Great Expectations

At Lindsell Train we spend a lot of time reading. Company reports, broker notes, media commentary, academic papers, business and investment texts; anything that we think might supplement our own analysis with external insight. To this end, I have, on occasion found myself reaching for a book on management strategy - a much maligned but rather prolific branch of nonfiction. The field has its shortcomings, but the best, if not quite page turners, are serious attempts at cracking a worthwhile yet particularly resilient nut: How to identify, and ultimately sustain 'greatness'.

In essence each volume reviews a series of company case studies, documenting previously successful management approaches, in an attempt to synthesise best practice from what's worked in the past. How-to guides for building and running great businesses. It's a noble aim, but a tough one to deliver on. Like much in economics, findings tend to rely on back-tested or mined data, suffer from an absence of control trials, and should be approached with caution. First, simply identifying appropriate role models can be challenging, meaning we often don't know if the 'great' companies profiled are intrinsically so, or have just been lucky, making them pointless to analyse (more on this below). Second, even if we can convince ourselves we're looking at genuine and deserving success stories, there are more pitfalls waiting when trying to work out how these were forged. For example, selection bias (if only winners are studied, we won't know whether failed companies also followed the same lauded practices) and causal fallacy (does good practice lead to success or vice versa?) have tripped up many a well meaning researcher, with 'correlation implies causation' a surprisingly easy trap to fall into. *Reductio ad absurdum*, all successful companies operate from buildings and the more successful, the bigger the building.

This can be hard to control for (particularly where survivor bias limits access to data from extinct firms), whilst narrative fallacy (whereby stories, even if misleading, tend to be more believable than facts) can further muddy the waters. Even if they can be derived, magic formulae that have worked for one business, at one instant in time may not translate between eras, sectors, companies or even people as each face their own unique challenges. As I say, it's a tough gig. Happily, these hurdles aren't wholly insurmountable (overcoming them is an important part of machine learning) but a truly robust test of any resultant hypothesis requires forward-looking, multi-year studies with sufficient sample sizes and relevant control sets. A bit of an ask for someone with a best-selling book to write.

Nevertheless, several authors have found fame and fortune attempting some approximation of the above. The best known works from the canon include Jim Collins' *Built to Last* (written with Jerry Porras) and *Good to Great* books, both of which reputedly sit pride of place on the bookshelf (or perhaps Kindle) of Amazon's Jeff Bezos. In each, Collins collates a list of top performers and tries to pinpoint the key factors leading to the creation of these 'sustainably great' businesses. Appealing stuff for managers and investors alike - except that as others have already noted, the subsequent results have been less than encouraging. *Good to Great* was published in 2001, meaning enough time has passed to appraise Collins' claims with some forward-looking hypothesis tests of our own. For example, in the subsequent years the shares of his sample of 'great' companies performed, at best, in-line with S&P500 index of average ones. (The median performance of the 10 companies detailed, yields an 8.1% total return pa from 16/10/01, the date of the book's publication, to 01/10/18 vs. the same 8.1% figure for the S&P. Following similar reasoning, the companies from Built to Last, published 26/10/94, narrowly underperformed with a median return of 9.9% pa vs. 10.1% pa. This updates similar tests performed by others.) The selection process required wide share price outperformance over the preceding 15 years, yet the list ended up a mixed bag of both hits (e.g. Philip Morris and Nucor) and misses (e.g. Fannie Mae and Circuit City).

