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Issues relating to the ontology, epistemology and philosophy of equity pricing research

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Abstract (168 words)
This paper is motivated by poor results achieved following decades spent applying the contemporary equity investment paradigm. In particular: every 7-10 years, markets have crashed and ushered in global recession; dominant institutional investors understand investment theory well, but neglect it; and investor confidence is so low that the majority of mutual funds in the US are passively managed. Similar weaknesses are seen in investment by corporates whose accounting rate of return has been steadily declining. The motivation of this paper is that a weak paradigm can only survive if the methodology used to link observed data to theory is inappropriate. Validation of this intuition proceeds in two steps: the first chooses five methodologies that are commonly used for evaluation of equity price and sets out relevant stylized facts; while the second reviews a selection of papers that apply the methodologies written by esteemed researchers in the pre-eminent *Journal of Economic Perspectives*. The conclusion is that the papers' analytical foundations are so weak that none should have been published.

Keywords finance theory; ontology; epistemology; philosophy of finance research; equity valuation

JEL Classification: G11 - Portfolio Choice; Investment Decisions

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Issues relating to the ontology, epistemology and philosophy of equity pricing research

> The ultimate goal of a positive science is the development of a 'theory' or 'hypothesis' that yields valid and meaningful (i.e. not truistic) predictions about phenomena not yet observed.
>
> Milton Friedman (1953: 7) *Essays in positive economics*

1. Introduction

Financial economics is strongly analytical, but rarely introspective. The implicit assumption is that robust analysis of data will provide the right answer, and little time need be spent pondering process: terms such as ontology, epistemology and research philosophy are rarely used by finance scholars[1]. This attitude seems common across disciplines that advocate the scientific method of observe-hypothesize-test-theorize for analyzing phenomena because it does not require understanding data as the basis for selecting an appropriate methodology, nor does it require understanding systems as a basis for selecting the appropriate theoretical structure. Moreover, few disciplines close the research loop by validating theory against field evidence.

This article can best be thought of as backtesting equity pricing research against an extended scientific method. The latter incorporates ontology which describes the nature of a phenomenon such as equities, along with epistemology which sets out knowledge or understanding of it. These additional steps shape the way that stylized facts about equities – or observed empirical findings that are consistent across time and geography - are characterized in an appropriate taxonomy and then assembled in explanatory hypotheses that can be aggregated to a unifying theory and validated against field data. Such an analysis establishes a robust research philosophy or search for knowledge of the precursors to equity prices, and evaluates the contemporary equity pricing paradigm against the Friedman (1953) test of whether or not it is appropriate.

A successful investment research strategy is socially important because the most important of all economic variables is the value of an asset. It sets the price of equities and bonds which are the cornerstone of a comfortable retirement for the globe's middle class; and it is used to rank capital investments and thus shape the mix of corporate and government assets, which affects everything from infrastructure to employment. Unfortunately, decades since the outline of modern investment theory was finalized in the 1970s have brutally exposed its weaknesses. As examples: every 7-10 years, markets have crashed and ushered in global recession; dominant institutional investors understand the theory well (most have CFA certification and a business MBA), but neglect it (see, amongst many: Amenc *et al.* (2011)

[1] A search of websites of leading finance journals for mentions of ontology, epistemology or research philosophy found one example in each of *Journal of Finance*, *Journal of Financial Economics* and *Review of Financial Studies*. By contrast, *Academy of Management Journal* had 20 mentions, *Accounting Review* had 27, *Journal of Political Economy* had 60, and *Cambridge Journal of Economics* had 698 mentions.

and Coleman, 2014)[2]; and investor confidence is so low that the majority of mutual funds in the US are passively managed (Morningstar, 2018). Similar weaknesses are seen in investment by corporates whose accounting rate of return has been steadily declining[3].

It is obvious that equity prices do not perform their central role as reliable signals for allocation of financial resources: businesses are making ill-founded investment choices, and retirees' lifestyles are under threat. This explains growing doubt about theoretical foundations of the modern finance paradigm that was captured in the conclusion by Harvey, Liu and Zhu (2015) that "most claimed research findings in financial economics are likely false." If such a dire situation were true of any other critical knowledge-discipline, there would be industrial scale development of better theory. None, though, is apparent in finance.

My paper contributes to upgrading the finance paradigm by evaluating common research methodologies with the intuition that poor theory can only survive if researchers are blind to its weaknesses. It seems that current equity research tools do not suit the character of the data, which axiomatically makes research findings ill-founded so they do not challenge deficiencies in theory.

This paper is structured around critical evaluation of five investment methodologies that are in wide use and support much of the contemporary asset pricing paradigm, namely: event studies; fundamental valuation of equities using accounting measures; ex post Fama-MacBeth style time series analysis; use of Sharpe Ratio to compare portfolio returns; and closed form theoretical models of equity price.

Each methodology is critiqued alone by setting out stylized facts that are relevant to the methodology which is then described; the second step identifies a recent paper that uses the methodology and was recently published by multiple authors from leading universities in the *Journal of Economic Perspectives*. This is not a gotcha! parody of a weak paper, but a critical review of the finance discipline's best scholarship. The intent is to show the inconsistencies and methodological weaknesses of common equity pricing techniques. Thus analyses are granular and rely on real-world datasets and uncomplicated evaluations to highlight issues.

This style of analysis follows authors who have begun pointing to methodological weaknesses in finance research, including Harvey, Liu and Zhu (2015), and Welch (2013). This paper is hardly unique, but is motivated by the determination of leading researchers and journals to continue publishing material that is known to be flawed. It is one of a growing number of papers that are questioning the refusal of scientists across disciplines to ponder how the sausages are made.

Looking ahead, the next section reviews the five selected papers, the paper then discusses the methodologies' shortcomings, and closes with brief suggestions on ways to strengthen finance theory.

