Contemporary Issues

Editors' Note: *Contemporary Issues* is a new section created to deal with recent developments and current thinking in finance. Potential topics for this section include (but are not limited to) discussions of recent significant changes in tax law or regulation. descriptions of innovative securities/strategies, brief analyses of major events, interesting descriptive material, comments by major figures in the field, and argument and opinions expressed by members of the profession. In a sense, this section might be called "the Editors' choice." As a consequence, some of the material appearing here has not been subjected to a formal refereeing process. This first installment consists of Richard Roll's keynote address at the 1993 FMA meeting in Toronto; William Beranek and Steven L. Jones's discussion of the recent growth in the trading of claims against bankrupt firms, along with some preliminary evidence; and Martin S. Fridson's argument that high-yield bonds do in fact contain an equity component. We hope you find our selections here, and in future issues, interesting.

What Every CFO Should Know About Scientific Progress in Financial Economics: What Is Known and What Remains to be Resolved

Richard Roll

Richard Roll is Allstate Professor of Finance at the Anderson Graduate School of Management, University of California, Los Angeles, California.

■ What topics in financial economics have empirical relevance? What are the contributions made by scholarly finance that a typical CFO should know? Which problems can be described as continuing research whose solution a CFO of the future will have to know?

Although most questions in financial economics remain in the unresolved category, there are some items that every CFO, even the president/CFO of a small firm, but certainly the CFO of any company of moderate size and larger, should already have in his repertoire of tools.

Option Valuation. The first candidate is the valuation of simple and complex options. Following the original Black and Scholes (1975) solution for a relatively simple option, the valuation of more complex options has been a major research success story in finance. There has been a tremendous volume of scholarly papers on this general subject, but success also can be measured by noting that complex option-valuation methods are widely employed by finance practitioners. Investment bankers, money managers, consultants, non-financial corporations, and government entities are today frequent implementers of various option-valuation techniques.

Option valuation is probably most frequently undertaken in the financial industry, but there are applications in other industries too. Many capital budgeting projects have option components, corporate debt is callable or convertible, bank lines of credit often contain contingent elements, labor contracts may endow options on workers (e.g., the choice of early retirement), real estate leases can be renewed, shelf space in supermarkets can be reserved at a price, mines can be opened and closed, etc. I would argue that option theory ought to be the first thing taught in finance, even before discounting arithmetic. A great thing in its favor is that we can actually tell business executives how to do something useful and important.

An interesting application of complex option valuation is in asset securitization. When I went to work at Goldman Sachs in September 1985, and began research on mortgage-backed securities, the secondary mortgage market already had a large volume of trading, but static mortgage yields were still employed when discussing value. An assumed constant rate of prepayment was used to calculate cash flows and the internal rate of return of the cash flows was computed and expressed as a yield spread over what was considered a "comparable" Treasury bond. Many market participants knew, of course, that this exercise was fraught with problems of an option-like nature. The decision to prepay is *contingent* on a number of factors: the refinancing rate, the value of the real estate collateral, the economic condition of the borrower, and perhaps the borrower's beliefs about the future course of interest rates. Put simply, the option to prepay is very complex.

Today, in contrast to 1985, all major dealers regularly attempt to compute the value of this option when conveying price information about mortgage products. The favorite metric in practice now is the "option-adjusted spread," the yield spread over Treasuries of the bond-qua-bond portion of the mortgage. Calculation of an option-adjusted spread is a multi-stage process based on a several assumptions about the state of nature:

1. *An interest rate "process"*: Typically, a factor model of the time series evolution of spot interest rates is used. The earliest models assumed a single factor, say the short interest rate, which drove all other rates along the term structure. A single-factor version of the Cox-Ingersoll-Ross (1985) term structure model is still widely used in practice, but more advanced valuations assume more complicated factor models, such as a two- or three-factor model or perhaps a model with stochastic interest rate volatility.

2. *A prepayment model*: This is often the product of an econometric investigation into observed historical prepayments patterns. (See for example Richard and Roll (1989).) Prepayment models regularly have a dozen or more explanatory variables; they display excellent empirical fit to prepayments on large aggregates of mortgages, for example, all GNMA pools with coupons of 8.5%. Their explanatory power is much worse for individual pools, of which hundreds of thousands now are being traded. In some cases, they have missed major influences. A recent example was their underestimation of the refinancing

incentives in ARMs, which became more important as short-term interest rates declined more than long-term rates in 1991. Since then, many prepayment models have been modified to contain an additional explanatory variable related to the term structure.

