A central obstacle in nutritional epidemiology is the relative inaccuracy of subjects’ habitual dietary intake estimates for specific food groups and nutrients. Random and uncorrelated measurement errors cause attenuation of relative risk estimates, and decrease the statistical power of studies. In addition, systematic (scaling) errors may cause problems of comparability across studies, or across sub-populations in multi-ethnic or multi-centre studies. To adjust for biases in relative risk estimates caused by measurement errors, it is increasingly being proposed that epidemiological studies of diet and disease risk should incorporate sub-studies for the validation and calibration of food-frequency questionnaire assessments of subjects’ habitual dietary intake. In this paper, we review recent developments in statistical methods used for such studies, with special emphasis on structural equation modelling. Particular attention is given to problems that arise because random errors in dietary questionnaire measurements cannot be assumed to be uncorrelated with those of measurements obtained by weighed food consumption records or 24-hour diet recalls.

The concepts of ‘validation’ and ‘calibration’

‘Validation’ is usually referred to as the evaluation of whether a measuring instrument really measures what is intended. When a gold standard measurement is available, validation consists of a comparison between the gold standard and a test measurement. If the errors in the test measurement are sufficiently small, it is considered valid, and may be substituted for the gold standard in future studies. This simple definition of validation cannot be applied in nutritional epidemiology, since a gold standard measurement does not exist. We therefore expand the
Dietary assessment errors and their effects on relative risk estimates

The dietary assessment instrument used most often in large-scale epidemiological studies is the food-frequency questionnaire. Initially, the measurement obtained from the questionnaire, $Q$, was assumed to be linked to the true habitual food intake value, $T$, and the measurement error, $e_Q$, by a simple measurement model which expresses $Q$ as the sum of two independent random variables: $Q = T + e_Q$. This is known as the 'classical' measurement error model. Here the latent variable $T$, the true intake, is assumed to have finite variance $\sigma_T^2$ and errors are assumed to have mean zero, constant variance and to be uncorrelated when measurements are repeated on the same subjects. More specifically, for a particular realisation of the measurement $Q$ on the $i$th individual and on occasion $j$, the classical model can be written as sum of two independent terms:

$$Q_{ij} = T_i + e_{Qij},$$

with

$$E[e_{Qij}] = 0$$

(assumption of global unbiasedness on the group level);

$$\text{Cov}[e_{Qij}, T] = 0$$

(assumption of no correlation of errors with true intake levels); and

$$\text{Cov}(e_{Qij}, e_{Qij'}) = 0 \text{ whenever } i \neq i' \text{ or } j \neq j'$$

(assumption of uncorrelated errors).

This model contains some strong assumptions that are unlikely to hold in practice. To improve the model, three major modifications were eventually proposed.

First, the assumption of global unbiasedness ($E[e_{Qij}] = 0$) was dropped, as it was recognised that intake could be either systematically overestimated or underestimated on a group level. As an example, consider a questionnaire in which questions concerning a non-negligible source of alcohol (e.g. strong spirits) is absent; then clearly there is a bias in the measuring instrument leading to a systematic underestimation of alcohol consumption.

Second, it was recognised that also the assumption of no correlation of errors with true intake levels ($\text{Cov}(e_{Qij}, T_i) = 0$) may not hold in practice. It is possible, for a variety of reasons, that individuals consuming large quantities of alcohol tend to underestimate their alcohol intake more than individuals with more moderate alcohol intakes. A more accurate model should therefore include a term to represent a covariance between the error $e_{Qij}$ and $T_i$.

Third, it was recognised that even after accounting for a possible covariance between $e_{Qij}$ and $T_i$, individuals who respond to the questionnaire on multiple occasions may vary in their tendency to under- or overestimate true intake. This means that even those deviations from the true value that are uncorrelated with true intake level $T$ can be decomposed into an individual specific bias $\delta_{Qij}$, which is random only between individuals but not within, and a portion $\gamma_{Qij}$ which varies randomly within individuals from one occasion to another.

