arthritis involving *I. belli* and *Cryptosporidium* infestation in patients with AIDS are not yet well known. Further studies should help clarify these questions. We are not aware of any previous report of reactive arthritis after enteric infection due to *I. belli* and we believe this to be the first such report.

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AUTHORS’ REPLY We are pleased that the first article in *LESSON OF THE MONTH* has attracted interest and correspondence.

In reply to Dr Edgar et al, we feel that the use of Bence-Jones protein (*BJP*) as a descriptive term for urinary monoclonal light chain is in such standard usage as to be fully acceptable; few clinicians or laboratory scientists would be in any doubt as to the meaning of this term.

The agarose gel electrophoretic assay used for the detection of urinary *BJP* in our laboratory has a sensitivity of 0–08 g/l, which after concentration of urine ×200 gives a lower limit of detection of approximately 0–001 g/l of monoclonal protein (*Sheldon J*, personal communication). In this case *BJP* was not quantified but was expressed as two faint bands of kappa protein at initial testing. The laboratory routinely expresses *BJP* calculated as a percentage of total urinary protein.

It is our experience, and that of our colleagues in the laboratory, that the term "benign" can be applied to the presence of a monoclonal protein in persons with no evidence of myeloma, Waldenstroms macroglobulinaemia, amyloidosis or other related B cell malignancy. We suggest that the term can only be applied once the condition is shown to be stable with time – five years for IgG and IgA and 10 years for IgM paraprotein. An alternative term monoclonal gamopathy of unknown significance (MGU) is better used when any doubt exists.

We agree with Dr Edgar et al, and hope that *LESSON OF THE MONTH* highlights the need to follow up the findings of even a faint band of *BJP* with serial *BJP* measurements. It was this omission which led to the difficulties encountered in this case. However, we appreciate the concern expressed by Dr Edgar that early bone marrow examination be undertaken if any *BJP* is detected and there is general agreement that this decision should be based on clinical judgement. Most clinicians would find it impossible, for reasons of resource limitation and clinical acceptability, to perform bone marrow examination on every patient with any detectable *BJP*, although this is a moot point. Certainly levels of >0–01 mg/l are more suggestive of malignancy and should be investigated with bone marrow examination. As far as other investigations are concerned plain radiographs are generally regarded as a more sensitive indicator of occult myeloma than other investigations, including early bone marrow examination and radioisotope scanning of bone. Only when negative results from these investigations are available would it be advisable to monitor the patient over time remembering that a repeat bone marrow examination is always an option at a later date.

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**Antiphospholipid antibodies (aPL) in systemic lupus erythematosus. Are they specific tools for the diagnosis of aPL syndrome?**

We read with interest the paper by Ghirardello et al on antiphospholipid antibodies (aPL) in systemic lupus erythematosus (SLE) but would suggest that their conclusion, "lupus anticoagulant (LA) but not anticardiolipin antibody (aCL) positivity is a specific tool for the diagnosis of thrombotic complications... in SLE", is interpreted with caution.

There are a number of methodological problems in setting up a study of this kind which should be highlighted:

1) This study was retrospective and it cannot be assumed that because aPL was present at the time of study, they were also aPL at the time of diagnosis of SLE. In fact, the authors do not specifically state in reference to the 47 patients who had experienced pregnancy, whether they were diagnosed as having SLE at that time. It is therefore likely that the recording of aPL complications using
this approach will be inaccurate. Ideally, a prospective study is needed with the enrolment of SLE patients at the time of aPL detection and close observation over a long follow-up time. 2) The lack of standardisation of both lupus anticoagulant and anti-cardiolipin tests continues to be a major problem in interpreting clinical data published on antiphospholipid antibodies. However, better interlaboratory agreement was achieved when IgG and IgM aCLs were recorded in a semi-quantitative fashion (that is, negative, or low, medium, high positive). Ghirardello et al do not specifically comment on such standardisation for their own laboratory. 3) The ELISA assay for aCL is clearly a more sensitive assay for detecting aPLs than the Russell viper venom test. Interestingly, in this study LA positivity was only found in those patients who were aCL positive. Furthermore, a significant association between high titres of IgG aCL and arterial thrombosis suggests that the aCL titre may be more important than the presence of aCL in SLE patients. Previous reports have suggested that aCL, rather than LA, is a better predictor of fatal death in SLE pregnancy, and that high titre aCL carries a worse prognosis. In this regard, Ghirardello et al have not studied aPL complications at different titres of aCL. Furthermore, patients in ‘risk pregnancies’ were treated with aspirin which may influence the frequency of complications observed.

In summary, larger prospective studies of SLE patients with aPL, better standardisation of aPL assays, and analysis of differing times of occurrence in respect of aPL complications are needed.

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Authors’ Reply Dr Hopkinson et al suggest caution in interpreting the conclusion of our study regarding the specificity of anti-phospholipid antibodies (aPL) for aPL complications in systemic lupus erythematosus (SLE). However, it seems they did not take into account the second part of our conclusion; that is, “the measurement of IgG anti-cardiolipin antibody (aCL) level seems to be of considerable clinical value to aCL positivity”. We believe therefore that our conclusion summarised our results regarding LA and both aCL positivity and level.

Dr Hopkins also comments adversely about some of the methodological approaches we used. 1) Our study was a cross-sectional rather than a retrospective study. In fact, individuals were considered ‘ideal’ for this purpose, some ethical considerations should be taken into account by authors who are interested in research as well as in patient care. In performing such studies, we consider there may be some anamnestic manifestations which occurred after review diagnosis to define individuals as ‘SLE with aPL complications’ and to be able to evaluate possible relationships between aPL and some low prevalence aPL complications. Such a study design, which has been widely used to investigate the clinical significance of aPL, has both well known advantages and disadvantages. As Dr Hopkins and colleagues pointed out, we recorded all patients that elapsed between the occurrence of anamnestic manifestations and the aPL determination may be a bias in our study. However, the relevance of this bias depends on some variables, particularly, on the total amount of anamnestic data recorded and the length of time elapsed from their occurrence and the aPL determination.

Although the prospective study may be considered ‘ideal’ for this purpose, some ethical considerations should be taken into account by authors who are interested in research as well as in patient care. In performing such studies, we consider there may be some anamnestic manifestations which occurred after review diagnosis to define individuals as ‘SLE with aPL complications’ and to be able to evaluate possible relationships between aPL and some low prevalence aPL complications. Such a study design, which has been widely used to investigate the clinical significance of aPL, has both well known advantages and disadvantages. As Dr Hopkins and colleagues pointed out, we recorded all patients that elapsed between the occurrence of anamnestic manifestations and the aPL determination may be a bias in our study. However, the relevance of this bias depends on some variables, particularly, on the total amount of anamnestic data recorded and the length of time elapsed from their occurrence and the aPL determination.

Antiphospholipid antibodies (aPL) in systemic lupus erythematosus. Are they specific tools for the diagnosis of aPL syndrome?

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