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[2] This is not generally appreciated because it is not contemplated by field studies such as those of Gompers *et al.* (2016) and Graham and Harvey (2001) which are remotely administered surveys and do not allow for answers outside normative finance concepts. For example, the Gompers *et al.* (2016) survey of private equity investors has a question 'On what fraction of deals do you use each of the following methods to calculate the exit value or terminal value of the model?' and provides only four possible answers: Comparable companies, Comparable transactions, DCF-based growing perpetuity, and Other. The result is to narrowly parameterize responses.

[3] A study by Deloitte reported that return on assets for US firms fell by three quarters during 1965-2012 (Hagel *et al.* (2013). Similarly, Kahle and Stulz (2017) found a significant decline in profitability of US firms in the 40 years to 2015 when measured by three common ratios.

2. Critical evaluation of leading finance methodologies

This section provides a detailed critique of the assumptions, methodology and conclusions of five widely used research methodologies that support much of the contemporary asset pricing paradigm. Each will be illustrated by a recent paper in the *Journal of Financial Economics* that has multiple authors from pre-eminent universities and applies the methodology to real world data. The aim is to use material prepared and published by the finance discipline's leaders as exemplars of its research. My aim is not to pass any unfair judgments, and so I emailed my comments to the corresponding author of each of the five papers that were critiqued seeking clarification. To date [none] has replied.

The following sub-sections each focus on a single methodology, and follow a three-step process: the first sets out stylized facts – or what we know with empirical certainty – of relevance to the methodology; the second step is to describe the methodology and use a recent exemplar to examine its suitability; and the critique closes with an evaluation of the methodology.

2.1. Event studies

Literally thousands of event studies have examined the response of equity prices to specific events or information which should intuitively affect stock prices (see: Corrado, 2011). Reflecting this effort, Kothari and Warner (2009: 5) report that: "A vast literature on event studies written over the past several decades has become an important part of financial economics". The authors go on to argue that "nonzero abnormal security returns that persist after a particular type of corporate event are inconsistent with market efficiency" which is an extension of the argument by Fama (1991: 1602) that event studies find that "prices on average adjust quickly to firm-specific information".

This reflects the all but universal conclusion of event studies that cumulative abnormal return (CAR) – which sums abnormal daily returns following an event - quickly decays to zero. That is, most event studies find that information brings a cumulative abnormal return of no more than a few percent that rarely lasts more than a day or two (Brown, 2011). The small signals of a wide variety of unanticipated corporate shocks and other surprising events soon disappear (Brooks *et al.*, 2003). Evidence is that significant, long-lived abnormal returns come only after true paradigm changes such as industry deregulation or similar 'Schumpeterian shocks' (Pettus *et al.*, 2009), and to targets of changes in control such as acquisitions (Moeller *et al.*, 2005). In most event studies, prices simply react to uncertainty created by the announcement, and - once details appear to show the event was to be expected and already built into valuations - prices quickly return to pre-shock levels

Certainly this shows that markets efficiently incorporate new information in price; but the reversal of price changes that is a corollary of zero CAR means that the information had no permanent impact and was either correctly anticipated by the market (which is hard to accept for true shock events) or the market decided it was irrelevant. This is consistent with extensive evidence that public data plays a limited role in equity price changes. As an example, Fair (2002) examined the S&P 500 futures contract between 1982 and 1999 to identify moves of greater than 0.75 per cent within any five minutes, which is about seven standard deviations above average, and – even well after the fact – could not discern the causes of 90 per cent of moves. A similar situation applies with individual stocks where Bouchaud *et al.* (2009) show that large price jumps (a one-minute return exceeding three standard deviations) typically occur in the absence of new public information. In other words, the largest changes in value of stocks and major stock indexes are almost always caused by factors other than new public information.

The net is that findings of event studies are routinely misinterpreted. In fact, they provide compelling evidence that information only infrequently has substantial permanent impact on equities' price: they are another widely used, but demonstrably unsuitable, finance research technique.

An example of the doubtful value or meaning from event studies is the analysis by Cohen and Wang (2017) which concluded that their study "provided evidence that market participants perceive staggered boards to be, on average, value reducing." The event comprises two court rulings in 2010 on whether a company bylaw that weakens takeover defenses is permissible or not. A Delaware lower court said yes, but was over-ruled on appeal. Although the authors provided detail of the firm and issue in an earlier paper, in this re-examination they do not identify which of two companies is involved and offer a somewhat opaque explanation of the event (page 639):

> These rulings addressed the permissibility of shareholder-adopted bylaw amendments that substantially weaken the antitakeover force of staggered boards at firms whose annual shareholder meetings take place late in the year but not at firms whose meetings take place early in the year.

The analysis involved comparing returns following announcement of the rulings for the firm and portfolios of companies which are most and least likely to be affected by the staggered board rulings. It identifies a two-day differential return of 0.96 percent, which is significant at the ten percent level. The two day event window reflects the fact that there was little or no time on the days of the rulings to assess their import. Neither paper considers the relevance of the window used, nor provides abnormal returns over longer periods.

The paper has other conceptual and methodological shortcomings that are common to event studies. The first relates to granularity of the observations. Early event studies used monthly returns data, then shifted to daily data, but - even though tick data are now readily available - few event studies use them. The times of the two court rulings in the study were identified, and prices could be tracked hourly from before the time of release that could have revealed insights into the flow of price-sensitive information over several days to show what happened.

This is significant because the few granular event studies show that most of the impact of even unexpected events is reflected in price within a few trading hours. As an example, a study of fatal industrial disasters and sudden CEO deaths – which are totally unexpected events that mostly occur in private - using minute-by-minute data found share prices reacted within an hour; half the fall was reversed by the time of the first media reports with the balance reversed by the next trading day (Coleman, 2011). Although few studies in finance have small samples, their granularity can provide fascinating insights (such as the comparison by Petersen and Thiagarajan (2000) of the structure and strategies of two US gold producers with opposite risk policies).

