

**FROM:** Ted Aronson

**DATE:** May 5, 2022

**RE:** “EVERYTHING YOU KNOW IS WRONG”

Larry Siegel’s review of *The Economics of the Stock Market* opens with:

Einstein’s theory of relativity advanced Newtonian physics. That did not mean Newton was wrong — only that his theories could be improved upon. In an ambitious new book, the economist Andrew Smithers rejects core “Newtonian” principles of economics, replacing them with radical departures from conventional wisdom.

Andrew Smithers is ambitious indeed, thus Larry’s title.



Einstein Discovers That Time Can Stop Completely

(Larry references his “Read Your Sharpe and Markowitz,” which we shared in 2014. A copy follows.)

Attached is our usual sampling: book jacket, foreword, contents, and introduction.

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## “EVERYTHING YOU KNOW IS WRONG”

Laurence B. Siegel

May 2, 2022

Einstein’s theory of relativity advanced Newtonian physics. That did not mean Newton was wrong — only that his theories could be improved upon. In an ambitious new book, the economist Andrew Smithers rejects core “Newtonian” principles of economics, replacing them with radical departures from conventional wisdom.

But as I will explain, unlike Einstein, some of Smithers’ theories fail meet the standard of empirical verification.

### LOOKING AT THE WORLD FROM A DIFFERENT ANGLE

Sometime in the early 1970s, the comedy troupe Firesign Theater recorded an album called “Everything You Know is Wrong.” It was a satire on “new age” thinking: Dogs flew spaceships! The Aztecs invented the vacation! Men and women are the same sex! Aliens are living like Indians in an Arizona nudist park.

You get the idea.

Smithers’s new book, *The Economics of the Stock Market*, presents bold and provocative theories that advance our understanding of financial theory. But there are moments that are reminiscent of Firesign Theater — at the extreme, leading one to believe that everything one knows about the stock market is wrong. Here’s Smithers channeling Firesign:

- The expected real equity return is stable, at 6.7%, no matter what is going on in the bond market.
- The risks of equities “fall sharply as the time horizon lengthens.”
- Bond yields fluctuate “within narrow ranges.”
- Companies don’t try to maximize profits.

It’s easy to dismiss these sweeping statements that contradict our understanding of markets. But it’s not easy to argue with a man who “in 1956... went up to Clare College Cambridge to read economics,” and who has been thinking about economic principles and putting them into practice for more than 60 years. Smithers is eminently qualified to question the tenets of neoclassical economics as it has been applied to finance in those 60 years, and he does so in an exemplary way: no math, crystal-clear writing, and a rich vein of data graphics.

### CLASSIC FINANCE IS STILL CORRECT AND RELEVANT — MOSTLY

Smithers’ central thesis is that companies try to maximize their stock price, not profits. Because of the structure of top-executive pay — “pay them in stock,” the legacy of Harvard Business School professor Michael Jensen — companies almost certainly favor maximizing the stock price over some other measure of profit when there is a choice to be made. (I will argue that classic finance is correct in asserting that the stock price is usually the best measure of long-

run profit expectations, so that paying executives in stock is appropriate — unless the pay levels get out of hand, a separate issue.)

Smithers then uses this observation to try to overturn classic finance, the work of Harry Markowitz, William Sharpe, Franco Modigliani, Merton Miller, Fischer Black, and Robert Merton.

He fails in that effort.

Smithers does what many critics have done when faced with evidence that their theory doesn't fit the facts: He seeks to overturn the whole thing. In place of classic finance, Smithers proposes a blend of Keynesian thought (consistent with his Cambridge education) and behavioral economics.

It's a seductive proposition but doesn't quite work. As I wrote in a 2014 essay, "Read Your Sharpe and Markowitz," published by the CFA Institute,

Some of [the] findings [of the long list of pioneering scholars] aren't exactly right. [But] classic finance forms a base case or null hypothesis against which empirical facts, new theories, and conjectures can be tested. Without it, we are lost. With it, we have a set of very useful guideposts, a little like Newtonian...physics.<sup>1</sup>

Newton's theory of gravitation ignored air resistance. That is not a bug but a feature. It says how gravity would behave in the absence of any complicating factors, air resistance being only one. It's a point of departure for a richer physics that does account for air resistance and any other frictions you encounter in real life. Without Newton, Einstein would have gotten nowhere.

And, while we may adopt some of the ideas suggested by Smithers in crafting a practical model of investment markets that includes "air resistance" — the frictions of living in an imperfect world — the baseline theories of classic finance are correct. We should learn them thoroughly before trying to identify the relevant exceptions.

## WHY THIS BOOK REVIEW IS UNCONVENTIONAL

I am not recommending that advisors read this book; it is too long, detailed, and technical for all but the most determined students of finance. But don't ignore it! That's what makes this an unconventional book review. Smithers's ideas, good (so as to include them) and bad (so one can understand and critique them), need to make their way into investor education as a *part* of the whole picture. That can only happen if the top-level investment thought leaders — writers, economists, consultants, strategists, and chief investment officers — either read this book or what others say about it and grasp its merits and shortcomings.

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<sup>1</sup> Siegel, Laurence B. 2014. "Read Your Sharpe and Markowitz." CFA Institute Magazine (September/October). I confess that, on re-reading my description of the "Read Your Bible" and imaginary "Read Your Darwin" banners in the Scopes Monkey Trial courtroom, I find it very poorly worded. I have re-writing this article on my bucket list.

I'll discuss Smithers's central point, that public corporations maximize their stock price rather than "profits." Smithers, along with many others, alleges that this behavior causes distortions: misallocation of capital, excessive risk-taking, and so on. I'll evaluate this claim. Then I'll poke at a few of Smithers' more doubtful claims. I'll close by praising his critique of macroeconomics.

## CORPORATIONS MAXIMIZE THEIR STOCK PRICE INSTEAD OF "PROFITS" — OK, MAYBE

In a book review written for Reuters, Edward Chancellor, himself a formidable commentator on economic and market matters, summed up Smithers's corporate-profits view as follows:

[Smithers] lifts the corporate veil to reveal a world in which the managers of public companies put their own interests first and seek to maximise current share prices rather fundamental values.<sup>2</sup>

Not a big surprise. The important question here is whether we have a better estimate of fundamental value than the market does. Corporate insiders might; active portfolio managers think they do, but the track record of active management says they don't.

Agency theory is the discipline that studies this issue. The theory, first discussed at length by Alfred Marshall (1842-1924), explores the consequences of hiring executives to manage the day-to-day affairs of a corporation that they do not own.<sup>3</sup> This "separation of ownership and control" (principal and agent) sets up a wedge between what is best for the corporation (meaning its owners, the shareholders) and what is best for the executives personally.