3. *Numerical integration or Monte Carlo simulation:* This combines the interest rate process and the prepayment model. Technically, this involves computing cash flows along a large number of different possible interest rate paths (the cash flows are path dependent), weighting the paths by probabilities of occurrence, and calculating a probability-weighted average of discount rates that equates the average net present value of the cash flows to the current observed price.

The same or a similar technique is applicable to a variety of other options. Of course, it is applied to mortgage derivatives such as collateralized mortgage obligations (CMOs), mortgage-backed bonds, and stripped mortgage-backed securities (Interest-Only IOs and Principal-Only POs), and to other interest rate-sensitive options, swaps, swaptions, call options on Treasuries, corporate debt, municipal securities, credit card receivables, automobile loans, and computer leases.

However, to point with satisfaction at the wide application of these techniques should not be done with too much pride. I have already mentioned that valuation of *individual* mortgage pools leaves a lot of unexplained variation. We know that some of this variation can be explained by regional, ethnic, and life-style differences among borrowers, but there still remains much left unexplained. Prepayment models are subject to considerable question, and term structure models are unfinished. In this latter area, multiple-factor models explain most of the price variation in long-term Treasuries, probably 98% or more, but only 50-60% of the price variation is explained in the short end of the yield curve, say for durations of less than 1.5 years. This lack of explanatory power is puzzling, and important, because the correct valuations of so many interest rate-contingent securities depend on a satisfactory dynamic model of the interest rate process.

Methods for Hedging. After option valuation, a logical second tool in the CFO kit would be methods for hedging. We still don't fully understand risk and return, but we are able, I believe, to offer advice and techniques for efficiently reducing risk by employing derivatives. I'm not arguing that businesses necessarily *should* engage in hedging, particularly when they are public corporations; but given that many businesses *do* hedge, we can help them toward an

optimal set of contracts. This may be particularly important for certain types of risks, not necessarily systematic risks, but risks such as those induced in some companies by exchange rates, commodity prices, tax rates, etc. You may ask why financial methods are not entirely obvious in this area. I would argue that in many situations, simply measuring the extent of the exposure can be difficult. Furthermore, businesses may want to simultaneously hedge several risks, which is a well-defined problem in portfolio theory: minimize the total volatility of fluctuations induced by a set of primitive risk exposures using a defined set of hedging tools.

Certain Economic Parity Conditions. Third, there are some concepts and topics in economics in which we can have confidence and that, if firmly grasped, can prevent common mistakes made by financial managers. One of my favorite examples involves the international parity relations, interest rate parity, purchasing power parity, and the international Fisher relation between real and nominal interest rates. Interest rate parity is a pure arbitrage condition, something that we can handle very well. If we consider nominal interest rates expressed in two different currencies, each on loans with zero (nominal) default risk and the same term, the interest rate differential is invariably within transaction cost bounds of the differential between the spot exchange rate and the forward exchange rate over the same term as the loans. For example, euro-currency rates conform perfectly to this parity condition. Indeed, we know that money center banks quote forward exchange rates simply by calculating what the parity relation requires the forward rate to be!

This is an important empirical fact for financial managers because they are frequently in the position of attempting to determine whether it is "cheaper" to obtain financing in one currency versus another. A multinational oil company, for instance, has the opportunity to obtain financing in a large number of currencies. Is there really one that is cheapest?

The exact empirical validity of interest rate parity for comparable contracts implies strongly that an apparent difference in borrowing costs between two countries is likely attributable to *non*-comparability of one of the contracts. If a U.S. firm borrows in deutsche marks instead of dollars, *but then hedges away the exchange risk,* the net cost should be identical. Indeed, this is the basis of pricing in the interest rate/exchange rate swap market. The empirical lesson is that perfectly risk-equivalent borrowing sources in two different countries are likely to have identical costs.

But what if the corporation doesn't hedge: can borrowing costs be cheaper on an expected basis in one currency rather than another? The empirical evidence seems to suggest that this might indeed be possible. There is pretty good evidence that real interest rates are slightly lower in some locations, at least temporarily. We might even speculate on a reason for this, such as differing propensities to save coupled with a greater perceived risk of investing in the non-local currency (unhedged). Note that *expected* unhedged borrowing costs could differ across currencies, but hedged costs ought to be the same.

