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Is there persistence in stock price movements?

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My 1963 paper *The Variation of Certain Speculative Prices* argues that changes in speculative prices should be considered as having an infinite variance. This seems an esoteric mathematical concept but, in fact, it is not. In the first part of this talk, I propose to discuss it in a new way, which I hope will make its meaning intuitive. The talk will continue by describing some fresh implications of infinite variance.

In order to assess quantitatively the risks relative to future market operations, it is necessary, in my opinion, to begin with an objective analysis of past price behavior. It may be that the subjective forecasts of skilled traders will turn out to differ from the inferences drawn from the objective examination of the past. If this is the case, we shall be confronted with a difficult problem of deciding whether those subjective forecasts are warranted. But I shall avoid this problem today. I shall also avoid the problem of distinguishing between factors relative to the market or to individual price series. I shall consider a *single* well-defined security or commodity and follow its *closing* price over successive days, months or years. There is, moreover, a great advantage in choosing a series that is very long even though it may necessarily be of limited interest today. This is one reason why I have paid special attention to cotton prices; but very similar results were obtained in all other cases by myself and by my student, Professor Eugene Fama.

If one plots the histograms of any kind of past price relatives, one always finds that it has a very pronounced bell in the middle, which means that most price changes are indeed small. But, whenever the series is long, and uncensored, one also finds that many of the largest positive and negative price changes are surprisingly large. What do I mean by surprisingly? After all anybody familiar with the history of the stock market (or even just with history in general) knows that extremely large price changes have occurred in the past. Well, data analysts have a very widespread acquired habit of considering that unless proven otherwise by overwhelming evidence all bell-shaped histograms are samples drawn from populations following the Gaussian law, or the Galton ogive. It is from this viewpoint, wrongly referred to as normal, that price change of such large size are found to have an extremely small probability, and are therefore extraordinarily surprising.

It is very tempting to try to sweep this difficulty under the rug. This is often done with the help of another concept, also referred to as normality. One claims *not* to be interested in the abnormal events, such as wars, depressions and the like, and one therefore eliminates all the changes associated with such major perturbations of the market. Such *a posteriori* censorship is naturally very effective in making the histograms acceptably normal in the sense of the Gaussian distribution. I don't think, however, that anyone will claim that it makes a difference whether the events that made one a rich or poor man were a posteriori felt to be normal market fluctuations, or were dignified by a special name and a place in history books. As a practical conclusion: when choosing the time period of the histogram of time to which one's histograms are devoted, it is necessary to disregard history and any other source of what statisticians call an optional stopping rule. This may sometimes imply that one cannot make use of the whole available sample, because the periods over which data are available are often bounded by major historical events. Insofar as possible, one must rather choose one's time span independently of the events that it contains.

One can also try to avoid dealing with the statistics of outsize events by claiming that their very size implies that they are due to a conjunction of causes so clear-cut that the economist should be able to link them together, thus giving an explanation and hopefully also allowing some degree of prediction. If and when such prediction becomes possible, it will obviously be taken into account, and only the unpredictable residuals will be treated as random. Until such a time, however, causal explanations of economics will be made a posteriori; this means that one has to say *not* that certain changes are big because they are causal, but rather that they are causal because they are big (and are therefore so important that a serious search for causes has been carried out.)

There is a third reason why the implications of price change histograms have not been faced in the past: It is widely believed that price series are not stationary, in the sense that the mechanism that generates them does not remain the same during successive time periods. This argument is used by some who believe that the price mechanism is fully rational and predictable, as well as by others who believe it to be fully statistical. They all claim that the laws of the market (whether rational or statistical) change from year to year, or even from month to month, and in any case, at least from one business cycle to the next.

Statisticians tend to be unaware of the gravity of this conclusion. Indeed, so little is known of nonstationary time series, that accepting nonstationarity amounts to giving up any hope of performing a worthwhile statistical analysis. Of course, one has no choice in some cases, but in most cases the evidence in favor of nonstationarity is exclusively based upon the wide variations in the values of the largest price changes observed over successive time periods, as well as in the subjective patterns that can be read into these changes.

The erratic behavior of the values of outsize changes is best expressed by the fact that the sample second moments (variances, standard deviations) are very different over different time spans. Let us, however, consider other methods of assessing the location and scale of the price histograms. The sample mean is not too erratic. Neither the sample interquantile range, i.e., the range between the one quarter largest changes and the one quarter smallest changes.

This evidence suggests that it may after all be possible to represent price changes by mechanisms that would be the same year after year. But these mechanisms must generate histograms whose central bell would be very similar between years, while their tails or wings would be very erratic. The mechanism proposed in my own work fulfill these conditions and are exclusively statistical. Some, but not all, allow no substantial prediction. Every form of statistical prediction can provide answers to actuarial questions of the following type: what is the minimum amount a broker must charge for allowing operations on margin, in order to keep his own probability of ruin under a certain prescribed threshold. The erratic behavior of the outsize price changes is taken care of by my principle of infinite variance, which it apparently took me a long time to get to in the present talk, and upon which I shall now comment.

