The Huginn Fund Frequently Asked Questions

Written and continuously updated by the fund manager, James Wilson.

Introduction

This document will hopefully serve as a useful accompaniment to the Owner's Manual I have written.

Upon discovering Huginn, the thoughtful long-term investors I am striving to collect will often ask similar questions.

This is a summary of the answers to those pertinent questions. It is my hope that the answers will foster further curiosity and help focus the follow-on discussions.

I have a wealth of other materials to share that really get under the skin of how I think and act, so please get in touch if you would like to learn more... huginn@pamp.co.uk

Q1. What are the Fees?

The Huginn Fund has no management fee. It is a performance fee only product which means we don't take any fees unless we **<u>out</u>**perform.

There are two fee structures to choose from:

Absolute – 20% of the gain above a UK CPI hurdle with a high watermark

Or

Relative – 27% of the outperformance versus a composite index (50% FTSE All Share Total Return & 50% MSCI World)

The aim of this structure is to create alignment. However, the best alignment comes from the fact that I have my entire pension in the fund - I eat my own cooking.

Q2. What is the market inefficiency implied by your investment framework and how do you intend to make money from it?

I suppose the overarching inefficiency implied by my investment framework is that from time to time, the market will behave in a predictably irrational way because the participants are humans.

To be more specific, it is the periodic irrational pessimism that my philosophy relies upon as opposed to the irrational exuberance in the other direction.

If you look at the overall GDP in an economy like the US, it swings between low single digit growth and low single digit decline. If you follow the revenue of the companies that represent the US economy (like the S&P 500), then a drop in GDP tends to be slightly magnified (c. -10% to -15%) because the sources of revenue for these companies are more discretionary than the entire economy which includes a large public sector. Then, because the companies tend to have operational and financial leverage, the earnings in a bad year will fluctuate even more (c. -20% to 30%).

Furthermore, the market also seems to apply a lower multiple to these temporarily lower earnings, resulting in even greater fluctuations in the overall stock market levels in a bad year (c. -30% to -40%).

With reassuring regularity, the earnings for these businesses rebound to even greater levels and resume their growth trajectory alongside the overall economy. Therefore, there is not much change to the value, but the price fluctuates widely.

In a generalised way, this is where I find opportunity - in the myopia and irrational pessimism that leads to a vast overreaction to short term news and temporary/cyclical setbacks in earnings. Furthermore, the pricing opportunity doesn't have to be some wider market cycle. These overreactions happen in individual stocks and industries independent of the economy.

A key ingredient is having thoughtful long-term investors for The Huginn Fund. They need to be willing to weather the volatility because they understand that hidden within the generalised panic are the most lucrative opportunities.

In 2019 Meir Statman, Professor of Finance at Santa Clara, wrote an excellent paper on the 'second generation' of behavioural finance. In the paper, he describes the five foundational blocks in this new generation. The fifth reads:

'Markets are not efficient in the sense that price always equals value in them, but they are efficient in the sense they are hard to beat. Investors seek to satisfy wants for utilitarian, expressive and emotional benefits from investments and investment activities and often commit cognitive and emotional errors on their way to their wants'. (Link)

So, it is a simple predictable behavioural inefficiency that I believe I will be able to exploit with regularity, but it is extraordinarily difficult to do well. Partly because I am also subject to the human condition.

The challenge for me is first and foremost to be as rational as I can be. I also need to constantly prepare by conducting deep independent research that helps me differentiate between the very small number of investable companies and everything else.

I then must be extraordinarily patient and wait for them to trade at the right price. Nobody knows when the opportunities will come, and sometimes they never do. I need to be very disciplined/careful about how I appraise value but also act decisively when it makes sense.

Q3. How has your framework changed over time?

A key development in the way I think about an investment opportunity is the importance of leadership in long term value.

I have had three public board positions which I used to embed myself in the business operations and learn how things work from the inside. I had lots of opportunities to get involved on both the operational and corporate side which all taught me the same thing – how important leadership is to the result for shareholders.

