

Wearable-Informed Energy Baselines for EPK

A narrative review of individual energy estimation, low-fat diets and performance fuelling

Billy Craig | Independent Researcher

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Central proposition. Repeated wearable observations can make an energy estimate more responsive to an individual's daily activity than a fixed resting-expenditure equation combined with an assigned activity level. This added information does not, by itself, establish greater calorie accuracy or improved outcomes.

Abstract

Energy Per Kilogram (EPK) expresses dietary energy intake relative to body mass. Its current operational approach uses wearable-estimated total daily energy expenditure, an estimated fallback when data are absent, and an initial five-day learning period. It then offers an energy-balance pathway intended to meet estimated expenditure and a performance pathway targeting approximately 15% above the estimated baseline. Both retain a low-fat, high-carbohydrate dietary orientation. These programme features are described by the developer; they have not been independently validated in this paper.

This narrative literature review examines whether repeated activity observations add useful individual information beyond resting metabolic rate multiplied by an assigned physical activity level. Selected validation studies support incremental information from movement sensing and, in some settings, improved estimation from combined heart-rate and movement data. Other studies demonstrate substantial individual errors in both wearable and equation-based estimates. The five-day period and 15% increment therefore remain design choices requiring evaluation. We distinguish information content, calorie accuracy and practical benefit, and propose separate validation and pragmatic outcome studies. Low-fat trials are assessed by prescribed and achieved fat intake, with 20% of energy or less used as the EPK review category. Positive dietary findings do not independently validate EPK. No new participant data are reported.

Keywords: energy expenditure; wearable devices; physical activity level; Mifflin-St Jeor; EPK; dietary adequacy; performance.

Scope and evidence selection

This targeted narrative review uses primary research and source records accessed through 2 October 2026. It is not a systematic review or an EPK validation study. Appendix A explains selection, appraisal and limitations.

1. From a static estimate to a daily baseline

Mifflin-St Jeor predicts resting energy expenditure using body mass, height, age and sex; its original derivation included 498 adults.[1] To estimate a whole day, a resting prediction is commonly multiplied by an assumed activity factor. Such a calculation is already individualised in its demographic inputs, but a fixed activity category does not directly observe whether today involved a desk-based routine, a long shift on foot, or training.

The proposed comparison is therefore between two estimation strategies, not between a formula and direct measurement. A consumer wearable also applies algorithms and may incorporate a predicted resting component. Its additional contribution is repeated sensor information. The research question is whether that information improves the relevant outcome beyond an explicitly specified equation and activity factor.

EPK exposure: dietary intake (kcal/day) / body mass (kg).

Equation comparator: predicted resting expenditure x assigned PAL.

Energy-balance target: B kcal/day.

Performance target: approximately $1.15 \times B$ kcal/day.

Here B denotes the estimated daily baseline selected by the programme. These equations describe targets, not measured energy balance.

The developer describes the current EPK sequence as: use wearable total daily expenditure where available; use an estimated baseline when it is absent; collect an initial five days of information; then offer the energy-balance pathway. The exact fallback equation, completeness rules, averaging method and later update schedule were not supplied for this paper. These details must be documented before the method can be reproduced. Recommendations below are prospective, not claims about implemented software.

1.1 Keep quantities distinct

Total daily expenditure already includes the resting component represented by the device. Adding a second resting estimate or another full-day PAL multiplier to that total would double count energy. Likewise, exercise sessions imported from multiple sources require deduplication. Device definitions of active, resting and total energy must be documented rather than inferred from labels.

PAL is a ratio: total expenditure divided by the specified basal or resting reference. Basal and resting measurements are not identical protocols, so the reference used by an activity multiplier must be stated. Diet-induced thermogenesis should not be added again if already included in a total-expenditure estimate or its multiplier. EPK intake per kilogram, estimated expenditure per kilogram, and sports energy availability are separate quantities; the latter also accounts for exercise expenditure and fat-free mass.

1.2 What is being claimed

The strongest present claim is greater observation of individual activity over time. Greater accuracy in kcal/day is a separate empirical claim, and improved dietary adequacy or performance is a third. None follows automatically from the preceding claim. A data-rich estimate can remain biased; an accurate expenditure estimate can still be an incomplete prescription for recovery or growth.

