

Do Algorithms Dream About Artificial Alphas?

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1.1 INTRODUCTION

The core of most financial practice, whether drawn from equilibrium economics, behavioural psychology, or agency models, is traditionally formed through the marriage of elegant theory and a kind of ‘dirty’ empirical proof. As I learnt from my years on the PhD programme at the London School of Economics, elegant theory is the hallmark of a beautiful intellect, one that could discern the subtle tradeoffs in agent-based models, form complex equilibrium structures and point to the sometimes conflicting paradoxes at the heart of conventional truths. Yet ‘dirty’ empirical work is often scoffed at with suspicion, but reluctantly acknowledged as necessary to give substance and real-world application. I recall many conversations in the windy courtyards and narrow passageways, with brilliant PhD students wrangling over questions of ‘but how can I find a test for my hypothesis?’.

Many pseudo-mathematical frameworks have come and gone in quantitative finance, usually borrowed from nearby sciences: thermodynamics from physics, Eto’s Lemma, information theory, network theory, assorted parts from number theory, and occasionally from less high-tech but reluctantly acknowledged social sciences like psychology. They have come, and they have gone, absorbed (not defeated) by the markets.

Machine learning, and extreme pattern recognition, offer a strong focus on large-scale empirical data, transformed and analyzed at such scale as never seen before for details of patterns that lay undetectable to previous inspection. Interestingly, machine learning offers very little in conceptual framework. In some circles, it boasts that the absence of a conceptual framework is its strength and removes the human bias that would otherwise limit a model. Whether you feel it is a good tool or not, you have to respect the notion that process speed is only getting faster and more powerful. We may call it neural networks or something else tomorrow, and we will eventually reach a point where most if not all permutations of patterns can be discovered and examined in close to real time, at which point the focus will be almost exclusively on defining the objective function rather than the structure of the framework.

The rest of this chapter is a set of observations and examples of how machine learning could help us learn more about financial markets, and is doing so. It is drawn not only from my experience, but from many conversations with academics, practitioners, computer scientists, and from volumes of books, articles, podcasts and the vast sea of intellect that is now engaged in these topics.

It is an incredible time to be intellectually curious and quantitatively minded, and we at best can be effective conduits for the future generations to think about these problems in a considered and scientific manner, even as they wield these monolithic technological tools.

1.2 REPLICATION OR REINVENTION

The quantification of the world is again a fascination of humanity. Quantification here is the idea that we can break down patterns that we observe as humans into component parts and replicate them over much larger observations, and in a much faster way. The foundations of quantitative finance found their roots in investment principles, or observations, made by generations and generations of astute investors, who recognized these ideas without the help of large-scale data.

The early ideas of factor investing and quantitative finance were replications of these insights; they did not themselves invent investment principles. The ideas of value investing (component valuation of assets and companies) are concepts that have been studied and understood for many generations. Quantitative finance took these ideas, broke them down, took the observable and scalable elements and spread them across a large number of (comparable) companies.

The cost to achieving scale is still the complexity in and nuance about how to apply a specific investment insight to a specific company, but these nuances were assumed to diversify away in a larger-scale portfolio, and were and are still largely overlooked.¹ The relationship between investment insights and future returns were replicated as linear relationships between exposure and returns, with little attention to non-linear dynamics or complexities, but instead, focusing on diversification and large-scale application which were regarded as better outcomes for modern portfolios.

There was, however, a subtle recognition of co-movement and correlation that emerged from the early factor work, and it is now at the core of modern risk management techniques. The idea is that stocks that have common characteristics (let's call it a quantified investment insight) have also correlation and co-dependence potentially on macro-style factors.

This small observation, in my opinion, is actually a reinvention of the investment world which up until then, and in many circles still, thought about stocks in isolation, valuing and appraising them as if they were standalone private equity investments. It was a reinvention because it moved the object of focus from an individual stock to

¹Consider the nuances in the way that you would value a bank or a healthcare company, and contrast this to the idea that everything could be compared under the broad umbrella of a single empirical measure of book to price.

a common ‘thread’ or factor that linked many stocks that individually had no direct business relationship, but still had a similar characteristic that could mean that they would be bought and sold together. The ‘factor’ link became the objective of the investment process, and its identification and improvement became the objective of many investment processes – now (in the later 2010s) it is seeing another renaissance of interest. Importantly, we began to see the world as a series of factors, some transient, some long-standing, some short- and some long-term forecasting, some providing risk and to be removed, and some providing risky returns.