I saw how toxic, myopic and misaligned leaders can shuffle a business towards their own selfish gains at the expense of all else. Similarly, I observed how passionate, conscientious and objective leadership were able to create a culture that focussed on doing more with less – a key ingredient for improving return on capital and ultimately shareholder value.

I was able to watch the competitive battles play out first hand and see how small and often unnoticeable nudges of good leadership were the keys to success.

I learned that I often wrongly viewed good companies as a 'dehumanised' well-oiled machine that can pump out a good set of accounts each year. The years I spent on public boards taught me that an organisation is a collection of people that can choose to do the right things or the wrong things at any time. Generally, this all flows back to leadership and the example they set and reinforce.

Q4. What is your investment time horizon?

It sounds slightly odd, but my time horizon is indefinite. I use discounted cash flow analysis for valuation, which means I have a terminal value. A terminal value extends to perpetuity and so it would be inaccurate to say I think of a shorter time horizon in this context.

However, if take the simple maths I put forward in the Owner's Manual regarding the time I expect it will take for the market to see the value I have discovered, then I need ideas to work within 5 years of the initial investment.

Conceptually, the best businesses are the ones you never have to sell because they compound away for very long periods of time without major issues.

The intrinsic value is not a static variable. Wonderful business and management combinations have a higher probability of improving value rather rapidly. It is my hope that the market appraised price of an investment is perpetually trying to catch up with the value that is being added year after year.

Q5. How do you define investment risk?

I want to avoid permanent loss of capital, not necessarily the temporary loss which the volatile market can imposes on the share price. Permanent loss of capital refers to the underlying value of the business. In a very simple way, if something changes that permanently reduces the marginal return on capital, growth rate and/or the per share value, then this can be deemed a permanent loss of capital.

I look for ways in which this could happen and try to consider their probability of happening. I suppose this framework forms part of my investment risk analysis.

The risk of permanent loss of capital can come from many places. The most obvious one is financial and operational leverage. Both factors can make a business more fragile to the vicissitudes of the various cycles of supply/demand in which a business sits.

However, it is not as simple as the presence of financial and operational leverage. One must perform a detailed analysis of the structure and the liquidity issues that could unfold causing dilution for shareholders or even losing ownership. For example, a business might have a lot of financial leverage relative to current profits, but it could be zero coupon debt that does not mature for 20+ years. Structures with long dated maturities and very low annual cash costs can be very low risk and add a lot of value for shareholders, yet they score poorly on the standard ratios that most analysts use.

There are also external factors that can increase risk for companies, like regulatory intervention. A company that performs some important societal need that also has a powerful market position can often come under the purview of new regulation that impacts pricing power or competitive dynamics, leading to lower returns on capital.
There can also be more left-field risks to consider like fraud, natural disaster or nationalisation by a local government.

An important part of the analysis involves searching for the risk factors and an estimation of their impact. Often this is part of the initial feasibility study, where one can quickly identify some risks which are just not worth taking.

Q6. How would you characterise your investment style?

I am looking for investments that offer excellent value relative to their quality and growth prospects.

I struggle to answer these types of questions because a flexible mindset and creative approach to finding opportunities in a wide

(Growth, Quality, Value etc.)?

variety of places is important to continued performance. Rigid thinking and commitment/consistency biases can creep in if one repeatedly classifies themselves with strict definitions.

However, I can say that I have been most often called a value investor by those that have investigated my decisions.

Q7. How do you generate ideas for investments?

In a previous job I had responsibility for maintaining and updating a large quantitative screening tool which plugged into the Bloomberg data feed.

In doing this for a couple of years and seeing the results, I realised that it does not work particularly well for the type of concentrated/bottom-up investing I strive to do.

I found the data which these tools scrape to be unreliable and inaccurate. I also realised that most of the rest of the industry does the same things with the same data, and to quote Sir John Templeton 'if you want to have better performance than the crowd, you must do things differently from the crowd'.

Therefore, I don't have a quantitative screening tool like most investors seem to.

I think you make money from discovering and deducing insights. I define an insight as 'seeing things that others don't'. I tend to look in the places that others aren't looking and ideally don't even know exist.

In a very simple way, smaller companies are a good example of this, especially in the US. There are tens of thousands of businesses listed over there that have never had a piece of research written about them and are probably too small for 95% of institutional investors to even consider.

