SUNFLOWER MEAL IN BROILER DIETS: A DOSE–RESPONSE META-ANALYSIS OF INCLUSION RATE, RESIDUAL FIBRE, AND BIRD AGE

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Sunflower meal offers a route to diversify the protein ingredients used in broiler diets, but recommendations expressed as a single inclusion ceiling obscure variation in meal composition, formulation and exposure history. This exploratory dose–response meta-analysis used performance data from three independent broiler experiments covering days 22–42. Fifteen treatment arms, representing 80 pens and 2,176 initially allocated birds, provided 12 sunflower-meal versus control contrasts. Correlated log response ratios were summarised by within-study generalised least-squares dose slopes and pooled using restricted maximum likelihood with modified Knapp–Hartung confidence intervals. Variances in two studies were reconstructed from reported coefficients of variation under an explicit residual-variance assumption. Pooled linear projections per 10 percentage points of inclusion corresponded to +1.04% for body-weight gain (95% confidence interval −5.80% to +8.37%), −0.25% for feed intake (−1.94% to +1.47%) and −1.15% for feed conversion ratio (−7.11% to +5.20%). Growth and feed-conversion slopes were heterogeneous, and some within-study responses were nonlinear. These results neither demonstrate equivalence nor identify an optimum. Reported meal crude fibre occupied a narrow range, approximately 21–22%, while dose and sunflower-derived fibre were inseparable within individual experiments. Repeated starter observations and changing feeding programmes also prevented an independent estimate of age modification. The evidence therefore supports composition-specific formulation and explicit treatment of exposure history, rather than a universal maximum inclusion rate. The analysis is a reproducible, retrieval-limited quantitative synthesis, not a comprehensive systematic review or a validated three-moderator prediction model.

Key words: *broiler chicken; sunflower meal; dose response; crude fibre; feed conversion; dietary substitution; meta-analysis*

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1. Introduction

The nutritional value of sunflower meal is best understood as a property of a meal–diet–bird combination. Its protein can replace part of the contribution normally supplied by soybean meal, and the product can consequently broaden the ingredient choices available to feed formulators. However, a tonne of one sunflower meal is not nutritionally interchangeable with a tonne of another: hull retention, residual oil, processing conditions and the composition of the remainder of the diet alter what reaches the bird. Composition studies have demonstrated substantial variation in the energy and amino-acid value of sunflower meals, although some such measurements were made in adult cockerels rather than growing broilers (Villamide & San Juan, 1998). The appropriate starting question is therefore not simply how much sunflower meal a broiler can tolerate, but under what compositional and formulation conditions a specified inclusion changes a specified outcome.

A dose–response analysis could improve that question by using the entire sequence of tested inclusion levels rather than selecting the largest dose associated with a nonsignificant comparison. Nevertheless, the apparent precision of a fitted curve can exceed the information supplied by the experiments. Increasing sunflower meal often simultaneously decreases soybean meal and cereal, increases supplemental oil and crystalline amino acids, and changes the physical structure of the feed. The measured response consequently describes a formulated dietary substitution, not a purified challenge with sunflower fibre. A useful synthesis must preserve that practical interpretation while asking whether the pattern is sufficiently consistent to transfer beyond the specific formulations tested.

The distinction between ingredient inclusion and nutrient replacement is especially consequential. A treatment described as replacing 30% of soybean protein does not necessarily contain 30% sunflower meal; the actual inclusion depends on the protein

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concentrations of both ingredients and on the original formulation. Furlan et al. (2001), for example, reported replacement categories alongside substantially lower dietary inclusion percentages. Treating the category labels as ingredient dose would stretch the exposure axis and distort every estimated slope. This is not a minor reporting preference: harmonising dose from the ingredient tables is a prerequisite for making experiments quantitatively comparable.

Residual fibre is a plausible explanation for divergent responses because the hull fraction affects both nutrient dilution and feed structure. Yet crude fibre is an analytical measurement, not a single physiological substance, and it cannot be used interchangeably with neutral detergent fibre, acid detergent fibre or insoluble non-starch polysaccharides. Different analytical procedures retain different fractions of plant cell walls (Van Soest et al., 1991). A synthesis that merges those measurements into one numerical moderator may appear comprehensive while combining unlike quantities. Accordingly, this article distinguishes the fibre remaining in the meal from the fibre concentration of the complete diet and treats missing or incompatible measurements as limitations rather than opportunities for speculative conversion.

Bird age provides a second plausible explanation because digestive function changes rapidly after hatch. Direct physiological measurements document age-related changes in digestion and absorption during early growth (Noy & Sklan, 1995). However, age is often accompanied by changes in nutrient specification, feed form, inclusion rate and cumulative exposure, so a difference between a starter and a finisher result cannot automatically be attributed to digestive maturity. The informative comparison would expose birds of different ages to otherwise comparable diets and separate current exposure from prior dietary history. Most available performance reports only partly satisfy that requirement, making an explicit account of exposure history essential.