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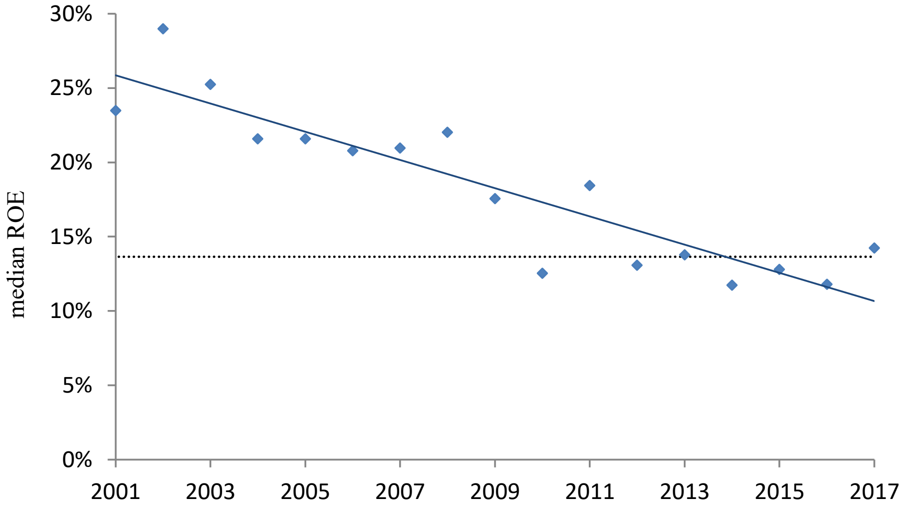
Of course share price performance, even over relatively extended periods, doesn't tell us everything. Collins isn't claiming prowess as a stock picker and the shares could simply have been expensive at the time of publication. However the underlying business record of his companies hasn't been that great either. Notably the median returns on equity (ROE) of the *Good to Great* set have fallen by over a third from an initially stellar 23% in FY2001 to a fairly middling 14% by FY2017, whilst the average across the S&P actually grew to match this from its post dotcom low of 6%. This looks suspiciously like mean reversion - as illustrated by the following chart which plots the median ROEs of the *Good to Great* companies since the book's publication (with the black dashed line showing the S&P 500's current level).



Median ROEs for the 10 *Good to Great* companies (namely Abbott Labs, Circuit City, Fannie Mae, Gillette/P&G, Kimberly Clark, Kroger, Nucor, Philip Morris/Altria, Walgreens and Wells Fargo) since 2001, the year of publication. R^2=0.81. The black dashed line shows the S&P500's 2017 level of 13.6%. Source Bloomberg, Jim Collins & Lindsell Train.

So perhaps Collins fell at the first hurdle and his selection criteria for greatness wasn't stringent enough? Or if his companies really were great, then this doesn't appear to have been uniformly maintained. Either way, it seems neither the greatness he documented, nor his formula for it, were as sustainable as hoped. This apparent body blow provides ample fuel for those who regard this entire prolific field as more 'pseudo' than science and I'd recommend following up here with the insightful 2009 Deloitte article *A Random Search for Excellence* (also published via the more rigorous journal article *How long must a firm be great to rule out chance?* Henderson et al. 2012), which uses statistical arguments to diagnose flaws in this and other well known studies.

But perhaps the criticisms go too far. Whilst it would be a mistake to embrace Collins' roadmaps as definitively prescriptive, his intent is laudable. 'Sustainable greatness' is a valuable thing to unearth, it's just that it also tends to be rather hard to spot. We should know - we spend much of our time trying.

The important question then is whether we're all wasting our time. Is sustainable greatness even possible, let alone identifiable? Arguing one side, Phil Rosenzweig, known for his 2007 book The Halo Effect (a direct critique on management strategists), is clear and damning with his take. To quote him in some length:

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"The delusion of lasting success is a serious matter because it casts building an enduringly high-performing company as an achievable objective. Yet companies that outperform the market for long periods of time are not just rare but statistical anomalies whose apparent greatness is observable only in retrospect. More accurately, companies that enjoy long-term success have probably done so by stringing together many short-term successes, not because they somehow unlocked the secrets of sustained greatness. Unfortunately, pursuing a dream of enduring greatness may divert attention from the need to win more immediate battles."

So is he right? Alas this is one of the many finance questions that can't be answered definitively, but both intuitively and empirically we believe not. In truth, this belief underscores our whole investment philosophy. Ok, we agree that the *average* company is probably just that, but we'd argue that for the exceptional few, the 'statistical anomalies', greatness can be both enduring and predictably so. We're not alone here; to summarise the findings of the above referenced Deloitte study, the authors (whilst critical of many of the methods employed to find them) eventually concede that the prevalence of 'defensibly exceptional' firms is high enough so as to be unexplainable by chance alone.

Similarly, whilst the median tally for the *Good to Great* group may have disappointed, within the mix there were some standout successes - not least tobacco company Philip Morris (aka Altria from 2003) which since 2001 has both conjured a 12-fold total return for its shares and maintained ludicrously high ROEs (never dropping below 30% throughout the 23 years for which we have Bloomberg data). The *Built to Last* companies also appear to have 'lasted' a lot better than those from *Good to Great*, at least according to our ROE test; with median ROEs of 20% in 1994 enduring and even growing to an impressive 22% by 2017[1]. In other words, when assessing Collins' record, beware incomplete data sets - he really did have some great companies in his repertoire, but perhaps, needed to apply a bit more qualitative selectivity to his quantitative screens. This may be where the 'art' element of stock picking takes over.