To minimize that conflict of interest, Michael Jensen, whom we met earlier, recommended paying executive bonuses in stock or options. Fine. Accounting profits (earnings) are not the profits a company should be maximizing; economic profits are, and Jensen's presumption was that the markets — in setting stock prices — estimate them more accurately than accountants and corporate CFOs do.

Smithers disagrees; let's see why.

The practice of paying executives in stock can be abused. Companies can boost apparent profits (accounting earnings) by writing off all failed projects in one quarter, called a kitchen-sink quarter, so the losses don't poison subsequent quarters' earnings. *If* such actions fool the market enough to raise the stock price above fair value, executives who are paid in stock or options will benefit at the expense of shareholders. That should not be allowed.

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<sup>2</sup> <https://www.reuters.com/breakingviews/global-markets-breakingviews-2022-04-14/>

<sup>3</sup> See Marshall, Alfred, 1890, *Principles of Economics*. Adam Smith [1776, *Wealth of Nations*, bk. V, ch. I, pt. III, art. I] had an inkling of it. [Gardiner Means \(1931\)](#) called it the separation of ownership and control, and this terminology became standard, as did "principal-agent conflict." Means's work prefigured extensive research in the second half of the last century by a series of superstars including George Stigler, Eugene Fama, and Michael Jensen. I call it agency theory. Smithers's book is in this tradition and is a strong contributor to it.

Another abuse is to time stock buybacks to maximize executive stock- or option-based compensation. While this practice may run afoul of insider trading laws, it happens. This, too, hurts shareholders.

These critiques resonate at an emotional level — how dare they? It's my money! Let's look a little deeper. If active managers could add alpha by observing and reacting to these abuses, as well as the many other opportunities to take advantage of corporate folly, their success would show up in the data. It doesn't. According to a 2014 article in the *Chicago Booth Review*, "Before costs and fees, active managers on average beat their benchmarks by 5 [basis points]. After costs and fees, they underperform the benchmarks by 5 [basis points]."<sup>4</sup>

Although the debate on whether active management adds value will never end, the study referenced in the Booth article is one of many documenting that stock prices, while never perfectly fair, are fair enough that they measure profit — specifically, the present value of all expected future profits — better than any accounting-based measure available to analysts. Pay executives in stock, and make sure they obey all applicable laws because the economic reward for cheating is so large.

Smithers' argument that companies maximize stock prices "instead of" profits is unconvincing. They try to maximize stock prices by maximizing profits. They do not always get it right. That is very different from saying that are trying to achieve the wrong goal.

On to Smithers's more Firesign Theater-like claims.

### **BOND YIELDS FLUCTUATE WITHIN NARROW RANGES — NOT!**

This claim is so silly that every experienced investor will see through it immediately. The 10-year U.S. Treasury bond yield has more than doubled just in the past year!<sup>5</sup>

Fortunately, he later changed the story to "real yields fluctuate within narrow ranges," which is more accurate but still wrong.

When you look at the long sweep of history, high volatility in bond yields isn't even rare. Let's stick with nominal yields. A hypothetical "consol" bond issued in the distant past (it doesn't matter when) with a par value of \$100 and a coupon rate of 3% would have been worth a

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<sup>4</sup> <https://www.chicagobooth.edu/review/why-active-managers-have-trouble-keeping-up-with-the-pack>. The study referenced in the article is not just another active management performance study. It is the now-classic study by the Booth-Wharton team of Luboš Pástor, Robert Stambaugh, and Lucian Taylor, "Scale and Skill in Active Management," published in the *Journal of Financial Economics* in April 2015. (I'll note, and then set aside, their implication that fees and costs add up to only 10 basis points; typically they are much larger.) More recent data indicate that average manager alphas have gotten worse, not better.

I owe it to my good friend Ted Aronson, who is himself an active manager and who is co-publishing this article, to note that while the average manager is not very good, the best managers are excellent, adding a lot of value over time. See [Siegel, Kroner, and Clifford \(2000\)](#).

<sup>5</sup> In fact, it very nearly tripled. Intraday low of 1.127% on August 4, 2021; intraday high of 2.981% on April 20, 2022.

measly \$20 when long-term-bond yields peaked around 15% in the early 1980s.<sup>6</sup> In 2020, when yields reached rock bottom, it would have been worth \$303. So much for nominal bonds staying within relatively narrow bounds.

## EXHIBIT 1 U.S. TREASURY BOND YIELDS (TYPICALLY 10-YEAR MATURITY) SINCE 1790



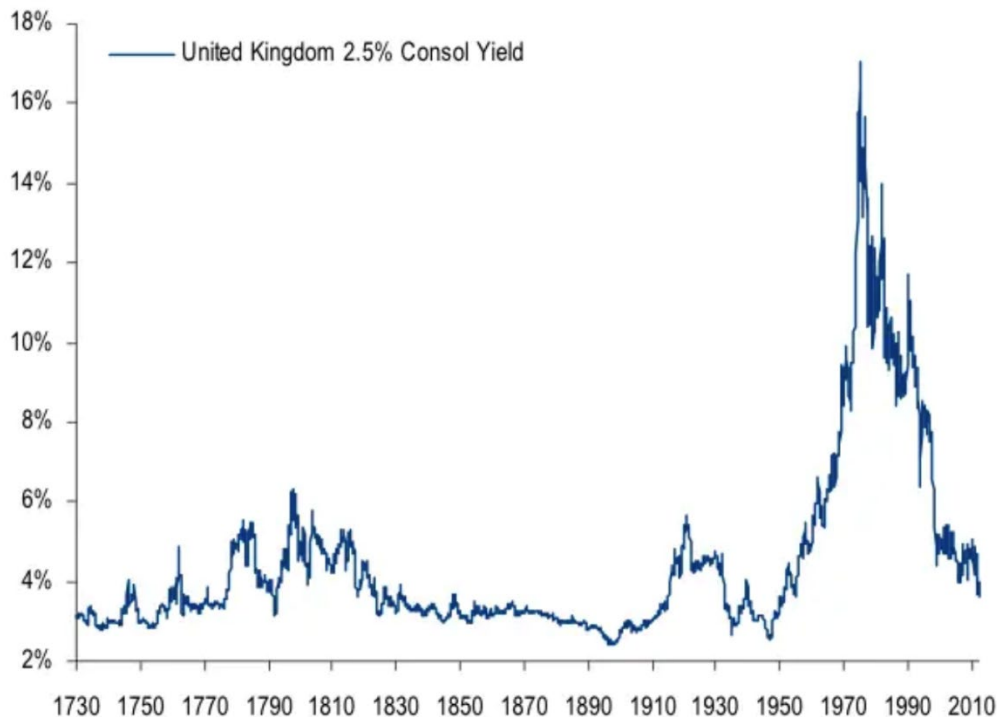
Source: <https://www.businessinsider.com/10-year-us-treasury-note-yield-since-1790-2012-6>, drawing on Michael Hartnett's report "The Longest Pictures," Merrill Lynch Equity Strategy Group, 2012.

