



An Uncritical Glorification of Hedge Funds

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Sebastian Mallaby's 2010 book about the hedge fund industry, *More Money Than God*, is a disappointment. Mallaby worked on the book for three years, conducting hundreds of interviews. He credits his agent with steering him to "grapple harder with the rich variety of thought that explains the success of hedge funds." As the director of the Maurice R. Greenberg Center for Geoeconomic Studies at the Council on Foreign Relations, he had plenty of support. He benefited from a series of dinners with investment experts, convened by Robert Rubin, to discuss portions of his manuscript. The book should have been an eye-opener.

But somewhere along the way Mallaby got captured by the industry, and in the end, he comes off as a hedge fund industry lobbyist. The book does shed some light on interesting events in hedge fund history and is strewn with a few valuable insights. Mostly, though, it is a work of serial hagiography. It seems designed to attract worshippers like those who drive by celebrity homes in Beverly Hills.

"Hedge funds" or "a few hedge funds"

A major criticism is that the book does not adequately distinguish between hedge funds in general and the very small number of hedge funds – about a dozen or so – that the book does cover. There are at least 8,000 hedge funds in existence. As Mallaby himself writes, 5,000 hedge funds went out of business between 2000 and 2009. So over the last 10 years, there have been at least 13,000 hedge funds. The small number of unusually large and successful ones – a disconcertingly large percentage of which actually crashed and burned – are certainly not representative of the entire population of hedge funds.

Mallaby does mention many of the doubts about hedge funds that have been raised. In almost every case he dismisses these doubts with weak or non-existent counterarguments. Toward the end of the book – in almost the only place where he addresses the distinction between prominent hedge funds and all hedge funds – he asks, "But in its focus on the pioneers who shaped the industry, a history of hedge funds is necessarily biased toward winners. Perhaps the average hedge fund that attempts these strategies loses money?"

He answers this by saying that the best evidence comes from a 2010 paper by Roger Ibbotson, Peng Chen and Kevin Zhu, which states that the average hedge fund earned 7.7% net return between 1995 and 2009, after correcting for survivorship and backfill bias



– including 3 percentage points of alpha. Mallaby fails to mention that the 7.7% return trailed the S&P 500 return over the same period. Granted, the authors calculated that risk-adjusted, the hedge funds earned an alpha of 3%. But they also caveat in a footnote that many hedge funds hold hard-to-price illiquid securities, which are priced in a manner that makes their movements asynchronous with monthly S&P 500 returns. This leads to understated estimates of market exposure. While the authors attempt to take account of this in their calculations, these securities contaminate the data in a way that could reduce betas and increase alphas.

Yet in other places Mallaby trumpets the returns of successful hedge funds by comparing them directly with S&P 500 returns. Why doesn't he mention that comparison here? Also, he does point out the problem of illiquid assets elsewhere in the book, explaining that they make hedge fund managers "smooth" their returns, artificially reducing their volatility. Why does he not mention this here?

The answer seems to be that Mallaby has become a salesman for hedge funds. Salesmen don't go out of their way to mention negative things about their products.

The interesting parts

That said, Mallaby does provide some interesting – and occasionally exciting – narratives about the peak experiences of famous hedge funds. In places, the narratives help to explain the excellent returns that these funds achieved and why such funds can be capable of achieving such returns.

Mallaby covers the campaigns of George Soros, Stan Druckenmiller and others against the British pound and the Thai baht. The stories show how a macro strategy, focusing on the wasteful things governments sometimes do, can yield excess returns. Governments occasionally engage in fruitless efforts to keep their currencies in fixed ratios to other currencies, throwing money against the winds of efficient markets in the process. Hedge fund managers like Soros merely act as the agents of these winds, gobbling up the wagers of governments that are determined, for one reason or another, to make bad bets.