Second, the only reported result is for a two-day window around announcement of the ruling. It seems that there was little or no time on the days of the rulings to assess their import, so two days were allowed. This still leaves the question of what happened before and after the two days. Did the abnormal return dissipate, indicating no substantial impact and match the typical finding that the event study just measured noise. The final issue relates to contribution of the paper, which involves a single subject firm and reports an abnormal return that is not statistically significant at any meaningful level (p>0.06).

To summarize, the authors conclude that they "provided evidence that market participants perceive staggered boards to be, on average, value reducing." In fact they report an event study that does not describe its event, includes abnormal return over just one two-day period, and is not statistically significant at any meaningful level (p>0.06). The study has a single subject firm whose granularity usually provides fascinating insights, but that opportunity is wasted. In keeping with most event studies, the researchers have merely measured noise, and have no justification to for proof of statistically significant permanent impact.

2.2. Fundamental valuation of equities

Fundamental analytical techniques involve "the use of information in current and past financial statements, in conjunction with industry and macroeconomic data to arrive at a firm's intrinsic value" (Kothari, 2001). These studies assume that data proxy for unobservable return generating processes, and incorporate normative intuitions such as a causal link between securities and goods markets whereby economic outlook predicts equity prices. To the contrary, forwards looking economic and financial indicators co-move (e.g. Campbell and Thompson, 2007), so that – as observed by Fama (1981) - "stock returns lead all of the real variables." It is important, then, to understand stylized facts about the relationship between equity returns and accounting variables that may color analysis; otherwise explanations may not be robust to empirical evidence and are merely spurious.

Fundamental valuation of equities invariably incorporates data from quarterly firm accounts. US listed firms release accounts for the first three quarters of the calendar year in 10-Q reports no later than 40 calendar days following the end of the quarter (45 days prior to the beginning of 2003); and their December quarter data is released in 10-K reports no later than 60 calendar days following year end (75 days prior to the beginning of 2006) (see www.sec.gov/rules/final/33-8644.pdf).

Many of these accounting datasets are autocorrelated. Those involving dividends and capital structure are made in light of previous decisions and the strategies of competitors (Coleman, 2014); and one-off transactions such as acquisitions and SEOs are timed to optimize outcomes and are procyclical (Brown, 2011). Also, while data in the balance sheet are levels, those in the income and cash flow statements are flows and subject to seasonality.

Determinants of price changes can be serially correlated, too. Thus investment analysts and ratings agencies change recommendations in increments (Cronqvist *et al.*, 2009); and autocorrelation arises endogenously when investors base decisions on past price changes and establish trending, or when prices serve as a co-ordinating signal for herding (Sias, 2004). A structural relationship arises because equity prices incorporate contingency from their real options properties (Myers, 1977), and non-linearity from feedback between investor decisions, which establishes a convex function between return and variables in firms' income statement and balance sheet (Zhang, 2000).

Just as there are stylized facts about equity price behavior, there are demonstrated bounds. An important limit is that less than about 20 percent of variation in equity returns can be explained ex post and ex ante prediction is markedly less successful (e.g. Welch and Goyal, 2008). Thus there is a large missing, or unmeasured, variables problem: most determinants of equity prices are unknown (or at least cannot be measured).

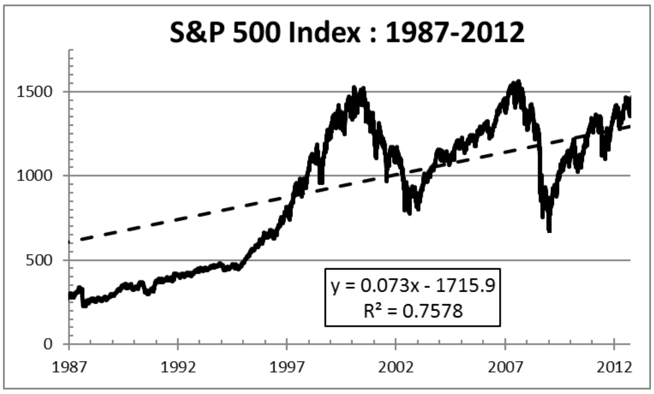
A good example of equity valuation using fundamental analysis comes from Bartram and Grinblatt (2018). They set out to determine the validity of agnostic investing by taking the view of a statistician with little knowledge of finance, and predict return in a regression against accounting variables to. Independent variables comprise 28 accounting items from the most recent 10-Q or 10-K report; and dependent variables are return and price obtained from CRSP. Samples comprise month end prices and the most recent accounting report, which the authors state reflects "the accounting information known to investors" (page 129). Data is culled by removing financial services firms, and firms with a share price below \$5 or negative NTA; variables are winsorized at the 95 percent level. Analysis begins with cross-section linear regression, and parameters are used to calculate the peer-implied fair value of each firm for comparison with market value. This identifies monthly mispricing errors, which are run against Fama-French and other factors in time series regressions to predict month ahead returns. The authors report that their mispricing signal "earns risk-adjusted returns of up to 10 percent per year and is economically significant."

This analysis is problematic for a number of reasons. Starting with the data set, US listed firms release their accounts between six and 12 weeks after the end of the quarter. The net is that prior quarter's accounting data are not available during the January, February, April, July and October months; and are only available for about half of the March, May, August and November moths. This is a fairly substantial misalignment

In addition, the dependent variable is monthly and the independent variables are quarterly. Of the independent variables, 12 come from the income or cash flow statements and monthly values are calculated by summing the four most recent quarters. Thus successive monthly values of each independent variables are identical within quarters, and – for flow variables - three fourths of successive observations are identical. This exacerbates already strong serial correlation. Moreover, the sample covers March 1987-December 2012, or 104 quarters, and there are 28 independent variables: so the data are seriously overfit.