<sup>6</sup> A consol bond is one that never matures, but pays its initial coupon rate forever. The bond can be sold to another investor. Consol bonds were traded in the U.K. from 1730 to 2014, as Exhibit 2 shows. The data were collected by [Michael Hartnett](#). The word "consol" is short for "consolidated annuity." In 2014 the bonds were "called," that is, redeemed whether the investor liked it or not. This did not violate the bond covenant because a call provision was written into the covenant, as it is for many bonds. (You should be skeptical of government promises to do anything forever, but the British government acted pretty responsibly in this episode.)

The volatility of the bond market in Smithers's home country, the United Kingdom, was even greater:

## EXHIBIT 2

### BOND YIELDS (CONSOLS, I.E, PERPETUAL BONDS) IN THE UNITED KINGDOM, 1730-2012



Source: <https://www.businessinsider.com/british-bond-market-1730-2012-6>, drawing on Michael Hartnett's report "The Longest Pictures," Merrill Lynch Equity Strategy Group, 2012.

(That little blip around 1776 had to do with some unpleasantness in the colonies. The next, larger blip around 1800 was the Napoleonic War. World War I shows up distinctly, and the much larger mountain after that is the great inflation of, roughly, the 1970s.)

You may say "no fair" to the consol example because almost nobody invests in such long instruments. But Smithers said that yields on both bonds *and cash* fluctuate within narrow ranges. Cash yields in the United States fluctuated almost exactly as much as long bond yields — from 13.99% in March 1982 to 0.01% in early 2021. Real yields on cash also fluctuated widely, although not nearly as much as nominal yields. Case closed.

Smithers is a distinguished and very senior economist. I hope his claim that "yields" (implying nominal) was just an editing mistake. He knows that bond yields fluctuate, causing great disruption. If it is no more than a typo, he will get an apology; either way, you just got a history lesson.

## DO EQUITY RETURNS MEAN-REVERT, REDUCING RISK FOR LONG-TERM INVESTORS?

A great deal of ink has been spilled on this question. My answer is complex. While equity returns may mean-revert, investors with long holding periods do *not* face less risk than short-term investors; they may even face more. Let's explore.

Every market will exhibit *apparent* mean reversion over some time frame, as long as that market was not destroyed by war or some other force. That doesn't mean the market is safe and that low returns will be followed by high ones — it means that a market survived because it *was* safe and low returns were followed by high ones. That's what survival is. If you can't know for certain that a market will survive, you should not count on mean reversion.

And some of the world's most promising markets — Germany, Austria-Hungary, Japan, Russia, China — did in fact fail. Although the U.S. and U.K. would have looked pretty good to a global investor in, say, 1900, so did all those countries that lost a war or suffered a Communist takeover. The fact that some markets later rose out of the ashes and succeeded (I am thinking of China — it's not clear what will happen to Russia) did not help the 1900 investor, who was wiped out. Those markets did not become safer with longer holding periods!

While the U.S. is a good bet for survival, nothing is guaranteed. We should invest *as though* risk expands with the holding period — Paul Samuelson said that “time spent recovering from crashes is also time spent waiting for more crashes”<sup>7</sup> — and then hope that the opposite happens.

## ARE EQUITY AND BOND EXPECTED RETURNS CO-DETERMINED?

This is a toughie, so I saved it for last. “Co-determined” is just econo-nerd talk for “determined by the same underlying factors, so that they move in a somewhat parallel fashion.” If they are not co-determined, there is no direct connection. The latter is what Smithers believes.

The expected return on equities cannot be directly observed, so it must be inferred from other information. There are as many ways of doing this as there are financial economists. With a colleague, I am compiling a book on the subject: *Equity Risk Premium Forum 2021*, to be published by the CFA Institute Research Foundation.

To say that equity and bond expected returns *are* co-determined means accepting the Ibbotson and Sinquefeld analysis: The expected return on an equity index equals the riskless rate (that is, the yield on Treasury bonds or cash) plus a risk premium for bearing the additional risk of equities as compared with bonds or cash.<sup>8</sup> The risk premium may be variable (the currently popular view) or stable.

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<sup>7</sup> Quoted in Siegel, Laurence B. 1997. “Are stocks risky? Two lessons,” *Journal of Portfolio Management* (Spring).

<sup>8</sup> Ibbotson and Sinquefeld's analysis begins with the Fisher equation,  $R_{fr} = R_f - i$ , which defines the real riskless rate,  $R_{fr}$ , as the nominal short-term (Treasury bill) riskless interest rate,  $R_f$ , minus inflation. It is due to Irving Fisher of Yale, the nice man who said that the stock market in 1929 had reached a permanently high plateau. He was a great economist. Really. (See Fisher, Irving. 1907. *The Rate of Interest*. New York: Macmillan, 1907; Mansfield Centre, CT: Martino Publishing, 2009.) The real expected return on bonds is then  $R_{fr}$  plus a horizon premium for bonds over bills; an equity premium for stocks over bonds; and so forth in a building-block manner.

Smithers' proposition, however, relies on the closely related but not identical Fisher *effect*, which is not a definition but a testable hypothesis: that nominal interest rates change to follow changes in the inflation rate. In this



To say that equity and bond expected returns are *not* co-determined means that the market arrives at them independently, driven by unrelated processes. In his construct, the stock return (on average over time) is equal to inflation plus 6.7%, a simple process if ever there was one. The bond return, according to Smithers, is determined as follows:

[S]avings and investment are equated by movements in the short-term interest rate and corporate leverage is balanced with the preferences of the owners of financial assets by variation in the bond yield.

Got that? It sounds complicated, but it's standard Keynesian stuff, taught in school.

### **STOCK AND BOND EXPECTED RETURNS ARE CO-DETERMINED**

In a proof by contradiction, I now provide evidence that stock and bond returns are inherently related. Real rates wander all over the place, so if equity and bond returns are determined independently then there is an opportunity to make money by market timing.<sup>9</sup> When real rates are negative or positive but very low, you borrow to buy equities because they will earn “real 6.7%” at a time when your borrowing rate is “real something-much-less.” When real rates are high, you do the opposite: Sell stocks short or just not buy them.

This strategy “worked” in the recent past. The further real interest rates fell in 2008-2022, the higher the stock market went. When real rates were higher, from about 2000 to 2008, the stock market did poorly (it fell in half, then doubled, then fell in half again).

But, over longer periods of history, the strategy is worthless. Negative real rates in the 1970s would have had you buying stocks, which performed terribly. Large positive real rates in the 1980s would have caused you to avoid the stock market during one of the great bull markets of all time.

