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Towards a second stage of indeterminism in science

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When the International Congress for Logic, Methodology, and the Philosophy of Science was held in Jerusalem, in September 1964, I delivered an invited address titled "The Epistemology of Chance in Certain Newer Sciences." But I hardly tried to prepare a text for the *Proceedings*, and for many years I kept resisting friendly suggestions – notably by the Berkeley molecular biologist Gunther S. Stent – that the draft be reworked, completed and printed. One reason was that success kept eluding repeated attempts to state a technical point, while also making clear its philosophical implications. But it is good to see the old text published at long last. It has been substantially edited for style and shortened, but not otherwise modified, and it is preceded by a few pages of miscellaneous observations, which have been recast in the form of a dialogue. References were updated in 2002.

1. Reflections from the perspective of 1987 on a premature fractal manifesto written in 1964

While the word *fractal* did not appear until 1975, this 1964 draft was important in the evolution of fractal geometry, an interdisciplinary enterprise I conceived in 1964, then developed. I have devoted to it almost all my creative life.

Question:
Why should this old text be of historical interest today?

B. B. Mandelbrot:
It occupies a critical position along the tortuous path that eventually led to fractals. This, and the fact that this text appears in a journal called *Interdisciplinary Science Reviews*, seems to call for a few philosophical and autobiographical comments. My research career, which must be described as "improbable," was triggered by a casual side interest in diverse isolated empirical regularities that everyone else viewed as of little consequence. As I look back, my life divides into three well-separated periods.

A period of gestation started with my PhD thesis in 1952 and lasted until 1964.

Jumping ahead, the third period that started in 1975, witnessed consolidation and increasingly broad, rapid, and smooth development, marked by books that do seem to involve an effective mix of technique and philosophy. Fractal geometry has the special charm of allowing uninterrupted interplay between concrete fields (ranging from widely practiced ones to the very obscure) and sophisticated pure mathematics. It has been successful as mathematics. In fact, it has shamed the iconoclastic tradition that ran from Laplace to Bourbaki by stimulating or reviving several mathematical theories; it has become a widely used tool in the description of nature and in the wide search for order in chaos; and finally, fractal art is now becoming widely admired as art, irrespective of its unusual origin.

The middle period lasted from 1964 to 1975. From the viewpoint of fractals' development, it was in many ways the most interesting but from a personal viewpoint it was most frustrating. This period was punctuated by successive *fractal manifestos*, the most notable ones having been a 1972 lecture at the Collège de France in Paris, which followed a Trumbull Lecture at Yale in 1970, and the even earlier 1964 Jerusalem lecture with which we deal here.

When chance or duty makes me reread this and other unpublished texts of the middle period, I am surprised at the precision and clarity given to many ideas that were not fully worked out until much later in my life. But my style failed to encourage the reader to plow through papers that had already acquired the reputation of advancing very disturbing ideas. It is useful, therefore, to state at this point one basic idea of fractals.

Why is school geometry so often described as “cold” and “dry?” One reason is this geometry’s inability to tell what shape a cloud is, or a mountain, or a coastline. Clouds are not spheres, mountains are not cones, coastlines are not circles, and more generally, man’s oldest questions concerning the shape of this world were left unanswered by Euclid and his successors, who concerned themselves exclusively with an unrealistically orderly universe. In order to achieve a handle on nature, a radically different geometry is needed, one that *must* contradict many old ideas that have become so familiar as to seem obvious and universally valid. However, to negate these ideas completely would be self-defeating because it would replace excessive order with utter chaos. Fractal geometry is a new and very different broad area of order within the domain of the old chaos. Some fractals imitate the mountains and the clouds, while others are wild and wonderful new shapes. More generally, the new fractal world is in some cases hard to tell from the real one, and in other cases it is of fantastic and surprising beauty.

Question:

Is there any relation between second stage indeterminism and chaotic fluctuations?

BBM:

The conventional wisdom has long been that the study of the weather and of economics is harder than the study of perfect gases, but will eventually use the same means to achieve the same degree of perfection. To the contrary, my work suggested a profound qualitative distinction between the underlying fluctuations, and as a result the theories of the corresponding phenomena were bound to differ sharply. On the one hand, the fluctuations that characterize the theory of gases should be viewed as “mild,” and the first stage of indeterminism in science was comparatively easy because of their being mild. On the other hand, the facts already established by 1964 indicated that the fluctuations of the weather and of prices were “wild.” I used to use “erratic,” an ill-chosen Latin word that did not last. My work invited the sciences to move on to a second stage of indeterminism.

