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November 18, 2025

GIPS Standards Technical Committee
CFA Institute
915 E High St.
Charlottesville, VA 22902

Re: Exposure Draft Guide for Best Practices in Return Attribution Reporting

Dear Sir/Madam:

“I’m going to speak my mind,
because I have nothing to lose.”
Semanticist E.I. Hayakawa

Thank you for the opportunity to offer comments on this document. This letter represents solely my opinions; the views disseminated within it are my own alone, or those I reference, and are not to be considered, in any way, the opinions of my firm¹ or any committee of which I am a member.

This is a subject that was first considered by CFA Institute² in 1997 (see Caccese (1997)). As Mike Caccese recently explained to me, this was dropped because of other priorities. Now, nearly three decades later, it has resurfaced.

You have elected to venture into an area that is fraught with controversy. In Spaulding (2004) I coined the “3 Cs of performance measurement”: confusion, controversy, and change. And attribution is filled with controversy, perhaps more than any other area of performance measurement. For example,

- Whether one should use geometric or arithmetic
- Whether you should report the interaction effect
- Whether transaction- or holdings-based is better.

This document demonstrates opinions in at least one of these, which only adds

¹ TSG will submit a separate, and no doubt much shorter, response.

² AIMR, or the Association for Investment Management and Research, at the time.

to the controversy and perhaps confusion. One might even suggest the paper is a *tad* biased in some points. While there are many books and articles to draw upon, you've limited yours, which perhaps adds to some of the bias that appears within. Was this a truly objective view? It's uncertain at the moment.

Preliminary comments

As is often the case, I have some remarks to share regarding the text, itself. I hope you find them of interest and value.

Previous though unreferenced works

While the guide's bibliography references the European Investment Performance Committee's 2004 paper, nowhere within the document is it cited. It is my understanding that all works cited within an article are to be in the bibliography; likewise, all items in the bibliography are to be cited within the article. This particular document, not being cited, may very well have been an oversight, and arguably an unfortunate one, at that.³ I did confirm that all other items referenced in the bibliography are cited.

It is unfortunate that the author(s) not only failed to cite this previous work, but also a proposed standard that was developed by the Performance Measurement Forum (see Spaulding (2002/2003)). Surely, these previously developed documents should have been considered for this undertaking.

As noted below, there are many works, including books and articles, that could have been used and cited, to perhaps provide a broader perspective. I think it is unfortunate that this wasn't done.

Is geometric attribution interaction effect free?

On page 11 we find, in referencing a book by Carl Bacon: "Finally, the geomet-

³ I Googled "are references in a bibliography to be cited within an article," and received the "AI Overview" which explained "Yes, every reference in a bibliography (more accurately, a reference list or works cited list) must be cited in the body of the article, and every citation in the text must correspond to an entry in the reference list."

ric method does *not calculate a separate interaction*. Instead, the interaction is included in the calculation of the selection effect.” <emphasis added> This seems to support the invalid premise that geometric attribution does not have an interaction effect; but, it can. As with the arithmetic version of Brinson-Fachler⁴ (and Brinson-Hood-Beebower, for that matter), the firm can use the benchmark weight for selection, and have an interaction effect, or use the portfolio weight, and combine interaction with selection.

While you cite one of Carl Bacon’s books, he acknowledged to me that geometric attribution can have interaction. Granted, Carl, not being a fan of this effect, avoids it, but it can exist in geometric attribution as it can in arithmetic.

To further demonstrate this, please consider Menchero (2000/2001), who provides a geometric model and states that interaction can be incorporated.

Spaulding (2003/2004) explains that Carl’s model is essentially the geometric version of Brinson-Fachler, and that an interaction effect can be employed in it if one wants it. The choice to include an interaction effect in a geometric model is no different than in using an arithmetic model. Please refer to Appendix A for a detailed explanation.

Treatment of interaction effect.