Factors represented the invisible (but detectable) threads that wove the tapestry of global financial markets. While we (quantitative researchers) searched to discover and understand these threads, much of the world focused on the visible world of companies, products and periodic earnings. We painted the world as a network, where connections and nodes were the most important, while others painted it as a series of investment ideas and events.

The reinvention was in a shift in the object of interest, from individual stocks to a series of network relationships, and their ebb and flow through time. It was subtle, as it was severe, and is probably still not fully understood.² Good factor timing models are rare, and there is an active debate about how to think about timing at all. Contextual factor models are even more rare and pose especially interesting areas for empirical and theoretical work.

1.3 REINVENTION WITH MACHINE LEARNING

Reinvention with machine learning poses a similar opportunity for us to reinvent the way we think about the financial markets, I think in both the identification of the investment object and the way we think of the financial networks.

Allow me a simple analogy as a thought exercise. In handwriting or facial recognition, we as humans look for certain patterns to help us understand the world. On a conscious, perceptive level, we look to see patterns in the face of a person, in their nose, their eyes and their mouth. In this example, the objects of perception are those units, and we appraise their similarity to others that we know. Our pattern recognition then functions on a fairly low dimension in terms of components. We have broken down the problem into a finite set of grouped information (in this case, the features of the face), and we appraise those categories. In modern machine learning techniques, the face or a handwritten number is broken down into much smaller and therefore more numerous components. In the case of a handwritten number, for example, the pixels of the picture are converted to numeric representations, and the patterns in the pixels are sought using a deep learning algorithm.

We have incredible tools to take large-scale data and to look for patterns in the sub-atomic level of our sample. In the case of human faces or numbers, and many other

²We are just now again beginning to prod the limits of our understanding of factors by considering how to define them better, how to time them, all the meanwhile expanding considerable effort trying to explain them to non-technical investors.

things, we can find these patterns through complex patterns that are no longer intuitive or understandable by us (consciously); they do not identify a nose, or an eye, but look for patterns in deep folds of the information.³ Sometimes the tools can be much more efficient and find patterns better, quicker than us, without our intuition being able to keep up.

Taking this analogy to finance, much of asset management concerns itself with financial (fundamental) data, like income statements, balance sheets, and earnings. These items effectively characterize a company, in the same way the major patterns of a face may characterize a person. If we take these items, we may have a few hundred, and use them in a large-scale algorithm like machine learning, we may find that we are already constraining ourselves heavily before we have begun.

The ‘magic’ of neural networks comes in their ability to recognize patterns in atomic (e.g. pixel-level) information, and by feeding them higher constructs, we may already be constraining their ability to find new patterns, that is, patterns beyond those already identified by us in linear frameworks. Reinvention lies in our ability to find new constructs and more ‘atomic’ representations of investments to allow these algorithms to better find patterns. This may mean moving away from the reported quarterly or annual financial accounts, perhaps using higher-frequency indicators of sales and revenue (relying on alternate data sources), as a way to find higher frequency and, potentially, more connected patterns with which to forecast price movements.

Reinvention through machine learning may also mean turning our attention to modelling financial markets as a complex (or just expansive) network, where the dimensionality of the problem is potentially explosively high and prohibitive for our minds to work with. To estimate a single dimension of a network is to effectively estimate a covariance matrix of $n \times n$. Once we make this system endogenous, many of the links within the 2D matrix become a function of other links, in which case the model is recursive, and iterative. And this is only in two dimensions. Modelling the financial markets like a neural network has been attempted with limited application, and more recently the idea of supply chains is gaining popularity as a way of detecting the fine strands between companies. Alternate data may well open up new explicitly observable links between companies, in terms of their business dealings, that can form the basis of a network, but it’s more likely that prices will move too fast, and too much, to be simply determined by average supply contracts.