2. Evidence for added information and its limits

Incremental information from wrist movement. Dougherty et al. studied 49 adults using doubly labelled water and accelerometry. Wrist movement explained an additional 4-8 percentage points of variance in total or activity expenditure beyond age, sex, height and body composition.[2] This supports added explanatory information. It is not a 4-8% improvement in calorie accuracy, a within-person change-detection result, or validation of EPK.

Combining physiological and movement signals. Brage et al. compared accelerometry, heart rate and combined models against doubly labelled water; 46 participants contributed to analyses. Combined sensing with individual exercise calibration showed the best precision, although its precision advantage over accelerometry alone was not statistically significant.[3] Laboratory calibration in that study must not be equated with five days of passive EPK data collection.

Wearable totals can obscure activity error. Murakami et al. evaluated 12 devices in 19 adults using a chamber and free-living doubly labelled water. Activity expenditure was generally estimated less accurately than total expenditure, which includes resting metabolism.[4] The devices were from an earlier generation; their errors should not be assigned unchanged to current models or software.

Equations can work well on average. Porter et al. analysed 1,488 older adults' total-expenditure records. Mifflin combined with the study's mean PAL estimates underestimated total expenditure by only 24 kJ/day on average, but the standard deviation of the difference was 1,401 kJ/day (approximately 335 kcal/day).[5] Small mean bias therefore coexisted with appreciable individual uncertainty. This was not a wearable-versus-EPK trial.

Activity level affects model performance. Prado-Novoa et al. evaluated ten total-expenditure models in 56 adults against doubly labelled water. Models generally underestimated expenditure, with substantial individual errors and accuracy affected by activity level.[6] This supports evaluating a range of activity patterns rather than assuming a model validated in one group transfers to all users.

Evidence synthesis. Sensor observations can add information beyond demographic inputs, but these studies do not establish that any wearable total is always more accurate than Mifflin x PAL. Nor do cross-sectional associations alone establish reliable tracking of a person's changing expenditure.

2.1 Accessible use and rigorous evaluation

Repeated laboratory assessment is impractical for routine consumer use. That supports developing accessible methods; it does not establish their validity. End users need not each undergo laboratory testing. A representative validation sample can assess the estimator, while larger real-world studies can assess feasibility and outcomes. INTERLIVE recommends context-specific calorimetry for activity validation and doubly labelled water for free-living total expenditure, together with transparent device and analysis reporting.[7]

3. The five-day learning period

Five days can provide a useful initial record of a person's routine, but this paper identifies no validation of five days as sufficient to calibrate the EPK baseline. It should be described as an initial learning period. A five-day window may omit weekends, a heavy training day or part of a rotating work schedule. Its usefulness depends on the completeness and representativeness of the observations, not elapsed time alone.

As a prospective implementation specification, eligibility should require complete daily records according to documented device-specific rules. A partial day should not be interpreted as low expenditure. Missing data, device non-wear and a genuinely quiet day should remain distinguishable. Where the programme falls back to an equation, the source of the estimate should remain visible and the change logged.

An ongoing estimate could use a defined rolling window, with comparison of five-, seven-, fourteen- and twenty-eight-day windows in validation. Shorter windows may respond faster but capture more noise; longer windows may better represent routine while responding more slowly to change. The optimal choice is an empirical question. Repeated observations do not remove systematic algorithm error.

3.1 Source and data-quality requirements

A reproducible EPK implementation should retain the device model, software version where available, source platform, date and time zone, units, completed-day status and the origin of each total. It should state whether resting expenditure is measured or predicted by the supplier. Data merged from multiple wearables or exercise apps should not be summed without overlap checks. Historical recalculations and device changes should be identifiable.

Reasonableness checks should flag missing, duplicate or implausible values for review without silently substituting low targets. The user should be able to understand whether the baseline is based on wearable data or a fallback estimate. Re-estimation after unusual travel, illness or a change in training should follow a documented rule; these are recommendations for evaluation, not verified current features.

3.2 What everyday observations can establish

Longitudinal records can describe estimated demand across work, rest and training days. Dietary records, body-mass trends, perceived energy, recovery and performance can help evaluate whether a pathway is useful. However, agreement with another app or stable body mass alone is not an independent validation of calorie expenditure. Intake reporting, water and glycogen changes, and changes in body composition can complicate interpretation.

