Table 1 Chlorine content found by electron beam induced X-ray analysis of the basis that the contents of Mn, Cr, Fe, and Cl add up to 99.99%

Extreme-pressure additive	Percentage of Cl: On wear print area	On blobs of debris	On arbitrarily selected spots
5% by wt chlorinated paraffin wax B	0.38	2.48	—
2% by wt chlorinated paraffin wax B (optimum concentration)	0.20	1.34	—
1% by wt benzyl chloride (optimum concentration)	Trace	—	0.10

Wear prints were obtained from 30 s runs with 0.9525-cm diameter En-31 steel balls at 1,500 r.p.m. and 21.6 kg wt.

We conclude from these observations that chlorinated paraffin wax is superior to benzyl chloride as an extreme-pressure additive. While the chemical reactivity of the additive plays some part in the mechanism of extreme-pressure lubrication, it does not lead to the formation of a wear-protective layer.

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SILVER AND MOULD REPLY—We were rather surprised that Schnurmann and Porter found "quite unacceptable" our view[1] that extreme pressure lubricants containing either sulphur or chlorine form with steel surface layers of iron sulphide or chloride which reduce metal wear and prevent seizure. This is not only our view but also that given by Rowe[2], Prutton[3], Davey[4] and many other workers in the field of extreme pressure lubrication. We were similarly surprised to read the statement that the chemical reactivity of the additive, although playing some part in the mechanism of extreme pressure lubrication, does not lead to the formation of a wear-protective layer. This too is in contrast with our findings[5], and the findings of others[6,7], of the relationship between chemical activity and extreme pressure performance.

However, it is possible that a misunderstanding has arisen over the definition of the lubrication condition known as 'extreme pressure'. By this we, and others, mean the conditions whereby lubrication is maintained by means of a surface layer of principally inorganic material. These conditions are operable under the severe loadings found in hypoid gears or in certain metal cutting operations, and are in contrast with those used by Schnurmann and Porter in their experiments. In lower loaded anti-wear tests[8], we found that chlorinated hydrocarbons gave a slightly better performance than benzyl chloride, although at the same chlorine concentration in each oil; however, we must question the validity of results obtained from blends of chlorine compounds as described by Schnurmann and Porter, each with such different chlorine contents.

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Interspecific competition in tits

DHONDT has recently made a case for the existence of interspecific competition between blue and great tits[1]. My first criticism is mainly statistical and concerns Dhondt's Fig. 3 in which he presents a Lotka–Volterra graph with the equilibrium lines for the two species, estimated subjectively by eight colleagues. I tried a discriminate function analysis as a more rigorous approach and found that the blue tit line had a slight, though insignificant, positive slope. Moreover, in a linear multiple regression of population change of one species on the density of both species, neither species had a significant effect on the other. Consequently, neither the competition coefficients nor the conclusions about niche breadth drawn from the coefficients can be upheld.

My second criticism concerns the interpretation of the significant regressions in Dhondt's Figs 1 and 2. Both figures show that relative to great tits, the reproductive success of blue tits increases as the combined density of the two species increases. Because of the greater variance of the great tit density the result, especially in Fig. 2, could be due to intraspecific, density-dependent effects. In a linear multiple regression of relative competitive ability (great tit fledged per pair divided by blue tit fledged per pair) on year, blue tit density, great tit density, and the ratio of great tit density to blue tit density, no one independent variable is significant although the whole regression is significant. Thus, the dependence of relative competitive ability on combined density seems uncertain and the possible contribution of the blue tit–great tit ratio should not be rejected. Finally, as there will be interspecific differential mortality, emigration, and so on, there is no reason to think that the equilibrium combined density should be where the reproductive abilities of the two species are equal.

British data support the trends reported by Dhondt except that in contrast to Dhondt's study, there is a strong positive correlation between changes in blue and great tit densities[2].

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DHONDT REPLIES—In my letter[1] I did two things (1) demonstrate that competition for food in the breeding season between great and blue tit results in a reduced fitness (fewer young produced) and (2) speculate how these titmice nevertheless coexist. Minot confirms point (1) but here criticises point (2).

The first part of Minot's criticism concerns the isoclines I tried to estimate. The method I used was unorthodox. I thought that fitting the lines by eye made it possible to correct for aberrant points caused by varying environmental factors. The more orthodox approach is to calculate a multiple regression equation in which the relative change in numbers (not the absolute value as Minot has done) is expressed as a function of each species' density. Doing this, leaving the 1962 point out (see ref. 1), results in isoclines very similar to the ones drawn in my Fig. 3. These are, for the great tit (GT) $1.817-0.075\ GT-0.045\ BT=0$ and for the blue tit (BT) $1.186-0.098\ BT-0.0087\ GT=0$. It is correct that change in numbers of either species is not significantly correlated with the density of the other species. However both partial correlation coefficients are negative and the one expressing the effect of the BT on GT change in numbers is almost significant ($r=-0.42$, $t_{14}=1.736$, $P\simeq0.05$, one tailed), although the other one is much lower ($r=-0.23$). We know however, that the key factor, contributing most of the variance to annual mortality is 'overwinter mortality'[2,3]. Therefore it would be surprising if change in numbers could be related directly to breeding density in the previous year, without correcting for the factors that cause this variance. I conclude

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