

## Nutrition Discussion Forum

**The energy cost of protein turnover is arbitrarily distributed between maintenance requirements and protein retention efficiency – response by Roux**

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In my investigation of the problem of the allocation of the cost of body protein resynthesis, I found in all the examples that I was able to examine that conventional multiple regression efficiency satisfactorily approximates turnover-related retention efficiency (defined in Roux<sup>(1)</sup>). I therefore developed the theory in Roux<sup>(1)</sup> from an axiom on the equality of these efficiencies. I took care to justify the use of this axiom empirically by a comparison between two different estimates of the cost of protein resynthesis from (protein breakdown)/6, in MJ/d. An estimate of (protein breakdown)/6 derived from the most reliable direct isotopic estimates of protein synthesis minus protein retention was compared with an indirect estimate of (protein breakdown)/6 obtained from the average regression protein efficiency estimate of twenty-two pig experiments collated from the literature. This was formally done in Roux<sup>(1)</sup>, by the comparison between the estimates contained in equations (11) and (12), and shows agreement between the direct and indirect estimates of protein resynthesis cost within the bounds of estimation error.

My formulation<sup>(1)</sup> should, furthermore, be understood in conjunction with two previous contributions<sup>(2,3)</sup> in which I proved that whole-body turnover-related protein retention efficiency ( $k_p$ ) can be approximated by:

$$k_p = (1 + (1 - (P/\alpha)^{(2/9)Q})^{-1}/6)^{-1}, \quad (1)$$

with  $\alpha$  the limit value of whole-body protein ( $P$ ) at maturity, so that the ratio  $P/\alpha$  represents the degree of maturity.  $Q$  is the proportion of nuclei active in cell growth or division, which can be represented by a scaled transformation of actual intake in terms of maintenance and *ad libitum* intake. Five worked examples from the literature show<sup>(2)</sup> that conventional multiple regression protein retention efficiencies satisfactorily approximate the turnover-related retention efficiency of equation (1), calculated at a given level of intake for the  $P/\alpha$  midpoint of the protein mass interval covered by the regression estimates.

It therefore follows from both Roux<sup>(1)</sup> and Roux<sup>(2)</sup> that the variability in regression estimates of protein efficiency is not due to an arbitrary distribution of the energy cost of protein turnover, but instead is, except for experimental error, attributable to the complete incorporation of the cost of protein resynthesis in the regression estimate. Consequently the variability of regression estimates is, like the turnover-related efficiency of equation (1), due to changes in protein maturity ( $P/\alpha$ ) with age and level of food intake quantified in terms of its effect on the proportion of active nuclei ( $Q$ ).

Problems associated with the variability of the regression coefficient can be avoided by the use of the theoretical efficiency of protein synthesis. The cost of protein resynthesis must then be included in maintenance. I showed that maintenance estimated from fasting heat production may be used to correctly include the cost of protein resynthesis<sup>(1)</sup>. Otherwise, maintenance can be estimated by subtraction from experimental food intake by employing the theoretical efficiencies of protein and fat synthesis<sup>(1)</sup>, in a process analogous to the estimation of the intercept in regression analysis from estimates of the regression coefficients.

For ease in presentation and biological understanding I inadvertently used the same symbol ( $k_p$ ) in the context of both turnover incorporation and multiple regression<sup>(1)</sup>. This seems to have caused the implication in Hall's reaction<sup>(4)</sup> to my paper<sup>(1)</sup> that I erroneously regarded the accommodation of the cost of body protein resynthesis in the multiple regression coefficient as a mathematical *a priori*, rather than an axiom in need of empirical justification. It, furthermore, seems that this misunderstanding led Hall to think that a different identity is necessary for the root cause of the defects in protein efficiency estimation by regression, than the one developed by myself<sup>(1)</sup>. However, with regard to the correction of the defects we seem to be in substantial agreement.

I declare no conflict of interest.

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