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Black Noise

Examining the influence of Fischer Black's 'Lost Decade'

On the day I finished reading Justin Fox's *The Myth of the Rational Market*, I received a review copy of *Business Cycles and Equilibrium*, by Fischer Black, which Wiley is republishing this fall. The combination kicked off a train of thought that resulted in a determination to explain why Fischer Black was so inspirational to many of us in the first generation of Wall Street quants.

The Myth of the Rational Market is an excellent history of the idea of rationality in finance, tracing it from isolated musings to a revolution in economics, to a force in the world, to a myth with a self-sustaining power that went beyond theory or evidence. While I would quibble with a few points of history and theory, I am deeply impressed by the overall quality, given the scope of the story and the complexity of the ideas.¹

Fischer Black is mentioned 38 times in the book, behind only Gene Fama (70), Irving Fisher (64), and Paul Samuelson (42), and Chapter 8 focuses on his work. But he pretty much disappears from view after 1973, when the Black-Scholes and Merton option-pricing papers came out, to 1984, when he pops up to give his famous "Noise" presidential address at the American Finance Association in New York. From my point of view, this missing decade was his most influential, and it culminates in the speech. During this time, a cohort of quants trained in quantitative finance Ph.D. programs, plus a few math and science quants who took finance courses at the same schools, moved to Wall Street, and changed it forever. It is also roughly the period over which



Business Cycles and Equilibrium was written, and the final chapter of that book is the article version of "Noise." His 1995 book *Exploring General Equilibrium*² is the more important work, but *Business Cycles and Equilibrium* was the more influential.³

Imagine yourself entering a quantitative finance Ph.D. program in the late 1970s or early 1980s. To a subset of us, the basic theory seemed to be in place, and incremental academic improvements were nowhere near as exciting as exploiting what we already knew. We called ourselves "rocket scientists,"⁴ which sounds quaint

today, but not to a generation with childhood memories of getting up early to watch space launches and a formative experience of the 1969 moon landing.

A real rocket scientist advances knowledge by using theories to accomplish a goal, not by thinking up new theories or designing controlled experiments to test them. She pushes the limits of technology and either expands them or verifies that they exist. She's a generalist on a cross-disciplinary team, and many of the scientific fruits will be outside her area of expertise, say in astronomy or planetary science. Her reward

is not publications or wealth, it's the adventure and the knowledge gained.

Rocket scientists viewed quantitative finance theories as tools, not principles. In class, we learned convincing theoretical arguments and overwhelming empirical evidence that securities were priced as if markets were efficient. In the world, we observed that people charged tens of billions of dollars in management fees to deliver worse than random portfolio selections, and that enormous energy and money was spent in frantic trading that had to net out to less than zero due to transaction costs. The world was changing, but very slowly. Only a few investors and no traders showed much interest in the academic results.

One kind of person thinks, "The theory must be incomplete or wrong, as it does not account for observation. Let's revise the theory." Another kind thinks, "People are stupid not to understand this beautiful theory. Let's put our effort into educating them." A rocket scientist thinks, "People obviously want something that theory says should be available for free. If I can figure out what it is they want, I can get it for free and sell it for a lot. If I succeed, not only will I make money, but I'll also prove the theory in the real world, and I'll change the real world. If I fail, I'll learn the defects of the theory. But wait! What if my changes disrupt the markets and cause disaster? ...Who cares? Let's light the fuse and find out what happens." People in that last group found Fischer Black's papers addressed them directly, while some other papers had to be mined for useful nuggets.

If this sounds too abstract, consider one practical example. Corporations issued bonds which were bought mainly to fund actuarial and contractual institutional liabilities. A number of intermediaries took significant fees for issuing, placing, managing, and trading bonds. For legal and traditional reasons, these bonds took forms that were not necessarily convenient to either issuer or investor: fixed rates, call provisions, periodic interest payments with bullet repayment of principal, and weak credit protection. One obvious inefficiency in the late 1970s was that issuers were having trouble coming up with the short-term cash for interest and repayments,

Robert Merton

Fischer Black thanked nine people for "comments on earlier drafts" of his "Noise" speech. Unfortunately, Peter Bernstein died recently, but I asked the others if they recalled anything about their comments and also what they thought of his "prediction: that one day these conclusions will be widely accepted." Robert Merton relates:

"I do recall introducing Fischer when he gave 'Noise' as his AFA presidential address in NYC. Breaking with tradition before and since, he spoke early in the morning instead of the late afternoon/early evening, so that the Wall Street crowd could attend on their way into work.