1.4 A MATTER OF TRUST

The reality is that patterns that escape our human attention will be either too subtle, or too numerous, or too fast in the data. Our inability to identify with them in an intuitive way, or to construct stories around them, will naturally cause us to mistrust them. Some patterns in the data will be not useful for investment (e.g. noise, illiquid,

³Early experiments are mixed, and adversarial systems have shown some of these early patterns to be extremely fragile. But as technology grows, and our use of it too, these patterns are likely to become increasingly robust, but will retain their complexity.

and/or uninvestable), so these will quickly end up on the ‘cutting room floor’.⁴ But many others will be robust, and useful, but entirely unintuitive, and perhaps obfuscated to us. Our natural reaction will be to question ourselves, and if we are to use them, ensure that they are part of a very large cohort of signals, so as to diversify questions about a particular signal in isolation.

So long as our clients are humans as well, we will face communication challenges, especially during times of weak performance. When performance is strong, opaque investment processes are less questioned, and complexity can even be considered a positive, differentiating characteristic. However, on most occasions, an opaque investment process that underperforms is quickly mistrusted. In many examples of modern investment history, the ‘quants’ struggled to explain their models in poor performance periods and were quickly abandoned by investors. The same merits of intellectual superiority bestowed upon them rapidly became weaknesses and points of ridicule.

Storytelling, the art of wrapping complexity in comfortable and familiar anecdotes and analogies, feels like a necessary cost of using technical models. However, the same can be a large barrier to innovation in finance. Investment beliefs, and our capability to generate comfortable anecdotal stories, are often there to reconfirm commonly held intuitive investment truths, which in turn are supported by ‘sensible’ patterns in data.

If innovation means moving to ‘machine patterns’ in finance, with greater complexity and dynamic characteristics, it will come from a leap of faith where we relinquish our authorship of investment insights, and/or from some kind of obfuscation such as bundling, where scrutiny of an individual signal is not possible. Either way, there is a certain additional business risk involved in moving outside the accepted realm of stories, even if the investment signals themselves add value.

If we are to innovate signals, we may very well need to innovate storytelling as well. Data visualization is one promising area in this field, but we may find ourselves embracing virtual and augmented reality devices quicker than the rest of finance if we are to showcase the visual brilliance of a market network or a full factor structure.

1.5 ECONOMIC EXISTENTIALISM: A GRAND DESIGN OR AN ACCIDENT?

If I told you that I built a model to forecast economic sector returns, but that the model itself was largely unintuitive and highly contextualized, would this concern you? What if I told you that a core component was the recent number of articles in newspapers covering the products of that industry, but that this component wasn’t guaranteed to ‘make’ the model in my next estimation. Most researchers I have encountered have a conceptual framework for how they choose between potential models. Normally, there is a thought exercise involved to relate a given finding back to the macro-picture and ask: ‘Is this really how the world works? Does it make sense?’ Without this, the results are easily picked apart for their empirical fragility and in-sample biases. There is a subtle leap that we take there, and it is to assume that there is a central ‘order’ or design to

⁴There is an entire book that could be written on the importance of noise versus signal, but I would suggest we suspend our natural scepticism and allow for the possibility that unusual patterns do exist and could be important.

the economic system. That economic forces are efficiently pricing and trading off risks and returns, usually from the collective actions of a group of informed and rational (if not pseudo-rational) agents. Even if we don't think that agents are informed, or fully rational, their collective actions can bring about ordered systems.

Our thinking in economics is very much grounded in the idea that there is a 'grand design' in play, a grand system, that we are detecting and estimating, and occasionally exploiting. I am not referring to the idea that there are temporary 'mini-equilibria' that are constantly changing or evolving, but to the notion that there are any equilibria at all.

Darwinian notions of random mutations, evolution, and learning challenge the very core of this world view. Dennett⁵ elegantly expresses this world view as a series of accidents, with little reference to a macro-level order or a larger purpose. The notion of 'competence without comprehension' is developed as a framework to describe how intelligent systems can come out of a series of adaptive responses, without a larger order or a 'design' behind them. In his book, Harari⁶ describes the evolution of humans as moving from foraging for food to organized farms. In doing so, their numbers increase, and they are now unable to go back to foraging. The path dependence is an important part of the evolution and constrains the evolution in terms of its future direction. For example, it is unable to 'evolve' foraging practices because it doesn't do that any more and now it is evolving farming.

