relying on intrauterine devices (IUD). This study has the advantage of providing its own built-in control group, but the age-distribution covers a wider range than in the Los Angeles study, so that the results must necessarily be less specific. The outcome is, however, again discouraging — all 13 cases of cervical cancer were found among the group using oral contraceptives. Again there seems to be a correlation between risk and duration of use but nobody would go to the stake in defence of any particular estimate of the risk. And again, the absolute risk is small — perhaps a doubling of the natural rate of something like 0.1 per cent.

The lessons to be drawn from this important work are several. First, for individual women wondering how with least risk to safeguard against unwanted pregnancy, especially when they are young, there is no substitute for a realistic appraisal of the small risks entailed and for an intelligent appraisal of the benefits and snags of other methods of contraception. As with other effective drugs, it would be surprising if oral contraceptives were free from side effects. But the risks are absolutely small — much smaller than those of regular cigarette smoking, for example. A decision to use oral contraceptives is neither irrational nor suicidal. For physicians, the new developments are yet another proof of how complicated the modern world has become — yet there is no civilized alternative to an honest attempt to explain what are the new developments, and to suggest what may lie ahead. Only public health authorities have a clear course before them — better facilities for the early diagnosis of the tractable forms of cancer, not simply those of the breast and cervix, are an urgent need. Will they be provided as generously as the circumstances require? □

Greenhouse research

There is a need for more research on CO_2 and climate. It should be directed where it matters.

Is the climate about to change because of the accumulation of carbon dioxide in the atmosphere? It is perhaps inevitable that the Environmental Protection Agency and the National Academy of Sciences in the United States should differ in their opinions on the future as sharply as reported on the next page. Much the same happened some years ago, when people were worried about the consequences for the ozone layer of the release of chlorofluorohydrocarbons into the atmosphere. But surely, there must come a time when further study is inappropriate, and when something will have to be done? Indeed, but not yet.

The academy report is itself a splendid pointer to the further study now needed, partly because of its omissions. First, there is general agreement on the fact of the accumulation of carbon dioxide — measurements are now much improved, and differences between predictions of future accumulations, based on different economic models dictating fossil fuel use, are not small but affect only the time-scale of events. There is more bite in what the academy's report has to say on interactions between the atmosphere and the oceans — solution of carbon dioxide accounts for 40 per cent of carbon dioxide released into the atmosphere by processes that are now reasonably well understood, and which are likely to become less efficient as concentrations in the atmosphere and surface layers increase. The potential role of the oceans as a massive heat sink for cooling the atmosphere is, by comparison, only poorly understood and cries out for further study.

On the evidence of this latest study, however, the greatest uncertainties attend the attempt to calculate what will be the climatic consequences of future carbon dioxide accumulation. On the grounds that a comprehensive survey of this part of the problem was published only in 1982 under the same auspices, this year's academy committee merely quotes the earlier study to the effect that the prospective warming of the surface of the Earth by the greenhouse effect is unlikely to be substantially reduced by negative feedback (such as could be caused by clouds). This is merely another proof that among the programmes of study now called for, the construction of more realistic models of the atmosphere is the most urgent — if the most difficult. □

Where was Hoyle?

It is not in bad taste to suggest another 1983 Nobel prize, sadly overlooked.

LAST week's clutch of Nobel prizes is more than usually interesting, as was that two weeks ago to Dr Barbara McClintock. In each case, the nominating committees and the Royal Swedish Academy seem to have gone out of their way to find people who are not only distinguished for the importance of their scientific work but who are also in some way acknowledged to be remarkable for the durability of their contribution to the understanding of science. The saga of Dr McClintock's long battle against prejudice and incomprehension is an heroic tale. Taube (this year's chemist) is plainly one of the most versatile of people. Chandrasekhar (one of this year's two physicists) has also in his time surmounted prejudice, mostly at Cambridge in the 1930s (and what a collaboration there would have been if Eddington had recognized that his younger colleague had such tools in his gift). But William Fowler (the other physicist) belongs in this group, even though his sunny temperament and engaging personality can never have invited prejudice, because of the distinction of his physics — especially his early decision in 1946 to embark on a study of nuclear reactions likely to be important on the Sun — as well as for his lasting influence on a generation of physicists at the California Institute of Technology. It may be especially important for the future that the Nobel committees have finally agreed that astrophysics is now properly within the scope of physics.

For all those reasons, it is important that what follows should be read with care, as meaning literally no more than what it says — and especially by those whose names are mentioned, all of whom are great friends of this journal. To adapt a now well-known phrase, however, it cannot have escaped most people's notice that the list of physics prizes does not include some of those whose names naturally come to mind in connection with the problem of nucleogenesis, several of whom are mentioned on page 759 and the most obvious of whom is Sir Fred Hoyle. To say this is in no sense to suggest that Fowler's work is unimportant or undeserving — on the contrary, everybody will be delighted that his massive contribution to physics has been acknowledged in this way. Fowler, generous man that he is, would probably have been even happier if he and his old collaborator had been joint prizewinners.

So what went wrong? It is entirely possible that the Nobel committees, inhibited by their by-laws, are saving up the names of contributors to the nucleogenesis problem for some future years, but given the pace of discovery, that would be a risky manoeuvre. It is also possible that the committees, or even the Royal Swedish Academy, were impressed that Hoyle differs from Fowler (and most other prizewinners) in that, always subversive as the best scientists are, he has recently been over-subversive, advocating with too much passion and too little evidence the unnecessary theory that living things on the surface of the Earth originally arrived from intragalactic space. By the general consent of this journal's referees, that is where the weight of opinion lies. Yet it would be a shabby circumstance if such considerations or fears of the embarrassment there would be if an unrefereed account of this theory were blazoned across the world at some prizegiving ceremony had deflected the committees from the logic of what might have been their intentions.

It would be improper but also impracticable to demand that the committees should at some point in the future make amends. If the Nobel prizes are to retain their high reputation, each year's prize must go to that year's obvious candidates. And nothing in the preceding sentences should be taken to mean more than the obvious — that Hoyle would have been a natural partner for Fowler, that the contributions of the other partners to their famous collaboration are in no sense belittled and that especially in the supposedly adventurous discipline of physics, it is unreasonable to expect that able men and women will always be people whose genius is never marred by eccentricity. □

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