His address also broke tradition in another way. As incoming AFA president, I gave the introduction. Usually, the address lasts 45–60 minutes, and so I thought that at least 3–5 minutes was needed for an appropriately weighted introduction. About 15 or maybe 20 minutes at most into his talk, Fischer paused, and one could hear the proverbial pin drop in the large ballroom. Now those who knew Fischer

while institutional investment portfolios were throwing off too much early cash and having trouble getting enough long-term, call-protected investments. Very long-term, noncallable, low, or zero coupon bonds would have served the interests of both sides, but few of them were issued. Another inefficiency was that the portfolio of available credit risk was not well-diversified across sectors. Bonds were expensive to trade, and impossible to short, so managers were stuck with unbalanced portfolios for years.

It was easy to find theoretical arbitrages in the system, ways to make large amounts of money with negligible risk, if you could go long or short individual components without transaction costs. It was also easy to find prices and transactions that appeared irrational.

One reaction to this situation is to assert that markets are irrational. Another is to claim that they are out of equilibrium. A third is to hypothesize unobserved factors that made everything efficient, despite appearances, or to deny the evidence of inefficiency. All of these seemed unsatisfying and, more importantly, offered no opportunity for profitable innovation.

knew that he would stop sometimes to write down an idea that came to mind before forgetting it (he claimed to have a poor memory), but relatively quickly I realized that he was done. So I started clapping.

Later Fischer quoted George Stigler, in saying that no audience complains about a speech being too short. Perhaps, but not on this occasion. That laconic style was his, especially in public speaking and in the classroom. While the address itself was surely the shortest in AFA history (and since), 'Noise' was not the shortest title: Richard Roll's 'R**2' wins that honor, especially when one adds that it compactly describes the initials of its author as well.

On whether Fischer's verdict has come to be: I don't think so yet, in the way he saw it, beyond a few folks deeply into finance... certainly not among the broad class of economists, who view what their models can forecast as the important focus and not what they cannot forecast. I do, however, believe that we are moving a bit closer as a consequence of this crisis."

There was one other key ingredient to this time and place: personal computers. Mainframe computers had become more widely available and flexible, but for creative work the personal machine had unparalleled advantages. In those days, that meant building your own machine, and programming your own operating system. Rocket scientists naturally did this for fun, but were also keenly aware of the advantage that a computer conferred. The financial system was still running on paper trade tickets and desktop calculators. We understood that finance was information processing, and with our computers we could do that thousands of times better than the best human, and that our power doubled every 18 months while the human brain required a thousand generations for meaningful evolution.

Fischer Black taught us to assume rationality and equilibrium, to exploit technology, to avoid complex assumptions, and to respect evidence, then to figure out what to do. As early as 1970 he wrote, "Thus, a long-term corporate bond could actually be sold to three separate persons. One would supply the money for the bond; one would

Richard Roll

"I tried to recall specific conversations with Fischer, but it has been quite a while, as you know. However, when I dug out his paper and read it again, I began to remember some of the arguments we had. There are two basic parts to the paper: the impact of noise on finance and the impact of noise on macroeconomic phenomena. Concerning finance, some of his arguments are well accepted, but they had already been pretty well accepted before his paper. I'm thinking in particular about the role of liquidity traders and noise traders. Most modern papers in microstructure take it as given that there are both informed traders and uninformed (noise) traders. Some of his other arguments, though, are less well appreciated, such as his contention that one cannot learn anything from studying price/earnings ratios, etc. I think he's absolutely wrong in this case and I told him so at the time.

In terms of macroeconomics, none of his ideas have been accepted, nor should they be. For example, his utterly ridiculous assertion that the rate of growth of the money stock has no impact on inflation. We have a tremendous amount of empirical evidence to the contrary from long histories in many countries. He's also all wet about exchange rates. His explanation of business cycles is not simply unaccepted, it's the subject of deri-

bear the interest rate risk; and one would bear the risk of default." This happened, of course, with the development of securitization funding, interest rate derivatives, and credit derivatives. All three, and hundreds of other financial innovations, were built by rocket scientists from the bottom up. We found niches we could exploit for profit; the niches expanded and overlapped with other niches, and grew into a standardized market.

