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Reply to Lindqvist *et al*. 'Does parental age difference affect offspring count in humans: comment on Fieder and Huber'

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Invited reply

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Obviously the earlier the onset of reproduction, the higher the offspring count, other things being equal. This was not the hypothesis of our paper; we were interested in the effect of parental age difference *per se*, for which we found an optimum value confirmed between estimates from independent samples of mostly (more than 96%) post-reproductive contemporary Swedes (we used actual reproduction partners instead of marital partners as the latter might blur results via premarital offspring, divorce, etc). To take issue with our estimate of this optimum spousal age difference, we believe, critics need to turn to a procedure that permits some other estimate of an optimal age difference. Lindqvist *et al.* (2008) have not done so.

Whereas the model that we fitted included the standard quadratic term in age difference (notated as x_1^2 in our letter, x_i^2 in their comment), thus making it possible to examine the data in order to locate an optimum age difference, Lindqvist *et al.* (2008) substitute a term that changes its sign whenever the wife is older than her husband. In this set-up, the two terms involving age difference have become highly confounded by design, rendering the estimates of their separate coefficients unstable and even the relative sign of these. For instance, in regression (2) of Lindqvist *et al.* (2008), there is a local optimum of age difference at +10.3 years, which certainly contradicts our data in figure 2; and in column (4), the same regression with one additional predictor, the net age difference component of the regression function has no maximum anywhere, but declines monotonically over its entire range. It has thus become impossible to talk about any optimum value for the male–female age difference, and so the regressions in the comment seem quite irrelevant to our original letter.

We are told that the results in column (2) of table 1 of the comment 'closely match' those of our letter. However, the women of the Umea study (Lindqvist *et al.* 2008) averaged 6.20 children (26 560/4285), close to the constant term in regressions (1) and (2). But in our study (figures 1 and 2), the highest lifetime mean number of children per marital age difference class never exceeded 2.4; we clearly live in a different demographic regime from that of the seventeenth through nineteenth centuries. Given that the regression

The accompanying comment can be viewed on page 78 or at http://dx.doi.org/doi:10.1098/2007.0514.

Table 1. Results of the quadratic regression of the partner's age in year 2003, age difference in months between partners, age difference squared, and woman's age at first birth on offspring count. (*$p<0.05$; **$p<0.01$; ***$p<0.001$.)

	offspring count	
	men	women
n	4852	5222
adjusted R^2	0.081	0.054
constant	4.5204***	4.1841***
age of partner 2003	−0.0222***	−0.0196***
age of partner/woman at first birth	−0.0459***	−0.0387***
age difference	−0.0128*	0.0147*
age difference square	−0.0012**	−0.001**

in their column (2) reverses the sign of the quadratic term for all women older than their husbands, the agreement they claim with our own results is surely accidental, and does not justify any argument that the result in their column (4), incorporating the additional predictor of woman's reproductive value, should be expected to be any sort of guide to the effect of adding a similar variable in our regression.

And that is indeed the outcome here. When we add an analogous variable (age at first birth) to the regressions we reported in figures 1 and 2 and the accompanying text, the optima we compute are essentially unchanged. The reader is referred to our new table 1, where woman's age at first birth—a better measure of reproductive value than 'woman's age at marriage' (cf. Low & Clarke's (1991) finding for nineteenth-century Sweden! that age-specific fertility is not closely linked to age at marriage), is added to the regression we reported earlier involving age difference, age difference squared and partner's age in 2003. Sample sizes decreased, owing to some missing data, but all coefficients remain significant and unchanged in sign. It is unwise to go further with the addition of linear terms to so subtle a curvilinear regression as what we showed in our earlier figures 1 and 2.

It is fair to conclude that while the inclusion of the term 'sign(x_i)' in the regressions reported by Lindqvist *et al* (2008). destroys the possibility of computing an optimal age difference, the potential confound of reproductive value that they put forward does not have anything like the claimed effect when the regression for convex-upward effect of age difference is specified correctly.

There is nothing 'more parsimonious' about a regression that destroys the very concept it was aimed to examine, namely, the estimation of an optimal age difference. Our critics do not comment on the strong resemblance between our regressions for men and for women (they cannot, as they have no corresponding dataset for males) and do not comment on the probable difference in the validity of offspring count for estimating fitness in an era when infant mortality was so high that an average of six children per married women corresponded to a population rate of increase over the last 94 years of their study (1749–1843) of only 83% (data from http://www.scb.se/templates/tableorchart____26047.asp, accessed 11/5/2007). Even if Lindqvist *et al.*'s (2008) regressions had been sound, which they are not, the

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relevance to evolutionary biology seems to have gone missing. We stand by the contents of our original letter, and we are further gratified by the stability of the results in our letter against challenge by the additional measure of reproductive value.

Addendum

In our reply to Lindqvist *et al.*, we argued that the transformation of the sign of one of the independent variables in the regression model (the quadratic term) as calculated by Lindqvist and his co-authors cannot lead to a maximum, but to a monotonic decline over its entire range. We have learnt that Lindqvist *et al.* revised the original version of their comment in the meantime, correcting that sign transformation. As we did not receive the revised version of the comment, however, our reply referred to the regression presented in the original version of their comment.

Irrespective of the actual version of the Lindqvist *et al*. comment, even after including age at first birth (a better measure of reproductive value than age at marriage, see reply) in our regression model our results remained essentially unchanged, confirming the validity of our arguments.

Editorial note

Biology Letters apologises that Fieder and Huber were sent only the original and not final version of the comment on their paper. In the final version the sign correction had been made by Lindqvist *et al.*, who did not realise that the earlier version had been sent to Fieder and Huber for a reply.

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NOTICE OF CORRECTION

The invited reply is now presented in the correct form.

19 December 2007