How was this invitation received? Certainly not to my satisfaction! On the one hand, many influential scholars considered my discoveries to be potentially important, and offered me a series of renowned pulpits from which to present them. Yet, until 1975, they were called controversial. In fact, they provoked little discussion, pro or con, to justify them being so called controversial. They failed to affect the work of numerous, diverse, distinguished and often well-disposed people who heard me. To use a term favored by Stent, my work suffered from being “premature.”

Question:

You have said that, in your work, a growing role is played by sophisticated graphics, dear to a geometer’s and an artist’s eye. Could you elaborate?

BBM:

Being premature is particularly painful when one’s whole scientific work has been interdisciplinary. Thus, it is unusual indeed that fractal geometry managed to survive and to become part of the mainstream, without having to be first forgotten and later rediscovered by others, when its time came. Why did its time come after 1975, but not before? We cannot be sure, except that an essential role has clearly been played by computer graphics – of which I became a pioneer by necessity.

Mention of Stent’s paper necessarily brings forth a thought concerning the issue of uniqueness in scientific discovery. Indeed, Stent draws our attention to the (hostile) review that the biochemist Erwin Chargaff wrote of *The Double Helix* by James D. Watson. In that review, we read that “*Timon of Athens* could not have been written, *Les Desmoiselles d’Avignon* could not have been painted, had Shakespeare and Picasso not existed. But of how many scientific achievements can this be claimed? One could almost say that, with very few exceptions, it is not the men that make science, it is science that makes the men.

What A does today, B or C or D could surely do tomorrow."

This may be true of many of the individual strands of fractal geometry. But fractal geometry is not merely a juxtaposition of its individual strands. It arose as an integrated whole, ruled by a philosophy that was conceived and developed under conditions that – for better or worse – were greatly affected by my peculiar life story. Would another individual, or some collectivity, have reached the same philosophy and built the same whole? A worthy question for the future, assuming that this whole actually survives.

2. Text of the Premature Fractal Manifesto of 1964

Since the turn of the century, acceptance of indeterministic stochastic theories in science has spread spectacularly. A new epistemology has arisen as a result, superseding the epistemology built upon deterministic causal theories. In certain areas of physics, the new approach was rapidly and strikingly successful, for example in the study of thermal fluctuations in gases and in solids, and in quantum mechanics. Elsewhere, progress has turned out to be slow, and the fulfillment of high initial expectations is continually postponed. Such is the case of meteorology and in most of economics. The present paper proposes to trace this difference to the existence of a deep qualitative contrast between the nature of the observed fluctuations in the "developed" and the "less developed" sciences.

2.1. Differences in scientific development

It is often asserted that differences in development between sciences are solely due to differences of "age" as measured from the earliest systematic investigation of the different topics. I disagree. Indeed, probability theory saw its first triumphs in physics, but first arose in the study of the statistical problems raised by economic-psychological choice. In the hands of Laplace (circa 1800), a probabilistic view of social science and an arch-deterministic view of physics had reached a high point at the same time. Even as late as 1912, statistical social science could still be presented as a model to be followed by statistical physics. Similarly, in the works of Boussinesq (1872) and Osborne Reynolds (1895), the statistical concept of turbulence in fluids was roughly contemporary with Maxwell's and Boltzmann's (1866) kinetic theories of gases. But stochastic theories dashed forward in the study of gases, while they still lag in the study of turbulence.

2.2. Articulation

Before proposing an explanation of this difference in fate, it is good to recall that an "articulation" is at the root of many statistical theories: small systems combine into big systems, and one is interested in a statistical theory that applies to the latter and is based on a limit theorem of probability theory. In the case of thermal fluctuations, the small systems have physical reality but are microscopic, that is, inaccessible to human perception. Only the large systems are on man's spatial and temporal scale. Moreover, the following ideas are held to be true:

. •. The details of the microsystems have no effect on the macrosystems, and brutal approximations concerning the structure of the former do not affect the effectiveness of a macroscopic theory.

. •. The "classical central limit theorem" is applicable. *A fortiori*, the "law of large numbers" is applicable.