This would have been impossible without the founders of modern finance. They encouraged and validated the process; for example, Merton Miller was a driving force in establishing public trading of equity options. Others developed theories, advised, lobbied, and lent their credibilities in support of innovation. At the same time, they trained a generation of MBAs to understand and accept these changes.

For all this, I think most rocket scientists would agree that they were distant, external

sion (perhaps somewhat unfairly). Again, I told him at the time that he was heading for trouble, but he persisted in presenting this to a wide range of audiences. I remember macroeconomists asking me, after they heard his talk on macro, whether finance people really held him in such high regard. It was a bit embarrassing.

It's interesting that he predicted not only that his ideas would someday be well accepted, but if they're not, he said, 'I will blame it on noise.' He also said, 'Research will be seen as a process leading to reliable and relevant conclusions only very rarely, because of the noise that creeps in at every step.' This is a very damning prognostication about economic science and it has not proven to be correct. His own research on options is an example of the contrary. Perhaps that would be considered "rare," but there are numerous other examples. We have come a long way in 25 years, though we certainly don't fully understand the macroeconomy.

Fischer had a thick skin. You could tell him flat out that he was full of baloney and he'd just laugh. Shortly before his death, he and his wife came to visit us in California and stayed at our ranch in Ojai. He had not been affected whatsoever by his illness. He was still filled with ideas, many wrong and others brilliant, and he wanted everyone's opinion of them, even my wife's! He was unique, but not always right."

allies, often unsympathetic to our problems and goals, not always comprehending our experience, not always supportive of nonacademic pursuits, slow to give credit to the quant on the Street. Although they were personally friendlier and more helpful than Fischer Black, he saw things the same way that we did, and they didn't.

The most obvious divergence of view was how we described what had happened afterwards. To a rocket scientist, the market had been efficient and in equilibrium, with an opportunity for a new invention. We had seen the need, invented, and commercialized the invention. Economic value had been created, just as if we had invented something that improved the fuel efficiency of cars or the processing speed of computers. The improved information processing moved the equilibrium to an overall better place.

Back at the university, the same events were

usually described as the removal of a market inefficiency. The equilibrium hadn't moved; people had gotten more rational and moved closer to where they should have been all along. There was no understanding of the reasons they had been at the old place. If there was credit to give out, it went to the professor who had written a paper pointing out the general inefficiency (with no hint on how to solve the problem – sort of like writing, "cars should run farther on the same amount of gasoline") or to the senior business executive in the innovating firm who couldn't write a line of code or sum a geometric series.

Fischer Black understood the market as something built by people. Rationality was natural, as was rapid progress toward equilibrium. But technology changed the equilibrium, and rocket scientists changed the technology. Improvements in computer and communications technology, along with application of mathematical theory to replace traditional rules of thumb, allowed more granular and accurate allocation of economic resources, and more total value.

The counterpoint to a rocket scientist was an "Einstein," someone who thought deep thoughts to understand fundamental truths. We did not use it as a complement. No one was described as an Einstein – the usual phrase was to "go Einstein," as in "she had a good insight, but she went Einstein with it," or "he got offered a tenure-track position and decided to go Einstein." The term was never applied to people born before 1950, the ones who built modern finance. Using our admittedly sophomoric terminology, they *were* Einsteins. They thought deep thoughts, tested them rigorously, and put them into simple equations that anyone could understand. The phrase was reserved for younger people, who seemed blind to implementation opportunities, contemptuous of their role in advancing knowledge, or unaware of how fast finance was changing.

Perhaps a better analogy is one I used in a 1983 presentation at Harvard Business School. I compared academic finance to archeology, the study of a dead financial system using records of trades. The problem with that is that you have

to guess what you could have executed and at what price, and even if you guess right, you're only explaining how things used to be. By contrast, the rocket scientists were anthropologists embedded in the living society that had replaced the old financial system. Rocket scientists learned the actual outcome of executing a strategy; we didn't have to make assumptions. I cringe a bit today at the arrogance of that talk, and the unintended condescension, but it captures our feelings of that time.