To accommodate the above departures from the original assumptions, a decomposition of the error term $e_{Qij}$ was postulated, including a linear relationship between individuals’ questionnaire measurements and true intake levels:

$$e_{Qij} = \alpha_Q + \beta T_i + e_{Qij},$$

where the total random error $e_{Qij}$ is decomposed into $\delta_{Qij}$ plus $\gamma_{Qij}$, and where $E[\delta_{Qij}] = 0$, $E[e_{Qij}] = 0$, $\text{Var}(\delta_{Qij}) = \sigma_\delta^2$, $\text{Var}(\gamma_{Qij}) = \sigma_\gamma^2$, $\text{Var}(e_{Qij}) = \sigma_e^2 = \sigma_\delta^2 + \sigma_\gamma^2$, and all terms may be assumed to be mutually uncorrelated. This led to the following measurement model:

$$Q_{ij} = \alpha_Q + \beta Q_i T_i + e_{Qij},$$

with constant variance and uncorrelated error assumptions now holding for $e$. This modified model implies a
Statistical models in dietary validation studies

more complex covariance structure for $Q_{ij}$:

$$\text{Cov}(Q_{ij}, Q_{i'j}) = \beta_{ij}^2 \sigma^2_T + \alpha^2_{Qij}$$

$$= \beta_{ij}^2 \sigma^2_T + \alpha^2_{Qij} + \sigma^2_{Qij}$$

and

$$\text{Cov}(Q_{ij}, Q_{ij}) = \beta_{ij}^2 \sigma^2_T + \alpha^2_{Qij} \text{ for } j \neq j'.$$

The parameters $\alpha_{Qij}$ and $\beta_{Qij}$ express constant and proportional scaling biases, respectively. The term $\delta_{Qij}$, which is systematic within subjects but varies randomly between them, has been termed subject-specific bias. It is the presence of subject-specific biases that implies a non-zero correlation for replicate questionnaire measurements $Q_{ij}$ and $Q_{i'j}$ taken from the same individual at different time points.

Within epidemiological studies, the effects of errors in dietary questionnaire assessment errors are several. First, and most importantly, random errors $\epsilon_{Qij}$ cause attenuation of relative risk estimates. Under the simplifying model assumptions of approximate normality and a linear measurement error model as under equation (2) above, the magnitude of such bias depends on how the relative risk (RR) is expressed. If relative risks are expressed as log relative risk estimates, then the correlation coefficient between them, has been termed subject-specific bias. It is the presence of subject-specific biases that implies a non-zero correlation for replicate questionnaire measurements $Q_{ij}$ and $Q_{i'j}$ taken from the same individual at different time points.

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The design and analysis of validation and calibration studies

**Classical approaches**

In practice, $\rho_{QT}$ or $\lambda_Q$ can be estimated only by comparison of dietary questionnaire measurements with measurements obtained by an alternative technique. The major problem, however, is to find good reference measurements for such a comparison.

For a long time, it was assumed that accurate reference measurements could be obtained by asking subjects to record their current intake on a number of specific days, using weighed food consumption records, food consumption diaries or 24-hour diet recalls. It was thought that as long as food portions were assessed accurately by weighing or using a scale of pictures, such records would lead to highly accurate measurements of intake on each given day. With respect to the subjects’ habitual dietary intakes in the longer term, the only major source of error left would then be due to within-subject, day-to-day variations in the actual intake of foods and nutrients. It was thus assumed that by increasing the number of recording days the mean of multiple food records would gradually converge to the individuals’ true habitual intake values. These considerations led to the formulation of the model:

$$R_{ij} = T_i + \epsilon_{eij}, \quad \epsilon_{[e]} = 0, \quad \text{Cov}(T, \epsilon_{e}) = 0,$$

$$\text{Cov}(\epsilon_{eij}, \epsilon_{e_{ij'}}) = 0 \quad (j \neq k).$$

An additional assumption was statistical independence between the random errors of food consumption records and those of questionnaire assessments, hence:

1. calculate the correlation coefficient between measurements $Q$ and the average of multiple days of food records ($R$);
2. estimate the correlation $\rho_{QT}$ between $R$ and true intake levels, by means of an analysis of variance (to estimate within- and between-subject variances in the food records); and
3. to correct the crude estimate, $\rho_{QT}$, for the correlation between $R$ and true intake levels (i.e. correcting for attenuating effects due to residual within-subject (day-to-day) variations in the average food record measurements); that is $\lambda_Q = \rho_{QT} / \rho_{QR}$.