On page 11 we find “the interaction should be combined with the selection effect or shown separately.” I heartily agree. Spaulding (2008) explains how interaction can be evaluated in order to treat it more appropriately.

The employment of a currency effect

On page 8 we find “currency effects should be presented *only when* currency decisions are part of the investment decision-making process.” <emphasis added> I do not understand the basis for this. In my view, currency effects should always be presented whenever a portfolio is comprised of assets with local currencies that differ or if the asset’s currency(ies) differ from the portfolio’s base currency.

Consider a simple case, where a U.S. based investor holds both USD- and

⁴ Bacon’s model is a geometric form of Brinson-Fachler.

Euro-denominated securities. The Euro-denominated can obviously be converted to USD for reporting. And so, the 1,000 shares held in BMW stock will be shown in USD. However, the movement of these Euro-denominated securities will be based on both the local market and the changes between these two currencies. To ignore currency would be to attribute these currency-driven changes to the selection effect.

A rather simple, naïve formula⁵ is:

$$CurrencyEffect = \left[w_{P_i} \times (r_{P_i}^b - r_{P_i}^l) \right] - \left[w_{B_i} \times (r_{B_i}^b - r_{B_i}^l) \right]$$

where:

l = local currency

b = base currency

P = portfolio

B = benchmark

w = weight

r = return.

Unintended cross-product

On page 11 we find: “In certain models that use the arithmetic method, the arithmetic calculations of the allocation and selection effects may create an unintended cross-product called interaction.”

What is meant by “unintended”? While interaction may not be “part of the investment decision-making process” *per se*, it is the result of two decisions: allocation and interaction. The inclusion of interaction is one of the areas of attribution where there are strong opinions on both sides. This paper should be objective, not biased. The wording you have chosen demonstrates a bias against interaction, which, in my view, is inappropriate for its purpose.

Geometric attribution and residuals

While you correctly cite both Bacon as well as Burnie, Knowles, and Teder, you failed to include Menchero (2000/2001). Menchero makes it clear that

⁵ The original source for this model is unknown to me.

geometric models, while not experiencing residuals through linking, as arithmetic models do, does affirm that they *require a smoothing agent* for their single period calculations.⁶ Bacon uses such a smoothing agent for his selection effect, which is also needed for interaction.⁷

On pages 13 and 14, you provide a detailed explanation on residuals. What is missing is that geometric attribution requires a smoothing agent in order to avoid having a single period residual.

Let's contrast the arithmetic selection effect with Carl's:

$$StkSel^G = w_{P_i} \times \left(\frac{(1+r_{P_i})}{(1+r_{B_i})} - 1 \right) \times \left(\frac{1+r_{B_i}}{1+R_s} \right)$$

$$StkSel^A = w_{P_i} \times (r_{P_i} - r_{B_i})$$

The reader can, no doubt, observe that:

- the arithmetic (A) and geometric (G) weights match up
- and the return differences do, as well,
- with the only difference being the geometric form expresses the difference geometrically (by division), while the arithmetic arithmetically (by subtraction).

We also see Carl's method includes an additional factor, that uses his "semi-notional return," which is absent from Brinson-Fachler. This agent is necessary to avoid a residual. I.e., this is a smoothing effect. Without it, we will not reconcile to excess return.

⁶ I feel it important to stand up for all who prefer the arithmetic model, to point out that geometric has its shortcomings, as well.

⁷ While preparing for the CIPM exam, some 20 years ago, I reached out to Carl to better understand his model. I couldn't comprehend one of the terms in selection. He explained that in order to get the math to work, he needed to use this factor. This factor is not found anywhere in the arithmetic version of Brinson-Fachler.

What this means is that both arithmetic attribution and geometric attribution are subject to residuals: geometric for single periods and arithmetic for multiple periods. But for each there exist smoothing methods to eliminate them.

What about linking?