Many of the pioneers of modern theoretical finance also made important practical contributions. But Fischer Black was the only rocket scientist among that group.⁵ He wrote mainly in practitioner journals, in simple English rather than equations.⁶ He had a distinguished professional career before his academic appointments, and then he left academia for a job at Goldman Sachs. He eschewed theoretical edifices for simple logic and "exploration" of "facts." He appeared to live only for ideas, and he would discuss the ones he was interested in with anybody. In my experience, he was not nurturing; he had no interest in

If you thought the market wasn't working, in our view, it meant you didn't understand what it was trying to do

your ideas that didn't intersect with his, and he was not good at introducing you to others who might help your thinking. But you never doubted that he gave you the full benefit of his brain; he didn't tell you things he didn't know, simply to impress you; he didn't hide things he did know, to protect them. He had an incisive, withering logic that could refine (or evaporate) your ideas ruthlessly.

Rocket scientists felt that the Einsteins and archeologists failed to appreciate the power of the combination of modern financial theory plus advances in technology. Some invented frictions or complexities to reconcile the simple models with real world behavior. None of this seemed to us to work very well, nor to be very

Lawrence Weiss

"I remember having lunch with Fischer in the Sloan School in the spring of 1980 as I was a visiting scholar from Yale, then trying to be a macroeconomist. I was working with Bob Litterman, a newly arrived and rather disaffected MIT assistant professor, who was teaching me good Minnesota (Sargent/Syms) time series econometrics. Bob and I wrote a paper in which we showed that real rates were 'exogenous' in a system containing nominal money, nominal rates, prices, and output. We took this to be critical of most existing work in macroeconomics, which is where Fischer agreed most. We agreed that money was mostly passive, that price level surprises had little to do with output surprises, and that most 'stylized facts' were dubious. Other than these kinds of nihilistic verities, I don't remember if he agreed

satisfying even if it worked. Often it seemed to be a game to force observed behavior into a mathematically tractable form, doing violence to the concept of "rationality" in the process. Fischer Black took our side in this matter, and crafted the beautiful sentence that became a proud rallying

cry, "In my model, markets work."

We needed markets to work. We viewed financial theory as a set of tools, and markets powered them. We had an instinctive revulsion at models that posited market imperfections or nonequilibrium results. If you thought the market wasn't working, in our view, it meant you didn't understand what it was trying to do. If the government had a foolish monetary policy, markets seamlessly developed their own forms of money. A lot of academic finance assumed at the outset that everything could be measured in money; Fischer Black challenged us to, "Imagine a world without money." Markets flowed around regulations and taxes, informational asymmetries and frictions, false beliefs, and manipulation; the way a flood

with our thesis that statistical exogeneity (in the sense of Granger or Syms) was a useful tool. I think he liked the conclusions more than the methodology.

I would be remiss to history if I did not comment on his lunch menu. After I sat down facing his desk, he went to his lavatory and poured himself six large Styrofoam cups of cold water. When he returned he offered me a cup. When I accepted, he got up from his desk and walked across the room to fetch my cup, leaving his inventory intact. He then pulled from his desk a full bag of Keebler's Pecan Sandies. He ate every one of those cookies, washing them down with copious quantities of prepositioned cold water. When he had finished the packet, he folded it up quite carefully, placed it back in the drawer and then pulled out a fresh packet. I don't think he finished the second packet."

ignores one-way street signs and traffic lights.

The rocket scientists took jobs on Wall Street, but never in research or analysis. We wanted to test ideas by trading or managing large amounts of money. While we were certainly not indifferent to wealth and fame, we thought of ourselves primarily as scientists. We were searching for knowledge and we shared it freely, with each other and in publications. That's not as disinterested as it sounds. We saw the financial world changing rapidly; in fact, we were changing it, and our best career option was to keep ourselves on the cutting edge rather than make a fixed investment in any single idea, however promising.