So, to what extent have we succeeded in our quest to unearth these elusive enduringly-high performers? Taking our Global Equity representative portfolio from its start in 2011 the component 24 companies had a weighted average ROE of 18%, which at the time was well above the 11% figure for the MSCI World Index as a whole. That's a nice start, but a single year doesn't tell us much. We could then point out that at launch, the median ROEs for those companies had been higher than the Index's average for all but two of the preceding 20 years. This seems to hint that our chosen few do (or at least did) possess the rare sustainability we're looking for.

But aren't we also just data mining here? If we start with a large enough sample of companies (and by most estimates there are at least 50,000 listed firms extant today) then as Rosenzweig warns, we should expect to find some that simply by chance have strung together years of high ROE's, creating the misleading impression of durability. Hence, whilst attempting a more modest task than Collins (we're not trying to divine the path to greatness, just find it) selecting companies based on performance after the fact could also lead us astray.

But how plausible is it to suggest that a company could consistently generate decades of success by chance alone? In the extreme case, if business outperformance (say generating a higher ROE than the average company) for any given year were entirely random, then to do so for 20 successive years would be akin to flipping 20 heads in a row on a fair coin. The chances of this happening, for a given company, whilst not impossible, are roughly one in a million[2]. Even with a global pool of 50,000 or so companies to select

[1]Interestingly, though perhaps only coincidentally, the *Built to Last* list shares seven companies with our LT global universe of stocks (and the European spin-off book a further five) whilst *Good to Great* shares just two.

[2]To adhere to the above example where the median ROEs of our 2011 portfolio companies managed to beat the MSCI's average in 18 out of 20 years, we should really calculate the odds of flipping 18 or more heads from 20 trials. This improves the chance of success to a more generous 1 in 5,000, but it's still miniscule.

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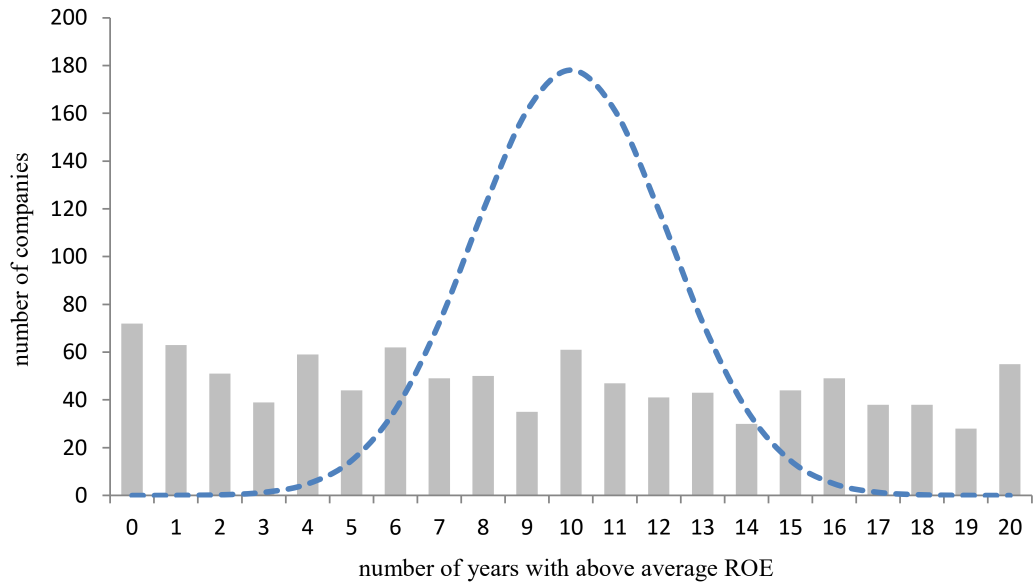
from, the likelihood of finding just one with such a string of success is still less than 5%. As it is, of the almost 1,000 companies listed in the MSCI World index with complete 20 year histories worth of data, 55 have clocked above average ROEs in each of those 20 years - that's 1 in 18. Even if the index were to represent the biggest and best the world has to offer, 55 appears to be far too high an occurrence to be down to chance alone. This can be visualised graphically as a histogram noting the number of years each company achieves an above average ROE. I've overlaid this with the typical bell-shaped normal curve which might be expected if outperformance from one year to the next really were both uncorrelated and 100% random. Quite clearly the empirical data and idealised distribution do not match[3]. On this basis then, the durability of high-performance can't be reasoned as a mere statistical anomaly. This isn't to say that the future has to look like the past, but it does help us reject Rosenzweig's hypothesis.