Efficient markets. After spending 25 years looking at particular allegations about market inefficiencies, and 10 years attempting to exploit them as a practicing money manager, I have become convinced that the concept of efficient markets ought to be learned by every CFO. Market efficiency really is a first approximation to reality and, perhaps more important, is an extremely helpful organizing paradigm of thought. When some vendor, investment banker, or employee contends that a particular action should be taken to exploit an inefficiency, the CFO should be suspicious. This implies that educators should spend a considerable amount of time teaching the logic of market efficiency, giving numerous examples, discussing why and when it might not be true, and providing robust techniques for detecting such occasions.

I am personally suspicious of many academic studies that have allegedly uncovered systematic inefficiencies in the liquid bond and stock markets. Over the past decade, I have attempted to exploit many of the seemingly most promising "inefficiencies" by actually trading significant amounts of money according to a trading rules suggested by the "inefficiencies." Here, I'm not talking about "anomalies" that have potential explanations based on mis-estimation of risks or costs. In this latter category, I would place empirical findings that small firms earn more than large firms (beta adjusted), that the stock market consistently underprices stocks with low PE ratios, that discounts on closed-end mutual funds are too high, etc. Instead, I'm talking about intertemporal pricing anomalies that would seem to be exploitable by a defined trading rule: e.g., the January effect, the turn-of-the-year effect, the day-of-the-week effect, findings that returns are reversed over a short period (see DeBondt and Thaler (1985)) or that they are positively correlated over somewhat longer periods (see Jegadeesh and Titman (1993)).

Many of these effects are surprisingly strong in the reported empirical work, but I have never yet found one that worked in practice, in the sense that it returned more after cost than a buy-and-hold strategy. Note too that I am often in a relatively advantageous position to exploit new research findings. Usually, I hear about these things through the academic grapevine and read working papers long before the findings are published in a peer-review journal. I should have

as good an opportunity as any trader to exploit the profit-making potential of the finding. On the other hand, my continued inability to turn these findings into hard cash has led to a different hypothesis: that I'm just not a very astute investor. I would certainly be the first to admit that particular trades I have made based on academic disclosure of apparent anomalies may not have been advisable. Someone else may be much better at exploiting these opportunities. On the other hand, when Peter Lynch confesses that one of his greatest investments, in a particular brand of hosiery, was suggested by his wife's fondness for the product, it makes me think that I'm not that incapable a trader, after all.

There has been a lot written about efficient markets, but the basic concept is simple. Competition will eventually assure that trading rules just cover costs. The logic of efficient markets doesn't require that all prices always reflect all available information. This is merely an idealized frictionless version, akin to saying that gravity doesn't matter when firing a rifle over a 50-yard range. *Of course*, prices cannot always reflect all the information known to every single individual; if this were true, there would be no incentive to ferret out information, no incentive to expend resources on analysis, no incentive to pay attention to persons with knowledge (such as corporate insiders). The efficient market equilibrium is an absence of a *marginal* incentive to study, analyze, or spend on information. There are and should be infra-marginal returns; for example, the first person who successfully builds a neural network for analyzing utility stocks will probably cover costs, but not the 10,000th person who designs such a network.

Mathematical models of market efficiency have counterparts in models of evolutionary biology. In the model of the "hawks and the doves," biologists note that competition for food results in a stable evolutionary equilibrium characterized by multiple strategies. When competitors meet at a food site, they can either fight over the prize and risk injury—the "hawk" strategy—or withdraw and lose the food—the "dove" strategy. If every individual fights, a mutant who withdraws would eventually have a greater probability of procreating than the average fighter because of the risk of injury and the fact that only one fighter can win. (The dove occasionally finds uncontested food.) On the other hand, if every individual followed the dove strategy, a single fighter would gain a lot of food. The evolutionary equilibrium can be shown to involve either (a) part of the population always follows the hawk strategy and the complementary part follows the dove strategy or (b) every individual follows a randomized strategy, sometimes behaving as a hawk and sometimes as a dove. We can definitely rule out a world in which everyone follows the same fixed strategy.