On page 10 we find “Arithmetic excess returns are simple to calculate and easy for users to understand. Arithmetic excess returns, however, do not compound—that is, they do not ‘add up’ over multiple periods. This combination of an arithmetic operation and a geometric operation (compounding) creates a residual, discussed in a separate section.”

Yes, it is true, arithmetic models are “linking challenged.” However, countless methods have been developed to “smooth out” these effects. While you reference this on page 14 (“Firms may choose to eliminate residuals by using a smoothing or linking algorithm in their attribution model to automatically allocate a residual to one or more other effects”), you fail to reference any. For this paper to have value, I suggest you cite, at a minimum, the following: Cariño (1999), Menchero (2000), and Frongello (2002).

Composite attribution must be aggregated

On page 3 we discover that if one wants to calculate and report attribution for a composite, it “must be aggregated.” Why?

First, Spaulding (2010/2011) provides a fair amount of general criticism for the aggregate method. As is pointed out, the aggregate return fails to match the definition of a composite return, and so should never have been allowed. To require that composite attribution be done against the composite’s return generated in an aggregate method almost guarantees the return will not align with the composite’s, if it was calculated using an asset-weighted approach. Furthermore, has the notion of asset-weighting the results of asset-weighted attribution been tested and dismissed? I will confess I have not looked into this, but would prefer to do so before abandoning it completely, as you are doing. If you have evidence that it will not work, please share it.

I think it is interesting to note that the word “must” only appears within this document three times, and two of those times relate to the GIPS standards. As for

this “guide,” the word appears only in regards to the case above. On the other hand, the word “should” appears 150 times. Therefore, one must conclude that none of those “should’s” were worthy of being elevated to being a “must,” and the authors felt that for the composite calculation, a “must” was warranted. I ask that this be reconsidered, unless, of course, you have demonstrated that any alternative method of deriving a composite’s attribution would be invalid.

Net of fee attribution

On page 4 you state that “Attribution can be calculated using gross-of-fees returns or net-of-fees returns.” I would say that *technically* you are correct. However, I am unaware of any sound method calculate net-of-fees attribution. Yes, some firms will create a “fee effect,” to go along with their other effects, so that everything ties to the net-of-fees return. But this is far from appropriate.

You mention “Local regulation.” I suspect you may be referring to the SEC Marketing Rule. There has been much, make that “a great deal,” of confusion as to whether firms must calculate net-of-fee attribution. It is my understanding that this is not required. Perhaps it would be inappropriate to go so far as to state this within the document, but I’d mention the challenges of doing true net-of-fee attribution as another justification to avoid it.

Speaking of regulations

Elsewhere, you reference the use of a representative portfolio for attribution. As you know, with the new SEC Marketing Rule, the use of representative portfolios is restricted. I would hope they will allow a firm to use this, but as for returns, they require that the composite the representative portfolio is in accompany any such reporting. Whether they would require composite attribution, as well, is unknown. You may want to reference regulatory restrictions regarding rep portfolios.

Risk Attribution

First, by risk attribution, I do not mean attribution that focuses solely on risk, but rather an attribution model that seeks to identify the contribution of risk to the excess return, resulting in a “risk effect.” See, for example, Ankrim (1992), Kophamel (2003), Menchero (2006/2007) and Spaulding (2016).

Finally, in response to your questions

Question 1: Should firms disclose that the policy for selecting representative portfolios is available upon request?

Yes. And also, if they're using a composite or rep portfolio.

Question 2: Does your firm show attribution for periods greater than one year? If so, what is the longest period shown? Are there challenges with presenting attribution periods greater than one year that are not addressed in this Guide?

N/A. This is an exceptionally interesting question; it is a topic that has been on my "to do list" for quite some time. I look forward to reading your responses.

As for the last question, I would say "yes," there are challenges. That is part of my interest in pursuing this topic. First, how is one supposed to understand the results, if they are spread across a long time period, where in some months allocation may have been superior, while in others, selection (or, dare I say it, interaction)? Guidance is needed here. Still something I want to look into.