A further issue is that the sample covers March 1987-December 2012, which – as shown in figure 2 - was a period of strongly rising prices when the S&P 500 index rose by a monthly arithmetic average of 1.3 percent, which was not quite matched by the mispricing signal's average monthly return of 0.92 percent.

Fig 2
S&P 500 Index from 1987 to 2012



The paper also fails to mimic the position of a statistician who has little knowledge of finance. Specific restrictions are imposed on the sample, and analysis does not employ a statistician's tools of trade such as considering the ontology of the data, taking into consideration mismatches of data frequency, and overfit of data. Also, although including only some factors parameterises the analysis and biases the investor's apparent skill, there is no explanation for how the agnostic statistician selects the independent variables.

The final issue is what practical implications are provided by the analysis. Whilst it uses data available in real time, the perspective is ex post. That is, if the statistician had known what data to choose and how to arrange appropriate portfolios, she *could* have made a profit. The predictive regressions are just ex post backfitting of data and so are purely explanatory.

To summarize, Bartram and Grinblatt (2018) provide retrospective cross-section and time series regressions between firm price and accounting variables. Accounting data they rely on were not released until weeks after beginning of return assessment periods; returns data are monthly, and the already highly correlated quarterly accounting variables are summed into rolling year totals that emphasizes auto-correlation and over-fits data. The paper lacks practical implications beyond reporting that an investor who knew what data to choose, and how to analyze it and arrange appropriate portfolios, *could* have made an average monthly return which is slightly less than that of an S&P 500 ETF. Overall, there is reason to doubt if agnostic pricing really could work.

2.3. Ex post OLS regression to analyze equity return time series

One of the most common methodologies in finance uses Fama-Macbeth regressions to analyze return time series (Fama and MacBeth, 1973). Since Carhart (1997), it has become standard to use this approach to model the performance of fund managers by regressing their portfolios' return against a set of return-relevant factors (such as market return, size, price-to-book and price momentum: Fama and French, 1993). The regression is assumed to adjust for that part of the portfolio's return which arises from exposure to a known pricing anomaly rather than any skill of the manager (Jordan and Riley, 2015). Pástor and Stambaugh (2002) state the obvious implication: "Alpha [the regression constant or intercept] is often interpreted as skill displayed by the fund's manager in selecting mispriced securities". The approach is widely used to examine more general influences on returns from factors as diverse as firm corporate social responsibility (Galema *et al.*, 2008) and investors' local knowledge (Seasholes and Zhu, 2010). With few exceptions (e.g. Edelen, 1999), regressions find an insignificant intercept, and support the common conclusion that investors have no skill.

A good example of the technique's application is an analysis by Jordan and Riley (2015) into the link between "Volatility and mutual fund manager skill". It explores incorporation of a volatility factor equal to the difference in returns of portfolios of historically low and high volatility stocks. The authors use a sample of mutual funds in their analysis, but for simplicity I use factors provided on Professor French's website to illustrate weaknesses in the technique (see: Israel and Ross, 2017).

Table 3 sets out some simple statistics in relation to the Fama-French factors that illustrate several issues.

Table 3
Annual Fama-French Five Factors 1964-2017

This table reports summary characteristics for annual values of the five Fama-French factors using data from Professor French's website. Panel A shows their average, and the proportion of years they are positive. Panel B shows univariate correlations.

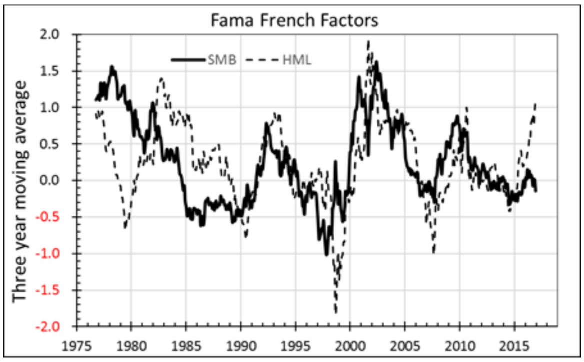
Annual Fama-French Five Factors: January-December 1964-2017						
	Mkt-RF	SMB	HML	RMW	CMA	RF
Panel A:						
Average: percent	6.90	3.64	4.69	3.12	3.74	4.80
Proportion of years >0	0.722	0.593	0.630	0.685	0.611	1.000
Panel B: Partial correlations						
Mkt-RF	1	0.226	-0.248	-0.271	-0.344	-0.184
SMB		1	0.113	-0.163	0.001	-0.055
HML			1	0.085	0.751	0.156
RMW				1.000	-0.075	0.035
CMA					1	0.163
RF						1

The first weakness in this methodology stems from the assumption that the constant in the regression – which is the mean of error terms, or what cannot be explained by independent variables – arises solely from fund manager skill. This is blind to the many other possible influences on return (such as semi-deviation and seasonality, and size of the fund family or the investment team), the manager's motivation to apply their skill to improving performance (especially a bonus tied to fund performance relative to total compensation), and mechanical complications (such as fund flows). It also omits factors that are intuitively important to the investor's opportunity cost such as return on interest bearing securities and international firms.

The strict parameterizing of analysis by including only some factors biases evaluation of the investor's apparent skill. This is particularly important because factors lack any economic justification, and are merely included because they have some long-term relationship with return, even though it is often of tenuous statistical validity. It is becoming more important as the technique is being used to "correct" for firm features such as political donations and SRI measures that have quite remote connection to returns.

A second issue is that the methodology applies an ex post adjustment to reported return which implicitly presumes that portfolio managers can accurately predict factor values, despite their instability. As reported in table 3, factors are positive in an average of 64.8 percent of years, indicating a sign change every two years or so. This is illustrated in figure 2 which plots average annual values of the well-known SMB and HML Fama-French factors that, respectively, show the excess return from portfolios of stocks with small capitalization and high book-to-market ratio relative to portfolios of large cap and low MTB stocks. The significance of the chart is to confirm that capitalization and value attributes of stocks do produce price anomalies, but these are quite unstable, to the extent that they change in sign every two or three years. Thus, while it may be justifiable to apply a long-term average of factors to correct for random return variations, retrospective adjustments are inappropriate penalties for lack of foresight and deny part of the skill of an investor who correctly predicts factors.