The analogy to market efficiency is immediate: Investors compete for the most "undervalued" asset. The hawk strategy is conducting security analysis. The dove strategy is passive investing: expending no effort on information analysis. Clearly, if everyone analyses securities, the benefits will be less than the costs. If everyone is passive, the benefits of analysis will be tremendous. The equilibrium is that some analyze, some don't. Does it sound familiar? Note that the final equilibrium is characterized by a situation in which it is not worthwhile for the marginal passive investor to begin analyzing nor for the marginal active investor to cease conducting security analysis.

Some academics and many practitioners question the logic of market efficiency because of supposed non-rationality in individuals. They point to psychological studies demonstrating that individuals do not conform to the Von Neumann/Morgenstern axioms of expected utility, that they ignore obvious patterns in psychological games, or that they systematically over- or underestimate probabilities. Personally, I believe psychologists are generally correct about individual behavior. (Anyone who has married and divorced at least once cannot fail to see some merit in the hypothesis of human irrationality!) Perhaps we should all pay more attention to psychological research results, for they may very well imply market inefficiencies, particularly if *all* individuals behave aberrantly in the same manner. This might cause fads and speculative bubbles, a possibility championed by Shiller (1991) and others.

Yet we should be mindful too that a market may have greater apparent rationality than its individual participating agents precisely because everyone is *not* aberrant in the same direction; there can be diversification across irrationalities. When diversification is present and the number of individuals is large enough, even the slightest thread of individual reason may result in what appears to be a highly intelligent market solution. This is what we in finance call "collective wisdom."

The biological analogy is to an organism having greater ability than its constituent parts or to an insect society seeming to have much more intelligence than its individual members. E. O. Wilson's book, *The Insect Societies*, (1971) contains page after page of examples. I am fond of a famous example from a much earlier scholar, Eugene Marais, described in his book, *The Soul of the White Ant* (1938). Marais noticed that the tops of African termite nests were always kept clean of plants and debris. They are always smoothed dirt. Clearly, a termite society systematically sweeps off the top of its nest. Marais didn't ask *why* the termites cleaned their nests, but only *how* they organized and accomplished it. He threw a few blades of grass on top of a

nest and waited to see what happened. Termites appeared in large numbers and each termite seized a blade of grass and started tugging backward. Often, there were termites on all sides of a given blade of grass; they engaged in a tug of war, pulling the blade this way and that, with no perceptible reasoning nor cooperation that might have been dictated by some grand strategy. A blade of grass might be pulled all the way from one edge of the nest to the other, and back again.

A termite psychologist would have readily identified a severe lack of reasoning; yet the termites persevered. After long observation, Marais documented the fact that an individual termite tugged in a perfectly random direction. If a termite were picked up with tweezers and turned through some angle, it would resume tugging, but backwards at the new angle! How, then, does the top of the nest get cleaned? What is the mechanism of the grand strategy? Marais eventually deduced the solution. Each termite follows a simple decision rule: if the blade of grass is on top of the nest, tug on it. Otherwise, ignore it!

We might at first think how marvelously organized are the termite colonies to identify a problem and carry out a solution strategy. Cleaning the top of the nest appears to be the result of an intelligent being. Yet it's really nothing more than a simple decision rule dictated by the genetic heritage of every individual termite. The rule is illogical and results in a lot of unnecessary effort, yet it brings about a systematic—and seemingly carefully chosen—result.

We should teach this concept: Most persons suffer from all sorts of irrationalities. Yet there is probably a small amount of rationality within each of us. This may be enough, when averaged over countless individuals, to bring a result that *appears* to be the product of sophisticated reasoning.

Portfolio Theory. Since Markowitz' initial specification of an algorithm to "optimize" portfolios of risky assets, the theory of portfolio selection has become well-developed and useful. I believe that every CFO should have a firm grasp of how individual assets can be combined to obtain a portfolio that has a differing character than the mere average of its constituent parts. This is conceptually useful not only for firms in the financial industry, where it has a direct application to the balance sheet, but also for firms contemplating the combination of multiple production components, regional manufacturing sites, factories, divisions, etc.