Let us recall the meaning of the terms used in the second statement. As applied to temporal means, the classical central limit theorem holds that the sum of $T^{-1/2}$ from $t=1$ to T of $[X(t)-E(X)]$ is approximately Gaussian for large T. As applied to means over large numbers of systems, this theorem states that the sum of $N^{-1/2}$ from $n=1$ to N of $[X(n)-E(X)]$ becomes approximately Gaussian for large N. The (strong) law of large numbers states that there is a probability equal to one that, for increasingly long samples, the sum of T^{-1} from $t=1$ to T of $X(t) \rightarrow E(X)$ as $T \rightarrow \infty$, and for increasingly large assemblies, the sum of N^{-1} from $n=1$ to N of $X(n) \to E(X)$ as $N \to \infty$.

First-stage indeterminism has the virtue of being closely related to causal theories. When it prevails, successful statistical theories can be constrained so that a "correspondence principle" holds: the mean E[the sum of N^{-1} from $n=1$ to N of $X(n)$], or the temporal trend E[the sum of T^{-1} from $t=1$ to T of $X(t)$] may be made to match those of an approximating deterministic-causal theory. In statistical mechanics, for example, the additional information provided by statistics is an important but detailed correction, an "error term," a "fluctuation around an equilibrium state."

It is usually felt that this correspondence principle is obvious and that scientists' universal reliance upon the law of large numbers and the classic central limit theorem requires no special justification. At best, a scientist may occasionally observe that the conditions of validity of these theorems are so undemanding, or weak, that they are overwhelmingly likely to be verified. But natural science exhibits very few cases (if any) where validity of these conditions is rigorously reduced to basic physical laws. Usually, their validity is listed as a kind of phenomenological principle that happens to be remarkably effective.

2.3. Less-developed sciences and articulation

With this in mind, consider less developed statistical theories that also involve a clear-cut articulation. Here is a first main point. My investigations lead me to believe that the less developed sciences are precisely those for which classical central limit theorem or even the law of large numbers fails to hold.

Does this belief imply that statistical techniques become helpless? It does not, by any means. However, and this is my second main point, the new models will necessarily differ in kind from the old ones. In other words, they will usher a new stage of indeterminism into science. The change will not only affect the details of the answers but the very characterization of what makes a question well-posed, or capable of being answered, and hence worth asking.

There are several possible reasons why the classical central limit theorem may fail to hold, and a corresponding variety of "kinds" of new statistical theories. No fallacious unity is therefore implied by referring to the aggregate of these theories as constituting a new second-stage indeterminism.

Also, this last term does not exclude the possibility that indeterministic theories may lie between the causal and the first-stage indeterministic theories, rather than beyond the latter.

Anticipating briefly questions to be discussed below, we may note that second-stage indeterministic models may be avoided by giving up the concept of statistical stationarity. If we do so, however, there could be no theory, and this would be a very poor bargain.

2.4. Possible reasons for failure of first-stage indeterminism

Even when the random quantities $X(t)$ or $X(n)$ are statistically independent, first-stage indeterminism fails when the distribution of $\boldsymbol{X(t)}$ is excessively "long-tailed" (that is, there is a very large probability of X being very large, for example exceeding 4 or 10 times the interquartile range). Let the distribution of random variable X be made increasingly fat-tailed. Sufficiently fat-tails cause the population variance to become infinite, and the classical central limit theorem necessarily becomes invalid. Later on, the population mean itself fails to converge, and the law of large numbers becomes invalid as well.

The mathematicians' search of interesting "pathologies" conceived these possibilities long ago, at least since Cauchy in 1853. But it is only recently that my work showed that these possibilities are not pathological, but practical and even indispensable. Examples occur in economics: Pareto's law of income distribution; the variation of speculative prices; the problem of industrial concentration; and so on. Examples also occur in physics, among them the flow of water from Lake Albert into the Nile River;

this is also the context in which one ought to re-examine the distribution of the energy of primary cosmic rays.

There is a second possible reason for first-stage indeterminism to fail. Even in cases when the distribution of $\boldsymbol{X}(\boldsymbol{t})$ itself is short-tailed (for example, Gaussian or even bounded), first-stage indeterminism fails when the intensity of the interdependence between $X(t)$ and $X(t+T)$, as measured by a correlation, decreases very slowly as $T \to \infty$. Indeed, as the span of interdependence lengthens, the classical central limit theorem eventually fails.

