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LETTERS TO THE EDITOR

Reporting partial screening results: is it confusing and unsatisfactory?

In their recent paper, Hackshaw and Wald (2001) suggest that reporting partial risk estimates in screening programmes for Down syndrome using the combined or integrated tests is not worthwhile. This conclusion is based on their estimate that only 0.05–0.1% of women would be expected to have such a high risk based on nuchal translucency and age alone and that they would remain screen-positive regardless of the values of first or second-trimester biochemical markers and that partial reporting of such high-risk results would be 'confusing and unsatisfactory'. Confusing and unsatisfactory for whom?

In 1998 we commenced an ongoing prospective comparative study of first and second-trimesters markers for Down syndrome in a multi-racial Australian population (The FaST Study). Women participating in the study give informed written consent and are aware that they will not receive the results of *any* of their individual screening tests until they have completed all of the tests (i.e., after second-trimester maternal serum screening). High-risk first-trimester results are not reported until completion of second-trimester serum screening and the availability of combined (first and second trimester) risk estimates. An important exception to this is if the nuchal translucency measurement is greater than 6mm. In this case the woman is informed immediately and withdrawn from the study. While this NT threshold may now seem excessively large, the study was designed prior to the establishment of a local normal range for NT measurement and prior to an appreciation of the relationships between NT and biochemical markers. Nonetheless, of the approximately 8500 women screened to date, five (0.06%) women have had such an NT measurement and were offered immediate diagnosis. Contrary to what Hackshaw and Wald suggest, the specific arrangements for these very small numbers of women were not problematic for our screening programme and were certainly not confusing. We would anticipate that similar arrangements for women with partial high-risk estimates ($> 1:2$, for example) would also be easy to administer.

As for being unsatisfactory, it is known that women who choose to have prenatal screening for Down syndrome prefer to have as early screening as possible, irrespective of the likelihood that earlier testing will detect pregnancies destined to miscarry (Kornman *et al.*, 1997; Mulvey and Wallace, 2000). In both of these studies the most common reasons given for the preference for earlier screening were the easier termination of an affected pregnancy and the shorter period of uncertainty, the same reasons given by those women who prefer CVS to amniocentesis (Spencer and Cox, 1987). Therefore, while there are no published data specifically addressing the preferences of women regarding partial risk reporting, what evidence does exist suggests that women would desire such reporting if subsequent components of a combined screening package could never return a low-risk result for them. We believe that withholding such results would be unsatisfactory. Instead, we would suggest that, for the individual woman, the reporting of partial risk estimates in these special cases would not only be best practice but would be found to be most satisfactory by women — if we only asked them.

Finally, in their paper Hackshaw and Wald (2001) state that there are no published data on the correlation between inhibin A and first-trimester markers for Down syndrome. We were the first to report that there is no correlation between nuchal translucency and the three most useful second-trimester markers AFP, fβhCG and inhibin A, and refer them to those data (Wallace and Mulvey, 1999).

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Reply to Mulvey and Wallace

Mulvey and Wallace suggest that a policy of reporting results on women with exceptionally high risks based on nuchal translucency and maternal age would be acceptable. They base this on the experience from their

research project in which all women have first and second-trimester screening tests performed after those with high NTs (>6 mm) are withdrawn. While this was not a problem in their research protocol, it is not the same as the situation described in our paper, which involves offering the 'combined' or 'integrated' test, and computing a preliminary risk based on nuchal translucency and maternal age; doing so could reduce the benefits of these screening tests.

A routine screening policy which, as a first step, reports partial results if the risk estimates are very high (say >1 in 10) may be acceptable if *only* women with such high risks were informed. However, once a risk estimate has been computed for all women the temptation to issue a partial report with a risk estimate that was not quite so high would probably be too great to resist, and this could compromise screening performance of the full test offered.

A policy of reporting partial screening results would be a confusing policy because there are likely to be some women with partial results who would go on to have the completed screening test leading to conflicting risk estimates in the same pregnancy. This would make the clinician's decision to offer and the woman's decision to accept a diagnostic test more difficult than if a single (and more accurate) risk estimate were reported. This would be unsatisfactory. It would also be unsatisfactory because it would be inefficient. Women who have partial screening results with, for example, risk estimates of 1 in 30, may choose to have a diagnostic test immediately, regardless of the intended screening policy. They would then not obtain the benefit from having the full 'combined' or 'integrated' screening test, which in many cases would correctly yield screen-negative results and so avoid an unnecessary invasive procedure.

We thank Mulvey and Wallace for bringing their published letter on the correlation between inhibin-A and nuchal translucency to our attention.

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Poland sequence and hyperhomocyst(e)inaemia

We have recently observed a child with Poland sequence (unilateral symbrachydactyly and ipsilateral aplasia of the sternal head of the pectoralis major muscle), whose mother was afflicted with moderate hyperhomocyst(e)inaemia. The hyperhomocyst(e)inaemia is an identified factor of thrombophilia, and in the pregnant woman is an admitted risk factor for several birth defects, such as neural tube defects (Graf *et al.*, 1996), microcephaly and congenital heart diseases (Andersson *et al.*, 1999).

Poland sequence (prevalence of about 1 in 20 000 newborns) is caused by subclavian artery disruptive events, which could also be related to thrombotic phenomena. Untill now, no cases of Poland sequence in newborns of hyperhomocyst(e)inaemic mothers have been reported in the literature. We think it is worth studying whether hyperhomocyst(e)inaemia or other thrombophilic states play a significant pathogenetic role in such a sequence.

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Microcephaly with dysgenesis of corpus callosum and colpocephaly in the survivor after the first-trimester death of a monochorionic co-twin

This was the first pregnancy of a 30-year-old healthy Chinese woman. She and her husband were non-consanguineous. There was no family history of diabetes mellitus or congenital malformations. She