Histogram showing the frequency of ROE outperformance for the 998 companies listed in the MSCI World index with a full 20 year data history over the period from FY1997-FY2016. Overlaid is the expected probability distribution, approximated as Gaussian via the central limit theorem, under the assumption that outperformance in any given year for this sample occurs with a 50% chance and that each company's results are independent both in cross-section and longitudinally. Note that here selection bias will have an impact as our MSCI sample of large liquid companies is unlikely to be representative of the global population of listed companies from which it is drawn. Source Bloomberg, MSCI & Lindsell Train.

Additionally, like the fabled Super Investors of Graham-and-Doddsville[4] we can narrow the odds even further of hitting false positives by pre-selecting a sub group to assess. If one or two MSCI sectors say, contain an abnormally high number of great companies, we might put that their success is down to something common to the group, and not just chance. It's notable then that of the 55 Index companies referenced above with 20 consecutive years of ROE outperformance under their belts, just under half hail from the Consumer Staples or Discretionary sectors.

[3]This perhaps isn't too surprising given the stringency of the last two conditions, but the extent of the deviation is nevertheless stark. Along these lines, I don't think Rosenzweig is necessarily arguing each year's performance as a random, independent event - management skill could still lead to stretches of good results even if the business itself lacks durability. But from an investors' perspective, if there is nothing to inherently differentiate a firm then estimating its long-term prospects (and hence market value) still reverts to guesswork. You could argue this as our most fundamental divergence from the management strategy gurus - not all businesses are created equal and we're looking for those with underlying, intrinsic greatness, not just those with the currently smartest management teams.

[4]This being Warren Buffett's classic riposte to the efficient market line of thought (which maintains that a streak of exceptional investment performance could also arise from simple statistical factors i.e. that by starting with a large enough sample of hopeful investors and then allowing randomness to filter out the 'losers', some 'winners' will naturally emerge). He points out that if the winners all share some preselected characteristic (he discusses nine investors/funds all of which are identifiably students of the Benjamin Graham & David Dodd school of investing) that the chances of registering type I errors are dramatically reduced.

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These include companies we own such as Unilever, PepsiCo and Brown-Forman alongside LT global universe stalwarts (which we don't own but at the right price would one day like to) such as Hermes, J&J and Coke. Several MSCI sectors (e.g. Utilities, Energy and Real Estate) contain none. Not coincidentally, consumer franchises speak for close to half the weighting of our Global Equity portfolio.

There will inevitably still be instances of unearned success, and not every company with a hot track record will remain durably great. Collins (whose selection requirements were different, but also exacting) had both big winners and losers within his *Good to Great* group of companies. As noted above, he perhaps just needed to be a bit more selective when compiling his lists. Easier said than done, but it's through this less quantifiable stock selection that we at LT attempt to add value over and above a simple screen for outliers. There's not space for a full discussion of our process here, but it includes a particular focus on the quality of a company's intellectual property, the best of which is perpetual, capital light and resistant to technological disruption, making it both durable and unusually valuable. Hence we limit our attention to just a handful of industries and sectors (consumer franchises, media content, financial marketplaces and healthcare) where we think the best IP flourishes.

At the end of the day, what really matters is whether all this has any forward or predictive ability. Again, we really need a longitudinal study to properly gauge our proficiency in picking durable companies. Our Global Equity representative portfolio has a 2011 start date, so whilst performance has so far been good, at just seven years in, it remains work in progress. All we can note at this stage is that the weighted average return on equity for our portfolio has so far sustained its high (and above average) level - see the table below. Given that the Fund's composition is largely unchanged (we still own all but two of the original 24 stocks with three new names added) the analysis seems relevant. We remain optimistic that our companies will build from their significant foundations and continue to generate abnormally high returns into the future as well. Correct or not (and only time, and eventually performance, will tell), identifying, buying and holding enduringly great businesses like these is the foundation of our investment philosophy.