Fig 2
Annual values of SMB and HML
This figure plots average annual values of two Fama-French factors.



A further methodological issue is that the right hand side, independent variables in most analyses include a benchmark return, along with a set of factors that describe portfolio traits. These factors are not independent as shown in panel B of tale 3, with many significant univariate correlations

Moreover, Fama and French (1993) style factors show the difference in return between high and low values of the factor, and effectively double the excess return from a correct style tilt. As shown in table 3, most of the factors are almost the size of market return. Thus the collinear variables contain an excess of information about returns, and much of their data is redundant. Importantly the multicollinearity stems from choice of independent variables that mean similar firm attributes, and so is structural.

Some of the subtleties of return regressions are shown in table 4. This follows Jordan and Riley (2015) to build a sample from the CRSP mutual fund database of actively managed US equity funds during 1998-2017 (N=935). The sample excludes multiple share classes, and drops funds identified by CRSP as an index fund, ETF or ETN, or variable annuity, and those with a Lipper fixed income code. Funds must have a minimum of $20 million in assets, and at least one calendar year of monthly data; monthly returns are winsorised at the 95 percent level. Returns are change in net asset value, which includes reinvested dividends, and are net of all management expenses and 12b fees (CRSP, 2018).

The first model runs a univariate regression of fund return on the S&P 500, and model 4 adds the standard Fama-French factors. These give a typical result where fund return after costs and fees slightly outperforms its benchmark.

The other models explore the impact from adding additional independent variables. Model 2 reflects the stylized fact noted earlier that equity returns are correlated and includes lagged return. This confirms studies showing that inclusion of lagged values substantially improves explanatory power (Coleman, 2014). Models 5 and 8 include lagged return in other regressions and also show substantial increase in R squared. . In each case the lagged term is highly significant (t>>3), adds materially to the models' explanatory power, and alters parameter co-efficients. This highlights the general point that many finance data series are autocorrelated and this should be considered.

Models 3 and 7 incorporate three asset classes that the domestic equity fund cannot access – namely return on the MSCI (excluding US) and Treasury bills and bonds – that could be considered as the opportunity cost of investment in US equities. In each case, inclusion of variables that are not related to US equity return roughly doubles the value of the constant.

Model 6 repeats the standard Fama-French regressions by incorporating squares. This shows that HML and SMB have concave relationships with return, which increases both explanatory power and the constant.

The intuition behind these regressions is that independent variables indicate factors beyond the ken of the portfolio manager, and the constant reflects their skill. With no independent variables, the constant equals portfolio return; each addition of an independent variable correlated with return changes the constant, with the implicit assumption that return is explained by fortuitous inclusion of the variable and not to skill. The constant in ex post regression of portfolio return, though, is simply the mean value of residuals not accounted for by the independent variables. Thus it is a measure of unexplained performance, and falls as collinearity between increases the model's explanatory power; it also falls with addition of correlated independent variables irrespective of their actual (i.e. non-spurious) contribution to the portfolio's return.

The nature of OLS regression is that the constant is changed by inclusion of independent variables that are correlated with the dependent variable. It matters naught if the added factors – which could be as diverse as seasonal dummies, measures of valuation, and return from other assets - are return generating or available to the investor. Including factors which are irrelevant to the portfolio's composition but have (spurious) correlation with its return alters fund manager performance. Put differently, the skill of the portfolio manager can be altered according to explanatory variables included in the regression, irrespective of their probative merit. This hardly seems a robust test of skill. As an understatement, it is not obvious how to interpret these regressions, but it is doubtful if they accurately depict fund manager performance and the constant is not necessarily a complete description of the portfolio manager's skill.

Table 4

Regression of portfolio returns against factors and opportunity costs

This table show regression of portfolio return against a variety of factors and opportunity costs. The sample is drawn from the CRSP mutual fund database of actively managed US equity funds during 1998-2017 (N=935). It excludes multiple share classes, and drops funds identified by CRSP as an index fund, ETF or ETN, or variable annuity, and those with a Lipper fixed income code. Funds must have a minimum of $20 million in assets, and at least one calendar year of monthly data; monthly returns are winsorised at the 95 percent level. Returns are change in net asset value, which includes reinvested dividends, and are net of all management expenses and 12b fees.

Variable	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Constant	0.107 ***	0.080 ***	0.170 ***	0.080 ***	0.060 ***	0.073 ***	0.128 ***	0.115 ***
Lagged return		0.154 ***			0.182 ***			0.293 ***
S&P 500 return	0.003 ***	0.003 ***	-0.003 ***				-0.006 **	-0.026 ***
MSCI excl US return			0.005 ***				0.004 ***	0.004 ***
3-month T bill			-0.016 ***				-0.004	0.010 ***
10-year T bond			-0.001 ***				-0.004 ***	-0.006 ***
Excess return (R_M-r_F)				0.003 ***	0.003 ***	0.003 ***	0.005 **	0.024 ***
HML				-0.004 ***	-0.005 ***	-0.003 ***	-0.005 ***	-0.005 ***
RMW				0.005 ***	0.005 ***	0.005 ***	0.007 ***	0.007 ***
CMA				0.003 ***	0.004 ***	0.003 ***	0.008 ***	0.011 ***
SMB				0.007 ***	0.007 ***	0.006 ***	0.003 ***	-0.002 ***
Excess return - squared						0.00001 ***		
HML - squared						-0.00004 ***		
RMW - squared						0.00002 ***		
CMA - squared						0.00005 ***		
SMB - squared						-0.0002 ***		
Adjusted R-squared	0.076	0.117	0.133	0.256	0.282	0.292	0.303	0.351
Observations	5760	4285	5760	5760	4285	5760	5759	4285

Even a simple analysis such as that above shows that regressions with correlated independent variables and spurious or irrelevant links between return and financial time series inevitably bias the constant. There is also a large missing variables problem from omission of lagged fund return and performance-related measures. Another gap in explanatory power comes from limiting analysis to linear relationships, Perhaps ex post regression is useful for its original purpose of explaining returns. But it seems inappropriate to assessment of an investor's ex ante skill by selectively correcting it for factors that are unstable and have little theoretical justification.

