in overwintering as far south as Tampico in 1977–78, 1978–79 and 1983–84.

Thus, eradication from Florida in 1957–58 and the virtual disappearance of screwworm from the southwestern United States since about 1979, must in part have been due to unfavourable climate. The coincidence between 'success' in the two major campaigns and the occurrence of record cold winters is inescapable and unlikely to be due merely to chance. New outbreaks could occur when climatic conditions again favour the pest.

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1. Krafsur, E.S., Townson, H., Davidson, G. & Curtis, C.F. *Nature* **323**, 495–496 (1986).
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KRAFSUR *ET AL.* REPLY—In our earlier letter[1] we showed that the effects of winter temperature on screwworm abundance were statistically insignificant over 22 years in each of seven climatic zones of Texas. We do not think this can be fairly described[2] as a light dismissal of Readshaw's views[3] that screwworm eradication by sterile males is a "grand delusion" and that the reduction in cases can be explained by recent cold winters.

Bartlett's test applied to the residual variances in the analysis of the data from the climatic zones shows that the pooled regressions are heterogeneous ($\chi^2 = 40.3$, 5 d.f., $P<10^{-6}$). If, nevertheless, one chooses to pool the number of cases for the whole of Texas and take state-wide temperature means for each winter, there is, as Readshaw says, a statistically significant association of screwworm cases with winter temperature; we find that $F = 8.62$, $P = 0.009$, and that this association accounts for 13.5% of the variance. But we doubt whether it is wise to take a fairly weak association of this kind for such a large and biogeographically heterogeneous state and promote it to a causal predictive indicator. If the association were meaningful, one would expect it to be sharpened by looking for it zone by zone, but instead it completely disappears.

We are not convinced by Readshaw's use[2] of a significant screwworm/winter temperature association between zones averaged over all years to "explain" variation from year to year — this kind of manipulation of statistical association is notoriously unreliable. We also believe his "overwintering temperature threshold" of 17°C to be fallacious. He derived this idea from a paragraph[4] containing data about the number of winter cases in different counties of southern Texas. But data in the same paragraph show that screwworm ovaries continue to mature even if temperatures drop to a mean of 15°C, which is scarcely consistent with the idea that at or below 17°C a population is wiped out.

In contrast with Readshaw's belief in the irrelevance of sterile male releases, the same paper[4] gives evidence that the manner of distributing sterile flies is critical to their achieving matings with wild females. Appropriate modifications to the sterile fly distribution techniques appear to have been decisive in reversing the fortunes of the eradication programme in 1977 in south and central Texas and in Tamaulipas, Mexico[5], in contrast to west Texas, New Mexico and Arizona, where these modifications were not made at that time.

Readshaw's model (see Fig. 4 of ref. 3) predicts that there should be plenty of screwworm cases in Texas, but in fact none has been reported since 1982 (ref. 6). This follows the massive sterile male release campaign throughout Mexico over the past 10 years. This campaign seems to us much more likely to be the major cause of the eradication than do a few cold winters. By 1985 screwworms had been confined to east of the 93rd meridian in southern Mexico[6]. In March 1987 the Mexico–American Commission for the Eradication of the Screwworm reported that the screwworm was now restricted to the international frontier of Mexico with Guatamela and Belize, and that no screwworm cases had been found in the Yucatan peninsula since 21 December 1986 (S.A. Cajero and J.E. Novy, personal communication). We have examined winter temperature data for a number of locations in Mexico for 1936–86 and can see no trends that correlate with the recent screwworm eradication. In the lowland sites for which we have data, winter temperatures remained well above Readshaw's supposed 17°C threshold throughout the period of the eradication.

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The multiple star system Sk −69 202

SIR—The star Sk −69 202, which is now firmly established[1] as the progenitor of SN 1987A, has two close companions. These companions[2,3] have $V = 15.3$, separation, d, = 2.65arc s (star 2) and $V = 15.7$, $d =$ 1.4arc s (star 3). The joint *a priori* probability that Sk −69 202 should have two such relatively close companions, is $\sim 1 \times 10^{-5}$. This shows that Sk −69 202 was almost certainly the brightest member of a physical triple system. If star 2 is an unevolved main-sequence star it has a mass of $\approx 10\ M_\odot$. As Sk −69 202 was brighter and more highly evolved than star 2, its main-sequence mass must have been $> 10\ M_\odot$.

White and Malin[2] have derived star densities from various photographic surveys of the region within ~ 30 arc min of SN 1987A. From these data they calculate the probability that stars of various magnitudes will occur near an arbitrary point in this field. From their data it is found that the *a priori* probability of finding a star as bright as star 2 within 2.65arc s of Sk −69 202 is $10^{-2.3}$ and the joint probability of finding stars 2 and 3 so close to Sk −69 202 is therefore 1×10^{-5}. In other words it is virtually certain that Sk −69 202 was the brightest component of a physical triple system. For an assumed distance of 50 kpc the projected separation of Sk −69 202 and star 2 is 0.6 pc. Because members of physical groups of stars are approximately coeval star 2 must be less evolved than Sk −69 202 and may therefore be used to set a lower limit on the mass of the supernova progenitor. A similar argument has recently been used by van den Bergh and Dufour[4] to derive a lower limit for the mass of the progenitor of the supernova remnant N63A in the Large Magellanic Cloud, which is located in the association NGC2030.

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Erratum

A different perspective on coral population genetics

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Nature **323**, 109 (1986).

Two references were incorrectly cited. They are correct as printed here.

1. Hodgson, G. *Mar. Ecol. Prog. Ser.* **26**, 61–71 (1985).
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