Time for tempting the Nobel fates

The Nobel season has come round again, but there is no closed season on speculation about this or next year's winners, irreverent though it may seem.

THIS is the season when it is reasonable to begin guessing at who will be in this year's list of Nobel prizes, due out less than a month from now. Stockholm committee-members, delphic as ever, probably have a good idea already. Others should be aware of the risks. One is that of losing money by injudicious bets, even at long odds. Another is that even a good guess may seem to delay a merited award by several years, as if the committees were abreactive to suggestions; for three years after the mention of the name in this connection, this journal felt apologetic to César Milstein, fearing that it had spoiled his chances. Those mentioned in what follows, even indirectly, are asked to be forbearing.

The ground rules are important. Although Alfred Nobel's will specifies that the three science prizes (physics, chemistry and physiology and medicine) will be awarded for discoveries in the previous year, the committees are evidently not strictly bound by that consideration. And although the three named prizes do not formally include astronomy and the Earth sciences, the rubric is suffiently elastic to have done some justice in these fields (but not, alas, to Hoyle). In passing, it is probable that Darwin, even had he been active later, would almost certainly not have qualified even under the most flexible interpretation; natural selection is certainly not medicine and is only at its most marginal physiology.

Techniques (as the example of chromatography shows) are not ruled out; given the present importance of the polymerase chain reaction (PCR) in amplifying small quantities (even single molecules) of nucleic acid polymers, that seems an obvious candidate. But Stockholm seems to prefer that the recipients of a single prize (the maximum is three) should be diverse in character; PCR will become even more awardable when an even more sensational use has been made of it than has yet been published.

Stockholm, like most journals, also seems to place particular value on a neat and well-rounded story. The now-successful hunt for the major cystic fibrosis gene has been exciting in itself, but will be irresistible in Stockholm when some physician has used what has been learned to cure patients. That prize cannot be long delayed. But there will be no prizes for, say, the nucleotide sequence of chromosome III of yeast; there are simply too many fingers in the pie, which is in any case a landmark rather than a discovery.

Much the same goes for the mechanism of gene control. Who could pick out three or fewer prizewinners from the army of people now working in the field? The best hope is that there will emerge some discovery about the mechanism of eukaryotic gene control suggesting unexpected continuity with the well-documented regulation of genes in bacteria. Much of the natural world is, in other words, unawardably complicated, although the regulation of the cell cycle may yet prove an exception. And the mechanism of the nervous system, unrecognized since the award to Hubel and Wiesel in 1981, is bound to come into its own again.

In the physical sciences, and chemistry in particular, there must be some place at Stockholm for C_{60}, now disarmingly called a novel allotrope of carbon. Its discovery has the classical virtues of seeming an historical necessity after the event. But it is probably now too soon to be celebrating. Stockholm may well prefer to wait until somebody has made an electronic device of it.

In that case, there is a case for doing something to applaud the importance of numerical techniques in understanding the ways in which molecules behave. Stockholm's reluctance to include machines among the recipients of its awards is understandable, but there is more than mere computer programming to the exploitation of, say, molecular dynamics in the understanding of complicated molecules. The difficulty would be in finding three or fewer people.

Straightforward physics is both more complicated and simpler. In one sense, there have been no sensations lately; the intermediate heavy bosons appeared on Stockholm's lists almost immediately after they were found, since when there has been endless argument about the nature of dark matter — and no top quark or Higgs boson. Anions are intriguing, but may not yet be prizeworthy.

A much better bet, perhaps the best this year, is linked with anions, whose statistical characteristics are usually defined by phase factors attached to their wave functions. It is now more than 15 years since Aharanov and Bohm pointed out the consequences of the equations of quantum motion of an electron in an electromagnetic potential.

Wave functions that solve Schrödinger's equation are complex quantities, with real and imaginary parts, but because their physical significance (the likelihood that a particle will be at some point in space) rests exclusively on their product with their own complex conjugate, they can be multiplied by an arbitrary number of unit modulus, most generally the quantity $e^{i\theta}$, where i is the square root of -1 and θ is any angle. But can this phase have no physical significance?

Bohm's interest in this question seems to have been clearly motivated by his long spell as one of Einstein's close colleagues when Einstein had set out to show that Bohr's willingness to embrace indeterminacy as the cornerstone of quantum mechanics must be mistaken. So might not the apparently irrelevant phase of the wave function embody the hidden variables' Einstein sought? That is how it must have seemed.

Most dramatically, what is called the Aharanov–Bohm effect is best illustrated by the thought-experiment (since realized) with electrons fired from a source at a screen carrying two parallel slits — the Young's slit experiment in quantum mechanics — in which interference patterns of electron waves are normally produced. The trick is to put a narrow conducting solenoid behind the screen but parallel with the slits. Although the field may be altogether too small to influence the electrons directly, their phase is affected by the direction in which they move relative to the magnetic field — and the interference pattern is shifted accordingly.

Stockholm need not be impressed, of course. Useful and interesting though the idea has proved to be, it might be thought a predictable effect, even a curiosity. But suppose it is put together with M. V. Berry's generalization of the argument that phase factors crop up repeatedly when the energy of a system is a function of some independent variable (as is the strength of the magnetic field in the electron experiment)? Berry's argument, first published in 1984, cannot be mistaken for fun and games. There is a growing band of people wondering what it means for the foundations of quantum mechanics. **John Maddox**