Machine learning, and models like random forests, give little indication of a bigger picture, or a conceptual framework, but are most easily interpreted as a series of (random) evolutions in the data that has led us to the current 'truth' that we observe. The idea of a set of economic forces working in unison to give rise to a state of the economy is instead replaced by a series of random mutations and evolutionary pathways. For finance quantitative models, the implication is that there is strong path dependency.

This is challenging, and in some cases outright disturbing, for an economically trained thinker. The idea that a model can produce a series of correlations with little explanation other than 'just because' is concerning, especially if the path directions (mutations) are random (to the researcher) – it can seem as though we have mapped out the path of a water droplet rolling down glass, but with little idea of what guided that path itself. As the famous investor George Soros⁷ described his investment philosophy and market: a series of inputs and outputs, like an 'alchemy' experiment, a series of trails and failures.

1.6 WHAT IS THIS SYSTEM ANYWAY?

Reinvention requires a re-examination of the root cause of returns and, potentially, abnormal returns. In nature, in games, and in feature identification, we generally know the rules (if any) of an engagement, and we know the game, and we know the challenges

⁵'From Bacteria to Bach and Back: The Evolution of Minds' by Daniel C. Dennett, 2018, Penguin.

⁶'Homo Deus: A Brief History of Tomorrow' by Yuval Noah Harari, 2015, Vintage.

⁷The Alchemy of Finance by George Soros, 2003.

of identification of features. One central element in financial markets, that is yet to be addressed, is their dynamic nature. As elements are identified, correlations estimated, returns calculated, the system can be moving and changing very quickly.

Most (common) quantitative finance models focus more on cross-sectional identification and less on time-series forecasting. Of the time-series models, they tend to be continuous in nature, or have state dependency with usually a kind of switching model embedded. Neither approach has a deeper understanding, *ex ante*, of the reasons why the market dynamics may change, and forecasting (in my experience) of either model tends to rely on serial correlation of states and the occasional market extreme environment to ‘jolt’ the system.⁸ In this sense, the true complexity of the financial markets is likely grossly understated. Can we expect more from a machine learning algorithm that can dig into the subtle complexities and relationships of the markets? Potentially, yes. However, the lack of clean data, and the likelihood of information segmentations in the cross-section, suggest some kind of supervised learning models, where the *ex-ante* structures set up by the researcher are as likely to be the root of success or failure as the parameters estimated by the model itself.

One hope is that structures of relationships suggested by machine learning models can inspire and inform a new generation of theorists and agent-based simulation models, that in turn could give rise to more refined *ex-ante* structures for understanding the dynamic complexities of markets. It is less likely that we can learn about latent dynamic attributes of markets without some kind of *ex ante* model, whose latent characteristics we may never be able to observe, but potentially may infer.

One thought exercise to demonstrate this idea is a simple 2D matrix, of 5×5 elements (or as many as it takes to make this point). Each second, there is a grain of sand that drops from above this plane and lands on a single square. Over time, the number of grains of sand builds up in each square. There is a rule whereby if the tower of sand on one square is much greater than on another, it will collapse onto its neighbour, conferring the sand over. Eventually, some of the sand will fall over one of the four edges of the plane. The system itself is complex, it builds up ‘pressure’ in various areas, and occasionally releases the pressure as a head of sand falls from one square to another, and finally over the edge. Now picture a single researcher, standing well below the plane of squares, having no visibility of what happens on the plane itself. They can only observe the number of sand particles that fall over the edge, and which edge. From their point of view, they know only that if no sand has fallen for a while, they should be more worried, but they have no sense as to the system that gives rise to the occasional avalanche. Machine learning models, based on prices, suffer from a similar limitation. There is only so much they can infer, and there is a continuum of complex systems that could give rise to a given configuration of market characteristics. Choosing a unique or ‘true’ model, especially when faced with natural obfuscations of the complexities, is a near impossible task for a researcher.

⁸Consider, for example, a classic state switching model, where the returns to a factor/signal persist until there is an extreme valuation or return observed, perhaps a bubble, where the state of the future returns turns out to be negative. Most forecasting models for momentum will have some similar structures behind them, where the unconditional returns are assumed to persist and are positive, until an extreme event or condition is observed.