All of this brings us to Fischer Black's speech as outgoing president of the American Financial Association (AFA). It was in New York, early in the morning, and the room was crowded with Einsteins, rocket scientists, and graduate students of unknown leaning. I had never seen Fischer Black give a public speech before. I had spoken to him many times in private and seen him in seminars (where he would sit motionless with a small smile, leading you to think he was not paying attention, then say something always unexpected, often outrageous, and sometimes incomprehensible – then return to repose, which was intensely irritating to most presenters), so I was not as surprised as others by his terse, even delivery and early abrupt end.

I was enthralled by the message. What he

labeled “noise” made markets possible, and was “a causal factor more powerful than ... large events.” Noise was responsible for business cycles and inflation, and rendered econometrics and government actions meaningless. Noise made price fluctuate more than value. Noise is what created opportunities and made them hard to exploit. He called noise “uncertainty,” but I think it is better translated today as “risk.”

Next he told us that apparent irrationality was due to people basing decisions on expectation

rather than certain knowledge. Noise means that rational expected utility calculations can lead to actions that are inconsistent with any single certain state of the world. This is only true for utility functions which are not time-separable; rocket scientists sneered at the time-separability of utility, while most Einsteins needed it to solve their equations. That’s why rocket scientists liked to divide expected return by variance (Kelly), which depends on bet size, but not on time; while Einsteins liked to divide by standard deviation

(Sharpe), which depends on time but not bet size.

All of this was expressed elegantly and succinctly, but was not altogether new. He continued with the statement that noise caused people to rely on rules of thumb. That sounds like behavioral finance, but he went on to say that he expected that science and technology would improve the rules. This was close to the rocket scientist view, except that we thought of the rules of thumb as equilibrium solutions, given the information, theory, and processing power

Meir Statman and Hersh Shefrin

“The ideas that Fischer Black expressed in ‘Noise’ must have been germinating in his mind for many years. Fischer’s ideas surely spurred ours, and perhaps our ideas helped Fischer in some small way to bring his ideas to fruition.

One of Fischer’s great qualities was his ability to place evidence, including everyday observations, next to theory. He was keen at seeing gaps between the two and ready to admit that he did not know how to bridge these gaps. He was also remarkably open to new ideas, his own and others’, about bridges between theory and evidence.

Fischer’s ideas and our own overlapped on the issues of dividends, noise trading, and behavioral finance more generally. That overlap started long before the term ‘behavioral finance’ was coined. Fischer placed Miller and Modigliani’s dividend theory against dividend facts, exposed the gaps between the two, and was not afraid to admit that the gaps remained a puzzle to him. The title of Fischer’s 1976 article in the *Journal of Portfolio Management* was ‘The dividend puzzle.’⁷ He ended his article with these words, ‘What should the individual investor do about dividends in his portfolio? We don’t know. What should the corporation do about dividend policy? We don’t know.’

In early 1982, we used Fischer’s article as a foil for our own, presenting a behavioral theory that proposed an explanation for

why individual investors would find cash dividends attractive, and not be subsumed by the usual considerations of risk and expected returns. In part, we said that people use dividends to control spending, using the rule of ‘spend from dividends, but don’t dip into capital.’ We submitted the paper to the *Journal of Financial Economics* and were very fortunate to have Fischer as the referee. Fischer was quite open to our ideas, writing, ‘This paper is brilliant. It rings both new and true in my ears.’ But if our ideas were new, they surely connected with ideas and facts that Fischer has known long before. He wrote, ‘For example, I have heard the MIT Treasurer say that he spends only dividends and interest from the endowment because that’s a way to control the operating division budgets.’

Fischer devoted a section of ‘Noise’ to dividends, writing, ‘In Black,⁸ I described the dividend puzzle. The solution to the puzzle, I now believe, is that we must put dividends directly into the utility function (see Shefrin and Statman⁹).’ Putting ‘dividends directly into the utility function’ is Fischer’s interpretation of our framework, but not language we actually used in our paper. Our recollection is that in our comments on an early draft of the paper, we mentioned that this language was a compact way of describing what was fundamentally different in our approach, and adding some technical issues about the difference between direct and indirect

utility functions in this regard.

Fischer was elected President of the AFA and was organizing the December 1984 meeting. He asked the two of us to organize a session devoted to what we know now as behavioral finance. We presented our paper on the behavior of what later came to be termed ‘noise traders.’ Specifically, we described the tendency of investors to be quick in the realization of gains and slow in the realization of losses. We called this tendency ‘the disposition effect.’ Twenty years later, Grinblatt and Han¹⁰ presented evidence that the disposition effect causes stock prices to exhibit momentum.