We cannot know whether the next period will be like the 1970s and 1980s or like the 2000s and 2010s. You *cannot* be assured of making money with a real-rate strategy of timing the stock market.

The hypothesis that stock and bond expected returns are independent, or “not co-determined,” is unsupported by the evidence.

### **MACROECONOMICS NEEDS A FACELIFT**

I promised to point out an area where Smithers's book is very valuable. It's in his critique of mainstream macroeconomics. Remarkably, modern macro *ignores the financial sector*. The financial sector doesn't exist in... wait for it... dynamic stochastic general equilibrium (DSGE) models of the economy, which are the ones used by most macroeconomists.

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model of interest-rate determination, the difference, the real interest rate, is exogenous (determined somewhere else in the economy, specifically by the supply and demand for money and other capital). I believe this hypothesis is correct.

<sup>9</sup> The hedge fund crowd would call this “risk arbitrage.” It is not a real arbitrage.

Why not? Mostly because it makes the math easier, but there is an underlying view among macroeconomists that financial markets are irrelevant because they are just claims on real economic assets, when only the real economy (of factories and trucks and software and patents and labor contracts) counts. Smithers argues that macroeconomists believe it's acceptable to omit the financial sector from their models because markets are efficient and prices are fair, causing financial aggregates to cancel each other out; I am not qualified to evaluate that claim.

We don't need a "new finance" to account for the failures of 2008 and other rocky periods in markets, or for recessions and depressions. We need a new macroeconomics. People at the frontiers of knowledge in the field are working on it. Smithers is not a lone voice crying out in the wilderness.

## CONCLUSION

Smithers has given us a lot to chew on. He proposes a revolution in many fields: corporate finance, security analysis, and macroeconomics, to name just a few. I propose an *evolution*. Our institutions and theories are not perfect, nor are they ready to be discarded. We advance, Thomas Kuhn reminds us, by making incremental changes in our ways of thinking until overwhelming evidence persuades us that we are barking entirely up the wrong tree. At that point we engage in what Kuhn calls a paradigm shift.

We are not at that point in investment finance or in the microeconomics of firms and their shareholders. We may be in macro. Smithers's book, difficult and frustrating as it is at times, will spur much valuable discussion about all these issues. That is how we make progress. Thank you, Andrew Smithers, for providing that service.

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## READ YOUR SHARPE AND MARKOWITZ

Laurence B. Siegel

In Jerome Lawrence and Robert Lee's classic play *Inherit the Wind*, based on the 1925 "monkey trial" in which John Scopes was accused of violating Tennessee law by teaching evolution, creationists rally support for their cause by displaying a banner saying, "Read your Bible!" Henry Drummond, lawyer for the defendant, wishes there were also a banner proclaiming "Read your Darwin." If you're going to argue for a cause, Drummond seems to be saying, you'd better know it backwards and forwards. And if you're going to try to overturn somebody else's views, you'd better understand those views even better than your opponent does!

Here, I'll argue that the great innovations of William Sharpe and Harry Markowitz and the other creators of classic finance theory in the 1950s and 1960s are worth studying very closely — even though some of their findings aren't exactly right. Classic finance forms a base case or null hypothesis against which empirical facts, new theories, and conjectures can be tested. Without it, we are lost. With it, we have a set of very useful guideposts, a little like Newtonian mechanics in physics — we know it's not exactly right but use it, where we can, because it is so useful. We need to read our Sharpe and Markowitz.

### WHAT'S THE MATTER WITH FINANCE TODAY?

The current state of knowledge in finance — and particularly investment management — is confusing, not only to many newcomers but also to some of us who have been in the business for decades. The efficient market hypothesis (EMH), a cornerstone of classic finance theory, says that security prices reflect all available information and that it's impossible to beat the market consistently. The EMH is on the ropes. Most finance practitioners make their living by violating it. They find inefficiencies in the market and exploit them, for themselves and for their customers, and charge high fees for doing so. This would be impossible if the market were as efficient as academics believed a few decades ago.

The related Capital Asset Pricing Model (CAPM), and the portfolio selection technique known as Markowitz optimization, are also facing challenges. A large body of evidence shows that the CAPM is not exactly right — it does not give very good forecasts of security returns, conditional on knowing what the market return is. Low-risk (low "beta") securities seem to beat high-risk ones even though CAPM predicts the opposite. Markowitz optimization, which is a way of putting numbers around the long-established practice of diversification, has been blamed for the failure of diversified portfolios to perform well in the crash of 2008.

## A SPLIT NOBEL

Meanwhile, in Sweden, the Nobel Prize committee has added to the confusion by splitting the 2014 economics prize between Eugene Fama, a leading advocate of the EMH, and Robert Shiller, who has devoted much of his career to overturning it. (Lars Hansen, an econometrician whose work has formed the foundation for much of the recent testing of theories in finance, also shared the prize.) Is the Nobel committee saying that both Fama and Shiller are right? That the EMH is valuable and so is the body of research casting doubt on it?

You bet. That's exactly what they're saying. But you might be wondering how two contradictory propositions can both be right.

## FIRST, WHAT'S A THEORY?

We're most accustomed to hearing the word "theory" used in connection with the natural or physical sciences: the theory of gravity, theory of evolution, and so forth. In the "hard" sciences, according to the American Association for the Advancement of Science,

A scientific theory is a well-substantiated explanation of some aspect of the natural world, based on a body of facts that have been repeatedly confirmed through observation and experiment. Such fact-supported theories are not "guesses" but reliable accounts of the real world. The theory of biological evolution is more than "just a theory." It is as factual an explanation of the universe as the atomic theory of matter or the germ theory of disease. Our understanding of gravity is still a work in progress. But the phenomenon of gravity, like evolution, is an accepted fact.<sup>1</sup>

## HOW DO THE EMH, MPT, CAPM FIT THIS CRITERION?

In this context, EMH is a hypothesis, not a fully developed theory. It is testable and, when we test it in detail, we find it wanting — markets are not perfectly efficient. In spite of this, EMH is a valuable hypothesis because it focuses our attention on what a perfectly efficient market would look like and how real markets differ from that ideal. As Thomas Coleman, a professor at Johns Hopkins University and the author of *A Practical Guide to Risk Management* (CFA Institute Research Foundation, 2011) and *Quantitative Risk Management* (Wiley, 2012), writes:

EMH is powerful not so much because it is right or wrong - but rather because it (1) reminds us that generating alpha is hard (markets are not grossly inefficient) and (2) pushes us to ask where, why, and by how much markets are inefficient.<sup>2</sup>

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<sup>1</sup> Quoted at [http://chemwiki.ucdavis.edu/Textbook\\_Maps/Map%3A\\_Petrucci\\_10e/01%3A\\_Matter-Its\\_Properties\\_And\\_Measurement/1.1%3A\\_The\\_Scientific\\_Method](http://chemwiki.ucdavis.edu/Textbook_Maps/Map%3A_Petrucci_10e/01%3A_Matter-Its_Properties_And_Measurement/1.1%3A_The_Scientific_Method), accessed on July 14, 2014.