1.7 DYNAMIC FORECASTING AND NEW METHODOLOGIES

We return now to the more direct problems of quantitative asset management. Asset pricing (equities) broadly begins with one of two premises that are usually reliant on your chosen horizon:

1. Markets are composed of financial assets, and prices are fair valuations of the future benefit (cash flows usually) of owning those assets. Forecasting takes place of future cash-flows/fundamentals/earnings. The data field is composed of firms, that are bundles of future cash-flows, and whose prices reflect the relative (or absolute) valuation of these cash-flows.
2. Markets are composed of financial assets that are traded by agents with imperfect information based on a range of considerations. Returns are therefore simply a ‘trading game’; to forecast prices is to forecast future demand and supply of other agents. This may or may not (usually not) involve understanding fundamental information. In fact, for higher-frequency strategies, little to no information is necessary about the underlying asset, only about its expected price at some future date. Typically using higher frequency micro-structures like volume, bid-ask spreads, and calendar (timing) effects, these models seek to forecast future demand/supply imbalances and benefit over a period of anywhere from nano-seconds to usually days. There’s not much prior modelling, as the tradeoff, almost by design, is too high frequency always to be reacting to economic information, which means that it is likely to be driven by trading patterns and to rebalance frequencies that run parallel to normal economic information.

1.8 FUNDAMENTAL FACTORS, FORECASTING AND MACHINE LEARNING

In the case of a fundamental investment process, the ‘language’ of asset pricing is one filled with reference to the business conditions of firms, their financial statements, earnings, assets, and generally business prospects. The majority of the mutual fund industry operates with this viewpoint, analyzing firms in isolation, relative to industry peers, relative to global peers, and relative to the market as a whole, based on their prospective business success. The vast majority of the finance literature that seeks to price systematic risk beyond that of CAPM, so multi-factor risk premia, and new factor research, usually presents some undiversifiable business risk as the case of potential returns. The process for these models is fairly simple: extract fundamental characteristics based on a combination of financial statements, analysis, and modelling, and apply to either relative (cross-sectional) or total (time-series) returns.

For cross-sectional return analysis, the characteristics (take a very common measure like earnings/price) are defined in the broad cross-section, are transformed into a z-score, $Z \sim N(0,1)$, or a percentile rank (1–100), and then related through a function f^* to some future returns, r_{t+n} , where ‘n’ is typically 1–12 months forward returns. The function f^* finds its home in the Arbitrage Pricing Theory (APT) literature, and so is derived through either sorting or linear regressions, but can also be a simple linear correlation with future returns (otherwise known as an information coefficient, IC), a simple heuristic bucket-sorting exercise, a linear regression, a step-wise linear regression (for multiple Z

characteristics, and where the marginal use is of interest), or it can be quite complex, and as the 'Z' signal is implanted into an existing mean-variance optimized portfolios with multitude of characteristics.

Importantly, the forecast of 'Z' is typically defined so as to have broad-sectional appeal (e.g. all stocks should be measurable in the cross-section). Once handed over to a well-diversified application (e.g. with many stocks), any errors around the linear fit will (hopefully) be diversified away. However, not much time is typically spent defining different f^* functional forms. Outside of the usual quadratic forms (typically used to handle 'size') or the occasional interaction (e.g. *Quality*Size*), there isn't really a good way to think about how to use information in 'Z'. It is an area that largely has been neglected in favour of better stock-specific measurements, but still the same standardization, and the same f^* .

So our objective is to improve f^* . Typically, we have a set of several hundred fundamental 'Z' to draw from, each a continuous variable in the cross-section, and at best around 3000 stocks in the cross-section. We can transform the Z into indicator variables for decile membership for example, but typically, we want to use the extreme deciles as indicators, not the middle of the distribution. Armed with fundamental variables 'Z' and some indicators Z^I based on 'Z', we start to explore different non-linear methodologies. We start to get excited now, as the potential new uber-solving model lies somewhere before us.

The first problem we run into is the question: 'What do I want to forecast?' Random forests, neural networks, are typically looking for binary outcomes as predictors. Returns are continuous, and most fundamental outcomes are equally so (A percentage by which a company has beat/miss estimates, for example). Before we choose our object, we should consider what kind of system we are looking to identify.