Initially, the practical implementation of calibration studies was described under the same assumptions as those of validation studies, namely that mean true intake levels of a population could be estimated correctly by average weighed food records or 24-hour diet recalls, and that random errors of such records or recalls would be independent of random errors in the dietary questionnaire (used for classification). In theory, the design of a validation study can be lighter than that of a validation study, in that a single (non-replicated) reference measurement $R_i = T_i + \epsilon_{e_{ij}}$ per person (e.g. a single day’s food consumption record or a single 24-hour diet recall) would be sufficient to estimate $\lambda_Q$, whereas validation studies would need at least one further measurement (e.g. a replicate measurement $R_{ij}$ or a biochemical marker of diet).

**Validation in terms of structural equation models**

In the mid-1990s, the statistical analysis of dietary validation studies received renewed attention. It was shown that statistical evaluation of dietary validation studies, based on the comparison of questionnaire measurements $Q$ with replicate daily food consumption records $R$, could be analysed conveniently by simultaneously taking into account the measurement errors in
both $Q$ and $R$ expressed by equations (2) and (5): this constitutes a simple structural equation model\(^{11-15}\). Structural equation models have been developed extensively in the field of psychometrics, which also suffers from the lack of gold standard measurements. In many situations, the number of unknown model parameters to be estimated equals the number of independent means, variances and covariances. Equating the theoretical and sample moments then results in a set of equations that can be solved directly to obtain the parameter estimates. This is illustrated in Table 1. In other situations, where the number of model parameters is smaller than the number of independent elements in the covariance matrix, a fitting algorithm can be used that is based on maximum likelihood or on minimum squared differences between the observed and theoretical moments.

The formulation of the measurement error problem within the framework of structural equation modelling has the considerable advantage of allowing generalisation to more complex study designs, such as those incorporating biomarkers of diet as a third type of measurement\(^{4-6}\). Furthermore, the introduction of structural equation modelling for dietary validation studies led to a greater awareness of essential model assumptions needed to allow estimation of the parameters of the error model. The most important of these assumptions is that random errors ($\varepsilon$) must be statistically independent between at least three measurements in the validation study, including the questionnaire measurement. This assumption means that correlations between any pair of these measurements should be entirely due to the fact that these all relate to the same latent variable – i.e. true intake – but not because the same subjects tended to make similar errors, in amount and direction, with each type of measurement. For example, if the validation study is based on the comparison of questionnaire measurements with multiple food consumption records, random errors must be assumed to be independent between measurements $Q$, $R_1$ and $R_2$. Alternatively, a validation study may be based on the comparison between three different types of measurement – e.g. from a questionnaire ($Q$), food consumption records ($R$) and a biomarker ($M$) – assuming independence of random errors between the three different types of measurement\(^{4,6,12}\). Besides independence of random errors, the statistical models show that at least one measurement ($R$) must provide a reference scale – i.e. $R_{ij} = T_{ij} + \varepsilon_{R_{ij}}$. Without these assumptions, statistical models cannot provide unique estimates for each of the unknown parameters in our measurement models, and the statistical model is called unidentifiable.

### The problem of correlated random measurement errors for different instruments

As indicated in the definition of the model represented by equation (5), random errors for different instruments such as questionnaires and food records were at first assumed to be uncorrelated. However, since publication of the first papers on the application of structural equation models to the analysis of dietary validation studies, the assumption of uncorrelated random errors between questionnaire measurements $Q_i$ and replicate weighed food consumption records $R_{ij}$ and $R_{ik}$ has been increasingly called into question. Doubts about the general applicability of this assumption were created especially by the comparison of total energy intake estimates with measurements obtained by the doubly labelled water technique\(^{13,14}\) or of protein intake estimates with measurements based on 24-hour urinary nitrogen excretion\(^{15}\). These comparisons showed that, irrespective of the dietary assessment technique used, obese individuals tend to underreport their total food consumption more than lean subjects. Such

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**Table 1** Validation of questionnaire measurements by structural equation models: comparison of questionnaire assessments ($Q$) with replicate food consumption records ($R_1$ and $R_2$)

