the endogenous one. Finally, heterodimerization could similarly explain how, when a human transporter[8] is introduced into chick embryo fibroblasts, its turnover is regulated in the same way as the endogenous transporter (unpublished data).

MARTYN K. WHITE
MICHAEL J. WEBER

Department of Microbiology,
University of Virginia Health Sciences Center,
Charlottesville,
Virginia 22908, USA

1. Landschultz, W.H., Johnson, P.F. & McKnight, S.L. *Science* **240**, 1759–1764 (1988).
2. Buckland, R. & Wild, F. *Nature* **338**, 547 (1989).
3. Langdon, R.G. & Holman, V.P. *Biochem. biophys. Acta.* A**945** 23–42 (1988).
4. Cuppoletti, J., Jung, C.Y. & Green, F.A. *J. biol. Chem.* **256**, 1305–1306 (1981).
5. James, D.E., Strube, M. & Mueckler, M. *Nature* **338**, 83–87 (1989).
6. Gould, G.W. *et al. J. biol. Chem.* **264** 2180–2184 (1989).
7. Asano, T. *et al. J. biol. Chem.* **264**, 3416–3420 (1989).
8. Mueckler, M. *et al. Science* **229**, 941–945 (1985).
9. Kayano, T. *et al. J. biol. Chem.* **263**, 15245–15248 (1988).
10. Birnbaum, M.J., Haspel, H.C. & Rosen, O.M. *Proc. natn. Acad. Sci. U.S.A.* **83**, 5784–5788 (1986).
11. Thorens, B., Sarkar, H.K., Kaback, H.R. & Lodish, H. *Cell* **55**, 281–290 (1988).
12. White, M.K. & Weber, M.J. (unpublished data).

Multiple sclerosis and paramyxovirus

SIR—Vandvik and Norrby[1] used a different technique to the ones we described[2]. They show the presence of intrathecally synthesized antibodies to the simian paramyxovirus SV5 (or related viruses) in the cerebrospinal fluid (CSF) of some multiple sclerosis patients. Contrary to our observations, however, none of the CSF samples contains significant amounts of such antibodies.

We have carried out further studies (with the help of B. Souberbielle and others) on the CSF samples from an additional 23 patients. Of these, eight contained SV5 (or related) antibodies but none had significant amounts of such antibody in their oligoclonal immunoglobulin G bands as judged by the effect of virus adsorption. Further experiments have also suggested that the relatively cathodic oligoclonal immunoglobulin Gs are very sensitive to changes in phosphate ion concentration and in pH, and it may be that some of the adsorptions noted in the original series of experiments were due to non-immune reactions. Nevertheless, not all our results (Fig. 3, ref. 2, for example) can be explained in this way. We conclude that antibodies to SV5 or related viruses constitute a major component of the CSF in some multiple sclerosis patients but that this proportion is much smaller than was originally claimed[2].

In some of the CSF samples examined by Vandvik and Norrby the oligoclonal bands revealed by blotting onto SV5 antigen cross-react either with PF-2 or with mumps. Other studies[3] have shown that such cross-reactions are characteristic of only a small proportion of the total antigen repertoire of these viruses and are mostly confined to internal antigens. This observation would be consistent with a persistent paramyxovirus infection[4]. Alternatively, it could be indicative of the involvement of another uncharacterized paramyxovirus elaborating only cross-reacting antibodies, the other more specific ones not being detected.

W.C. RUSSELL
R.E. RANDALL

Department of Biochemistry and Microbiology,
University of St Andrews,
St Andrews,
Fife KY16 9AL, UK

K.K.A. GOSWAMI

Virology Section,
Department of Medical Microbiology,
University College and Middlesex Hospital Medical School,
London W1P 7LD, UK

1. Vandvik, B. & Norrby, E. *Nature* **338,** 769–771 (1989).
2. Goswami, K.K.A., Randall, R.E., Lange, L.S. & Russell, W.C. *Nature* **327,** 224–247 (1987).
3. Randall, R.E. & Young, D.F. *J. gen. Virol.* **69,** 2051–2060 (1988).
4. Randall, R.E. & Russell, W.C. in *'The Paramyxoviruses'* (ed. Kingsbury, D.) (Plenum, New York, in the press).

Validity of sexual selection in birds

SIR—Read and Harvey[1] suggest that a reanalysis of comparative data on the relationship between avian plumage, brightness and parasite intensities does not substantiate our[2] or Read's[3,4] previous findings that brighter species are more infected with haematozoa. I believe that the general finding of higher parasite prevalence in brighter, more sexually selected species remains valid.

First, I question the use of a two-tailed statistical test of the correlation. Read and Harvey[1] state, "we believe that the null hypothesis would have been rejected had a significant negative relationship been found". In view of our clear statement of our hypothesis about interspecies differences, I cannot imagine how a significant negative correlation could be construed as supporting it. Similar recognition of the one-sided nature of the hypothesis appears both in Read's analysis of haematozoa in North American and European passerines[3], and in his review of recent research in the field of parasites and sexual selection[4]. The point is a critical one, because the use of one-tailed tests renders nearly all of the across-species correlations using the scores of the six respondents used by Read and Harvey significant at the 0.05 level, and the mean correlation significant at $P < 0.05$ as well. (Given the extremely non-normal distribution of the scores, it might be argued that the mean is not the best summary statistic to use, and a modal score might be more appropriate, along with a non-parametric test.) The reanalysis of Read and Harvey[1] thus reveals differences in degree rather than kind. When these results are considered together with the finding for European birds[2] ($P = 0.002$) the overall case for an association remains very strong.

Nonetheless, the reported correlations between parasite prevalance and brightness, whether still significant or not, are generally lower than those calculated by us[2] or by Read[3]. In the re-ratings there may have been less emphasis on structural and display characters than I applied. Boat-tailed grackles, for example, received lower scores than common grackles from several of the respondents and none rated them higher (A. Read and P. Harvey, personal communication). I did rate them higher by one point; the common grackle is perhaps more iridescent but lacks the exaggerated tail which gives the former species its name. In ranking the original species[2], I gave species 'credit' for characters such as long tails or crests. This, together with unavoidable interest in forms of showiness that might reveal health may explain some of the discrepancies in scoring. In addition, my scores relied only partly on field guides and were constructed from personal experience of seeing virtually all the species in the wild. This perspective may give a more accurate version of the 'true' brightness of the species than merely using paintings or photographs from field-guides, which, as many birdwatchers know, can be misleading concerning the appearance of a bird under natural conditions. The real point is not showiness but the ability of females to detect health[5].

I am also puzzled by the rejection of Baker and Parker's[6] method for evaluating showiness. Use of this technique by Read[3] resulted in a highly significant positive correlation between brightness and parasite prevalence of 113 European passerine species. In Read and Harvey's reanalysis[1], however, the method of Baker and Parker is dismissed on the grounds that the originators of the method "had their own view concerning the evolution of bright colours". But the important point is not whether the individual scorer has any ideas about the evolution of bird coloration, because many people (including, perhaps, the six ornithologists used as scorers by Read and Harvey) have some notions on the matter, but whether the scoring was performed blind, with the scorer ignorant of any associations between the variable in question and the items being scored. All that one can strive for in the present case is that the scorers are ignorant of the parasite data, which was true both for Baker and Parker, as their paper preceded ours, and for our