During the time between Fischer’s referee report in 1982 and the time he finalized the ‘Noise’ paper in 1986, we had many conversations and correspondence about the issues of noise. We wish we had kept that correspondence, but, alas, we did not.

It is plainly evident that Fischer’s prediction that, ‘someday these conclusions will be widely accepted,’ is coming true in connection with the issues we discussed with him. For example, we know now, from work by Graham and Kumar¹¹ and Baker, Nagel, and Wurgler¹² that people consume differently from dividends and capital. As to the disposition effect, initially Odean¹³ and then many others provided strong evidence that the effect is strong both for individual and institutional investors.

We also know that overconfidence leads investors to trade (e.g., Statman,

Thorley, and Vorkink¹⁴), and that traders enjoy trading (e.g., Statman¹⁵). ‘Or perhaps they just like to trade,’ is now widely accepted. In this last respect, there is a recent interesting article by Grinblatt and Keloharju¹⁶ on ‘thrill seeking.’ So, Fischer’s conjecture about traders who trade because they ‘think that the noise they are trading on is information’ is now well established in the literature.

One of the most important aspects of ‘Noise’ is Fischer’s discussion of what market efficiency means. Fischer made the point that one result of noise is that nobody really knows how to measure fundamental value precisely, and as a result noise will cause gaps between market prices and fundamental values. An example of such a gap is momentum. Fischer suggested that we characterize a market as being efficient if ‘price is within a factor of two of value.’ Interestingly, Shefrin’s asset pricing book¹⁷ also uses the ‘Black noise’ play on words when modeling Fischer’s precise notion of trading on noise as if it were information. The main theme of the book is the importance of extending the traditional notion of a pricing kernel to accommodate ‘noise,’ along with the presentation of empirical evidence about whether the pricing kernel does indeed reflect noise. In the spirit of Fischer’s characterization, noise typically prevents all traders from precisely knowing the true pricing kernel.”

available. For the most part, behavioral finance people respected rules of thumb as functional adaptations, but didn't seem to respect the amount of work it took to improve on them. I had several conversations with Fischer Black on this subject, but never got a clear idea of whether he considered "rules of thumb" to be optimal solutions, given the available technology or inferior devices adopted out of ignorance and laziness. Asking the question a different way, was a rule of thumb a rational equilibrium when information and processing has costs, or an irrational solution accepted because it was tolerably close to equilibrium? Did it take a rocket scientist with a computer or a psychologist to improve things?

The final point he made that resonated from the speech is that reactions to noise entered directly into utility functions. Two specific examples were dividend preference and trading without information. Financial anomalies were

explained as multidimensional utility functions, not irrationalities. This is a dangerous step; once you start adding to utility function arguments you risk explaining everything as an additional parameter, and thus explaining nothing. But rocket scientists had chafed under, reducing everything to the one dimension of expected present value (or even its uncomfortable hybrid, risk-adjusted expected present value). Here was permission from the AFA president to embrace a more complex rationality.

Fischer Black was not the first person to say these things, but his stature and the occasion gave them more impact than they had in a graduate decision sciences seminar debate. Even more than the ideas, the boldness of the speech was inspiring. He wrote off huge sections of academic economics, in a few simple words, without elaboration. He predicted that everyone would agree with him someday, and gave no hint that he cared how fast that happened, nor that he might

be the one to change his mind. Most effectively, he said what he had to say, then stopped. It could have been disrespectful to the audience, but it wasn't. The message I got was that this stuff really matters; it's not about polite convention or consensus or credentials, it's about getting some useful work done. "The markets open soon, time to get to work." Twenty-five years later, I still take courage from that 15-minute talk.

This is only intended to be a personal narrative, not a history, nor a manifesto for all rocket scientists of that era. I have no doubt that others at the same speech, and others on Wall Street at the same time, will have different views on everything I've described. I care too much about these things to be a reliable reporter. It was only after reading the bare facts of the events as related in Justin Fox's book that I felt able to add some color, based on personal experience – otherwise, I wouldn't have known how to start. I still don't know how to end.