I have found these types of markets to be much less efficient, and quantitative screening would often exclude them because not much effort is made to document their KPIs/accounts in the various databases one might use.

I read widely into many different topics and disciplines that are not necessarily associated with my direct investments. I seek to constantly build a better understanding of the world around me, but I am always looking for nuggets of evidence that might suggest a place to look for competitively advantaged businesses.

I also regularly question long held beliefs to see whether my thinking has become rusty. For example, I had long held the belief that oil and gas businesses were uninvestable for capital allocation reasons, but also more recently the threat of substitutes from Solar and Wind power. I spent 18 months considering the evidence again and came out with an idea that resulted in my largest position at cost.

The oil and gas revisit was also timely because vast swathes of the investment industry have decided to divest their holdings, almost irrelevant of the facts or the value. I find hunting in those places that others deem uninvestable is also a great way to find ideas.

To reiterate a phrase I use in the Owner's Manual – I strive to be an empiricist. I am merely looking for the best risk adjusted returns, wherever they may be. This is not a science and most of the leads I purse for new ideas are dead ends. However, by thumbing through the forgotten, unknown and unloved parts of the market, welcoming the work required to understand the esoteric and reading widely off the well-beaten path, I have accumulated some great investments.

Often, I find that the key limitation is not new ideas to investigate, but the time to investigate them. Therefore, more important than the generation of ideas is the ability to prioritise the most promising ones and to eliminate the dead ends quickly.

Q8. What are the key components of your investment process?

Once I have ascertained an idea is worth further study, then the first stage of work is digesting the historic accounts in detail.

I go through a rather laborious process of transcribing all the historic accounts, ideally over many decades, into a spreadsheet. I also collect any useful notes and KPIs that may be disclosed in other regulatory filings, including the Quarterly reports, call transcripts and presentations.

- In collating this information in a format I find useful, I am looking for a great many things, including:
- An understanding of the inner workings of the engine of value creation through the accounts to see how the leadership, company and external market have come together to produce the returns on capital.
- Insight into the decisions regarding capital structure, and how they may have imperilled the business or added value.

- The presentation/explanation of the accounts which tells us a great deal about the intentions and focus of the leadership. Inspecting how they have accounted for revenues/profits and how it all translates into the actual cash generation of the business over time often speaks volumes about the nature of the leadership.

- Tracking how leadership decisions and specific projects/initiatives flow through to either success or failure, and inspecting the reasons given often gives you insights into not only how shrewd they have been, but also their level of humility and willingness to learn from mistakes.

In tandem with this historical accounting study, I will also attempt to understand the broader picture of the market and economy in which the company operates. I will study the reports and accounts for their competitors, both public and private. I will also seek out a collection of books written by industry insiders/experts over the years to understand how the market has developed and why. In particular, I find studying failures is a valuable use of time.

After absorbing the accounts and surveying the market/competitors, then I will usually have some more specific questions/hypotheses about how the customers behave, or perhaps some technical questions about the relative merits of certain products or services.

I will have accumulated these questions because the desk-based study will not have been able to answer them, and so I must look elsewhere. This normally involves ‘fieldwork’. Creativity is key to answering these questions and the primary research can take many forms, for example:

- Visiting stores for a retailer to speak with customers, staff and check prices.

- Contacting ex-employees to understand how the culture is at head office.

- ‘Mystery shopping’ where I pose as a potential customer and walk through the buying process in their shoes.

- Contacting local regulators to request historic records for mineral drilling productivity.

- Speaking with university professors to understand the ‘bleeding edge’ of technological substitutes.

- Joining a trade body to access industry data.

I often find that all the answers are out there, but you need to develop of craft of knowing how and where to find them. I also often find that this is the hardest and most time-consuming part of the research process. As a result, I also find that most competing funds either don't do it well or don't do at all, which should provide some edge over time, hopefully.

The combination of the desk-based study and the fieldwork can take many months, and sometimes more than a year if an industry is particularly complex and I need to hunt around a lot for the best participants. Patience and rigour are key.