Recommended user-facing description. After an initial five-day learning period, EPK offers a target intended to meet estimated baseline needs. Wearable-informed estimates add personal activity observations; they remain estimates and can be updated as the record develops.

4. Energy balance and performance pathways

The energy-balance pathway aims to match intake to B, the estimated daily baseline. Meeting this target is not proof of zero actual energy imbalance. If B underestimates expenditure, intake at B may remain insufficient; if B overestimates it, the same target may create a surplus. The programme should distinguish target adherence from achievement of physiological energy balance.

The performance pathway targets approximately 15% above B, with the intention of supporting growth, repair and performance. This is an operational starting setting, not an established optimum for all users. Its evaluation should consider training experience, desired outcome, dietary adequacy, body-composition changes and the uncertainty in B. Improved performance, muscle gain and tissue repair are distinct outcomes and cannot be inferred from one another.

Helms et al. randomised 21 resistance-trained participants to maintenance, estimated 5% or estimated 15% surplus prescriptions during eight weeks of training. Faster body-mass gain was more clearly associated with increased skinfolds than with greater hypertrophy or strength.[8] Garthe et al. studied 39 elite athletes over 8-12 weeks; nutritional counselling increased weight and fat gain relative to ad libitum intake, without a between-group difference in lean-body-mass gain.[9] These findings argue against treating a larger surplus as automatically more productive. Neither study tested EPK or all forms of athletic performance.

4.1 A nominal surplus is not a measured surplus

The following arithmetic illustrates sensitivity to baseline error, not an observed EPK result. Assume true expenditure is 2,400 kcal/day. A baseline underestimated by 15% is 2,040 kcal/day; multiplying it by 1.15 gives 2,346 kcal/day, still below true expenditure. A baseline overestimated by 15% is 2,760 kcal/day; multiplying by 1.15 gives 3,174 kcal/day, approximately 32% above true expenditure. The 15% performance setting therefore cannot be interpreted independently of baseline uncertainty.

4.2 Is a 15% surplus required for growth?

The reviewed evidence does not identify 15% as a universal threshold below which growth cannot occur. Helms et al. directly included maintenance, 5% and 15% prescriptions; its small sample and short duration limit precision, but its results do not establish 15% as necessary.[8] The quantity that matters physiologically is actual energy availability relative to expenditure and other demands, not a fixed percentage above an uncertain app estimate.

Longland et al. randomised 40 young men to different protein intakes during a four-week energy deficit with intensive exercise. The higher-protein group gained more lean body mass while losing fat.[20] Lean mass is not a direct measure of new contractile muscle, and this setting does not represent experienced athletes, recovery from undernutrition, childhood growth or disease. Nevertheless, it argues against a universal requirement for a positive dietary energy balance before any lean-tissue gain can occur.

EPK can retain approximately 15% as a testable starting prescription intended to provide additional energy. It should not describe it as the amount scientifically required for growth. Validation should compare maintenance, a smaller increment and 15%, with comparable protein and training, repeated body-composition measurements, performance outcomes and sufficient follow-up. Excess mass gain alone is not evidence of productive growth.

4.3 Dietary composition is a separate part of the method

Both pathways retain the developer's low-fat, high-carbohydrate orientation. Wearable validation studies do not establish the superiority of that dietary pattern. A reproducible dietary protocol must additionally specify carbohydrate, fat and protein targets, essential-fat provision, food quality and relevant individual adaptations; the developer now defines the low-fat category as 20% of energy or less. A lower bound, absolute intake and the remaining macronutrient targets still require specification.

Horton et al. demonstrated different expenditure and oxidation responses to carbohydrate versus fat overfeeding in a short crossover study, but most surplus energy was stored in both conditions and measured body-fat gains did not differ significantly.[10] This motivates recording composition as a modifier of response, rather than claiming that carbohydrate-rich intake guarantees a particular outcome.

Finally, current expenditure should not automatically define the intake required for recovery from prolonged restriction. The adequacy of either pathway in that context requires specific study. No universal kcal/kg minimum or clinical treatment claim is established by this paper.

