevertheless, this study is based on data from the Eureakhedge database, which we purchased specifically for this project with the use of funding kindly provided by the Charles University Grant Foundation (GAUK). The contract with the data provider allows me to use the data for research purposes and publish aggregated results, but it does not allow us to make the observations for individual hedge funds publicly available.

REED: I am satisfied with the response.

Date:	September 15, 2024
Opponent's Signature:	
Opponent's Affiliation:	Prof. Robert Reed Ph.D. (University of Canterbury)