Comments on *Synchrotron fibre diffraction identifies and locates foetal collagenous breast tissue associated with breast carcinoma* by V. J. James (2002). *J. Synchrotron Rad.* 9, 71–76

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We read with great interest the paper recently published by James (2002), as the work is in a similar field to our own. Upon careful reading we have developed some significant misgivings and concerns regarding many aspects of this particular paper and we felt compelled to bring these to your attention.

(1) It appears as though the author has confused *breast tissue* with *skin tissue* of the breast. These are completely different organs containing different structures. Furthermore, hyalinized tissue is common in breast tissues and is certainly not uniquely associated with cancer. The current consensus is that the hyalinized tissue is a response to inflammation following DCIS and not, as suggested, a pre-cancerous change. Within the UK, at least, hyalinized tissue is certainly not referred to as elastosis.

(2) We do not believe it is possible to distinguish, by eye, normal tissue from diseased tissue as is claimed in §2.

(3) The normal breast tissue data shown in Figs. 2(*b*) and 2(*c*) are absolutely identical to that previously presented as foetal tendon and skin tissue in James *et al.* (1998) (Figs. 3*a* and 3*b*). We are therefore confused regarding which tissue was really measured and why no reference is made to the original (1998) data source.

(4) There are omissions of several key experimental details that prevent any repetition of the work. These include the X-ray wavelength that was employed, the physical dimensions of the X-ray beam, the volume/mass of specimen that was experimentally interrogated and the actual method used for determining peak positions and intensities. Further, it is wholly inappropriate, in this case, to determine peak positions from second-order derivatives as the background is rapidly varying. Background fitting with a boxcar filter is also not appropriate as this would result in a non-physical average, which following subtraction will result in negative X-ray fluxes.

(5) Many of the features that are referred to within the text as being clearly apparent within Fig. 3 are not clear. No labels are included in Fig. 3 to aid recognition. This results in a great deal of ambiguity when trying to relate the features described in the text to those in the images. A clear understanding of the features is critical to the interpretation of the data. Specifically:

(i) The author claims that all samples show 20 orders of the 353 nm spacing in the equatorial region. These are simply not evident in any of the images presented in Fig. 3.

(ii) We are unable to find any diffraction rings that index to 32 nm or 43.8 nm shown in any of the images of Fig. 3.

The text claims that Figs. 3(*b*)–3(*d*) show no fat. There are clear rings due to fat in Figs. 3(*c*) and 3(*d*).

Fig. 3(*a*) does not appear to contain a 'wide diffuse ring' despite the claim in the text.

(6) A reference is made to the work of Lewis *et al.* (2000) as supporting the observation of a variation in collagen fibril radius. This claim is erroneous, as the Lewis paper contains no mention of variation in fibril radius.

(7) The 'fine rings appearing in all pathological tissue' are claimed to be randomly oriented structures. None of the images presented in Fig. 3 indicate scattering from randomly oriented structures other than that of the fat.

A major conclusion of the work is that the findings show structural changes preceding breast cancer. We do not believe that this can be supported by any of the data presented within the paper.

We regret having to bring these points to your attention but we have been frustrated and surprised at the number and extent of our concerns. At best, one may regard some of the problems as arising from poor presentation. However, this does not explain many of the points above.

References

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