Weighted Average ROEs	2011	2012	2013	2014	2015	2016	2017	2018
Global Equity Rep. Portfolio	18%	19%	17%	16%	20%	23%	25%	22%
MSCI World Index	12%	11%	12%	12%	10%	10%	12%	13%

Weighted average ROE's for the representative Global Equity portfolio for each year of its life since inception (with December fund weightings used in each case) vs. the equivalent MSCI World calendar year figures. In each year so far the fund's ROE has exceeded the equivalent year's average for the MSCI Index by a reasonable margin. Source Bloomberg, MSCI & Lindsell Train.

In the unlikely event that I've made the investment challenge sound easy, I want to add one more cautionary coda: that even if sustainable greatness both exists and can be identified, things can still go wrong for an investor. The future is inherently unpredictable and no business model is completely invulnerable, no matter how robust it appears under normal conditions. Black swans can fly over even the deepest moats, whilst sufficient neglect will erode even the thickest fortifications. For example, perhaps Collins had correctly mapped the path to greatness but the route was simply abandoned by some of his company's management along the way. This is one of the reasons we monitor our companies closely, paying particular attention to their balance sheets and capital allocation decisions - both areas that if improperly managed could potentially herald disaster. If and when we find cause for concern we can and will act on it, though whether quickly enough is perhaps a different question.

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I'll conclude here with one final example[5] to further illustrate this last point - that even with the best will in the world, predicting the future is tough. I'll refer you then to the very mildly infamous Bain publication *Profit from the Core*, written by Chris Zook and also released in 2001. Here it's argued that companies prosper only when focusing on their strengths and avoiding unsound diversification. Again, sensible in theory, but unfortunately under the test of time some of the conclusions haven't weathered well. Most memorably Zook admonishes the then fledgling Amazon for its expansion beyond books (asking "Will Amazon's new 'all things to all people' model pay off? Or, as we suggest in the book, should Amazon have been faithful to its roots and focused on expanding its core business? Time will tell.") and instead tips such future luminaries as Egghead Software, Enron and eYak.

Ok, it's easy to poke fun with the benefit of hindsight, but again, let's not judge too harshly. Sure Amazon has been a spectacular success, but was this preordained by 2001, or could it have taken a different path? How big a role has luck played, and what's coming next? Sadly, I don't think we know the answers, and frankly back in 2001 we didn't either. Indeed we've never invested in Amazon precisely because we've lacked the foresight to guess which strategic shift would pay off next - the Amazon of today is already a very different creature to that of 2001. Amazon Web Services for example only launched in 2006 yet contributed over 100% of last year's operating profit. It really is hard to foretell the future - a fact we find particularly true when assessing young, innovative stocks with new and as-yet untested business models.

Our approach then, a result of our myopic clairvoyance, is to trust in the track record, and stick to already-successful success stories. In the main, these are companies with well established, cash generative business models that have already demonstrated their durability over long periods of time. We do think that sustainable greatness exists, that it can be identified, and as a result past business performance does guide our thinking as to what might come next. This means we probably will miss the next Amazon, but instead continue to own long-term compounders like Shiseido (est. 1872), RELX (est. 1880), Kao (est. 1887) and Astellas (est. 1894) all of which are well into their second centuries, yet all have seen their share prices more than triple since we've held them. The shares of one of our most venerable companies, the London Stock Exchange (which can trace its history back over 300 years) have delivered a compound total return of 27%pa since our initial purchase for our Global Equity representative portfolio in 2011 - not bad, even compared to some of the FAANGs. This doesn't mean we'll never make mistakes (try looking back at these notes in 17 years time!) but we hopefully lessen our chances of ending up with an Egghead Software or an eYak.

James Bullock, Portfolio Manager

Lindsell Train Limited

[5]In case you're still after more, then both Clayton Christensen's *Innovator's Dilemma* and Paul Carroll & Chunka Mui's *Billion Dollar Lessons* - which weave convincing narratives around lists of business failures - are instructive examples of the alternative 'what not to do' approach. As a guide for prospective managers these reductive approaches fish from a broader pool, making them more likely to hit their prophetic mark. Likewise, if you're curious to see where it all began, then McKinsey alumni Robert Waterman & Tom Peters' 1982 book *In Search of Excellence* is the place to look - apparently accredited by none other than Warren Buffett as "A landmark book, without question the most important and useful book on what makes organisations effective ever written."

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Lindsell Train Limited
66 Buckingham Gate
London SW1E 6AU
ENGLAND

Tel. 020 7808 1210
Fax. 020 7808 1229
www.LindsellTrain.com
Info@lindselltrain.com