To summarize, Jordan and Riley (2015) use Fama-MacBeth and Fama-French regressions which parameterize analysis by omitting intuitively important factors such as other portfolio traits and opportunity costs. It adds further bias because an investor who correctly predicts factors loses much of the credit due to her skill. Volatility's significance is assessed after sorting portfolios by volatility, which persists so the high volatility portfolio axiomatically has a lower Sharpe Ratio even when returns are identical; and a comparison of sample means between high and low volatility portfolios shows they are barely significantly different ($p<0.06$). The conclusion in favor of volatility's significance is doubtful.

2.4. Sharpe Ratio for comparison of portfolio returns;

Where the previous section reviewed the Carhart technique of explaining mutual fund performance which dominates academic research, an alternative approach of particular interest to investors and practitioners (as well as researchers) is comparison of funds' performance. There are now many measures: for instance Farinelli *et al.* (2008) examine 11 ratios, Caporin *et al.* (2013) include 24 performance measures in their survey of this literature, and Cogneau and Hübner (2009) list 107. The most popular technique, though, remains the Sharpe (1966) ratio which is the value of a portfolio's excess return above the risk free rate divided by standard deviation of its returns. A mean-variance optimizing investor will prefer the fund with a higher Sharpe Ratio (hereafter SR).

SR is universally seen as a logical criterion for an investor who has chosen exposure to equities and has been used in numerous comparisons of portfolio performance. A recent example is the study by Barroso and Santa-Clara (2015) which uses the Sharpe Ratio as the sole criterion to compare the return to portfolios of stocks with recent price momentum relative to the market and to portfolios constructed around value and size factors. Their analysis approvingly finds that "momentum has offered investors the highest Sharpe ratio" (where SR for momentum is the annual average of monthly excess return for the momentum factor divided by standard deviation of its monthly returns).

This result actually arises from a bias in the Sharpe Ratio where it favors portfolios that have fewer negative years. This is because in a year when two portfolios have identical negative returns, the Sharpe Ratio is higher for the portfolio with the higher volatility: thus an investor using SR to choose between firms with identical negative returns mistakenly chooses the higher, not lower, volatility firm. Although the authors' conclusion is mathematically correct, once this SR's bias is taken into account it is simple to show that momentum does not give investors a higher volatility-adjusted return.

Table 4 reports aggregates of Fama-French factors by calendar year during 1927-2017. Out of the 101 calendar years, momentum had 18 of negative return, and RM-RF had 25 losing years. In these years, average RM-RF return was -16.5 percent and only one tenth below return of momentum; the latter had a much higher standard deviation, though, and its Sharpe Ratio averaged one seventh more than RM-RF. Thus momentum appears superior to market. However, if the Sharpe Ratio is corrected for sign in the negative years (calculated as: return*standard deviation rather than return/standard deviation), that for momentum would be about one fifth below RM-RF: momentum is inferior to market.

Table 4

Comparison of statistics for momentum and market factors

The table uses a sample of the Fama-French momentum and market factors by calendar year during 1927-2017 to compare their return and Sharpe ratio. Panel A reports data for years when the S&P return was positive; panel B relates to years when the S&P return was negative; and panel C includes all 101 years.

	Year	Average Return	Average Standard Deviation	Average Sharpe Ratio
Panel A: Years of negative return				
Momentum	18	-14.9	18.7	-0.75
RM-RF	25	-16.5	13.9	-0.85
Panel A: Years of positive return				
Momentum	73	13.5	8.3	1.34
RM-RF	66	17.3	11.0	1.50
Panel C: All years				
Momentum	91	7.90	15.82	0.93
RM-RF	91	8.02	19.24	0.86

To summarize, Barroso and Santa-Clara (2015) use the Sharpe Ratio to compare the return to momentum with that from the market and value and size factors, and concludes that "momentum has offered investors the highest Sharpe Ratio". While this may be mathematically correct, the Sharpe Ratio favors high volatility stocks in years of negative return (of two stocks with identical negative returns, the one with the higher volatility has the higher Sharpe ratio). Once returns are disaggregated between up and down years, the volatility-adjusted return of the market is superior to that of momentum. Sharpe Ratio does not show investors the actual higher volatility-adjusted return. In practice, for most equity investors the Sharpe Ratio is not fit for purpose.

2.5. Simplified, closed form asset pricing models

Previous sections examined methodologies that explain equity prices in terms of observable data and firm traits. This section turns to the theoretical link between data and return which encompasses the locus of investor decisions. Because this area is opaque and little known, it is often described theoretically by models that are built up from assumptions about investors and their motivations.

In reality little is known about how investors make decisions. One fact is that all but a trivial portion of equity transactions involve financial institutions or other firms[4], so most investors act in the role of agents, not principals. Institutions' business model is built around commissions from funds under management or transactions; decisions are made by salaried managers whose compensation is minimally related to performance of funds under their stewardship; and – given annual turnover in excess of 100 percent (Edelen, 1999) – their investment horizon is less than a year. Institutions are risk neutral, and respond to their business needs. They have no control over fund flows, and make decisions involuntarily in response to exogenously determined fund flows: market makers trade to balance holdings in light of liquidity, while institutions compete within global oligopolies for clients who are net savers because of fiscally-promoted retirement plans. As noted earlier, many decisions by analysts and corporates are serially correlated, and price establish correlation by co-ordinating trends and herding.