S_N being a sum of N independent variables $X(n)$, consider the relative contribution to S_N coming from the largest among the $X(n)$. When the expectation of X is zero and $S_N / N^{1/2}$ tends to the Gaussian as $N \to \infty$, a theorem says that the relative contribution of the largest among the $X(n)$ vanishes asymptotically. When $S_N / N^{1/2}$ does not tend to the Gaussian, the situation is sharply different: as $N \to \infty$, the relative contribution of the largest $X(n)$ may tend (in statistical law) to a nonvanishing limit. In such cases, the few largest contributions to S_N stick out, and there is a strong temptation to censor them *a posteriori*, calling them outliers, and use first-stage indeterministic analysis to study what is left.

2.5. Permanence of second-stage indeterminism

The features described in the preceding two paragraphs both raise a question. How final or permanent one may expect the second-stage indeterministic models to be? Classically, of course, there have been at least three distinct views of the roots of indeterminism:

A. Some stochastic models are held to be irreducible; this is the Copenhagen school's view of quantum theory.

B. Other stochastic models are held to describe the state of ignorance of some observer. For Laplace, once the past is fully known, so, potentially, will be the future. If so, the observer's state of ignorance will eventually reduce, more or less thoroughly, to an interplay of causal relationships. As was already mentioned, the usual argument holds that, when these causes are very numerous, and each contributes negligibly to the whole, one should expect the whole to be ruled by first-stage indeterminism.

C. The third classical view is favored by some extremist historians, who claim that the past can be of interest only for its own sake, and not as a basis of forecasting. In particular, statistical regularities in past records can be of no predictive interest.

Turn now to problems accessible to second-stage indeterministic models. They do fall between Laplace's conception of physics and the extremist's conception of history. Therefore, they introduce a fourth possibility.

D. One cannot always expect the causal reduction of fluctuations described under B to be possible. It may, however, be possible to reduce a second-stage indeterministic model to a mixture of the causal and the first stage indeterministic approaches.

Reductions of this kind are reassuring, because one has learned how to live with the two classical approaches. From the viewpoint of historical description, they are feasible *ex post facto* and are obviously a useful tool. In other contexts, they may also help classify and name the parts of the Unknown, a function whose performance is known to procure a feeling of power.

As a comment upon this last feature, observe that ordinary vocabulary is biased against terms that denote entities with a very long-tailed statistical distribution. For example, the size distribution of "human agglomerations" is much longer-tailed than those of "cities," "towns" or "villages," considered separately. Do the distinctions between city, town and village also have other foundations, more intrinsic than the desire to avoid a long-tailed distribution?

However, similar distinctions often lack other motivation. If so, Descartes' precept, "subdivide the difficulties into parts" may be very dangerous. Common sense and the old informal science embedded in the vocabulary both involve a preliminary processing that risks disfiguring the results of experience. In the areas where second-stage indeterministic models are indicated, examples abound where such *faux pas* are to be feared. For example, the flow of water alternates between being laminar and turbulent; hence the temptation to try to study the two kinds of regimes separately. In fact, my work suggests that the study of the whole natural flow may be simpler than that of its turbulent inserts taken alone. Similarly, in studying economic records, it may very well be preferable to avoid the temptation to attack periods of crisis separately, as if the economy changed in kind during the major depressions and booms.

To decide between alternatives A and D is a task that a scientist must not face on grounds of *a priori* epistemological preferences. Most scientists, however, agree with the Copenhagen view of quantum theory, that, as long as A has not been disproved by explicit construction, it is pragmatically unattackable. Most will also agree that one may conceive of cases when A has been disproved, but the best rules of behavior continue to be the same as if A held true. For example, the engineer's attitude towards thermal fluctuations is hardly affected by the fact that they are ultimately explainable by the kinetic theory of gases. *A fortiori*, it is likely in many fields that the best action will long continue to be based upon a second-stage indeterministic theory without concern about whether it may theoretically be reducible.

2.6. Meaningfulness of hierarchical descriptions

An important special issue is the problem of the meaningfulness of hierarchical description. I have found that samples generated by a second-stage indeterministic model often seem "stratified" or "hierarchical" even though no hierarchy had been built into the generating model. For example, generate an economic time series by a stationary stochastic process ***a***, with a continuous spectrum and a smooth spectral density with no local maximum but a pole at the frequency zero. Usually the sample functions of such a process seem to exhibit long Kondriatieff-like cycles that recall ordinary business cycles, and so on down to short-period wiggles much like the speculative fluctuations. A similar process engenders stars that group into galaxies, and then into clusters and then super clusters of galaxies, and so on. Obviously, the presence of such striking patterns in "forgeries" engendered by processes with no built-in hierarchy has far-reaching consequences.