5. What does a low-fat trial actually test?

A label such as low fat is insufficient to define an intervention. This review adopts the developer's operational category of 20% of total dietary energy or less, and separately describes diets above 20%. This is an explicit review convention, not a proven biological threshold. Prescribed targets, food provided and intake actually reported or measured must be kept distinct. A trial targeting 20% but averaging 29% tests the effect of offering that programme; it does not establish the effect of sustained adherence at 20%.

The suggestion that habitual fat intake is around 35% has support in a specific population: CDC reports mean intakes of 35.6% for men and 36.1% for women aged 20 or older in US 2015-2018 data.[11] These historical US averages should not be represented as a current estimate for every Western country, nor as evidence of a causal obesity threshold.

5.1 Two instructive examples of achieved intake

DIETFITS. In 609 adults, the healthy low-fat group averaged 29% of energy from fat at 12 months. Weight loss was approximately 5.3 kg, compared with 6.0 kg in the healthy low-carbohydrate group; the difference was not statistically significant.[12] This supports the criticism that a low-fat label may describe a diet above 20%. It does not support saying that such studies never yield benefits: both groups lost weight. A lack of superiority is different from a lack of change.

Women's Health Initiative. This trial randomised 48,835 postmenopausal women. The intervention targeted 20% fat alongside more fruit, vegetables and grains, but reported means were 24.3% at year one and 28.8% at year six. Cardiovascular events were not significantly reduced in the original intervention-period analysis.[13] A separate weight report found an intervention-control difference of 1.9 kg at year one and 0.4 kg at 7.5 years, without a weight-loss goal.[14] Thus the same programme can have modest weight effects and a null cardiovascular result. Neither outcome alone characterises every low-fat diet.

5.2 Targets do not guarantee superiority

Sacks et al. assigned 811 adults to four energy-reduced diets, including 20% and 40% fat targets. At two years, mean weight loss was 3.3 kg in both fat-target comparisons.[22] This is evidence about the offered dietary programmes, whose achieved intakes did not fully maintain the intended separation. It cannot establish an exact physiological effect of sustained 20% versus 40% intake, but it also prevents selective presentation of only favourable trials.

Required extraction fields. Record the fat target, achieved fat percentage and grams, energy intake, duration, comparator, food quality, protein, support intensity, adherence, attrition and outcome. Do not classify a study by its title alone, or remove a null result because it is inconvenient.

6. Evidence at substantially lower fat intakes

Controlled fat restriction: Hall et al. (2015). Nineteen adults with obesity completed a metabolic-ward crossover comparison of equal calorie reductions produced by reducing fat or carbohydrate. The reduced-fat diet supplied about 7.7% fat; the reduced-carbohydrate diet was not ketogenic. Over six days, calculated body-fat loss from metabolic balance was greater with fat restriction despite the greater fat oxidation on carbohydrate restriction.[15] This illustrates that burning more fat is not equivalent to losing more body fat: dietary fat intake must also be considered. The study was short, energy-restricted and too small to establish long-term superiority or EPK outcomes.

Ad libitum feeding: Hall et al. (2021). Twenty adults were randomised to the order of two-week inpatient diets: plant-based low fat (10.3% fat offered) and animal-based ketogenic (75.8% fat offered). Mean energy intake was 689 kcal/day lower during the low-fat period.[16] This provides evidence that a carbohydrate-rich diet need not cause greater spontaneous intake in that setting. Plant/animal sources, energy density, fibre and other features differed, so the effect cannot be attributed solely to fat percentage. The short crossover design and adaptation/carryover interpretation have been debated in subsequent correspondence.[18,19] The original result should be reported with that limitation, not extrapolated into proof of a durable metabolic advantage.

Free-living intervention: Kahleova et al. (2020). In 244 adults, a 16-week vegan programme targeted approximately 10% fat without calorie restriction and was compared with no dietary change. The between-group weight effect was -5.9 kg; insulin-sensitivity measures improved. Reported energy intake fell, and lean mass also declined.[17] The programme changed multiple dietary features and provided weekly classes. It therefore supports the efficacy of that package for selected outcomes, not an isolated effect of fat percentage or proof of adequate fuelling for growth. Achieved total-fat percentage was not extracted as a directly reported outcome here; the prescribed 10% must not be substituted for it.