ENDNOTES

1. I will allow myself one quibble here. You might get the impression from the book that Gene Fama in particular, and efficient market supporters in general, were blinkered zealots until the early 1990s. But Fama's class for finance Ph.D.s in 1980 and the Chicago Ph.D. qualifying exam covered alternative views thoroughly, including those of Kelly, Mandelbrot, Shiller, Kahneman, and others. Fama was neither unaware nor dismissive of the variety of serious opinion about financial markets. For Fox's purposes, this only clutters the story; he can't describe every nuance of every person's thoughts. But this is one that matters, to me anyway.

2. Published posthumously. In his last communication with me, Fischer wrote that he did not expect to live to see it in print and he did not expect it to do well because "economists do not take well to criticism."

3. Its influence predates its publication, as much of it circulated as articles or unpublished working papers, or the ideas were communicated by Fischer in person.

4. Emanuel Derman, in his excellent *My Life as a Quant*, makes fun of this designation, saying it was based on the belief that rocket science

is the most difficult quantitative pursuit. He was part of a second, much larger wave of quants who were trained mostly in mathematics and sciences, without much instruction or prior interest in economics or finance. Unlike the first wave, a majority were educated outside the USA, often in communist or former communist countries, and so had even less exposure to capitalism. This group also had a profound effect on finance, but had different attitudes, backgrounds, and early experiences on Wall Street. The biggest difference is that there were too many of them, dispersed among a much larger global financial system, so they did not develop as close a group identity as the first generation.

5. Okay, I know a lot of people will ask, what about Ed Thorp? All rocket scientists had come across *Beat the Market and Beat the Dealer*; in fact, you recognized a fellow traveler by seeing those books on the shelf. Ed was tremendously inspiring to us; he did what we wanted to do. But he was 15 years ahead of us – 25 really, because he skipped the step of acquiring experience with a big Wall Street firm and went right to running his own hedge fund. Like Fischer, he published in practitioners' journals, started in a nonfinancial career, and moved

to a financial job. But he didn't have the connections to the great financial theorists; he had never been a finance academic (he was a math professor). Of course, he did have other connections we admired, especially to Claude Shannon. Ed demonstrated it was possible, but he didn't prove you could do it with financial theory as opposed to general quantitative reasoning. I realize, writing this now, that it makes little sense, but it was how we felt at the time.

6. Of course, lots of people write without equations. But Fischer Black was an excellent mathematician, who had refined his ideas to the point where he could express them in clear, nontechnical English.

7. See Black, F. (1978). The dividend puzzle. *Journal of Portfolio Management* 2, 5–8.

8. See Black, B. *op. cit.*

9. Fischer cited a manuscript version of the paper, but you can find the published version as Statman, M. and Shefrin, H. (1984).

Explaining investor preference for cash dividends. *Journal of Financial Economics* 13, 253–282. It was reprinted in an excellent book, Thaler, R., ed. (1993). *Advances in Behavioral Finance*. New York, NY: Russell Sage Foundation.

10. Grinblatt, M. and Han, B. (2004). "The Disposition Effect and Momentum." Working paper.

11. Graham, J.R. and Kumar, A. (2006). Do dividend clienteles exist? Evidence on dividend preferences of retail investors. *Journal of Finance* 61, 305–336.

12. Baker, M., Nagel, S. and Wurgler, J. (2007). The effect of dividends on consumption. *Brookings Papers on Economic Activity*. Economic Studies Program, The Brookings Institution. Vol. 38, pp. 231–292.

13. Odean, T. (1998). Are investors reluctant to realize their losses? *Journal of Finance* 53, 1775–1798.

14. Statman, M., Thorley, S., and Vorkink, K. (2006). Investor overconfidence and trading volume. *The Review of Financial Studies* 19, 1531–1565.

15. Statman, M. (2002). Lottery players/stock traders. *Financial Analysts Journal* 58, 14–21.

16. Grinblatt, M. and Keloharju, M. (2006). "Sensation Seeking, Overconfidence, and Trading Activity". NBER Working Paper No. W12223.

17. Shefrin, H. (2008). *A Behavioral Approach to Asset Pricing*. Academic Press Advanced Finance Series. ISBN 978-0123743565.