There will come a point during the process where I either find the 'Achilles heel' for a company/industry and reject it as an investment, or I decide I have enough to pull together an investment case.

If I decide that I have covered enough ground to piece together the rationale for an investment, then I will attempt to summarise my work in a report of around 10-15 pages.

The deeper research part is about assimilating a great deal of information. The process of writing a report is about identifying the key factors and explaining them succinctly. I will often not have a clear idea of the conclusion when I start writing the report. I have found this writing process key to helping me plot a logical route through the findings to reach a useful conclusion.

I will regularly reveal gaps in my knowledge while doing this and so must go back into the field to find more answers, or perhaps read a few more books.

It is only once I have completed this study and report that I will consider myself competent enough to meet management and ask the right questions. Often, I will deem the case compelling enough to make an investment without management access, but sometimes there are questions that only the leadership can answer and so I will seek them out.

However, due to the fund size and my relative obscurity it is not always possible to win time with management. I have developed some ways and means of persuading busy leaders to meet with me, but they don't always work. The role of a fund manager involves judgement under uncertainty and an important part of the skillset is knowing how much uncertainty you can have when making an investment. I would never make any investments if I had to wait for all the answers to be in place.

If I can finish my research, enquiries and summary report with an investable conclusion, the final stage is often valuation scenarioizing.

Valuation can take many forms, and over the years the structure of my models has gotten simpler. A discounted cash flow is a dangerous tool because it is so sensitive to small changes in the inputs. There are many ways in which one can fall victim to biases in constructing a model. I control for this by keeping things as simple as possible. This can extend sometimes to a model where I merely choose the rate at which leaders can compound the assets over the long term, which normally occurs when I am assuming the past is a very good guide to the future. The complexities creep in when there is some fundamental change that requires more careful inspection of the new cash flow dynamics.

Once I have gotten comfortable with the worst feasible scenario and my point estimate of intrinsic value, I will then wait patiently for the company to trade at the right price, if it ever does.
If I do make an investment, then I will continue to read widely around the company and stay close to competitors/customers.

Once this monitoring stage starts, I am really looking for evidence that the rationale and/or valuation are wrong. If I discover a thread of evidence which suggests I may be wrong, then I will investigate it fully. This often involves more fieldwork, but in a more surgical manner.

If I confirm I have gotten a key part of the investment case wrong which could feasibly lead to permanent loss of capital, or there is perhaps something more sinister happening then I will sell the holding. I have learned that being decisive in these cases is highly valuable. The longer you leave an obvious mistake festering, the longer you are likely to let endowment/inertia biases creep in and cloud your thinking.

Similarly, I sometimes discover I have been too conservative with the way I have evaluated value. This can lead to a material upgrade in intrinsic value and a reassessment of the weighting upward in the portfolio.

In terms of primary differences to the Phoenix UK fund, a lot of it comes down to the fact that it is just me working on Huginn. I do all the heavy lifting alone, and then discuss interesting findings or problems with the team at the weekly meetings. I leave the door open to working more collaboratively if my work piques the curiosity of the analysts, which I have found leads to the most useful outcomes.

However, I simply don't have the time to create as much process-oriented paperwork as a team of c. 10 analysts.

I use the Phoenix DREAM framework for guardrails, but instead adopt to write a summary report to document the investment rationale instead of filling out the form line by line. I find it much more difficult to write the report than to tick each category off in DREAM one at a time. I think it is the need to link everything together into a coherent and flowing argument that is most useful about the report writing process.

Due to the time constraints, I also cannot physically do as much monitoring as a team of 10 people. I am more surgical with my post-investment fieldwork and will focus in on an issue for a month or two where needed. I try to control for this lower level of monitoring by having a much higher hurdle for leadership quality and track record.

Another interesting difference is the split of my time between new ideas and monitoring. I split my time roughly 60:40 in favour of new ideas, whereas I believe most analysts working on the UK fund are closer to 80:20 in favour of monitoring.

I have found that I have a particular vulnerability to 'falling in love' with holdings if I spend too much time becoming an expert to the n'th degree. I fool myself into thinking that this extra knowledge of the up-to-date minutiae helps me make better investment decisions. However, I have found that past a certain point it just clouds my judgement.