6.1 Relevance to EPK

These trials justify examining genuinely low-fat, carbohydrate-rich patterns separately from diets nearer 30% fat. They do not identify 20% as a sharp response threshold. Crucially, evidence of spontaneous eating reduction or weight loss is not evidence that an EPK maintenance or performance pathway meets energy needs. In an adequately fuelled EPK trial, energy adequacy, lean-tissue preservation, recovery, adherence and performance require direct measurement alongside fat mass.

Interpretation should distinguish reducing energy density from prescribing insufficient total intake. A low-fat pattern may make spontaneous intake lower; this could be helpful for some study outcomes but counterproductive when a user needs to replenish energy. The EPK hypothesis is best tested with explicit fuelling targets rather than treating all weight loss as a successful result.

7. Carbohydrate oxidation and dietary fat storage

The proposed mechanism has a defensible core: carbohydrate availability changes fuel selection, and fat balance depends on both dietary fat entering the system and fat being oxidised. Horton et al. found that carbohydrate overfeeding increased carbohydrate oxidation and expenditure more than fat overfeeding; fat overfeeding was stored more efficiently.[10] This supports investigating mixed high-energy diets in which carbohydrate meets oxidative demand while dietary fat remains available for storage.

However, post-meal storage is not equivalent to permanent fat accumulation. Stored fat can subsequently be mobilised and oxidised; the relevant outcome is net balance across time. It is therefore too categorical to state that all dietary fat goes directly into lasting adipose tissue, or that carbohydrate cannot contribute to fat gain. Baseline diet, duration, activity and total intake affect the result.

Acheson et al. studied massive carbohydrate overfeeding after glycogen depletion in three men. After glycogen stores filled, substantial de novo lipid synthesis occurred.[21] This extreme protocol is not ordinary mixed eating or EPK, but it demonstrates the capacity to make fat from carbohydrate. Together with Horton, it argues for differences in storage pathways and efficiency, not immunity to fat gain on very-low-fat diets.

Proposed EPK mechanism for testing. At a defined energy target, a carbohydrate-rich diet with 20% fat or less may alter substrate oxidation, dietary fat balance and spontaneous intake relative to a higher-fat comparator. Whether this improves function, lean-tissue gain or body composition must be tested under matched and clearly measured conditions.

7.1 Percentage and absolute fat intake

Fat percentage alone does not specify grams. Using the conventional 9 kcal/g conversion, 20% of 2,000 kcal is approximately 44 g fat, whereas 20% of 3,000 kcal is approximately 67 g. These are arithmetic examples, not individual recommendations. Studies and EPK protocols should report both quantities, fat quality and essential-fat provision. A ceiling does not establish a nutritionally adequate minimum, and evidence in adults should not be generalised unchanged to pregnancy, children or clinical disease.

7.2 Isolate the components experimentally

Three independent propositions require testing: whether wearable data improves energy estimation; whether the dietary composition improves relevant outcomes at comparable energy intake; and whether the performance increment improves growth or performance. A favourable finding for any one component does not validate the other two. Factorial or sequential studies would be more informative than attributing every change to the EPK package as a whole.

8. A proportionate evaluation programme

Stage 1: verify the calculation. Audit source data and software before participant comparisons. Confirm that imported totals, fallback values, date boundaries and missing-day rules produce the intended target. Document the five-day algorithm and distinguish it from device-level calibration. This establishes computational correctness, not physiological accuracy.

Stage 2: compare information and repeatability. Prospectively record wearable totals, activity schedules, fallback estimates and baseline revisions. Examine representation of workdays, weekends and training cycles, and sensitivity to window length and missing data. Ask whether wearable-informed estimates respond to independently recorded changes in routine. Responsiveness alone does not establish correct calorie magnitude.

