The importance of pre-operative axillary ultrasound and intra-operative sentinel lymph node frozen section analysis in patients with early breast cancer – a 3-year study

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COMMENT ON

Sanders MM, Waheed S, Joshi S, Pogson C, Ebbs SR. The importance of pre-operative axillary ultrasound and intra-operative sentinel lymph node frozen section analysis in patients with early breast cancer – a 3-year study. *Ann R Coll Surg Engl* 2011; **93**: 103–105
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There are several things in this communication that cause me deep concern. In Figure 1, it claims that there were 583 breast cancers newly diagnosed during the study period. Out of these, 272 were deemed to be advanced breast cancer or unsuitable for surgery. This is hugely outside any previous publication in this country or for any other country. In general terms, we would have no more than 5% of patients presenting with disease unsuitable for surgical intervention. This invalidates anything that has been said further in this paper.

Furthermore, while the authors are keen to say they are exploring NICE guidelines, they themselves are not adhering to these guidelines. First, NICE explicitly recommends that if sentinel node biopsy is carried out it should be carried out using the triple technique of scintigram, gamma probe and blue dye rather than blue dye alone. Using just blue dye is inaccurate and should not be recommended.

Furthermore, NICE guidance also states that frozen section is not recommended. There are indeed many publications in the literature that show that frozen section of the sentinel node has utility; in other words, when it is positive it is accurate on the whole. However, it has a significant high false-negative rate and therefore has been superseded by other methods such as preliminary change reaction molecular analysis.

Clopidogrel: is a surgical delay necessary in fractured neck of femur?

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COMMENT ON

Nwachuku IC, Jones M; Clough TM. Clopidogrel: is a surgical delay necessary in fractured neck of femur? *Ann R Coll Surg Engl* 2011; **93**: 310–313
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I read with interest this paper that addresses a common orthopaedic dilemma regarding timing of hip fracture surgery in patients on antiplatelet therapy. Clarification of a few points may help myself and other readers.

First, data for type of operation were recorded but not presented. Many would argue that there is significant variation in blood loss among operation types.

Second, the data between Tables 1 and 2 appear incongruous. I would expect the totals for complications and units of blood transfused to be the same in both tables as they are the same patient cohort.

Third, the method of blood loss estimation using pre/post-operative haemoglobin levels has been identified correctly as a limitation of the study. When comparing early versus late surgery, a statistically significant difference in blood loss is described but, given that Table 1 shows the same transfusion rate between groups, is this difference clinically significant?

Last, patients on clopidogrel and dipyridamole only were described. Out of a total of 619 neck of femur fractures, one would expect a large number to be on long-term aspirin. Comparison between matched groups of aspirin and other antiplatelet drugs would have been interesting but most informative of all would be the impact of dual antiplatelet therapy.

Author's response

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I would first like to thank you for your interest in our paper and am very pleased you have taken the time to offer critique and feedback. I aim to respond accurately to each of the various points in turn.

First point: *Data for type of operation were recorded but not presented.*

Yes, the data for type of operation were not presented in an attempt to keep the article word count down but I do concede that the type of surgery is an important factor in ascertaining why a certain cohort would lose more blood. I have been through my data again to give you these details.

The extracapsular fracture group was defined as those patients who sustained an extracapsular fracture and underwent either a dynamic hip screw (DHS) fixation or intramedullary nailing (long gamma nail or long proximal femoral nail). Of the 16 patients in that cohort, 14 were treated with DHS fixation, 1 with a proximal femoral nail and 1 with a gamma nail. The intracapsular cohort included any patient with an intracapsular fracture who underwent either a hemiarthroplasty, a total hip replacement or a cannulated screw procedure. The numbers were 10 hemiarthroplasties, 3 total hip replacements and 2 cannulated screws procedures, totalling 15 for that group. Some would argue the cannulated screws have reduced the blood loss rate but at 2 out of 15 I think this is not the case and this will not affect our results.

Second point: *Data between Tables 1 and 2 appear incongruous.*

You are correct in mentioning the incongruence between the transfusion rate results of Tables 1 and 2. I have meticulously revisited the data and found there has been some confusion on my part regarding the number of patients receiving a transfusion and the number of units being transfused. The table should read:

	Total units of blood transfused	Total patients transfused	p-value
Early surgery	16	6	0.9180
Late surgery	15	5	

	Total units of blood transfused	Total patients transfused	p-value
Extracapsular	20	7	0.3689
Intracapsular	11	4	

As you can see, the results are statistically insignificant. Nevertheless, we now have more blood units transfused to the extracapsular group, which further confirms our argument of the extracapsular patients being more at risk of blood loss.

Third point: *Is the difference in blood loss between the early surgery and late surgery groups clinically significant?*

I would agree with you that there is a discrepancy between statistically significant data and clinically significant data. One would comment that the effect size (if calculated) between the early and late surgical groups should also be

small. However, when comparing blood transfusion rates we must be mindful of many alternative factors governing whether a patient receives a blood transfusion on the ward. This variance is a reason why we did not use the rates of transfusion as a contributing indicator for blood lost. The fact that the blood lost between the early and late groups appears clinically insignificant fits in nicely with our final conclusions that a surgical delay before treating extracapsular fracture patients on clopidogrel should be allowed.

Last point: *Comparison between matched groups of aspirin and other antiplatelet drugs would have been interesting but most informative of all would be the impact of dual antiplatelet therapy.*

Aspirin is an interesting issue. In our trust, patients on aspirin were all treated the same way (with early surgery) and formulating comparison groups was therefore not possible. We attempted grouping our data to see if we could incorporate another cohort group involving aspirin and clopidogrel, or different combinations, or dipyridamole, clopidogrel and aspirin. However, the groups generated were far too small to warrant any significance so this was left out of the analysis.

I hope this has been of help.

The value of hyperbilirubinaemia in the diagnosis of acute appendicitis

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COMMENT ON

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We read with interest the article by Emmanuel *et al* investigating the usefulness of hyperbilirubinaemia in the diagnosis of acute appendicitis. The pilot results of our research show similar findings but we have studied a greater number of patients ($n=975$), of which 892 patients (91.5%) had appendicitis. Bilirubin was raised in slightly fewer patients with acute appendicitis (22.4%) and in those with a non-inflamed appendix (5.26%) ($p<0.001$) than was shown by Emmanuel *et al*. The odds ratio of our work was 4.25 compared with 3.25 in their study. The specificity and sensitivity in our study were 95% and 99.4% respectively. We also found that a perforation had increased bilirubin levels ($p<0.001$) compared with non-perforated appendicitis, with a specificity of 88.3% of bilirubin $>21\mu\text{mol/L}$.

Ultimately, our results correlate very well with those of Emmanuel *et al* and this contributes further weight to the use of bilirubin as a marker for acute appendicitis and the need to use this test in the assessment of acute appendicitis.