1. I want to forecast a company's choice to do something, e.g. firms that 'choose' to replace CEOs, to buy or sell assets, to acquire competitors. I then hope to benefit from returns associated from these actions. But how do firms make these choices? Do they make them in isolation from economic factors, is there really unconditional choice, or are these firms already conditioned by some kind of latent economic event? For example, firms rarely cancel dividends in isolation. Typically, the choice to cancel is already heavily influenced by very poor market conditions. So our model may well be identifying firms that are under financial duress, more than those that actually 'choose' to cancel dividends. Think hard as to what is a 'choice' and what is a 'state', where certain choices are foregone conclusions.
2. I want to forecast wrongdoing by the firm and then make money by shorting/avoiding those firms. Intentional or not, firms that misreport their financials but then are ultimately discovered (we hope!), and therefore we have a sample set. This is especially interesting for emerging economies, where financial controls, e.g. for state-owned enterprises, could have conflicting interests with simply open disclosure. This feels like an exciting area of forensic accounting, where 'clues' are picked up and matched by the algorithm in patterns that are impossible to follow through human intuition alone. I think we have to revisit here the original assumption: is this unintentional, and therefore we are modelling inherent uncertainty/complexity within the organization, or is it intentional, in which case it is a 'choice' of sorts.

The choice of independent variables should inform both ideally, but the ‘choice’ idea would require a lot more information on ulterior motives.

3. I just want to forecast returns. Straight for the jugular, we can say: Can we use fundamental characteristics to forecast stock returns? We can define relative returns (top decile, top quintile?) over some future period ‘n’ within some peer group and denote this as ‘1’ and everything else as ‘0’. It is attractive to think that if we can line up our (small) army of fundamental data, re-estimate our model (neural net or something else) with some look-back window, we should be able to do crack this problem with brute force. It is, however, likely to result in an extremely dynamic model, with extreme variations in importance between factors, and probably not clear ‘local maxima’ for which model is the best. Alternately, we can define our dependent variable based on a total return target, for example anything +20% over the future period ‘n’ (clearly, the two choices are related), and aim to identify an ‘extreme movers’ model. But why do firms experience unusually large price jumps? Any of the above models (acquisition, beating forecasts, big surprises, etc.) could be candidates, or if not, we are effectively forecasting cross-sectional volatility. In 2008, for example, achieving a positive return of +20% may have been near impossible, whereas in the latter part of 2009, if you were a bank, it was expected. Cross-sectional volatility and market direction are necessarily ‘states’ to enable (or disqualify) the probability of a +x% move in stock prices. Therefore, total return target models are unlikely to perform well across different market cycles (cross-sectional volatility regimes), where the unconditional probability of achieving a +20% varies significantly. Embedding these is effectively transforming the +20% to a standard deviation move in the cross-section, when you are now back in the relative-return game.
4. If you were particularly keen on letting methodology drive your model decisions, you would have to reconcile yourself to the idea that prices are continuous and that fundamental accounting data (as least reported) is discrete and usually highly managed. If your forecast period is anywhere below the reporting frequency of accounting information, e.g. monthly, you are essentially relying on the diverging movements between historically stated financial accounts and prices today to drive information change, and therefore, to a large extent, turnover. This is less of a concern when you are dealing with large, ‘grouped’ analytics like bucketing or regression analysis. It can be a much bigger concern if you are using very fine instruments, like neural nets, that will pick up subtle deviations and assign meaningful relationships to them.
5. Using conditional models like dynamic nested logits (e.g. random forests) will probably highlight those average groups that are marginally more likely to outperform the market than some others, but their characterization (in terms of what determines the nodes) will be extremely dynamic. Conditional factor models (contextual models) exist today; in fact, most factor models are determined within geographic contexts (see any of the commercially available risk models, for example) and in some case within size. This effectively means that return forecasting is conditional based on which part of the market you are in. This is difficult to justify from an economic principle standpoint because it would necessitate some amount of segmentation in either information generation or strong clientele effects. For example, one set of clients (for US small cap) thinks about top-line growth as a way of driving

returns, while another set of clients (Japan large cap) looks for something totally different. If the world was that segmented, it would be difficult (but not impossible) to argue for asset pricing being compensation for some kind of global (undiversifiable) risk. In any case, conditional asset pricing models, whatever the empirical methodology, should work to justify why they think that prices are so dynamically driven by such different fundamentals over the relatively short period between financial statements.