I wish to emphasize, however, that I'm thinking here strictly about the *normative* theory of portfolio selection, not its positive application to risk/return equilibria. (More about that later.) In the normative theory, we should teach how a set of primitive characteristics for individual components combines into aggregate characteristics. For instance,

diversification renders a large portfolio's return less volatile in a very precise way that is determined by the *dependence* among individual assets. Every CFO should know, I think, that the asymptotic volatility of a portfolio is its average covariance, that mean/variance efficient combinations are not necessarily equal- or value-weighted, and that they have particular common properties (such as being positively correlated.)

The CFO should also have a keen appreciation of the fact that when inputs are measured with error, the errors are likely to be magnified in an "optimization." This can be taught easily with simulations, and it gives the student an insight as to when portfolio theory is highly useful and when it may lead to significant errors of judgment.

That ends my list. I can't think of anything else of empirical relevance that we can say with absolute confidence, "This is something every CFO should know." There are, however, areas of ongoing financial research that the CFO of the future may have to learn, once the scientific facts are settled.

There is a plethora of poorly understood financial phenomena. We shouldn't be too embarrassed by this; after all, the same thing is true in scientific disciplines with longer histories. A good analogy is to medicine. Medical scholars now understand the exact cause of Wilson's disease: It is the inability of the body to process minute quantities of copper. This inability is probably of genetic origin, but that is irrelevant because there is a 100% effective medicine, which simply eliminates excess copper from the body. Without that medicine, however, Wilson's disease causes severe dementia and often death. The scholars who figured this out transmitted their knowledge to practicing physicians through the usual channels, scholarly publications, such as the *New England Journal of Medicine*.

The diagnosis of Wilson's disease is also simple once a physician knows what to look for: it is a telltale ring of copper-colored deposits in the iris, surrounding the pupil. Yet it is believed that even today there may be tens of thousands of patients who have languished for lifetimes in mental hospitals around the world because they display the outward symptom of dementia. They would be cured within days by a simple drug.

This is a success. Medical researchers (analogy: finance academics) have provided an empirically relevant result to physicians (analogy: business practitioners.) Yet there are many unsolved problems in medicine. In some cases, the disease is well-studied, but the cure has not been discovered, for example, an AIDS vaccine. In other cases, even the mechanism of the disease is unknown, such as cancer of the stomach. Sometimes, there is a revolutionary development

that clarifies a problem, such as the recent discovery that duodenal ulcers are probably caused by a particular bacterium, a possibility that was dismissed for decades on the grounds that the stomach's digestive juices create a sterile environment. It turns out that at least one bug can live in those juices after all—and it causes ulcers! However, no one knows for sure yet how this bug is transmitted from one individual to another. It cannot live in an oxygen rich environment, so airborne transmission seems unlikely. Some researchers think it is acquired by kissing an infected person who has just belched, but other, even less palatable modes of transmission have been suggested. Imagine the medical experiments that will settle this issue!

So when a CFO asks us for an answer to some important unsolved problems, we shouldn't squirm too much when admitting that we don't know the solution.

Perhaps the most important unresolved problem in finance, because it influences so many other problems, is the relation between risk and return. Almost everyone agrees that there should be *some* relation, but its precise quantification has proven to be a conundrum that has haunted us for years, embarrassed us in print, and caused business practitioners to look askance at our scientific squabbling and question our relevance. Without a risk/return model that allows one to quantify the required rate of return for an investment project, how can it be valued? The only alternative to a risk/return assumption is a valuation based on arbitrage, but this requires the existence of an exact duplicate pseudo-project that can be constructed from a collection of assets with *known* market prices.

Probably the single greatest risk/return innovation was the Sharpe (1964)/Lintner (1965)/Mossin (1966)/Black (1972) Capital Asset Pricing Model (CAPM.) Its great insight was that priced risk is non-diversifiable; it cannot be eliminated through portfolio averaging. It is a simple, elegant concept that most financial scholars believe *ought* to be true.

Unfortunately, the CAPM has had a rough empirical ride. We know now that there is a perfect equivalence between the CAPM and the market portfolio proxy being optimized in the sense of Markowitz. But direct tests of market proxy optimization have often rejected that hypothesis and indirect tests of the relation between average sample returns and estimated systematic risk coefficients (betas) have not usually been encouraging. Indeed, an outsider might wonder why we are persisting in testing the CAPM when it has so often been an empirical failure. Almost three decades of tests, dating back to the well-known paper of Black, Jensen, and Scholes (1972), through papers by Reinganum (1981), Lakonishok and Shapiro (1986), and most recently Fama and French (1992), have all reported troubling empirical deviations from the CAPM's predictions.