The central objective was to estimate the direction and uncertainty of inclusion-related performance changes in a small, internally comparable body of verified experiments, and then evaluate whether residual fibre and age could explain those changes. A broad, definitive regression was attractive in principle, but the recovered evidence did not provide enough independent variation or sufficiently complete uncertainty reporting to support it. The quantitative component therefore estimates simple within-study dose summaries for days 22–42, while the wider review examines mechanisms, alternative explanations and transferability. This separation addresses inclusion rate, residual fibre and bird age without suggesting that their effects have all been independently estimated.

The contribution of this article is an auditable distinction among three questions: what changes within an experiment as meal inclusion increases, what differs between experiments using different meals and formulations, and what remains unidentified. Those questions are closely related, but treating them as equivalent produces misleading nutritional recommendations. A positive response within a carefully balanced diet does not establish a universal benefit of fibre, just as a negative response in an energy-diluted diet does not establish an intrinsic defect of sunflower protein. The synthesis developed here uses the disagreement between experiments to define the conditions under which further evidence would be genuinely informative.

2. Materials and methods

2.1. Scope and evidence retrieval

Searches combined sunflower meal, sunflower seed meal, broiler, inclusion, fibre, fiber, age, performance, feed conversion and dose response, supplemented by Portuguese terms including farelo de girassol and frangos de corte. Author-and-title searches were used to recover cited experiments, and primary articles were located through journal websites, PubMed records and institutional or author repositories. This targeted web search may have missed eligible studies, including records indexed in agricultural bibliographic databases.

Bibliographic identity was checked against the primary article or its publisher or institutional record, including author order, year, journal, pagination and DOI where available. Search-engine crawl dates and repository upload dates were not treated as publication dates. However, bibliographic verification alone does not verify a numerical result, and an accessible abstract cannot substitute for a treatment table.

Quantitative extraction consequently required the relevant original table, whereas abstract-only evidence could identify a potentially relevant study or a remaining retrieval gap. This distinction was retained throughout the evidence audit to prevent plausible but unobserved numbers from entering the dataset.

The reporting principles of PRISMA 2020 favour a transparent account of identification, selection and synthesis (Page et al., 2021). The present retrieval did not supply the deduplicated database exports, complete screening history or independent human assessments needed for a conventional systematic-review flow diagram. Accordingly, the manuscript reports an exploratory meta-analysis of the verified, comparable subset, accompanied by a source-level inclusion and exclusion table. Its protocol was not prospectively registered.

2.2. Eligibility, products and experimental units

At the synthesis stage, the common days 22–42 window was selected from the retrieved evidence. The core quantitative set comprised controlled broiler feeding experiments that reported at least three dietary levels including a zero-meal control, actual meal inclusion, performance specifically during days 22–42, replicate numbers and an uncertainty statistic permitting a stated variance calculation. Body-weight gain, feed intake and feed conversion ratio were analysed separately. This common age window improved endpoint comparability but did not remove the effects of diets previously consumed by the same birds. The resulting estimand is therefore the response during days 22–42 under the experimental feeding histories, rather than the effect of initiating sunflower meal for the first time on day 22.

Sunflower cake, high-oil mechanically extracted products, whole seeds and isolated hulls were retained as mechanistic or contextual evidence rather than silently combined with the core meal series. Product names are not perfectly standardised across countries, and not every meal paper described its extraction process fully. A designation such as meal was therefore supplemented with reported oil, protein and fibre information whenever available. This approach reduces obvious product mixing without asserting a processing uniformity that the primary reports do not establish; the core results apply to the reported meal diets, not to every oilseed coproduct bearing a similar name.

The pen was the experimental unit for group-fed performance outcomes. Individual birds sharing a feeder did not constitute independent replicates of the dietary treatment, even when hundreds of birds were allocated to an experiment. This distinction is easy to lose when reconstructing variance from an SEM or CV, because using bird counts would produce spuriously narrow intervals. Replicate numbers were therefore extracted as pens per treatment, with birds per pen retained as a design descriptor. The total number of birds describes the scale of the experiments but does not determine the effective sample size of the meta-analysis.

Multiple papers with similar authors, bird numbers, age ranges, treatment structures and doses were screened for possible cohort overlap. The closely matching de Araújo reports required particular caution, while factorial experiments also raised the possibility of counting several formulations from one experiment as independent studies. Unresolved overlap was not converted into additional replication. For the core analysis, each independent experiment contributed one dose slope per outcome and phase, ensuring that extra doses and repeated observations did not artificially increase the number of independent studies.

