New ways with classical mechanics

For about a century, the chief defect of classical mechanics has been that most problems within its ken have been insoluble. Now there is some hope that complex systems will be tractable.

THE optimism of nineteenth-century physics in its belief that all problems could be solved by Newton's laws and their later elaboration is now well recognized and widely scorned, but not for all of the right reasons. The nineteenth century was justly proud of the sophistication of schemes such as hamiltonian mechanics, which offered the solution of all problems provided that the arithmetic (or algebra, or calculus) was feasible.

The usual complaint is that the optimists overlooked the threat to the foundations of mechanics embodied first by Maxwell's theory of electromagnetism and, later, by even more subversive developments. More recently, people have also been saying that it was foolish of the nineteenth century to suppose that the arithmetic of the solution of real problems, with numbers of variables of the order of 10^{23}, would be simply a procedural matter. But now, almost a century after the full flowering of nineteenth-century optimism, the question arises whether the critics might not themselves have used the interval more constructively.

Classical mechanics is fine so far as it goes, but that is not very far. Only the simplest problems are soluble. For the rest, it is possible to extract from, for example, the hamiltonian formalism a few 'constants of the motion' which may, in special circumstances, be physically relevant. But what use can it be, for example, in an attempt to calculate the Curie temperature above which a ferromagnetic material cannot be permanently magnetized, to know that the physical linear and angular momenta of the specimen will be conserved? The quantities that naturally arise in nineteenth-century mechanics are often not particularly interesting.

That is why there has recently been such interest in other ways of calculating the physical properties of even classical complex systems. The twentieth century's shame is that most of these schemes are only recent. One landmark is the exact calculation, by Onsager in the 1940s, of the properties of a two-dimensional Ising lattice, most simply a square lattice whose vertices are of two kinds and in which only pairs of nearest neighbours interact. This can serve both as a model for order–disorder in alloys such as β-brass and for ferromagnetism. Another closely related approach to the calculation of complex systems is that represented by cellular automata, which may be likened to living organisms that replicate themselves from one generation to the next by means of formal rules.

The objection to most of these methods is that they tend to require numerical simulation, so that general principles can be discovered only with difficulty and enormous labour. One of the most familiar illustrations is the use of T.A. Witten's model of the process of aggregation for studying the formation of aggregates in circumstances where the process is limited by the rate of diffusion (Witten, T.A. & Sander, L.M. *Phys. Rev. Lett.* **47**, 1400; 1984); the proof that aggregates are fractal structures has nevertheless helped to develop scaling laws relating the total mass of an aggregate to its linear dimensions, for example.

Two other kinds of problems conventionally tackled numerically have now been given more general treatment, suggesting that people's ambitions are now rising to the point at which it may be feasible to ask interesting questions of some simple models of complex behaviour. One such model is that of the process of diffusion on a tree-structured lattice, which is in principle a model for all kinds of systems. If the 'leaves' of a tree-shaped graph are the blind end-points most distant from the root, for example, it is a fair question to ask how an arbitrary distribution of some attribute among the leaves will rearrange itself statistically if particles, or attributes, can move from one leaf to another only by the branches of the underlying tree.

Constantin P. Bachas and B.A. Huberman rightly note, in an article just published, that the rearrangement of some attribute among the leaves of a tree is a process representative of the behaviour of systems organized on hierarchical lines (*Phys. Rev. Lett.* **57**, 1965; 1986). In this spirit, for example, one might ask how the microscopic properties of protein molecules are determined by the hierarchy of amino-acid sequence and domain structure. The particular interest of what they have now accomplished is what seems like an analytical solution of the problem of diffusion on a tree-shaped graph even if the underlying sturcture (for example, the number of branches at each vertex) is far from simple.

Bachas and Huberman work with the concept of the redistribution of probability among the leaves of a tree, but the idea is more generally applicable. One of their conclusions is that the process of relaxation, or rearrangement towards a stable configuration, is most rapid for fat than for thin trees, which may not in itself be surprising; several branches at each vertex will give both a more bushy tree and more opportunities for lateral transport. It is more surprising that they conclude that diffusion is inherently more rapid for uniform trees (with a fixed number of branches at each vertex) and for random trees than for all intermediate structures.

The succeeding article in the same issue of the same journal is a step in the same direction by Martin Grant and J.D. Gunton of Temple University in Philadelphia. What they have done is to construct a continuum analogue of the cellular automaton construction — and to solve it (*ibid.*, p.1970). The essence of the problem is that, unlike the microscopic dynamics of the nineteenth century, this problem is irreversible in time. There is an obvious connection with irreversible dynamics, where the transformation of the state of a mechanical system from one instant to the next is represented by a matrix which is not unitary (or hermitian). These are also of course the systems in which people such as Ilya Prigogine have argued that highly organized systems (such as living things) may emerge not merely as accidents but because they are enforced by the underlying dynamics.

Grant and Gunton give an elegant account of how it may be possible to isolate from all the variables of a complex system those which vary only slowly (compared with others) in time, and of how dissipation can force the 'slow' variables to grow exponentially with time, at least at the beginning of the evolution of a system. The argument makes believable the observation, in numerical simulations of cellular automata, of recurring apparently well ordered patterns. The authors refer in passing to the familiar rapid growth of dendrites in crystallizing systems; these are structures far from ordinary thermodynamic equilibrium that appear to be forced by the underlying physics. Whether Grant and Gurton's conjecture of "irreversibility leading to [thermodynamic] instability" will be found valid, and whether it will, for example, account for the existence of self-organizing systems such as living things, is for the time being an open question. But it is an important question, far removed from hamiltonian dynamics.

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