Stage 3: criterion validation in a funded subset. Compare wearable-informed EPK, prespecified Mifflin x PAL, and alternative suitable equations against concurrent free-living doubly labelled water. Measure resting expenditure by indirect calorimetry where feasible to separate resting-model error from activity error. PAL for prediction must be assigned independently, not calculated from the reference total being predicted. Prespecify devices, populations, wear-time requirements and sample size based on desired precision.[7]

Analyse the same time windows for all methods. Report mean signed bias, mean absolute error, limits of agreement and the proportion within prespecified error bands in both kcal/day and kcal/kg/day. Stratify by activity pattern and device when adequately powered. Correlation alone is insufficient. Doubly labelled water estimates expenditure averaged over its measurement period; it cannot independently validate every daily fluctuation.

Stage 4: pragmatic pathway evaluation. Compare wearable-informed and equation-informed baselines while holding dietary support and macronutrient guidance comparable. Prespecify adherence, perceived energy, recovery and performance outcomes, retention and adverse outcomes. Evaluate the 15% pathway separately, potentially against maintenance and a smaller surplus, with matched training and protein provision. Evidence of practical benefit would not automatically establish superior calorie accuracy.

9. Limitations and conclusion

This paper is a narrative literature review and programme description, not a validation trial. It supports specific elements of the rationale, not proven efficacy of the combined EPK programme. Older devices, research-grade sensors, population differences and incomplete access to some full texts limit transferability. No EPK user-level dataset or software audit was available. Current algorithm details require fuller specification, and evidence does not establish the optimal learning window or surplus.

Wearable-informed EPK has a defensible rationale: repeated personal activity observations can add information beyond a fixed demographic prediction and assigned PAL. Its accessible design can be evaluated without requiring laboratory access for every user. The appropriate present claim is an individually informed estimate, with greater accuracy, improved fuelling and performance benefits remaining testable propositions.

Disclosure: The author develops EPK. No new human-participant research is reported. This working paper is not peer reviewed. Author-owned text: CC BY-ND 4.0; third-party works retain their own rights. Repository deposition provides a persistent record; it does not constitute peer review.

Appendix A. Search and appraisal approach

Searches were performed using web-indexed PubMed records, PubMed Central and original journal pages, with searches completed on 2 October 2026. This is a targeted narrative review, not a registered systematic review. No exhaustive database export, duplicate independent screening, PRISMA count or formal GRADE certainty assessment was performed. The approach is appropriate for identifying a rationale and its limits, but cannot estimate the prevalence of supportive findings across the entire literature.

Search concepts. Wearable or accelerometer with total energy expenditure, doubly labelled water, resting metabolic rate, Mifflin-St Jeor or PAL; energy surplus with maintenance, 5%, 15%, hypertrophy or lean mass; low-fat diet with 20%, achieved intake, DIETFITS or Women's Health Initiative; carbohydrate overfeeding with dietary fat storage or de novo lipogenesis. Targeted searches checked named primary studies and relevant later correspondence. A CDC surveillance source was used only to contextualise habitual US fat intake.

Selection. Priority was given to human criterion-validation studies, randomised dietary or training trials, controlled feeding studies and original surveillance. Studies with null or contrary findings were retained. Mechanistic experiments, free-living efficacy trials and consumer-device validation were treated as different evidence types. Short experiments were not treated as long-term adherence studies. Existing EPK descriptions were programme information, not outcome evidence.

Extraction and appraisal. The synthesis records population and duration where relevant, comparator, prescribed versus achieved dietary exposure, outcome and a principal limit to application. Full-text tables were checked for DIETFITS, WHI and Kahleova where accessible; source records and abstracts supported additional studies. No individual-level data were reanalysed. Interpretation considered duration, sample size, measurement error, adherence, differing dietary components and whether the result directly addressed the EPK claim.

Principal gaps. No included trial compares this EPK implementation with a fixed resting-equation x PAL strategy. No trial validates its five-day algorithm. No identified trial establishes 15% as a universal growth requirement or 20% fat as a universal efficacy threshold. Evidence supporting low-fat interventions mostly concerns weight or metabolic outcomes rather than adequate fuelling under EPK maintenance and surplus prescriptions.

Research question. Compared with a prespecified resting-expenditure x PAL estimate, does wearable-informed EPK improve energy adequacy, adherence and functional outcomes when dietary composition and support are held comparable? Separately, does a diet at 20% fat or less improve these outcomes, and which surplus, if any, provides the best balance of lean-tissue gain, performance and fat gain?

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