<sup>2</sup> Personal communication. This article was critically reviewed by Professor Coleman, a longtime friend and occasional collaborator.

If EMH is an imperfect yet valuable hypothesis, Modern Portfolio Theory or MPT rises to the level of an invaluable theory. I define MPT broadly as a collection of major propositions in finance starting with Markowitz optimization (1952) and ending with Black-Scholes-Merton option pricing (1973). Let's enumerate the elements of MPT broadly construed (in no particular order):

The seven great ideas of modern finance:

- Dividend or cash-flow discounting (asset price as a present value)
- Interest rate expectations hypothesis
- No-arbitrage condition
- Market efficiency
- Portfolio efficiency (mean-variance optimization and related concepts)
- CAPM (relation between correlated risk and expected return)
- Optionality and option pricing

Another strong candidate is:

- Arbitrage pricing theory (mapping security returns into multiple factors).

And if we want to be ecumenical and bring in corporate finance, let's also include:

- Capital structure indifference, and
- Dividend indifference.

This is a pretty powerful body of knowledge. (From this point forward, I'm going to use "MPT" as shorthand for the whole list.) It is integrated — the parts fit, with each proposition consistent with all the others. It is testable and falsifiable. But the evidence on major parts of it, particularly market efficiency and the CAPM, don't rise to the standard of "overwhelming evidence." There are major doubts. So what is it useful for?

MPT's propositions are *useful as a null hypothesis and point of departure*.

Take, for example, the CAPM. The CAPM says what the return on a security *should be*, given the market return, the riskless rate, and the beta or correlated risk of the security. We know that the actual return on the security will differ from the CAPM's prediction. We call the excess return "alpha," and we credit the manager who picked that security with skill if the alpha is positive at a statistically significant level.

We know, then, that the CAPM cannot be exactly right because, if it were, all alphas would be zero (on average over time). There would be no manager skill to measure. But we also need the CAPM to provide the benchmark for measuring the managers whose ability to generate alpha has invalidated the CAPM!

In other words, the *null hypothesis*, what we should believe for the time being until the data convince us otherwise, is that the market is efficient and the CAPM gives accurate forecasts. This is what a manager asserting skill seeks to disprove, and our bias should be to require quite a lot of evidence. The return forecast given by the CAPM is also the *point of departure* for an inquiry into whether a manager has earned an alpha that is (1) positive, (2) statistically different from zero, and (3) sustainable or repeatable. If a manager doesn't pass those tests, he or she can be judged as having delivered the return that the CAPM predicted, and that could therefore be earned by combining a market index fund and a long or short position in the riskless asset (without paying the manager for any value added).

Without the CAPM, we wouldn't be engaging in scientific performance measurement. We'd be saying, "This return seems pretty good. It's better than what Steve at the country club got." There would be no thought of levering the market return up or down to create a neutral, objective benchmark.

The other propositions in the list above are similar. They're not universal truths, but are neutral base cases or starting points for an investigation.

So, Eugene Fama is right that the EMH is a vitally important concept against which all claims of market inefficiency or alpha generation can and should be tested. Robert Shiller is right that the EMH fails the test much more often, and more convincingly, than can be accounted for by accident and random variation; the market really isn't perfectly efficient.<sup>3</sup>

### HAVE WE BEEN USING TOO TOUGH A STANDARD FOR JUDGING MPT AS A THEORY?

So far, we've been evaluating MPT and its components as scientific theories, and they fall somewhat short. But economics is not a natural science. It's a social science. Some might say — and I'm inclined to agree — that it's a branch of animal behavior. What's a theory in the social sciences? Is MPT a theory in that context?

The sociologists Hans Joas and Wolfgang Knobl write,

Theories should be understood as generalizations. To put it the other way around, which may be easier to grasp, we might say: every generalization is already a theory. We use theories of this kind all the time, particularly in everyday life...The modern social sciences...now feature...a plethora of competing theoretical schools.<sup>4</sup>

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<sup>3</sup> See also a very valuable article by Cliff Asness and John Liew, "The Great Divide over Market Efficiency," *Institutional Investor*, March 3, 2014, <http://www.institutionalinvestor.com/Article/3315202/Asset-Management-Equities/The-Great-Divide-over-Market-Efficiency.html>

<sup>4</sup> "What is Theory?" in "Social Theory: Twenty Introductory Lectures," Cambridge University Press, [http://assets.cambridge.org/97805218/70634/excerpt/9780521870634\\_excerpt.pdf](http://assets.cambridge.org/97805218/70634/excerpt/9780521870634_excerpt.pdf), pp. 2-4, accessed on July 14, 2014.

If, in the social sciences, a theory can be just a working hypothesis or set of conjectures, subject to empirical check and countered by opposing or contradictory theories, then MPT is much better than that. MPT is a network of interrelated propositions, developed to describe a specific aspect of the way the world works, that is supported by enough evidence that well-informed people take it seriously as the starting point for further investigation. It is not “exactly true” but there is no alternative set of propositions that is “more true” or even “just as true.”

## CONCLUSION

And that is where MPT, the list of 10 great ideas shown above, stands. There are competing ideas but none of them hangs together as an integrated body of theory. Nor do the competing ideas have anything like enough evidence behind them to overturn or replace MPT. Behavioral finance is a start, but I regard it as an enhancement to MPT or, more finely understood, a set of exceptions to a general rule — a list of situations where MPT only gives you a pretty good answer instead of a great one.

Theoreticians should keep working on alternatives to MPT. But they should give proper respect to the body of knowledge they’re seeking to overturn. Meanwhile, practitioners should continue to pursue alpha. It’s out there. The market is not efficient. But it’s efficient enough that most investors will not beat the market with any consistency after proper adjustment for the risks taken and the explicit and hidden costs incurred. A few will.

Meanwhile, we’ll be building portfolios with an eye to risk, return, and correlation all considered simultaneously, as Harry Markowitz would have us do, albeit with some variations and enhancements. And, dear managers, if circumstances call for us to hire you to manage our assets, we’ll be mindful of the temptation to claim that you don’t pay attention to benchmarks and only buy the securities that go up. So we’ll be measuring you. And we’ll be using CAPM-based techniques, pioneered by William Sharpe, to do so.