Beyond these and a few other stylized facts, little is known about investors decision process. Certainly studies claim to report relevant field evidence such as Faff *et al.* (2016), Gompers *et al.* (2016), and Graham and Harvey (2001). But these are usually surveys that restrict respondents to choosing from normative answers. In most cases they are closed form, with alternatives drawn from theoretical explanations in the academic literature and few open choices. They are classic, albeit qualitative, examples of in-sample validation. As a simple illustration of what is missed, no surveys offer choice of payout and capital structure in light of competitor decisions or expectations of influential clienteles despite their well-recognized importance (Baker and Wurgler, 2004).

To fill these gaps in understanding equity return generating processes, a rich finance research literature has developed that builds closed form solutions to fully explain observed return distributions. They have diverse approaches, but models typically explain a dataset by making a number of assumptions, imposing bounds, and linking mathematical expressions. Assumptions include some of the following: the process underlying equity returns – such as dividends - is random; there are no market frictions; there are two groups of representative investors who have identical risk and time preferences; and there are two investment alternatives, namely equities and a risk free asset. Bounds include: only positive changes in levels of returns; constant values of risk free return, risk and time factors; and time-invariant model parameters.

A good example of this process is given by Barberis *et al.* (2015) who develop an "X-CAPM" in a logical process that sets out stylized facts about equity prices, then moves through hypothesis development and empirical testing to advance an innovative theory.

Amongst the paper's principal assumptions are that price is driven by dividend, which follows a random process. As noted earlier, dividends are strong autocorrelated, and not all firms pay dividends. Another assumption is that variables driving the relationships are constant including investor preferences (risk aversion, γ, and time discount factor, δ), the risk-free rate and volatility. Although these can be stable for a period, it is clear that equilibria are regulalrly punctuated and values can swing sharply. A third assumption is that investors have exponential utility, whereas all but a trivial portion of trading volume is through firms

[4] Data on trading volume by investor type are scarce. However, Kaniel *et al.* (2008) show that individuals are responsible for about four percent of trading volume on the NYSE; and Evans (2009) reports NYSE data that put it at less than half that level. According to NYSE, designated market makers account for 12 percent of volume (www.nyse.com/publicdocs/nyse/markets/nyse/designated_market_makers.pdf).

and financial institutions which are risk neutral with a short term horizon. A final assumption is that the value of the stock market is determined endogenously, despite much of the authors' own work showing extensive exogenous influence.

Several times the analysis is parameterized so that it does not reflect real world conditions. This is particularly true in relation to sentiment, S_t, which is the model's main driver of price. It is proxied by weighted past price changes, dP. and calculated by integrating the expression $e^{-\beta(t-s)}dP_{s-dt}$ where β is the weighting on lagged price changes and s refers to lags.This has a significant missing variables problem because it ignores the price level relative to history (which is known to be important through the wealth effect, expectation of mean reversion, disposition effect, etc), exogenous conditions (macro variables, returns on other asset classes) and stock-specific considerations. No explanation is given for the expression used. It is intuitively likely that most recent prices have greatest influence on extrapolators, but an exponential decrease is just one way to parameterize this, and work by two of the authors (Greenwood and Shleifer, 2014) suggests a simpler weighting such as λ^s, where λ is a constant. The way S is constructed is important because conclusions are an artefact of the specification and tautological.

There are further tautologies. One starts with the assumption that constants in univariate regressions of future returns, dividend growth and dividend-price ratio against current dividend-price ratio are approximately equal to 1,0 and 0 at long horizons. Thus no variables other than the current dividend-price ratio explain future returns, even though current dividend-price ratio is known to explain a small fraction of future returns. Not surprisingly the resulting expression shows that dividend yield predicts price changes. A second tautology starts with the observation that "price-dividend ratios are highly correlated at short lags" (which contradicts the assumption that dividend level follows a random walk) and defines the autocorrelation as equal to e^{-kt_1}, which introduces another negative exponent whose decay matches empirical facts.

This approach to modelling has three serious limitations. The first is that the assumptions are excessively simplified. It is generally accepted that returns are not random given evidence of their predictability; and virtually all trading is by institutions which are not unconditional performance optimizers and whose decisionmakers are salaried employees who are probably risk neutral. Even casual observation of market behavior shows its features are time varying due to shocks, cyclical influences and shifting determinants. The second limitation is that models omit multiple variables and influences; are blind to seasonality and structural breaks; and ignore information innovation and shared co-ordinating signals or shocks. The third limitation is that expressions are usually not justified theoretically or empirically, and invariably have negative power relationships.

In short, each model is an ansatz with simple structure, imposed boundary conditions, and unsupported equations that are assumed to depict relationships. Moreover, it is almost always impractical for any reader to readily replicate the study; and parsimonious depictions that cater for non-specialists are rare.

When over 80 percent of returns cannot be explained, it seems conceptually hard to understand how it is possible provide a closed form solution that relies on simplified assumptions that are not explained, unlikely parametrizations, and unsupported relationships, including one that contradicts empirical findings of two authors. The answer is that equity prices – like data in many natural systems - are lognormally distributed because they are driven by numerous multiplicative non-linear parameters. Non-linear pricing models with negative exponents will – even in the absence of any causality – generate a lognormal or fractionally integrated data generating process. Thus it becomes a simple matter to ex post align the combined system with observed lognormal data, even in the absence of any causality. Naturally these models cannot be applied or quantified and have zero predictive capability, which is why they are never validated by out-of-sample tests. Models merely show that stylized facts about equity prices can be explained by inverse power relationships that matches prices' lognormal distribution.

