saturation with respect to aragonite when P_{CO_2} attains approximately three times its present value. The biological implications of such a change are unknown. The attainment of the saturation level may in itself be less significant than the accompanying fall in the *p*H of the mixed layer.

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Atmospheric halocarbons and stratospheric ozone

LOVELOCK[1] reports atmospheric analysis data for the fluorochlorocarbons (FCCs) CCl_2F_2 and CCl_3F in addition to the chlorocarbons (CCs) CCl_4, $CHCl_3$, CH_3Cl_3, $CHClCCl_2$, and CCl_2CCl_2. The synthetic FCCs are presently of concern as possible catalysts for the destruction of stratospheric ozone. Although Lovelock states that "Molina and Rowland[2] rightly warn of a potential hazard to stratospheric ozone should the emission of the FCCs continue to grow unchecked", the levels of the CCs reported led him to conclude that "unless there is some special additional effect from the release of chlorine at higher altitudes, there are no grounds for singling out the FCCs as more hazardous than the other halocarbons which penetrate the tropopause".

I do not disagree with that as a statement of the present state of affairs, but I suggest that the conclusion, as stated, is likely to cause confusion as it does not distinguish between the present condition and the probable future values of the FCC/CC ratio, the issue of central concern. The following points seem to have been made by Lovelock or might be reasonably concluded from information presented by him: (1) Present halocarbon levels are at the edge of significant participation in stratospheric ozone destruction cycles. (2) CCl_4 and other CCs are present in far larger amounts than can be explained by reasonable estimates of synthetic sources; the present concentrations of CCs then represent essentially natural levels and are therefore not likely to change significantly in time. (3) The FCC analysis data suggest, however, that most of the FCC production to date is still present in the troposphere and lower stratosphere. So the sinks for the naturally-occurring CCs do not seem to be significant for the FCCs. For example, use of Lovelock's mole fraction of 1.0×10^{-10} for CCl_2F_2 and an atmospheric scale height of 7.3 km lead to a value of 1.3×10^9 kg for the amount of this compound in a shell 10 km thick around the Earth. This estimate is based on a low value for the tropospheric volume, neglects stratospheric content, and uses an analysis significantly lower than reported elsewhere[3]. Use of a recent production rate of 0.5×10^9 kg yr^{-1} and exponential growth with a 3.5-yr doubling period observed back to 1960 (ref. 4) gives an estimate of 2.5×10^9 kg for industrial production to date. Thus a large fraction of the FCCs ever produced can still be accounted for in the atmosphere. The only significant loss process would then seem to be diffusion past the tropopause and subsequent photolysis in the middle stratosphere.

I conclude that the problem is not the consideration that the FCCs might have some special stratospheric reactivity in contrast to the effects of the CCs, as suggested by Lovelock, but rather that the FCC concentrations are likely to continue to grow to critical levels. This has been illustrated by the model calculations of Cicerone, *et al.*[4]. Thus the FCC/CC ratio in the troposphere is most likely to increase steadily with time to a value significantly greater than unity with resulting future consequences for the ozone levels.

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Matters arising

Matters Arising is meant as a vehicle for comment and discussion about papers that appear in *Nature*. The originator of a Matters Arising contribution should initially send his manuscript to the author of the original paper and both parties should, wherever possible, agree on what is to be submitted. Neither contribution nor reply (if one is necessary) should be longer than 300 words and the briefest of replies, to the effect that a point is taken, should be considered.

PROFESSOR LOVELOCK REPLIES—If the models of stratospheric ozone destruction by odd chlorine are assumed, then I agree with Chesick that the uninterrupted exponential growth of FCC emissions must eventually establish an atmospheric concentration hazardous on a global scale. In this limited context the only point at issue is when this concentration will be reached. The atmospheric significance of a compound depends on the rate at which it releases chlorine in the stratosphere and this is a function of residence time as well as of abundance. The FCCs have residence times of more than 50 yr whereas the CCs other than CCl_4, have residence times of less than one year. A large fraction of the FCCs entering the stratosphere are returned to the troposphere unchanged, but most of the CCs entering the stratosphere are destroyed there.

I suggested that there might be a large natural chlorocarbon cycle[1]. Evidence gathered during the past six months considerably strengthens this conclusion. R. A. Rasmussen (personal communication) has found substantial concentrations of methyl chloride in rural air on the West Coast of the USA. I have confirmed this finding for air coming from the Atlantic with concentrations of methyl chloride now exceeding 10^{-9} by volume. As our investigations proceed, the sum total of atmospheric chlorine carriers discovered grows much more rapidly than does the release of FCCs. The measured ratio of CC to FCC chlorine is now approaching four.

The models of ozone destruction by odd chlorine are plausible and do give cause for concern but in the debate it tends to be forgotten that the models are entirely unconfirmed by direct observations in the stratosphere.

When all these factors are taken into account it does seem that there is time to make those stratospheric measurements from which a realistic upper limit to the concentration of anthropogenic chlorine compounds can be decided. Tropospheric measurements are also needed to complete our understanding of the natural chlorine cycle.

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Alteration of chicken melanocytes by DNA

LANZA has reported that injection of Harco chicken DNA into 5-d-old White Plymouth Rock embryos permits production of melanin beyond day 14 of incubation, in contrast to uninjected embryos[1]. He cites Hamilton's work, in which melanocytes were observed in embryos of all fowl examined, including breeds with white plumage; the melanocytes of white breeds were initially capable of melanin synthesis but degenerated