*"Your mother called to remind you to diversify."*



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The current consensus economic model, the neoclassical synthesis, depends on aprioristic assumptions which are shown to be invalid when tested against the data and fails to include finance. Economic policy based on this consensus has led to the financial crisis of 2008, the 'Great Recession' which followed, and the slow subsequent rate of growth. In *The Economics of the Stock Market*, Andrew Smithers proposes a model which is robust when tested, and by including the impact of the stock market on the economy, overcomes both these defects. The faults of the current consensus model are shown to result typically from an unscientific methodology in which assumptions are held to be valid despite their incompatibility with data evidence. Smithers demonstrates examples of these faults: the Miller-Modigliani Theorem (the assumption that leverage does not affect the value of produced capital assets); the assumption that short-term and long-term interest rates, and the cost of equity capital, are co-determined; and the assumption that the decisions of corporate managements aim to maximize the present value of corporate assets ('profit maximization') rather than the value determined by the stock market. *The Economics of the Stock Market* proposes a model which includes and explains the stationarity of real returns on equity, based on the interaction of the differing utility preferences of the managers of companies and the owners of financial capital. These claims are highly controversial, and Smithers proposes that the relative merits of the neoclassical synthesis and this proposed alternative can only be properly considered through public debate.

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# Foreword

*Andy Haldane*

Chief Executive, the RSA

Larry Summers once famously observed, tongue-in-cheek, that much of modern-day finance theory concerned itself with pricing a second bottle of ketchup, once the price of the first bottle was already known. While an overstatement, this quip contained an element of truth. Much of modern finance, and in particular asset pricing, theory relies on arbitrage relationships between different asset types (the second bottle of ketchup), largely taking as given macroeconomic fundamentals (the first bottle). Finance and economics were, in this sense, fundamentally detached.

That theoretical detachment between asset pricing and fundamentals, or between the financial and real sides of the economy in these models, prevailed only under some strict behavioural assumptions. Prominent among these were that the limits to arbitrage between asset types (bonds and equities, long- and short-duration assets) were small and short-lived and that the behaviour shaping these markets could be well approximated by that of a single representative investor and household. In other words, companies' behaviours typically played a somewhat secondary role, if at all. And so too did heterogeneity between investor and household types.

Although highly stylized, this approach to modelling found its way into many mainstream macroeconomic models from the 1980s onwards. Many of these models viewed both money and finance, and the corporate sector, as a 'veil' which could effectively be looked through when understanding the drivers of the economy. Financial factors—the financial sector and financial markets—had at best a secondary role in explaining business-cycle dynamics and likewise the financing and investment choices of companies. These omissions were a not insignificant contributor to the intellectual oversight that culminated in the Global Financial Crisis of 2008/09.

This stylized approach to asset pricing and financial choice also dominated academic discourse over much of the past half-century. Often, this focused on the co-determination of asset prices across different risk classes and durations. In the 1970s and 1980s, this led to the identification, and indeed proliferation, of various asset pricing puzzles—the 'equity premium' puzzle, the

'risk-free rate' puzzle, the 'uncovered interest parity' puzzle. These were puzzles that were believed to be rooted in the limits to arbitrage. And attempts to explain them were typically found by tweaking the behavioural assumptions underpinning the representative investor.

The past decade has seen a serious questioning of these approaches. The Global Financial Crisis, and empirical failures of the workhorse asset pricing and macroeconomic models, have led to a re-evaluation and re-think of modelling practices. For example, when it comes to understanding the economy, newer strains of models have emerged in which finance in general—and banking in particular—play a much more central role in shaping its dynamics, not just during times of crisis, but over the normal course of the business cycle. That has left us much better placed to assess, for example, the dynamics of the economy after the Covid crisis than after the Global Financial Crisis.

When it comes to academic research on asset pricing and finance, several new and exciting strands of thinking have emerged over recent years to help resolve some of the puzzles in finance and to link financial factors to observable macroeconomic behaviours. That has included an increased focus on so-called behavioural finance—the application of psychology to understanding the behaviour of investors, individually and collectively, and its implications for asset price dynamics. And it includes models which explicitly take account of heterogeneity in the behaviour and balance sheets of different investor types. These have helped resolve various asset pricing puzzles and close the gap between theoretical models and empirical practice.

This provocative and stimulating book by Andrew Smithers provides a different take again on the determination of asset prices and their link to the macroeconomy, based on his own research and practical investment experience over a long and distinguished career. There are a number of ways in which Smithers' model deviates from the mainstream. Let me highlight two. First, it puts centre stage an aspect of finance often less focused on—namely, the economics of the *stock market*. Second, it puts centre stage the behaviour of the managers of *companies*, as distinct from their household owners, linking this to the fortunes of the stock market.

While these might sound like relatively modest modifications of the core model, Smithers draws out a set of implications that are often significantly at odds with conventional thinking, both when it comes to the determination of asset prices and the functioning of the economy. To give an example, in Smithers' framework the level of bond yields and equity returns are not co-determined. That leaves the gap between them—the equity risk premium—able to deviate significantly from 'normal' levels for protracted periods.

Smithers' brings empirical evidence to bear, largely drawn from the United States, to explain and justify these alternative hypotheses.

Putting the behaviour of managers of companies at the centre of a joint explanation of asset market and the macroeconomy is, I think, a significant step forward in enhancing our understanding of both. The way the risk appetite of company managers is shaped by stock market performance, in ways which affect investment and financing choices, are issues not yet fully, or adequately, incorporated into mainstream macroeconomic models. Certainly, that is true by comparison with the effort devoted to studying consumption choices by households.

One of the signature macroeconomic challenges of our time—a challenge made more acute by the Global Financial Crisis and by Covid—is the global productivity slowdown. This has many and various causes. In a growth accounting sense, a significant part of it reflects a slowing in the rate of capital accumulation of companies in a large number of advanced economies. Smithers' model provides a clear and compelling explanation for this behaviour, rooted in the short-term behaviour of managers of, and investors in, companies, with the performance of the stock market at its centre.

Many in the economics profession, the investment management profession, and beyond will probably take issue with some (or many) aspects of the model, the evidence, and the hypotheses Smithers sets out here. In fact, it would be disappointing if they did not. Because this book provides, for me, just the sort of revisionist thinking and intellectual challenge the economics, investment management—and indeed policymaking—communities need if our models are to match the data, in particular over the longer run, and if they are to offer a more useful guidepost for decision-making in the future than has been the case in the recent past.