<table>
<thead>
<tr>
<th></th>
<th>$Q$</th>
<th>$R_1$</th>
<th>$R_2$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$Q$</td>
<td>$\beta_Q \sigma_Q^2 + \sigma_Q^2$</td>
<td>$\mu_Q + \beta_Q \mu_T$</td>
<td>$\lambda \mu_Q + \beta_Q \mu_T$</td>
</tr>
<tr>
<td>$R_1$</td>
<td>$\mu_Q + \sigma_Q^2$</td>
<td>$\sigma_Q^2 + \sigma_R^2$</td>
<td>$\sigma_Q^2 + \sigma_R^2$</td>
</tr>
<tr>
<td>$R_2$</td>
<td>$\mu_Q + \sigma_Q^2$</td>
<td>$\sigma_Q^2 + \sigma_R^2$</td>
<td>$\sigma_Q^2 + \sigma_R^2$</td>
</tr>
</tbody>
</table>

Parameter estimates

- $\mu_T = 6.20$
- $\sigma_T^2 = 1.47$
- $\rho_{QT} = \sqrt{\frac{1}{1 + (\sigma_Q^2/\sigma_T^2)}} = 0.69$
- $\sigma_Q^2 = 0.97$
- $\sigma_Q^2 = 1.93$
- $\lambda = \frac{\mu_Q \sigma_Q^2}{\beta_Q \sigma_Q^2 + \sigma_Q^2} = 0.43$

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systematic differences in the subjects’ tendencies towards underreporting will generally cause a positive correlation between the random errors of foods and nutrient intakes measured by questionnaires, food consumption records and 24-hour diet recalls.

It can easily be shown that a correlation between random errors $e_Q$ and $e_R$ will lead to an overestimation of the calibration factor $\lambda_Q$, if the statistical models used to estimate $\lambda_Q$ do not simultaneously estimate (and hence adjust for) this error correlation. A general consequence of such bias in estimates of $\lambda_Q$ is that calibration adjustments to dietary questionnaire measurements will provide only a partial correction for bias in relative risk estimates. The magnitude of bias in the estimated calibration factor $\lambda_Q$ depends not only on the strength of the correlation $\rho_{Q,R}$ between random errors of $Q$ and $R$, but also on the random error variances $\sigma^2_{Q,r}$ and $\sigma^2_{R,r}$ relative to the variance $\sigma^2_T$ of true intake levels, and hence on the magnitude of the correlations $\rho_{QT}$ and $\rho_{RT}$ (Table 2).

The coefficient of correlation $\rho_{QT}$ between questionnaire measurements and true intake levels will also tend to be overestimated if a positive correlation between random errors $e_Q$ and $e_R$ is ignored by the statistical model used for analysis. On the other hand, a positive correlation between the random errors $e_{R,i}$ and $e_{Q,i}$ of replicate measurements $R$ may cause an underestimation of the correlation $\rho_{QT}$. In most practical situations it will be unclear which of these two biases, upwards or downwards, predominates. More extensive theoretical simulations and sensitivity analyses, showing the magnitude of bias in estimates of $\lambda_Q$ and $\rho_{QT}$ when a correlation between the random errors $e_Q$ and $e_R$ is ignored (i.e. incorrectly assuming this correlation to be zero), have been presented by Spiegelman et al.\textsuperscript{10}, Wong et al.\textsuperscript{17} and Kipnis et al.\textsuperscript{18}.

**Biological markers of diet: a solution for the error correlation problem?**

In order to avoid biases in the estimation of $\sigma^2_T$, $\sigma^2_{Q,r}$ and $\beta_Q$ (and hence of $\rho_{QT}$ and $\lambda_Q$), statistical models should take account of the correlation between the random errors in $Q$ and $R$, as well as between the random errors in replicate measurements $R_{i1}$ and $R_{i2}$. A fundamental problem, however, is that the number of error parameters to be estimated – which include the error covariances Cov($e_{Q,i}, e_{R,i}$) and Cov($e_{R_{i1}, e_{R_{i2}}}$) – will be larger than the number of variances and covariances observed, as long as the validation/calibration study is based on comparisons between measurements of $Q$ and $R$ only. These statistical models thus remain unidentifiable. To solve this identifiability problem, a third type of measurement must be found, for which random errors can be assumed to be uncorrelated with those of $Q$ and $R$, and for which random errors are also uncorrelated if the measurements are replicated over time in the same individuals. In practice, the only category of measurements that may fulfil these criteria is biochemical markers of dietary intake.