Mallaby points out that not only governments, but also institutions, can be straightjacketed by constraints that cause them to throw money against market forces. For example, some institutional funds have requirements that the bonds they hold must be investment grade. Hence, when there is a downgrade, these funds must – predictably – sell the bonds en masse. Other institutional straightjackets are a little softer, but knowledgeable players like Tom Steyer of Farallon or John Paulson (before his big 2008 success) could take advantage of them. In the case of Steyer, he knew more than most other people about antitrust regulations and regulators and had a better chance than others of guessing right in merger arbitrage situations. John Paulson knew a lot about the legal nuances of the seniority structure of the various bonds issued by a corporation and was able to profit in that area.



And as to Paulson's truly extraordinary performance in 2008, Mallaby remarks in his acknowledgements that "John Paulson's memo to his investors laid out the logic of betting against the credit bubble so clearly that it makes you wonder how anyone could have been on the other side." Well yes, it does make you wonder.

The asymmetric bet

Mallaby mentions repeatedly what we assume good hedge fund managers look for assiduously – an asymmetric bet. A manager looks for situations in which the odds are "heads I win big; tails I break even or lose just a little." Such an asymmetric bet might be made when everything looks stable but there is a hidden danger of a sudden crash. If one can make a number of asymmetric bets, there's a high probability that one will win big on average. Hedge fund managers have more flexibility to place these kinds of bets than do, for example, mutual fund managers.

But as we've seen, even apparently asymmetric bets can go badly wrong. Both George Soros and Julian Robertson failed in the tech bubble market of 1998-2000 by trying to go against what they thought was the lunatic tide before finally giving up and diving in – at just the wrong time.

The tendency to crash

In fact, as I mentioned, a disconcerting number of these funds crashed with terrible returns in their final year or two. This applied to funds run by Michael Steinhardt, Julian Robertson, George Soros, and others. But while some funds, such as Long-Term Capital Management, Amaranth, Sowood and Askin, are now known principally for having crashed and burned, Steinhardt, Soros and Robertson are still considered hedge fund gurus.

This raises an uncomfortable question. Managers' investment talents have long been measured by their so-called time-weighted rate of return, ever since it was recommended as the proper measure in an influential 1968 study by the Bank Administration Institute, [Measuring the Investment Performance of Pension Funds](#). The time-weighted return is considered the proper measure because it is unaffected by investors' inflows and outflows, which are assumed to be irrelevant to how well the fund is managed.

The result, however, is that managers can rack up a record of outstanding time-weighted returns that lasts for years, even if their market-beating returns occurred only in their early years when they were managing a small asset base.

The bulk of investor funds typically flow in after that stellar record has been achieved. In the case of hedge funds and probably mutual funds too, it becomes much more difficult to produce positive outlier returns when the fund becomes massive. In some cases the



managers try to behave as they did when they could manage a fund nimbly, but they crash when they find they can't move quickly enough.

To demonstrate this situation, I created a hypothetical fund that starts with only \$20 million and racks up 30% returns for nine years. As news of its spectacular performance spreads, new funds flow in from awed investors at an accelerating rate, with new investments increasing 100% a year. In the tenth year the fund suddenly experiences a negative 55% return. The following table shows the results:

Year	ROR	New investment (\$ millions)	Assets end of year (\$ millions)
1	30%	20	26
2	30%	40	86
3	30%	80	216
4	30%	160	488
5	30%	320	1,051
6	30%	640	2,198
7	30%	1,280	4,521
8	30%	2,560	9,206
9	30%	5,120	18,623
10	55%	10,240	12,988

The time-weighted rate of return is 16.9%, appearing to show that the manager still performed extremely well over the 10-year period in spite of the last year's drop. But the dollar-weighted return is -22.4%. Hence, the average dollar invested in the fund lost 22.4% of its value annually.

Mallaby does address this issue in an endnote, observing, "Some commentators have suggested that [Michael] Steinhardt's dollar losses in 1994 were so large as to outweigh the gains over the rest of his career." But Mallaby responds to this by simply saying, "This claim is not borne out by an examination of Steinhardt's internal records." This is an inadequate response to a serious reservation.

