characterising organisms whose mitotic mechanisms resemble those currently found only in primitive forms such as the hypermastigote flagellates, dinoflagellates and certain other protozoa[4]. Also, while there are minor modifications, the fundamental characteristics of mitosis in Bangiophycean and Florideophycean red algae appear to be the same (ref. 5 and our work in preparation with C. Bosco and J. Thomas). Furthermore, it has been suggested that an interrelationship exists between red algae and certain fungi[6,7]. The presence of morphologically similar, NE-associated SPBs in both groups, along with other common ultrastructural cell division features[4,8] may serve to strengthen this idea.

KATHLEEN L. SCHORNSTEIN
JOE SCOTT
Department of Biology,
College of William and Mary,
Williamsburg, Virginia 23185

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BRONCHART AND DEMOULIN REPLY—Schornstein and Scott have added some interesting observations to our data on mitosis in *Porphyridium*, and their interpretation of spindle pole body behaviour is especially welcome. Their main criticism of our paper seems to concern the use of the word 'unusual', a very subjective point. Our use of that word is due to the association of microbodies with the spindle poles, a fact confirmed by Schornstein and Scott. Neither unpublished reports of such a situation in some other algae nor speculation about its meaning change our opinion that our use of that adjective was legitimate. Our use of the word unusual did not result from a belief that either kinetochores or spindle pole bodies were absent.

Kubai[1] gives a lengthy table (Table 6) of organisms displaying microtubules terminating in chromatin without differentiation of kinetochores. We are aware that improved fixation and staining, or better luck in obtaining the right section, may eventually have revealed in those organisms, structures that might be described as kinetochores. Thus we did not stress the fact that we had not seen them in *Porphyridium*.

There is nothing unusual in the absence of a spindle pole body which is the rule among higher plants, but, as we said in our report, we observed and photographed (Fig. 2) such an organelle in *Porphyridium*. We spoke, however, of a microtubule-organising centre, for we were sure only of its presence in prophase and were not certain of its existence at the spindle poles in metaphase. We did not feel the small dark masses present at the poles in metaphase (our Fig. 1 and Fig. 1*c* of Schornstein and Scott) could qualify as a 'well defined pole body'. It is to the merit of Schornstein and Scott to have shown their constancy and relation to part of the prophase organelles. Those observations however do not make mitosis in *Porphyridium* closer to that in *Membranoptera*[2], because except for a qualification of the kinetochore situation (kinetochores are well defined in *Membranoptera* and inconspicuous in *Porphyridium*) every difference we noted remains true (presence of microbodies; lack of polar ring, well condensed chromatin and nuclear envelope doubling by endoplasmic reticulum). As to the relationship to fungi, there is certainly some similarity to the situation in Basidiomycetes. We already had that feeling in 1977 but did not stress it for fear of being accused of stretching the evidence in favour of a red algal ancestry for higher fungi, to which one of us is devoted.

R. BRONCHART
V. DEMOULIN
Département de Botanique,
Université de Liège,
Sart Tilman,
B-4000 Liège, Belgium

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Cancers and the immune system

EXCEPTIONALLY high levels of antibodies against the Epstein–Barr virus have been found in the blood of children who later developed African lymphomas[1], and this finding is widely regarded as evidence of a viral origin for these unusual tumours[2]. However, the tumours are little more than exceptionally localised (and easily cured) lymphatic neoplasms, and they are all but confined to areas of holoendemic malaria where the diffuse disease (lymphatic leukaemia) is conspicuous by its absence. In these same regions, over 50% of childhood cases of myeloid leukaemia have localised collections of myeloid cells (chloromas[3]) as the presenting symptom. Therefore, we could be witnessing the effects of general constraints on cancers of lymphatic and haematopoetic tissues in regions where a combination of high infection risks and low standards of nutrition makes the acquisition of exceptionally high levels of immunological competence during infancy a condition of further survival.

This hypothesis assumes that although the immune system may have more difficulty in recognising quasi-foreign cells (for example, mutants) than wholly foreign cells (microorganisms), nevertheless, it is playing essentially the same role in infective and neoplastic diseases. Therefore, an appropriate test would be one which compared the illnesses and immune reactions of European or American children with lymphatic leukaemia and more localised lymphatic neoplasms such as lymphosarcoma and Hodgkin's disease.

A. M. SETWART
G. W. KNEALE
Regional Cancer Registry,
Queen Elizabeth Medical Centre,
Birmingham, UK

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DE THÉ AND GESER REPLY—The comments of Stewart and Kneale on the contrast between the preponderance of localised lymphomas in tropical climates versus diffuse lymphoid malignancies in temperate climates are interesting. The prospective study carried out from 1972 to 1978 in Uganda showed that up to 4 years before Burkitt's tumour development, the future cases were marked by high antibody titres against Epstein–Barr virus (EBV)-specified viral capsid antigens, when compared with the general child population of the same age, sex and locality. The study further showed that other herpes virus antibody levels were not elevated in the Burkitt's lymphoma (BL) candidates and we consider that this specific stimulation of EBV antibody production contradicts the view implied by Stewart and Kneale that BL is caused by an exceptionally high level of immunological competence during infancy, rather than by a specific EBV-related event. As discussed elsewhere[1], Burkitt's lymphoma develops, as do most cancers, as the result of a multi-stage process, each step having specific causes. That high levels of immunological stress could promote the development of localised lymphatic neoplasms (BL) rather than leukaemias as seen in temperate climates, is an old and conceivable hypothesis, which does not contradict the involvement of a specific initiating viral event, as proposed for BL.

We agree with Stewart and Kneale that a comparative epidemiological, clinical and immunological study of lymphatic leukaemias versus localised lymphosarcomas, in temperate and tropical regions, would answer the question of whether children with localised lymphomas are more immunologically competent than those with diffuse lymphoid malignancies.

GUY DE-TÉ
CNRS, Faculté de Médecine,
Alexis Carrl, Lyon
and CNRS, Cancer Institute,
Villejuif, France

ANTON GESER
International Agency for
Research on Cancer, Lyon,
France

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