Much of the error occurring in the more traditional measurements of diet may be due to subjects’ failure to recall or report accurately their intakes. Biochemical markers can be considered more ‘objective’ because they do not depend on a subject’s report. It thus seems reasonable to assume that random errors in biomarker measurements will generally be independent of the random errors in questionnaire measurements of dietary intake or of food consumption records and 24-hour diet recalls. However, the assumption of independence of random errors in replicate marker measurements obtained from the same individuals is often more problematic.

Two categories of biomarker of diet can be distinguished: those based on a concentration and those based on recovery\textsuperscript{12,18}. In the sections below, it is discussed whether and how these markers of diet can be of use in dietary validation/calibration studies, to overcome the problem of correlated random errors between questionnaire measurements and food consumption records.

**Use of concentration-based markers**

Concentration-based markers, as their name indicates, are based on the measurement of a concentration, at a given point of time, of a specific compound. Concentration measurements may be made in blood plasma (e.g. vitamin C, specific carotenoids), within a given blood lipid fraction (e.g. the relative fatty acid composition of circulating phospholipids), in an adipose tissue biopsy (e.g. the relative composition of fatty acids), in urine (e.g. sodium) or in other tissues (e.g. red blood cells) and body fluids (saliva). One key characteristic of this class of markers is that they do not have a time dimension; that is, their levels are measured and expressed without any time unit. A second characteristic is that these markers generally do not have the same quantitative relationship with dietary intake levels for every individual in a given study population. Concentration-based markers therefore cannot be translated into absolute intake levels per day, but at the very best can provide only a correlate of dietary intake levels. One consequence of this is that, if the objective is to estimate constant and/or proportional scaling factors $\alpha_Q$ and $\beta_Q$ (for calibration), weighed food consumption

### Table 2 Bias in estimates of the calibration factor $\lambda_Q$, when random errors of questionnaire assessment and reference measurements are correlated ($\rho_{R,Q} = 0$)

<table>
<thead>
<tr>
<th>$\rho_{R,Q}$</th>
<th>$\rho_{QT}$</th>
<th>$\rho_{RT}$</th>
<th>0.3</th>
<th>0.5</th>
<th>0.7</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.3</td>
<td>0.3</td>
<td>0.3</td>
<td>1.60</td>
<td>2.50</td>
<td>2.62</td>
</tr>
<tr>
<td>0.5</td>
<td>0.5</td>
<td>0.5</td>
<td>1.65</td>
<td>2.60</td>
<td>2.99</td>
</tr>
</tbody>
</table>

(Multiplicative) bias $= 1 + \rho_{Q,R} \sqrt{\left(\frac{1}{\rho_{Q,T}^2} - 1\right) \left(\frac{1}{\rho_{R,T}^2} - 1\right)}$. 

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Parameters to be estimated

\[
\begin{pmatrix}
\sigma_Q^2 & \sigma_{Q, R}^2 & \sigma_{Q, M_1}^2 & \sigma_{Q, M_2}^2 \\
\sigma_{Q, R}^2 & \sigma_R^2 & \sigma_{R, M_1}^2 & \sigma_{R, M_2}^2 \\
\sigma_{Q, M_1}^2 & \sigma_{R, M_1}^2 & \sigma_{M_1}^2 & \sigma_{M_2}^2 \\
\sigma_{Q, M_2}^2 & \sigma_{R, M_2}^2 & \sigma_{M_2}^2 & \sigma_{M_1, M_2}^2 \\
\end{pmatrix}
\]

Table 3 Theoretical covariance matrix in validation studies with a concentration-based biomarker

- \( \sigma_Q \): measurement error
- \( \sigma_R \): random variation
- \( \sigma_M \): between-individual variation
- \( \sigma_{M_1, M_2} \): covariance between \( M_1 \) and \( M_2 \)

Records or 24-hour diet recalls must provide a reference scale (i.e. by assuming \( \alpha_R = 0 \) and \( \beta_R = 1.0 \)).