In summary, the marriage of large-scale but sensitive instruments like machine learning methodologies to forecasting cross-sectional returns using fundamental information must be done with great care and attention. Much of the quantitative work in this area has relied on brute force (approximations) to sensitivities like beta. Researchers will find little emphasis on error-correction methodologies in the mainstream calculations of APT regressions, or of ICs, which rely on picking up broad, average relationships between signals (Z) and future returns. Occasionally (usually during high cross-sectional volatility periods) there will be a presentation at a conference around non-linear factor returns, to which the audience will knowingly nod in acknowledgement but essentially fail to adjust for. The lure of the linear function f^* is altogether too great and too ingrained to be easily overcome.

In the past, we have done experiments to ascertain how much additional value non-linear estimators could add to simulation backtests. For slower-moving signals (monthly rebalance, 6–12-month horizons), it is hard to conclusively beat a linear model that isn't over-fitted (or at least can be defended easily). Similarly, factor timing is an alluring area for non-linear modelling. However, factor returns are themselves calculated with a great amount of noise and inherent assumptions around calculation. These assumptions make the timing itself very subjective. A well-constructed (which usually means well-backtested) factor will have a smooth return series, except for a few potentially catastrophic bumps in history. Using a time-series neural network to try to forecast when those events will happen will, even more than a linear framework, leverage exceptionally strongly on a few tell-tale signs that are usually non-repeatable. Ironically, factors were built to work well as buy-and-hold additions to a portfolio. This means that it is especially difficult to improve on a buy-and-hold return by using a continuous timing mechanism, even one that is fitted. Missing one or two of the extreme return events through history, then accounting for trading costs, will usually see the steady-as-she-goes linear factor win, frustrating the methodologically eager researcher. Ultimately, we would be better served to generate a less well-constructed factor that had some time-series characteristics and aim to time that.

At this point, it feels as though we have come to a difficult passage. For fundamental researchers, the unit of interest is usually some kind of accounting-based metric (earnings, revenue, etc.), so using machine learning in this world seems analogous to making a Ferrari drive in London peak-hour traffic. In other words: it looks attractive, but probably feels like agony. What else can we do?

1.9 CONCLUSION: LOOKING FOR NAILS

It is for scientifically minded researchers to fall in love with a new methodology and spend their time looking for problems to deploy it on. Like wielding your favourite

hammer, wandering around the house looking for nails, machine learning can seem like an exciting branch of methodology with no obviously unique application. We are increasingly seeing traditional models re-estimated using machine learning techniques, and in some cases, these models could give rise to new insights. More often than not, if the models are constrained, because they have been built and designed for linear estimation, we will need to reinvent the original problem and redesign the experiment in order to have a hope of glimpsing something brand new from the data.

A useful guiding principle when evaluating models, designing new models, or just kicking around ideas in front of a whiteboard is to ask yourself, or a colleague: ‘What have we learnt about the world here?’ Ultimately, the purpose of empirical or anecdotal investigation is to learn more about the fantastically intricate, amazing, and inspiring way in which the world functions around us, from elegant mathematics, to messy complex systems, and the messiest of all: data. A researcher who has the conviction that they represent some kind of ‘truth’ about the world through their models, no matter what the methodology and complexity, is more likely to be believed, remembered, and, ultimately, rewarded. We should not aggrandize or fall in love with individual models, but always seek to better our understanding of the world, and that of our clients.

Strong pattern recognition methodologies, like machine learning, have enormous capability to add to humanity’s understanding of complex systems, including financial markets, but also of many social systems. I am reminded often that those who use and wield these models should be careful with inference, humility, and trust. The world falls in and out of love with quantification, and usually falls out of love because it has been promised too much, too soon. Machine learning and artificial intelligence (AI) are almost certain to fail us at some point, but this should not deter us; rather, it should encourage us to seek better and more interesting models to learn more about the world.