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COMMENT ON

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Diagnostic value of inflammatory markers

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I read this paper with interest and, having previously published a similar study in the same journal (unreferenced in the current paper),¹ would be grateful for the opportunity to offer comment.

The conclusions of the authors relate to the diagnostic value of the inflammatory markers in the diagnosis of acute appendicitis and to the value of the markers in distinguishing acute uncomplicated from complicated appendicitis. To address the latter point first. In the current paper, a total of 20 patients were included in the analysis of uncomplicated versus complicated appendicitis. In my study, 47 patients were analysed for C-reactive protein (CRP) and 49 for white blood cell count (WBC) with the conclusion that WBC does not distinguish the subtypes and that elevated CRP is suggestive, but not diagnostic, of abscess. It would seem curious to present this as an original finding without reference to my previous similar conclusion based on a larger data set.

Regarding the perhaps more important diagnostic value of the tests, I have sought to explain why the ability of the WBC and CRP to exclude appendicitis in the current study is so much more convincing than in my previous paper. I have recalculated my diagnostic values based on a reference range of ≤ 10 as normal for CRP with the following results: sensitivity 79%, specificity 38%, positive predictive value 79% and negative predictive value 38%. For a WBC of ≤ 11 , the values are 78%, 81%, 89% and 50%. These still differ considerably from those of Sengupta *et al.* The difference must, therefore, lie in the populations under study. Clearly, if the cohort present with non-specific abdominal pain with (in the algorithm of the authors) 'the diagnosis of appendicitis ... considered unlikely,

and no other obvious diagnosis of concern ... being considered', then the diagnostic yield is much more likely to favour a high negative diagnostic value for the inflammatory markers as the prevalence is much lower. This is particularly critical when considering predictive values. I can only assume that it is a sampling variant which explains the complete exclusion of appendicitis in the current study based on normal WBC and CRP combined.

This brings me to a central criticism of the key conclusion of the paper. If the aim is to avoid unnecessary hospital investigations, then it is counter-intuitive in a population with such a low prevalence of surgical disease requiring admission and surgery to base your assessment in large part on laboratory tests – are the authors proposing we treat patients, laboratory results or a hybrid? Surely, a more useful conclusion would be that in the assessment of non-specific abdominal pain there is little value in requesting blood tests if your clinical suspicion is that there is no significant pathology. In order to answer this more fully, the authors should present the number of patients with no clinical stigmata of significant disease who were admitted on the basis of an elevated WBC and/or CRP, and the proportion of these who subsequently required surgical or medical management within the hospital. If, as I suspect, the number is low, then there is no argument for blood testing in this group.

Reference

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AUTHORS' RESPONSE

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In answer to Birchley's comments, his study,¹ where all patients underwent appendicectomy, and ours, where all-comers with lower abdominal pain were included, are clearly representative of different populations and, therefore, the results cannot be compared. Furthermore, recalculating predictive values in Birchley's study population using a reference range from the Edinburgh laboratory is unlikely to provide useful information.