Given the above, and dropping the subscripts for simplicity, we write the measurement error models as:

\[
Q = \alpha_Q + \beta_{Q, R} + e_Q,
\]

\[
R = T + e_R,
\]

\[
M_1 = \alpha_M + \beta_{M, T} + e_{M_1},
\]

\[
M_2 = \alpha_M + \beta_{M, T} + e_{M_2},
\]

where \( \text{Cov}(e_Q, e_{M_1}) = \text{Cov}(e_Q, e_{M_2}) = 0 \) and \( \sigma_{e, M_1}^2 = \sigma_{e, M_2}^2 \). It should be noted that here we are no longer assuming \( \text{Cov}(e_Q, e_R) = 0 \) and \( \text{Cov}(e_{M_1}, e_{M_2}) = 0 \). The population means and covariance matrix for the measurements \( Q, R, M_1 \) and \( M_2 \) are given in Table 3. Since the measurements \( M_1 \) and \( M_2 \) have the same variance, and because the covariances with measurements \( Q \) and \( R \) are also the same for \( M_1 \) and \( M_2 \), the covariance matrix in Table 3 contains only seven independent entries. However, there are eight unknown parameters in the model, so that the model is not identifiable.

The only way to make the model identifiable is to assume one or more parameter to be known. The most obvious additional assumption to be added would be \( \text{Cov}(e_{M_1}, e_{M_2}) = 0 \), as other assumptions – e.g. \( \text{Cov}(e_Q, e_R) = 0 \) or \( \beta_M = 1.0 \) – do not seem reasonable. Unfortunately, however, the assumption \( \text{Cov}(e_{M_1}, e_{M_2}) = 0 \) is also unlikely to hold for concentration-based markers.

The reason to reject this assumption is that between-subject variation in concentration-based markers is generally determined not only by dietary intake of a given compound, but also by variations in digestion and absorption, distribution over body compartments, endogenous synthesis and metabolism, and excretion. For example, plasma levels of \( \beta \)-carotene depend not only on intake levels, but also on factors affecting absorption (e.g. depending on cooking method and on amounts of co-ingested fats), internal metabolism (e.g. conversion into retinol, retinal or retinoic acids, by endogenous dioxygenases) and non-enzymatic internal breakdown of \( \beta \)-carotene because of smoking and other factors that may increase oxidative stress. Likewise, the fatty acid composition of plasma phospholipids or of adipose tissue depends not only on the intakes of specific fatty acids, but also on the internal synthesis of fatty acids from carbohydrates, and on the elongation and (de)saturation of polyunsaturated fatty acids. Generally, these non-dietary determinants are very likely to vary systematically between individuals, so that part of the random variations in the marker (i.e. variations not determined by diet) would tend to be correlated over time \( 12,18 \).

Assuming that there is positive correlation between the random errors \( e_Q \) and \( e_R \), all that concentration-based biomarkers can add to validation studies is the estimation of an upper limit for \( \rho_{QT} \) and \( \lambda_{QR} \). Although such estimation may remain relatively imprecise if the marker does not correlate strongly with true intake levels \( 19 \).

**Use of recovery-based markers**

Recovery-based markers are based on precise and quantitative knowledge of the physiological balance between intake and output of a compound or chemical element. One example is the urinary excretion of nitrogen over a 24-hour period, which for any given individual in energy and protein balance is known to be approximately equal to 80% of nitrogen intake. Moreover, since nitrogen is present in the diet mostly in the form of protein, whereas the concentration of nitrogen in different types of protein is relatively constant, the 24-hour urinary nitrogen excretion can be translated into a valid estimate of an individual’s daily protein intake. Another example is urinary excretion of potassium, which also represents a relatively constant proportion of potassium intake. Since a very large proportion of potassium intake comes from vegetables, the urinary potassium excretion can be used as an approximate marker for total vegetable consumption \( 20 \). A third example is the doubly labelled water technique for the assessment of daily total energy expenditure, which for subjects in energy balance is very close to daily energy intake. This technique is based on the measured difference in recovery of \( ^{1}H \) and of \( ^{18}O \) in urine, after drinking a known amount of water that is doubly labelled with these two stable isotopes. From this difference in recovery, it can be computed how much \( CO_2 \) has been produced in the body by metabolism, and hence how much energy the body has produced \( 21 \).