My work concerns the logarithmic price relative; the logarithm of todays price minus the logarithm of yesterday's price! Let me begin by reassuring you that an infinite variance is not the consequence of my mistakenly taking the logarithm of zero. The concept of infinity, as used in engineering and physics, is not the same as the mathematician's infinity. Consider the example of a camera: there is a *finite* distance from the subject beyond which focusing at infinity is a completely safe procedure, as well as a tremendous simplification. This distance will depend upon the nature of the box, of the lens and of the film, and upon how wide the lens has been opened. Similarly, there is a range of cases where a variance that is really finite can be safely considered infinite. This situation prevails whenever all the samples that one encounters in practice are small, in the sense that the sample variance can never be expected to attain its limit, the population variance. In some cases, this happens as soon as the population variance exceeds ten times the interquantile interval; in other cases, it will have to be at least twenty times larger. As long as the population variance exceeds the lower limit relevant for the given sample size, its exact value becomes quite insignificant. I was led to postulate an "infinite variance" as the simplest way of mathematically describing the outliers, and of emphasizing their very strong contribution and their very erratic behavior from one year to the next. Any other way of dealing with these facts would have been extremely involved and would require large numbers of ad hoc and variable assumptions that would hopelessly overload the theory.

Let me now move closer to home, and show what the infinite variance means from the viewpoint of a natural classification of the risks associated with market trading. I will show that three or four parameters are necessary to describe the structure of the histograms of price relatives.

The first characteristic of a distribution is its mean, a location parameter. In the case of logarithmic price relatives, the mean is the average expected rate of growth.

The second characteristic is a scale parameter, such as the interquantile, which represents the risk associated with the scatter of one half of the possible outcome. One needs a scale factor that does not take very much account of the actual size of the largest price changes. Even when the variance is finite, the interquantile is a safer statistic, and I strongly suspect that many methods that claim to be based upon the variance actually use some scale factor like the interquantile or perhaps some other interquantile.

Now consider extreme events, favorable as well as unfavorable. For some reason which may reflect mathematicians'. outlook on life the problems relative to these outliers are all called problems of ruin, even when they consist in winning a jackpot. If the distribution were Gaussian, location and scale would determine it fully and could be used to evaluate the risks of ruin. Moreover, the Gaussian assumption leads to the conclusion that price is a continuous function of time. If this were the case, ruin could always be prevented by using any of a variety of stop-loss rules.

Actually, it turns out that the Gaussian estimates of the probability of ruin are absurdly low, and that stop-loss devices are often inapplicable when needed most: as, for example, when prices tumble down straight through the stop-loss line. This has several important consequences. First of all, it is prudent to separate the risks of extreme events from those of events in the interquantile range, and to estimate the probabilities of ruin separately. If the distribution of price changes is itself symmetric, a single parameter can cover both the favorable and unfavorable events. A large number of price histograms are indeed symmetric or very close to being so. This is for example, the case for the price relatives of cotton. But the price relatives of soybeans are very asymmetric and this is more complicated case requires four distinct parameters.

Suppose, then, that one wants to indulge in stock market engineering, to assess risks mathematically. To combine various kinds of securities or commodities into a balanced portfolio, one must not be content with mean and location parameters; the tails deserve separate attention. Moreover, the matter of how much to weigh the interquantile and the tails cannot be decided on a priori grounds, such as the arguments from the theory of economic utility, combined with various approximations that were suggested by Harold Markowitz.

By now, I hope to have motivated the principle of infinite variance and the need for a theory with 3 or 4 parameters. I found the starting point of such a theory in Paul Levy's esoteric concept of stable random variable and stable random process. Of course, it was originally constructed simply for its mathematical beauty, but one more beauty of mathematics is that every so often, a very abstract method turns out to be very practical. I would like in particular to provide a connection to the talk by Professor Fama which will follow mine, by pointing out how a theorem on stable processes has allowed me to analyze certain methods of trading akin to stop-loss rules, and to show the fallacy in S. S. Alexander's conclusion that his method of filters is better than buy and hold. Given their power, I cannot see how one could do any further theoretical work on prices without using stable distributions. In practice, however, it may often be possible to divide the distribution of price changes back into the middle part and the tails. Insofar as one asks questions about the probability of ruin, the tail is going to wag the dog.

As my last topic, I would now like to move from the distribution of price changes to the behavior of prices in time, and what has been called the random walk hypothesis. When I drew a histogram of price changes, I just took a long period of time and plotted the distribution of the sizes of the corresponding price changes. Now what about the possibility of interdependence between successive price changes? The random walk hypothesis, which is involved in most calculations of probability of ruin, consists in denying the possibility of any such interdependence, and data conform very well indeed to the random walk idea. That is, in my view the situation is the following: Although there are strong indications of various kinds of structures in price series, the market is sufficiently perfect, in the sense of economic theory, that it is very unlikely that any trading advantage could be drawn from such structures. Since no time is left to discuss the topic suggested by my title, let me simply mention that a paper of mine on martingales will discusses certain striking configurations that modify an underlying random walk without necessarily affecting a trader's expected gain.