Failure to explain

In several cases Mallaby took his book agent's advice to "rethink my original proposal and grapple harder with the rich variety of thought that explains the success of hedge funds," but he still fails to explain those successes. Perhaps some instances of outstanding performance simply can't be explained. Mallaby takes pains to prove, and succeeds in proving, that Julian Robertson's Tiger Management and its offshoots truly did outperform the market (albeit without accounting for the reservation I just stated about the measure



and meaning of outperformance). He convinces us that Paul Tudor Jones outperformed the market. And we all know about James Simons' legendary Renaissance Technologies hedge fund, which has averaged 35% annual returns after fees in the last 20 years while charging unusually high fees. But when it comes to explaining why or how these three outperformed, Mallaby comes up short.

In the case of Jones, it seems that he just has greater poker-playing skill than the other players – he can tell when to hold and when to fold, and he can psych out when others are going to hold or fold. It is possible. Mallaby's book makes it plain that a lot of market movements are really driven by the herd movements of these hedge fund players – if you think ahead of the herd or cunningly drive their movements so as to take advantage of them, you'll come out ahead.

When it comes to Robertson, Mallaby really doesn't explain – Robertson didn't do anything fancy, he was just a stock-picker. There may be a clue in this statement: "Tiger's roster of investors was crammed with captains of industry and finance, and Robertson never hesitated to call on them for insights." A little subtle tapping of insider information perhaps?

But there's also this: "To those who watched Robertson up close in his heyday, there was no doubt about his talent. He could drop in on a meeting with a chief executive and demonstrate a grasp of company detail that rivaled that of the analyst who tracked it. He could listen to a presentation on a firm he knew nothing about and immediately pounce on the detail that would make or break it." This is the sort of skill we attribute to Buffett; maybe it truly exists and only a few have it.

When it comes to Renaissance, Mallaby adds a little but not much. Apparently its managers really do just look for data patterns, but with extraordinarily intense and expert diligence. The best Mallaby can offer – which is actually not that bad when you think about it – is this Zen-like explanation from Renaissance executive Robert Mercer: "The signals that we have been trading without interruption for fifteen years make no sense. Otherwise someone else would have found them."

Mallaby's argument against regulation

Mallaby has come out as an opponent of hedge fund regulation, both in this book and in op-ed articles he has written. But in the book his argument is that hedge funds shouldn't be regulated because they aren't too big to fail — until they do become too big to fail, which they probably will. He also argues that they self-regulate well because most of them have their own founders' wealth invested in them — until they go public, as can and has happened, in which case the founders' skin is no longer in the game.

His main hedge fund defense is that hedge funds aren't as systemically risky as banks – at least not until they get as big as banks. This is a defense? He is right in saying that hedge funds pose less systemic risk because they are generally not as highly leveraged as



banks. But there are exceptions, such as Long-Term Capital Management, that do pose a systemic risk.

Mallaby raises and then answers several criticisms that have been lodged against hedge funds. In answer to the complaint that they contribute to the “troubling gap in modern society between the superwealthy and the rest,” Mallaby responds that “the answer is not to smother their trading. It is progressive taxation.”

Has he, therefore – like Warren Buffett – written prominent op-ed pieces calling for closing the tax loophole that awards hedge fund and private equity managers a regressive tax rate by allowing them to pay only long-term capital gains taxes? If so, I’m not aware of it.

The social question

Mallaby may think hedge funds are unfairly castigated in some quarters and that they need defending. Perhaps he may be right, but I’m not personally aware that they receive the critique they deserve – especially the idea that hedge funds in general are great investments.

But a defender who is nevertheless objective – as we would expect a researcher for the Council on Foreign Relations to be – needs to acknowledge the blemishes better than Mallaby does. He touts the fact that hedge funds have allowed universities to afford more facilities and scholarships and philanthropies to give out more money. Where does the money come from? If hedge fund managers beat the market by being better poker players than others, they must take the money away from suckers – which might be mutual funds, public pension plans, individual investors or taxpayers, when the suckers are governments — in short, masses of ordinary people who can’t defend themselves.

Mallaby should but doesn’t address this question: Do hedge funds amass wealth only for an elite in-group, including the Yale endowment, the wealthy hedge fund managers and a few other well-placed investors who are able to access the few market-beating hedge funds while subtracting equal wealth in other places? Or do they actually create net social wealth? If they do not do the latter, what good are they?



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