Since the recovery (or difference in recovery of two different chemical elements, for the doubly labelled water method) is known to be a fixed proportion of intake for any given individual, the random variations in the marker that may occur over time can be assumed to be uncorrelated, provided that the time interval between successive biological samples is sufficiently large. In addition, since the quantitative relationship between recovery-based markers and dietary intake is known (especially for urinary nitrogen and the doubly labelled water technique), these markers can also provide a valid
reference scale. We can thus write the following measurement error models and model assumptions, for a validation study based on the comparison of questionnaire measurements (Q) with weighed food records (R) and two replicate measurements of a recovery-based marker:

\[ Q = \alpha_Q + \beta_Q T + e_Q. \]
\[ R = T + e_R \quad \text{(alternatively,} \quad R = \alpha_R + \beta_R T + e_R). \]

**Model without biomarkers**

\[ M_1 = T + e_{M1}, \]
\[ M_2 = T + e_{M2}. \]

where \( \text{Cov}(e_Q, e_{M1}) = \text{Cov}(e_Q, e_{M2}) = 0, \quad \sigma_{M1}^2 = \sigma_{M2}^2 \) and \( \text{Cov}(e_{M1}, e_{M2}) = 0. \) The expected covariance matrix and parameters to be estimated in these models are shown in Table 4. Thus, for recovery-based markers, the only non-zero error correlation is that between \( e_Q \) and \( e_R \), whereas for concentration-based markers non-zero correlations are allowed between \( e_{M1} \) and \( e_{M2} \) as well as between \( e_Q \) and \( e_R \). It should also be noted that, using the above model assumptions, both the marker and the recording method are assumed to provide the same, valid reference scale for intake measurements. It would be possible to relax these assumptions, however, and to add for instance constant and proportional scaling biases to the measurement model for food consumption records (parameters \( \alpha_R \) and \( \beta_R \)), to be estimated in the validation study.

For illustration, we applied this model to data from the pilot-phase validation studies of the European Prospective Investigation into Cancer and Nutrition (EPIC), for measurements of protein intake in men and women in Italy and in the Netherlands (Table 5). Measurements in this study were obtained by a food-frequency questionnaire (Q), by the average of 12 replicate 24-hour diet recalls (R) and by urinary nitrogen in two different urine samples. As shown in Table 5, the model of equation (7) gave mostly lower estimates for both \( \rho_{QT} \) and \( \lambda_Q \) compared with models without the biomarker, where the correlation between random errors \( e_Q \) and \( e_R \) was simply assumed to be zero. A similar analysis, using also urinary nitrogen excretion as a marker of protein intake, was performed by Plummer and Clayton."
eliminate the problem of correlation between random errors $e_Q$ and $e_R$ of questionnaire assessments and consumption records by taking the place of $R$, plus the basic assumption that random errors in the marker are uncorrelated with those in measurements $Q$.

The question then is how, in the absence of further recovery-based markers for nutrients other than protein, energy or potassium, further progress may be made in the area of dietary validation and calibration studies. One possible way out of the problem of correlated random errors might be to assume that such correlation diminishes strongly when intake estimates are adjusted for total energy intake.

It is widely recognised that, before incriminating any specific nutrient or food in the aetiology of a disease, epidemiological studies should show that the nutrient or food remains associated with disease risk after the adjustment for total energy intake. The reason is that total energy intake is itself determined by factors such as body size, usual physical activity and metabolic efficiency, which may each also have effects on disease risk.

To account for the possible confounding effects of factors that lead subjects to eat more, or less, of all possible foods and nutrients, epidemiological analyses of diet–disease associations should be adjusted for total energy intake. Disease risk will thus no longer be related to absolute intake levels of nutrients or foods but rather to a measure of relative dietary composition, and methodological sub-studies on measurement error should also focus more on the validation or calibration of energy-adjusted intake levels of nutrients or foods. An interesting additional aspect of the total energy adjustment is that it might decrease substantially the correlation between random errors in different types of dietary intake assessment, if such correlation of errors were due mainly to variations in the degree of systematic underreporting by each assessment method. This will be true especially if one can assume that underreporting on average does not affect one type of nutrient or food more than another. More research should be done to verify if the latter assumption is generally reasonable, or to check whether at least it leads to a smaller degree of bias in estimates of $p_Q$ and $\lambda_Q$ than when random errors are assumed to be statistically independent for the non-adjusted intake variables.

References