2.3. Extraction, exposure definitions and data checks

Extraction recorded the publication, location of the source table, genotype and sex when reported, pen replication, exposure days, actual ingredient inclusion, meal composition, complete-diet fibre, formulation basis, treatment means and dispersion statistics. Means were preserved in their reported units; relative effects subsequently removed the distinction between grams and kilograms without changing the underlying outcome definition. However, a cumulative gain and a phase-specific gain remain different estimands even when both are expressed in grams. Each outcome was therefore linked to its exact measurement interval before any numerical comparison was calculated.

Dietary dose was expressed as percentage of the complete feed, with 10 percentage points corresponding to 100 g meal/kg diet. Sunflower-derived fibre can, when moisture bases match, be calculated by multiplying the meal inclusion fraction by its fibre concentration. That contribution is not the same as the change in total dietary fibre because the displaced ingredients also contain fibre. For example, adding 200 g/kg of a meal containing 220 g fibre/kg contributes 44 g fibre/kg diet before accounting for displacement. This arithmetic illustration clarifies the exposure definition; it is not an additional trial observation or a universal composition assumption.

Reported means were checked for plausible units, consistent treatment ordering and agreement with the experimental design. Where available, feed intake divided by weight gain was compared with reported FCR, recognising that mortality correction and averaging pen ratios can prevent exact equality. Phase sums were also compared with cumulative totals. Such checks identify questions, not permission to rewrite a paper: differences that exceeded a simple rounding explanation were documented, while the original values were retained or the affected report was excluded. No missing treatment mean, uncertainty measure, fibre concentration or replication count was fabricated.

Two core studies supplied outcome-specific CVs rather than directly reported arm standard deviations. Under the conventional interpretation that CV equals 100 times the residual standard deviation divided by the grand mean, a common residual SD was reconstructed as the arithmetic mean of the balanced treatment means multiplied by CV/100; the arm-mean SE was that SD divided by the square root of pens per arm. This is an explicit model-based reconstruction, not a recovered measurement. Because the papers did not fully document every covariance convention, the quantitative results are conditional on this interpretation, and sensitivity analyses altered the assumed sampling variances.

The third core study reported a common SEM for each outcome and phase, which was treated as the standard error of a treatment mean. The distinction between a mean SE, an SD, a standard error of a difference and a statistic pooled across all animals is fundamental; they cannot be exchanged simply because each is presented after a plus-or-minus symbol. Reports with ambiguous dispersion were retained in the evidence review but did not acquire an assumed inverse-variance weight. This conservative boundary sacrifices sample size while protecting the meaning of the calculated intervals.

2.4. Effect sizes and two-stage synthesis

For each nonzero dose, the effect size was the natural logarithm of the treatment mean divided by the same-study, same-phase control mean. The log response ratio accommodates positive continuous outcomes and gives a direct relative interpretation after exponentiation (Hedges et al., 1999). A positive value denotes greater gain or intake, whereas a positive FCR value denotes more feed required per unit gain. These directions were kept explicit because an apparently favourable negative number has different practical meanings for intake and efficiency; lower intake alone is not evidence of an improvement.

Shared controls were accounted for rather than replicated as independent observations. The approximate variance of a contrast was the sum of treatment and control mean variances divided by their respective squared means. For two contrasts sharing one control, the corresponding covariance was the control mean variance divided by the squared control mean, following the correlated-design framework discussed by Lajeunesse (2011). This construction assumes independent treatment errors conditional on the original design; it does not recover unreported block or longitudinal covariance. Its purpose is to retain the dependence that can be reconstructed, while stating the dependence that remains uncertain.

Within each experiment, generalised least squares fitted a linear log response to dose measured in units of 10 percentage points, with the curve constrained to pass through zero at zero inclusion. In matrix notation, the slope was $b = (x'V^{-1}x)^{-1}x'V^{-1}y$, with estimated variance $(x'V^{-1}x)^{-1}$, where y contains the log ratios and V their sampling covariance matrix. The zero constraint reflects the identity of a control with itself, not a claim that all biological responses are linear. Each fitted coefficient is therefore a summary of the observed dose range, and

marked departures from linearity restrict its use as a predictive curve.

The three study slopes were combined with a random-effects model, estimating between-study variance by restricted maximum likelihood. A modified Knapp–Hartung interval used a Student t distribution with two degrees of freedom and a variance adjustment bounded below by one, reflecting the small number of independent experiments (Röver et al., 2015). This procedure widens uncertainty relative to a simple normal interval but cannot repair biased study selection or uncertain variance inputs. Pooled log slopes and interval limits were converted to percentage changes using 100 times the exponential minus 100; they were not interpreted as percentage-point changes in FCR itself.

Between-study inconsistency was described using Q and I^2 calculated from the independent study slopes, rather than from the 12 correlated treatment contrasts. These measures characterise the disagreement in this particular set and are themselves unstable with only three studies (Higgins & Thompson, 2002). A large I^2 does not identify its biological cause, while a small I^2 does not prove consistency of complete dose curves. Accordingly, heterogeneity measures were read alongside the raw dose responses and the residual lack of fit within each study, not used as an automatic decision rule for pooling.