3. Discussion: methodological shortcomings in finance research

The findings above were a shock to me. The five articles subjected to detailed analysis were published in the *Journal of Financial Economics* which is the finance discipline's leading generalist outlet with an impact factor second only to the *Journal of Finance*; each had at least two authors, and they came from the world's top universities. The articles are marked by impressive analyses, often faithfully replicating methods that have survived with little change since the 1970s. They epitomize the discipline's best. They also epitomize the scathing observation by Nobel economics laureate Paul Krugman in his regular *New York Times* column (2 September 2009)[5]: "The central cause of the profession's failure [to foresee the global financial crisis] was the desire for an all-encompassing, intellectually elegant approach that also gave economists a chance to show off their mathematical prowess." The articles critiqued here had simplistic or non-existing theoretical justification and/or report trivial or irrelevant results. None should have been published.

Although it is sometimes argued that weaknesses in economic theory are due to economists' predilection for mathematical modelling (amongst many see Lawson, 2009), this review suggests that it is probably more accurate to blame inappropriate analytical methodology, no matter whether it is mathematical or not. The papers display four particular weaknesses: failing to design methodology in light of equities' data and characteristics; neglecting to incorporate the objectives and capabilities of investors; incorporating data that is unknowable at the time; and preferring mathematical tractability over reality. Each of these leads to neglect of unique features of the equity return generating system and its data: consider them in turn.

Few of the papers thought about their data or methodology. They just bored ahead – citing the precedent of some Beatles-era guru – to compile a sample and get straight into analysis which displays researchers' strengths. The most obvious omissions are preference for large samples without examination or validation of the individual data, and adopting overly rigorous analytical techniques despite susceptibility of the data to estimation or measurement errors. The major shortcoming in the studies is not their mathematical rigor, but the absence of any critical evaluation of what underlay the elegant maths.

A second methodological weakness is that virtually all the papers' data depends on human actions and decisions, but their analyses and theories are rarely informed by detail of investors' capabilities and objectives. It is standard to describe investors as risk-averse individuals who optimize volatility-adjusted return over the securities' life, and ignore markets' obvious dominance by risk-neutral institutions seeking to maximize their own

[5] www.nytimes.com/2009/09/06/magazine/06Economic-t.html

profitability over a one-year holding period. Nor do researchers consider the consequences of human involvement in decisions that leads to serial correlation of prices and return generating processes. Too many analyses are structured around disproven normative paradigms, and impose them through parametric analyses.

The third weakness of financial modelling is to incorporate data that is unknowable at the time. There is a tendency to think that simply incorporating lagged variables into an ex post analysis is predictive; but analysis is ex post and of little use in making economically significant predictions, which explains why out-of-sample tests are so rare. The final weakness is to prefer mathematical tractability over reality, which leads to imposition of artificial constructs that oversimplify and parameterize models to stay within normative bounds.

These methodological weaknesses result in studies that only provide the correct answer in years of positive returns; model equity returns lucidly even though ex post theoretical analysis can explain relatively little; report the influence on returns of variables that are not released until after the evaluation period closes; and enthusiastically proclaim the significance of statistically insignificant findings from small samples. There is little to justify the authors' confidence in their conclusions.

4. Conclusion: Building an improved philosophy of finance research

The dictionary meaning of theory is 'a system of ideas explaining something', and this article makes the case that equity pricing theory fails that test. The discussion above has highlighted the need to strengthen traditional analytical methodologies by paying greater attention to research philosophy, especially its ontology which examines the properties of data and relations between its measures, and epistemology which addresses analysis and scope of the discipline's knowledge. This brings together deeper understanding of the nature of data and its determinants and measures, pinpoints suitable methodologies to analyze the data and draw testable hypotheses, identifies appropriate theoretical frameworks, and validates the resulting paradigm against real world observations and puzzles. This extends the observe-hypothesize-test-theorize scientific method by explicitly matching data to appropriate analytical methodologies and theoretical framework, and overlaying another step which evaluates conclusions.

It is rare to see a paper in any finance journal that does not have a theory section, which typically provides multiple mathematical relationships and supporting economic explanations that motivate the study. As noted above, most of the assumptions do match the reality of markets; their techniques do not stand up to even mild scrutiny and prove unable to meaningfully predict asset prices. Most studies are retrospective, at best using lagged values of explanatory variables to explain the cross-section of returns, or analyzing time series ex post. Virtually none provide evidence from field studies or out of sample tests to support their analysis and conclusions.

Compounding the weakness of financial analyses is their lack of scientific discipline. Most studies are little more than sophisticated data mining, or re-analysis of relationships in data sets that have already been mined. Few studies begin with the observe-hypothesis strategy of the scientific method; most lack a rigorous intuitive base. More broadly, researchers do not share a grounded description of how markets, investors and asset prices actually work, so much analysis is not well-informed. This article examines research into equity pricing through the lens of articles written by leading researchers, that credit many commentators, and have been published in the discipline's best journals. Each is unsound and contains little theory with its meaning of explaining equity prices.

Finance researchers dismiss the inability to apply many theories, endorsing them as the best there is and tractable for teaching and research. Unfortunately there is clear evidence that inadequate asset valuation tools have steadily rising systemic opportunity costs, particularly through declining profitability of firms, declining earnings yield on equities, and investors' increasing preference for passive management of their savings. When a similar situation has arisen in sectors of other important aspects of life across medicine, engineering and science, there has been industrial scale efforts to improve knowledge. Finance theory now needs its own extinction event, and concentrated efforts to develop replacements.

The solution is clear, if not simple. University curricula should explicitly teach research students how to examine the ontology and epistemology of their data (good starting points are Lawson (2003) and West and Hepworth (1991)). Conferences and journals should announce three changes to their policy on paper acceptance: each paper should include a methodology section that clearly describes the nature of the data and the system under study, and what is factually known about their determinants and behavior; preference will be given to papers that progress grounded theory, including field studies and more appropriate research methodologies; and papers that incorporate assumptions which are not consistent with facts must justify their relevance to other researchers. Progress could be accelerated by conferences and special issues with the theme of developing real-world finance theory.

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