The Fama/French paper reports virtually no support whatsoever for the CAPM's central proposition: a positive cross-sectional relation between expected return and beta in which beta is the *only* relevant explanatory variable. Instead, beta hardly explains *any* of the observed cross-sectional variance in average returns while other variables, such as market capitalization and market/book ratio, do! We know this could conceivably be attributable to an inappropriate proxy for the market portfolio, but we can't be sure that that is really the correct explanation.

The empirical problems with the CAPM have led many scholars to consider more general models of risk and return, such as Ross' (1976) Arbitrage Pricing Theory (APT). The APT shares the CAPM's principal feature: only non-diversifiable risk is priced, but it deviates from the CAPM by allowing for multiple causes of such risks. Many academics and practitioners have documented the fact that multiple factors affect the observed time series of returns. Indeed, the existence of multiple factors predates Ross' discovery of the arbitrage conditions underlying the linear APT return/risk model. King (1966), for instance, documented the importance of industry factors. Many other studies have subsequently found empirical relations between stock returns and interest rates, investor confidence, real output, the money supply, exchange rates, oil prices, aggregate consumption, and a host of other variables.

There is no doubt that observed equity prices respond to a wide variety of unanticipated factors, but there is much weaker evidence that *expected* returns are higher for equities that are more sensitive to these factors. There does appear to be some empirical support for a cross-sectional relation between expected returns and sensitivity coefficients, as the APT requires, but scholars have questioned these results on a variety of grounds.

Putting the very best face on this work, we are still a long way from being able to confidently describe the underlying reasons for cross-sectional differences in average returns. Part of this doubt, however, is clearly attributable to the large volatility in unanticipated returns, which makes the job of precisely estimating expected returns a multi-decade task. Indeed, for some classes of assets, it is difficult to reject the hypothesis that all assets in the class have the same expected return. Furthermore, there is some indication that expected returns are non-stationary over time. If expected returns are changing faster than the reduction in the standard error with increased sample size, no amount of empirical evidence may be adequate to resolve the risk/return conundrum.

The problem of risk and return is even more daunting when we contemplate it from an international perspective. There exist some seemingly sound theories of international asset pricing, but if empirical work is unfinished domestically, it is even further from completion internationally. There are further unresolved questions here including, but not limited to, the pricing of exchange risk, cross-country differences (if any) among risk premia or real interest rates, and the sources of international diversification. Some evidence seems to point to a well-integrated international market, but other evidence seems to suggest less integration, particularly for developing countries.

In summary, I have mentioned five topics in financial economics that have been scientifically settled, at least to the extent that every CFO should be aware of the findings and be able to use them in financial engineering. These are: (1) Option valuation; (2) Methods for hedging; (3) Certain economic parity conditions, such as interest rate parity; (4) Efficient Market Theory; and (5) Normative portfolio theory. Given the present state of knowledge, I can't think of any other scientific problem that is sufficiently settled to make the results essential to a CFO.

Among the currently unresolved questions that science will someday answer, perhaps the most important in finance concerns the relation between risk and return. This permeates many practical financial engineering applications, such as the selection of investment projects. Most scholars feel that risk and return should be related, but sadly, the exact quantitative specification is still beyond our comprehension.

There are, of course, many other unresolved problems. In corporate finance, we are still studying dividend policy, leverage, and financial signaling. In capital markets, we are still puzzling over the waves in mergers, the price performance of initial public offerings of securities, the salaries of investment bankers, and the many aspects of corporate control. We have a lot to learn about the microstructure of securities trading. We are only beginning to understand the influences of macro-economic factors (inflation, real output, monetary policy, etc.), on asset markets.

Given that so few topics are resolved and so many remain to be solved, it may seem that the science of financial economics has not been very successful. But the field is really only a few decades old, though there were isolated scientific discoveries extending back somewhat further in time. By comparison to historical developments in other sciences, the pace of discovery in finance has been respectable. Also, we should keep in mind that coming to realize what is *not* known represents almost as much progress as the discovery of an ultimate solution. Finance has set a dizzying pace in this regard. Much of what was once considered financial "knowledge" has been unlearned. ■

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