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# 1

## Introduction

Economic theory has changed little over the past fifty years. The consensus, termed the neoclassical synthesis, has been subject to refinements and tinkering, but left fundamentally unchanged. Despite attacks, such as Nicholas Kaldor's comment on 'the intellectual sterility engendered by the methods of Neo-classical Economics'<sup>1</sup> it was, until recently, believed to provide a solid basis for policy—a view which was, however, treated with scorn by Hyman Minsky who wrote that 'Modern orthodox economics is not and cannot be a basis for a serious approach to economic policy.'<sup>2</sup> It was previously assumed that by following the precepts of the neoclassical consensus the economy could be kept in balance, with mild fluctuations in unemployment and inflation occurring around a trend of steadily rising output. But following the financial crisis and the great recession of 2008 the questioning of this view has become increasingly vociferous.

The major weakness of the previous consensus is seen by many to lie in the failure to incorporate finance into its economic models. Half the US economy's output is produced by companies whose behaviour is determined by the fact that their shares are quoted on the stock market. Once this is accepted, the economic model that follows is very different from the neoclassical consensus. Unlike the latter its assumptions are testable and prove robust when tested and it radically changes our understanding of how the economy operates and leads thereby to different policies, largely because it shows that corporations behave differently from households and quoted companies differently from unquoted ones. The failure of the current consensus is shown by its dependence on assumptions which either are untestable or, if not, fail when tested. The determination to stick to accepted assumptions and ignore the evidence that they are invalid shows that neoclassical economists have much in common with Hobbits who 'liked to have books filled with things that they already knew, set out fair and square with no contradictions'.<sup>3</sup>

<sup>1</sup> 'Marginal Productivity and the Macroeconomic Theories of Distribution: Comment on Samuelson and Modigliani' by Nicholas Kaldor (1966) *Review of Economic Studies* 33.

<sup>2</sup> *Stabilizing an Unstable Economy* by Hyman P. Minsky (2008) McGraw-Hill.

<sup>3</sup> *The Lord of the Rings—Part 1: The Fellowship of the Ring—Prologue Chapter* by J. R. R. Tolkien (1954) George Allen & Unwin.

## 2 The Economics of the Stock Market

The main faults in the consensus arise from two aspects of its construction. Firstly it largely ignores the strength of the 'corporate veil' and assumes that the private sector behaves as it if were the same as the household sector and secondly it holds that a satisfactory model can be constructed in which finance has little place. I avoid the first and seek to make a significant step towards rectifying the second. The stock market is not the only way that finance needs to be included in a valid model of the economy, but it is an important part of it.

The main concern of the owners of shares in quoted companies, and even to a greater degree those who manage their money, is the current value placed on them by the stock market, and this short-term assessment has naturally absorbed much of the vast resources of the financial services industry. It has also deflected the attention of economists from the market's longer-term behaviour, so that we need to shift our main concern from its unpredictable short-term fluctuations, which are simply noise in the statistical sense, to its longer-term more predictable ones. To understand how the stock market works we need to explain two of its key characteristics which include the stability and level of the long-term real return on equities and the stability and strength of the mean reversion of their cumulative return to trend.<sup>4</sup>

While mean reversion cannot be proved, it can only be shown to be probable, there appear to be two other important constants which are otherwise rare in economics and seldom known precisely. These are the produced fixed tangible capital output ratio and the labour share of output. The value of one of these three constants can be deduced from that of the others and as they are also derived independently of each other and are mutually consistent they provide strong evidence for mean reversion of each one considered separately. I put great emphasis on the stability of the real return on equities which has gained far less attention than it deserves. One reason for this lacuna is that much of the financial literature concentrates on excess returns (the return on equities over a supposedly 'risk-free' asset such as cash) which, contrary to the authors' usual expectations, are not mean reverting. The importance of the stationarity of the real return on equity comes not only from the conclusions that follow from it but also because of the support it gives to the stability of both the capital output ratio and the labour share of output.

The stock market's value, which I shall use to describe the level at which it would be if it were neither over- nor under-priced, varies around a stable mean and, if these fluctuations could be forecast, they would be arbitrated

<sup>4</sup> At longer horizons real equity returns exhibit negative serial correlation. Periods of high returns tend to be followed by periods of low returns and vice versa.

away in the pursuit of profit. This has not happened and as the years have passed the fluctuations have not become smaller, indicating that attempts to forecast short-term variations have so far failed. It thus appears that the market's short-term fluctuations are random and unpredictable. Arbitrage would even eliminate the longer-term blips if its rewards were sufficiently sizeable and rapid for those engaging in it to become rich at the expense of other participants. Either the longer-term predictability is not sufficient, regarding the level of mis-valuation and the timing of its correction, to make this worthwhile, or the transfer of wealth from those who mistime these longer-term fluctuations to those who benefit is not sufficient to alter market behaviour—each generation produces a new supply of winners, who do not then become so wealthy that they dominate market behaviour, and a new supply of relative losers to finance the winners.

To understand a complicated system, it is not sufficient to describe it, we also need to discard its inessential features and show that the resulting simplified model accurately simulates the system's behaviour. To do this I ignore the market's short-term fluctuations and seek to explain its longer-term stabilities. My first step is to show that its key features follow from the risk aversion of investors. I present a model which shows why this determines the long-term stable return on equities around which the shorter-term returns vary, being driven by the unpredictable fluctuations in investor sentiment. The attitudes to risk of investors and the managers of quoted companies determine the relative long-term risk-free real returns on bonds of different maturities (the yield curve), the negative serial correlation of real equity returns, the preferred proportions of debt and equity in household portfolios, and the level of corporate leverage.

Acting in isolation the key variables, such as growth, the proportion of the return paid to shareholders, leverage, and demography, should naturally result in changes to the return on equity but so far as can be observed, this has not happened. It appears that the fluctuations in these variables have offset each other. It is improbable that this has arisen by accident, but rather through a natural process which involves corporate behaviour responding to the stability of equity returns induced by investors' risk aversion. This produces a stable long-term return on corporate equity, the 'hurdle rate' below which companies do not invest, but above which they must, as failure to do so renders them liable to losing market share.

Changes in the proportion of household wealth that investors wish to hold in debt or equity assets must match the wish of companies to be financed by debt or equity—an important identity which seems previously to have been ignored by economists. When there are *ex ante* mismatches between the two, the effect is similar and parallel to *ex ante* mismatches in net savings. It requires

#### 4 The Economics of the Stock Market

changes in monetary or fiscal policy to bring net savings into equilibrium under conditions of full employment, while market-driven changes in long bond yields achieve the balance between corporate leverage and investors' portfolio preference. Variations in the risk-free rate of interest and the yield curve have historically been the main ways through which these two separate equilibria have been realized, though fiscal policy also affects both.

The preferred level of equity held by households can change, not only with short-term swings in confidence but over the longer term, for example with changes in demography, and these need to be reflected in changes in corporate leverage. Household portfolio preferences are insensitive to changes in long-term interest rates, but corporate leverage is sensitive to them. These differences in elasticities result in the required change in leverage being realized through small changes in long-term risk-free interest rates, while the equilibrium return on equities remains unchanged.

