

## NOTES AND COMMENTS

### COMMENTS ON: "A SUBORDINATED STOCHASTIC PROCESS MODEL WITH FINITE VARIANCE FOR SPECULATIVE PRICES," BY PETER K. CLARK

BY BENOIT B. MANDELBROT

THE WORK CITED [1] reports on experimental finds about price changes, which I believe useful, novel, and worth pursuing, and it advances another possible alternative to my stable Paretian model [2], notably to my infinite variance hypothesis. It may be interesting to elaborate upon some aspects of the relation between Clark's alternative model and mine. The main fact is that both can be viewed as "stochastic processes subordinated to Brownian motion," and even though the subordinating processes we advance are ostensibly different, they happen for most practical purposes to be surprisingly close to each other. Consequently, the paper cited involves no real difference about concrete predictions. However, it raises again an issue of scientific judgment I have often encountered: either Clark and numerous earlier critics are sensible in their belief that infinite variance per se is a feature so undesirable that, in order to paper it over, the economist should welcome a finite-variance reformulation, even when—as we shall see is the case with Clark's—it is marred by otherwise undesirable features; or I am sensible in my belief that stability in Paul Levy's sense is a feature both convenient mathematically and illuminating of reality, so that to achieve it one should learn to live with infinite variance. Dr. Clark's scholarly effort brings up this issue from a novel angle, which I believe deserves a fairly detailed discussion.

There is—as I have said—no dispute between us about the value of the concept of subordinated process. Clark's approach is an interesting and natural modification of one described by Mandelbrot and Taylor [4]. The notion is that price change would cease to be erratic and would reduce to the familiar Brownian motion if only it were followed in an appropriate "local time" different from "clock time." Taylor and I had thought that local time might coincide with transaction time, while Clark links it with volume. He also has the merit of having investigated this hunch empirically—as we shall see. However, it should be kept in mind that if price variation is to proceed smoothly in local time, then local time itself must flow at random and a highly variable rate. Consequently, as long as the flow of local time remains unpredictable, concrete identification of the applicable local time leaves the problems of economic prediction unaffected.

*The issue:* There is no longer any disagreement among economists that the distribution of speculative price changes is extremely long-tailed. To describe such distributions, applied statisticians have long noted one can use mixtures of Gaussian distributions with different variances. Price change distributions being nearly symmetric, the Gaussian components can have zero means. The concept of subordination of stochastic processes is "merely" a concrete interpretation of such mixing. In the present instance it is agreed that the mixing distribution of the variance is unimodal, with low probability for very small variance and appreciable probability for very large variance. The question is, which is the precise form of this mixing distribution? My 1963 model of price variation has been shown by Mandelbrot and Taylor to be strictly equivalent to postulating that the mixing distribution is "Lévy's positive stable distribution of exponent  $\alpha/2$ "—with  $\alpha$  usually near 2. Clark proposes instead a log-normal distribution.

This rephrases the new disagreement between Clark and myself in terms of a very old story, since the positive stable and the lognormal distribution have already clashed in the past repeatedly, notably in such contexts as the distribution of personal incomes, of firm sizes, and of city populations. As one may suspect from the very fact that each alternative has been defended by sensible statisticians, their actual graphs look very much alike, and I have noted several instances when different fitting criteria leave the field to different contenders. Therefore, even though I have numerous reservations about Clark's statistical techniques—especially about the estimation of  $\alpha$ —his statistical scores leave me unimpressed. They will be mentioned again, but to dwell upon them would distort priorities.