Carl Jung once said, 'people don't have ideas, ideas have people'. I have found that spending most of my time looking for even better investments than those in the portfolio is an excellent antidote to becoming too attached to my holdings. It gives me some excellent perspective and helps me stay focussed/clinical about the most important parts of the investment case(s).

Q9. What macroeconomic insights and/or themes

The macroeconomic equation is too complex for me to solve. I don't think I have ever really had a thought about macro that I could call an insight.

However, the simple macroeconomic realities that occur can provide excellent starting points in searching for ideas. In times of

do you use in the investment process?

stress in an industry or economy, investors tend to fall victim to irrational pessimism and so sell shares despondently.

During such times, prices can drop well below intrinsic values and even liquidation values. As such, macroeconomic shocks can provide a fertile hunting grounds for ideas.

Q10. How do you make use of 3rd party research from investment banks and other providers?

I actively avoid all sell side research.

I have previously been part of a team that used external research often. I found the combination of the wrong incentives and very clever people at investment banks was injurious to my clarity and independence of thought. I also found it shifted me into thinking much more myopically.
It also seemed apparent to me that most other fund managers were reading this stuff and basing their decisions on it.

Independent thought and long-term thinking are two increasingly rare attributes of investors I find. Therefore, I see them as a huge advantage and hold onto them as tightly as I can. One of the many ways to do that is avoid sell side research.

Q11. How are risk and probability incorporated into valuation?

I deal with risk outside of the direct maths in valuation (i.e. I don't change the discount rate). Risk to me is the risk of permanent loss of capital, not price volatility. It is captured in the analysis of aspects like financial leverage or regulatory risk. If these types of risk are too high and are likely to lead to permanent loss of shareholder value, then I will reject the idea.

Probability comes into the valuation when thinking about the range of outcomes. Every business has a probability of becoming worthless. There is always some way to break apart an investment case and turn it into a zero.

For example, aliens may make first contact tomorrow and gift us perpetual energy generation technology. This would change the core economics for most businesses and rewrite macroeconomics. However, I deem this scenario to be only a very remote possibility and so I don't include it in my worst feasible scenario.

I am wary of being too scientific about the probabilities in valuation because we cannot be precise about forecasting. In a simple way, I think about a range of outcomes that includes c.3 standard deviations around the mean. Another way to think of it is I believe that 1/100 events are likely to cause some permanent loss of capital to my investments.

I hope to have an investment career that spans at least 3 more decades and so hoping to avoid many 1/100 events over this time period seems reasonable.

However, the risk management all comes back to my ability to analyse risk appropriately and size up the probabilities (and probability distributions), which is extraordinarily difficult.

I recognise that this is difficult which is why I demand a large margin of safety when I invest.

I agree with Carveth Read when he said *'It is better to be vaguely right, than exactly wrong.'*

Q12. What is your approach to portfolio construction?

I think the benefits of diversification turn into drawbacks beyond 10-15 positions, and I think the best performance comes from a high concentration in the positions with the best downside protection and most upside.

Therefore, the weighting process is a combination of objective judgements about downside protection and upside potential, and more subjective judgements about depth of knowledge and understanding.

The starting point for a new position in a company that is in a new industry to me is c. 3%.

For a company that I know well, and have followed the industry for a while, which is trading at a price equating to the worst feasible scenario and offering at least upside of 100%, the weighting would be c.10%.

I suppose the difference between a 3% and a 10% weight is time. There is no substitute for living with a holding/industry for a few years while you broaden and deepen your knowledge base.

For an extremely attractive valuation in a company/industry I know well with c. 200% upside and the usual downside protection, I would consider a c.15% weight.

For an exceptional situation where there is more than 200% upside to intrinsic value, but also 50% upside to the worst feasible scenario, a maximum of a 20% weight can be considered.

It is also worth noting that the position limits are measured at cost price. If I am right about my best investment opportunities, then investors should expect to see a lot of concentration as good ideas compound their way upwards i.e. I have the flexibility to hold on to winners if that is the rational thing to do... which it often is.

James Wilson

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