This process of adjustment would not be possible if the fluctuations of equity returns were related to those of bonds, or if long-term bond yields changed only in line with short-term interest rates. But bond and equity returns are independent of one another, contrary to assumptions often made regarding their relationship, and while short- and long-term interest rates are related, their changes are partly independent so that the yield curve can vary. The partial independence of short-term rates and long bond yields allows the latter to stimulate changes in corporate leverage so that these can match changes in the preferred portfolio balance of investors. The lack of relationship between bond and equity returns is shown both in the long and the short term. The former is due to the insensitivity of equity returns to changes in bond yields, within the narrow range of their long-term variations. The lack of any short-term relationship is the natural result of the way in which short-term fluctuations in investors' confidence often respond to changes in expectations for profit which tend to offset changes in interest rates, whose fluctuations also affect investors' hopes and fears. High expectations from growth are often, but not always, accompanied by rising interest rates and low expectations by falling ones. The variations, both over the long and the short term, in the yield curve, interest rates, and equity returns ('equity risk premiums'), are thus necessary to allow markets and the economy to rebalance towards the equilibrium conditions needed both for full employment and the debt equity balances in household portfolios and corporate leverage.

The faults in the neoclassical synthesis have been made possible by failures to test assumptions, or to ignore evidence against them: 'one must not fall into the error of supposing that assertions about reality can be derived from *a priori*

assumptions. Whether well-behaved homogeneous-and-linear production functions exist or not is a question of fact. They cannot be presumed to exist as a consequence of some basic postulate.<sup>5</sup> I show that such failures include the assumption that leverage does not affect the value of corporate assets, that interest rates and the cost of equity capital are co-determined and that the decisions of corporate managements aim to maximize the present value of corporate assets, rather than the value determined by the stock market.

The resulting explanation of the economics of financial markets raises other important issues, which include the differences in the returns from different international stock markets, the way in which stock markets can be valued, the issue of market efficiency, the rationality of investors' behaviour, and the problems of measuring returns. Expected real returns from different stock markets cannot diverge, as investors would buy those with higher ones, but the portfolio preferences of domestic investors are likely to do so as the demographics of individual countries move independently. Nominal long bond yields, however, differ between countries, and these rather than real yields determine leverage. Because nominal bond yields and leverage can differ internationally between companies and financial markets, expected real returns on equities can be the same despite differences in demographics and the consequent differences in portfolio preferences.

To be valid a model must be testable and prove consistent with the evidence. To test mine I will mostly turn to the United States where the data for equity returns are available for much longer than elsewhere. In addition, the United States has avoided massive capital destruction in wartime and hyperinflation, both of which have created data problems in other major countries which I examine. I show, however, that negative serial correlation of equity returns can be observed in all stock markets whose economies have avoided massive capital destruction, indicating that the risk aversion of households is a universal attribute and not confined to the inhabitants of the United States. Changes in the tax system and interest rates are not trivial, as they can affect the growth rate of the economy, but they do not change the long-term return on equity.

This book's key conclusions are as follows, together with the chapter numbers which particularly address these issues:

- (i) The real return on equity is mean reverting at approximately 6.7 per cent p.a. This follows from the risk aversion of the owners of capital (Chapter 15).

<sup>5</sup> Kaldor (1966).



## 6 The Economics of the Stock Market

- (ii) This applies worldwide, with outturns deviating from it only through major periods of capital destruction in war (Chapter 16).
- (iii) Corporate decisions on investment, pay-out ratios, and leverage are made by managers whose behaviour is determined by a utility function which is different from that which determines the portfolio preferences of the owners of capital (Chapter 4).
- (iv) Companies seek to avoid equity issues. Their pay-out ratios vary with growth which can thus be financed without needing changes in private-sector savings. The ratio of corporate interest payments to profits is stationary (Chapter 4).
- (v) With stable demographics the risk aversion of the owners of capital is stationary. It changes with ageing and the structure of retirement savings, but due to the high elasticity of leverage to long bond yields and the low elasticity of the portfolio preference of the owners of capital, these changes are accommodated by changes in long bond yields leaving the return on equity unchanged (Chapters 12 and 13).
- (vi) Corporate capital can be usefully divided into short-term debt ('cash'), long-term bonds ('bonds'), and equity. Their returns are derived independently: savings and investment are equated by movements in the short-term interest rate and corporate leverage is balanced with the preferences of the owners of financial assets by variation in the bond yield: equity returns are stationary (Chapters 7, 8 and 10).

The neoclassical consensus is not necessarily a completely unified or internally coherent body of doctrine, but the conclusions set out above differ sharply from those usually held in the following ways:

- (i) Companies do not seek to maximize the present value of their net worth (often termed profit maximization). If they did they would vary their rate of investment with the cost of capital. Because the return on equity is stationary the cost of capital is known, and the rate of corporate investment does not vary with it (Chapters 18 and 19).
- (ii) Bond yields and equity returns are derived independently. The gap between them (the equity risk premium) is not mean reverting and bond yields provide no information on future equity returns (Chapter 9).
- (iii) The cost of capital varies with leverage. The Miller-Modigliani Theorem (MMT) assumes otherwise and is thus demonstrably false (Chapter 19).

'Awe-inspiring, encompassing, convention-flouting analysis, hard stick-your-neck out empirical discoveries, and counter-intuitive hypotheses. Endlessly stimulating and intensely useful.'

AVNER OFFER, Chichele Professor Emeritus of Economic History, University of Oxford

'This is a bold book that questions virtually all the assumptions of prevailing neoclassical theory. By rejecting the concept of the "representative agent", proposing instead that households and corporate management have totally different motivations, Smithers shows how finance plays a crucial role in explaining developments in the real economy. Leverage, ultimately driven by demographics, raises growth but threatens it too. His attacks on theory, particularly the Modigliani-Miller theorem, are supported by careful empirical work that also reveals important and unexpected financial constants. Throughout, the book sparkles with insights based on decades of close observation of how financial markets really work. Theorists must confront this reality, not try to ignore it.'

WILLIAM WHITE, Senior Fellow at the CD Howe Institute and former economic adviser to the Bank for International Settlements

'Most economists tend to think of financial markets as being efficient, notwithstanding an increasing body of evidence that there is a predictable component to movements in share prices. In this book Andrew Smithers sets out a view very different from the mainstream position of how share prices are determined. A key feature of his analysis is that periods of high returns are likely to be followed by periods of low returns—there is low-frequency negative serial correlation. The book provides an account of the factors that can give rise to this, based around the argument that different market participants have very different objectives and views of what risk is. The book poses a substantial and important challenge to financial economics. It is therefore important that the book should be published and the author's views debated.'

MARTIN WEALE, Professor of Economics, Kings College London

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