I believe indeed that *scientific model making is not primarily a matter of curve fitting*. Ordinarily, if one fits different distinct aspects of the same phenomenon separately, the best fits are both mutually incompatible, and unmanageable outside of their domain of origin. Hence, a combination of simultaneous fits of several aspects is unavoidable. For example, theories centered upon the Gaussian distribution are used in physics even when it is recognized that each specific aspect of the same phenomenon could be fitted better by some alternative. The Gaussian has special virtues: it is linked with the classical central limit theorem; it is stable, meaning the sum of any number of independent Gaussians is itself Gaussian; and it is analytically simple. Therefore, one is satisfied with a Gaussian theory as long as it fits everything reasonably well. Though I have often argued against specific Gaussian theories, I do consider the above attitude to be correct, because the virtues listed are real. Stability especially is linked with the potent scientific method of "invariances" (see, for example, [3]). In other words, I believe one should make it a principle not to give up the listed virtues without necessity.

An added virtue of the Gaussian is that its moments are finite, but after all, moments are an acquired taste. In this light, let me run through several comparisons between my model and Clark's alternative.

*Motivation for the stable model* (translated from the actual motivation, which was not relative to mixing distributions but to the resulting price change distributions): Among mixing distributions of the right general shape, the positive stable is the only one to be (i) stable—meaning that the mixing distributions corresponding to time spans  $T$  of one day, one week, etc., are identical except for scale, and (ii) related to a usable form of (non-classical) central limit theorem. Additional (late dividend) asset: since the implementation of fast Fourier transform methods, stable densities have become easy to calculate.

*Motivation for the lognormal alternative*: It is not described in the paper, but presumably many candidates were tried and the lognormal scored best. (If this guess is correct, the tests used by Clark would be questionable because they were designed to test between two hypotheses, not between a hypothesis and the best of several alternatives.) Of course, I am aware of the extensive generative theory for the lognormal through "multiplicative effects," but I don't see how those models can apply in this instance.

*Tests of the stable model*: They had been fairly rough and graphical (less so in recent work than in my 1963 paper [2]), but they have covered a broad variety of different properties of changes of different prices over different time spans.

*Test of the lognormal alternative*: It is less rough than in [2], but is limited to one series, the prices of cotton futures averaged over different dates of delivery. If it is indeed true that fit is improved using Clark's model for this particular series, the largest changes in this series would be smaller than predicted by my model. Even if this finding is confirmed, it may be of limited validity since the law imposes a ceiling on maximum change of the prices of cotton futures. Therefore, for a broad interest study of large price changes, cotton futures are not good material.

*Finite variance—pro and con*: I suspect that the above arguments would have settled the issue were it not for the matter of finiteness of moments. For the symmetric stable distribution of price changes the variance is infinite and for the positive stable mixing distribution even the mean is infinite, while in Clark's alternative, all moments are finite. However, though the use of infinite variance distributions was called a "radical departure," I don't believe it should continue to arouse emotion. The issues have been discussed elsewhere at length—particularly by me and E. F. Fama—but a few words may bear repetition. Since practical sample sizes are never very large, practical sample moments are only affected by the bulk of the distribution and not by its tail. Since we know that in their central portions all reasonable alternatives are very close to each other, the same should be true of the corresponding distributions of sample moments. If sample sizes could grow indefinitely, then in one case (stable) the distribution of sample moments would drift out to infinity, while in the other case (lognormal) it would thin down to a point. But those familiar asymptotics of the lognormal tell us nothing about distributions of small sample moments. When the lognormal is very skewed, its "small sample"



properties are extremely erratic, and calculations of sample distributions are very complicated. Thus, the reputed "tameness" of the lognormal is entirely based on its asymptotics, and in practice it has little value. On the contrary, the stable's asymptotics are a precise image of its finite sample behavior. As to the moment behavior of the data (which we cannot avoid and are supposed to describe), it is very erratic, so the reputed "wildness" of the stable appears to give a realistic picture of the economy.

*Brief comments on Clark's experiments:* They have shown that the daily increments of local time are like daily volume to the power of 2.13. This empirical discovery seems very interesting and deserves careful thought.

*I.B.M. Thomas J. Watson Research Center, Yorktown Heights, New York*

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#### REFERENCES

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