Prospect for a new year

The temptation to guess what lies even immediately ahead must be resisted, as the prospects for fundamental physics show all too plainly.

PRETENDING to predict the future of discovery is not merely foolish, but a contradiction in terms. Discoveries are, of their nature, unexpected to some degree. What follows should therefore kindly be regarded not as a set of predictions but as a list of wishes. The exercise is worthwhile not for its content, but because it reflects the way that at any time there is a sense of expectation attached to some fields of research, and of dormancy to others. Perhaps a later Thomas Kuhn will think it worthwhile to compare these expectations with the realities that emerge.

Expectations are most vividly engendered by big machines, most of which are built to timetables advertised in advance. The year ahead should see the commissioning of two machines of great importance — the new electron-positron collider (LEP) at the European high-energy physics laboratory (CERN) at Geneva and the Hubble Telescope, which should at last be launched when the US shuttle is back in service and has room to spare. Curiously, neither machine has done as much to lift the spirits of those concerned as might have been expected.

The difficulty at CERN is that, this time, there is no obviously spectacular prize to win, such as the predicted discovery of the mesons that mediate the electro-weak force, and whose masses were predicted to be within the range of collisions between protons and antiprotons in CERN's super-synchrotron. Instead, as if discomfitted that the top quark has not turned up in the same range of energy, and as uncertain as ever where the Higgs particle may lie, people are now hoping that 'new physics' will provide LEP's fun and excitement. Nobody belittles the importance of being able to measure the electro-weak bosons in plenty, but the hazard of new physics is the difficulty of telling the difference between the real thing and spurious data.

The disappointments of the Hubble Telescope are different: frustration and continuing uncertainty. The fact that an instrument which, if it works half as well as planned, is likely to do more to change our view of the Universe than all this century's optical instruments put together should still be sitting on the ground at Los Angeles after three years, waiting for a firm launch date, may help researchers appreciate their properly modest place in the wider scheme of things, but it speaks wonders for the endurance of the human spirit in the face of frustration that the Space Telescope Science Institute and its principal investigators remain cheerful.

Some say the delay may have helped ensure success. The guide stars on which accurate pointing of the telescope will rely have been more accurately and comprehensively measured, while ground access has got rid of an optical misalignment together with a huge amount of unwanted dust. But the telescope has been frozen in design during a period when telescope designers have been more innovative than ever. There is also some concern that the modified shuttle may not be able to put the telescope into a safe orbit.

Either of these machines might help throw light on the great cosmological issues — but not in their first year. Appropriately, the Hubble Telescope should eventually provide a reliable distance scale for the Universe, and thus decide what Hubble's constant is, but everybody understands what painstaking work that must be. Even the issue of what quasars are will take time, though opinion does seem to be drifting towards the view that the nuclei of most kinds of active galaxies embody black holes; sadly, less attention is being given to questions such as the lifetime of the active phase and the related question of whether an active phase is an evolutionary stage of most, perhaps all, galaxies.

Expectations of the contributions likely to be made by particle physics to cosmological understanding are now less uniform than a few years ago, when the Big Bang seemed to many to be nature's practical demonstration that the particles of the known hierarchy (nobody calls them "fundamental" any more) are related to each other by the progressive breaking of a natural symmetry in a mass of expanding and thus cooling gas. Part of the trouble is psychological; no sooner had the particle theorists sketched in the details of the Big Bang in the first few seconds and minutes (with observationally important predictions of deuterium abundance and spatial uniformity in the early Universe) than, it seemed to cosmologists, they fell under the spell of string theory, which is a way of accounting for the differences between particles by means of dynamical variables in dimensions (most commonly, there are six of them) which are hidden from observation, or are "collapsed".

In principle, of course, particle physicists may justly say that the attractiveness of string theory does not imply an abandonment of the exciting vision of how particles run down the heirarchy in the Big Bang, transforming from one species to another in the process, but merely a quest for a more natural explanation of the relationships between particles.

But they have nevertheless damaged the simple view that particle physics and cosmology would come together through the Big Bang in at least two ways. First, the mere suggestion that there may be unobservable dimensions is an unsettling reminder of other conundrums, the quantization of gravity, for example. (The question whether the hidden dimensions may embody the hidden variables for which Einstein fought is too little discussed.)

Second, if the dimensionality of space–time is to have a place on the agenda of discussion, should not attention also be paid to the arguments in the second half of Stephen Hawking's recent book *A Brief History of Time* that the Big Bang is an illusion brought about by occupancy of what is bound to seem a Universe in a corner of a larger structure?

It would be good if there were some progress in dealing with this muddle in the year ahead, because people's instincts are sound and the thinking will have to be done at some stage. On a less abstract but equally teasing plane, it does seem certain that 1989 will be the year of Berry's phase — the notion that for any mechanical system represented by a complex field (such as a common-or-garden electronic wave function), whose phase is supposed to have no physical significance, each characteristic of the system can be used to define a phase in such a way that supposedly identical states have different phases, which may be measured as, or inferred to be, different. Abrahamov and Bohm, independently of M.J.Berry, have shown that the supposedly insignificant complex phase of Maxwell's electromagnetic potential is measurable. Is this another way of importing hidden variables? People are now so busily generalizing Berry's argument that we should know what it means before the year is out. The Nobel physics committee would be well advised to plump for this trio before it is denied the topic by an embarassment of choice. After all, the looked-for explanation for high-temperature superconductivity cannot now be found before 1990.

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