Question 3: For periods greater than one year, does your firm calculate attribution on an annualized or cumulative basis? What factors influence your decision?

N/A. That said, I am unaware of ways to annualize attribution. We annualize returns, but attribution? Something to reflect upon.

Question 4: Is there any other information related to cash that firms should disclose?

The benchmark won't have cash. And so, what do we do with the portfolio's or composite's weight and return of cash?

Spaulding (2017) mentions how Campisi (2008/2009) finds a benchmark to serve as a proxy for cash, resulting in both allocation and selection effects; Spaulding prefers to treat cash as an allocation decision. How to align anything that is not in the portfolio/composite is something that should be considered, and probably included in the firm's P&P.

Question 5: Is there any other information about currencies that firms should disclose?

If the firm does not make currency decisions, then a naïve method to derive the currency effect is probably sufficient. However, if currency management is employed, then a model such as Karnosky-Singer (1994) should be used, as it bifurcates the currency effect into the impact from market and currency management. As noted above, if the portfolio or composite has multiple currencies represented, regardless of any currency management, a currency effect should be calculated.

Question 6: Please share if there is any additional information about the treatment of leverage and derivatives in return attribution that should be disclosed.

As with cash, how the weights and returns are to be handled is important. These can be labeled as “off-benchmark-bets,” with their effects simply shown, with no representation on the benchmark side, or to find a proxy(ies), as Campisi would, I believe, recommend.

While the work you’ve begun has value, I am concerned with its lack of objectivity. Topics such as geometric vs. arithmetic, the inclusion/exclusion of an interaction effect, and the existence of residuals in arithmetic but not geometric models, is a bit troubling.

I am hopeful that the next version will be more objective, and perhaps with a greater representation of materials from the rather extensive and broader performance attribution literature that is available. There are many experts on the various aspects of attribution, several of whom, I am sure, who would be willing to participate in this effort. I, for one, would be pleased to join in.

Sincerely,

David D. Spaulding, DPS, CIPM

Appendix A: A Demonstration that Geometric Attribution is not Interaction-free

Source: Spaulding (2003/2004)

What about Geometric Attribution?

Some people think that geometric attribution is interaction free; that it doesn't have an interaction effect. This interpretation is incorrect. Having an interaction effect is optional, just as it is with arithmetic models.

For example, if we refer to the article Carl Bacon wrote on his model (Bacon, 2002), we find the following formula to be used for security selection:⁸

$$StkSel = W_p \times \left(\frac{(1 + R_p)}{(1 + R_b)} - 1 \right) \times \left(\frac{1 + R_b}{1 + R_s} \right)$$

where

R_s = semi-notional return

R_p = portfolio return

R_b = benchmark return

W_p = portfolio weight.

Carl's method is essentially the geometric implementation of the Brinson Fachler model. And, as with its arithmetic equivalent, we have the option of having a separate interaction term or grouping interaction with selection, simply by deciding whether to weight the excess return by the benchmark or portfolio weight. If we want to show interaction, we simply substitute the benchmark weight:

$$StkSel = W_b \times \left(\frac{(1 + R_p)}{(1 + R_b)} - 1 \right) \times \left(\frac{1 + R_b}{1 + R_s} \right)$$

What would the interaction effect be geometrically? I apologize that I haven't had the time to validate this but believe it would be:

$$Interaction = (W_p - W_b) \times \left(\frac{1 + R_p}{1 + R_b} \right)$$

which is actually the way the arithmetic version of the BHB defines interaction. Consequently, going geometric will not eliminate the presence of an interaction effect.

Please note: the author has validated that this form of Interaction does, in fact, work. Further proof can be provided.

⁸ See page 29 of Carl's article. (Bacon, 2002) His notation is slightly different than mine, but accomplishes the same thing.

Appendix B: References

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