2.5. Sensitivity, bias and interpretive boundaries

Sensitivity analyses omitted each study in turn, doubled or halved the assumed mean sampling variances, and repeated the same analysis for the reported starter period. Starter and grower results were never stacked as independent observations because the same pens contributed both phases. The analyses also did not test the difference between age-specific pooled slopes without the missing cross-phase covariance. These choices allow the reader to inspect whether conclusions depend on one experiment or one variance convention, while avoiding a falsely precise age interaction derived from repeated measurements.

No multivariable fibre-by-dose-by-age regression, spline optimum, publication-bias test or numerical noninferiority claim was fitted. Such analyses can be useful in sufficiently informative datasets, but three independent experiments cannot support the requested interactions simply because they contain many treatment rows. Cluster-robust methods and small-sample corrections address particular variance problems; they do not create independent variation in a missing moderator (Hedges et al., 2010; Tipton, 2015). The absence of those estimates is therefore an evidential finding, not a model-selection failure concealed by more elaborate mathematics.

Risk appraisal focused on allocation, the experimental unit, formulation comparability, outcome completeness, missing variance, duplicate reporting and internal consistency. Animal-study risk-of-bias guidance provides relevant domains, although it was not converted here into a purportedly validated poultry-specific quality score (Hooijmans et al., 2014). Incomplete reporting was distinguished from demonstrated poor conduct, and no undisclosed human double-screening process was claimed. The extraction dataset, calculation script and results accompany the manuscript so that a reader can change an assumption, correct a source value or add a newly recovered trial without reconstructing the analysis from prose.

3. Results

3.1. Verified quantitative evidence and its limits

The core dataset contained three experiments, 15 treatment arms and 80 pens, representing 2,176 birds at initial allocation. Twelve nonzero-dose contrasts were available for each of the three outcomes during days 22–42. Actual dietary inclusion extended to 21.15% in Furlan et al. (2001), 20% in Tavernari et al. (2009) and 21% in Nassiri Moghaddam et al. (2012). These are substantial multi-arm experiments, but their combined bird count does not overcome the fact that only three independent meal–formulation settings were available. The source and design summary in Table 1 therefore provides the relevant context for the pooled estimates.

Table 1. Core experiments and the days 22–42 exposure window.

Study	Meal inclusion (%)	Pens × birds per arm	Meal crude fibre (%)	Dispersion
Furlan et al. (2001)	0, 4.23, 8.46, 12.69, 16.91, 21.15	4 × 50	21.73	Residual CV; SE reconstructed
Tavernari et al. (2009)	0, 5, 10, 15, 20	8 × 20	22.37	Residual CV; SE reconstructed
Nassiri Moghaddam et al. (2012)	0, 7, 14, 21	4 × 11	21.20	Reported treatment-mean SEM

Sources: original diet and performance tables in the cited articles. Fibre percentages are retained as reported; no dry-matter conversion was imposed. All three cohorts had prior starter-phase exposure. CV = coefficient of variation; SE = standard error; SEM = standard error of a mean.

The numerical data reveal disagreement before any model is fitted. At the highest tested grower dose, reported gain relative to the respective control was 4.89% lower in Furlan, 1.45% higher in Tavernari and 8.10% higher in Nassiri Moghaddam. These are calculated comparisons of published means, not adjusted causal effects or equivalence tests. Their opposing directions cannot be resolved by selecting one preferred study or quoting the largest nonsignificant dose from each. Instead, they establish that the common ingredient name and similar nominal inclusion ranges conceal materially different experimental responses.

The meal crude-fibre values reported in the core papers were 21.73%, 22.37% and 21.2%, respectively. Complete grower-diet crude fibre nevertheless rose across different ranges: 3.15–6.21%, 2.74–6.94% and 2.61–6.78%. The meal values are reported on the papers' stated or incompletely specified compositional bases and were not converted to a supposedly common dry-matter basis. Their narrow spread, combined with only three independent batches, provides little information about the response to a genuinely low-fibre meal. Thus, the core evidence evaluates conventional fibrous meal formulations; it cannot validate a contrast between broadly defined low- and high-fibre products.

3.2. Dose summaries for gain, intake and feed conversion

For body-weight gain, the pooled linear summary corresponded to +1.04% per 10 percentage-point higher inclusion, with a 95% confidence interval from −5.80% to +8.37%. The individual projected changes were −1.39%, +0.70% and +4.41% for the three studies in chronological order, and I^2 was 85.75%. The small positive pooled centre is therefore compatible with both a meaningful reduction and a meaningful increase in growth. Its appropriate interpretation is a poorly determined average of heterogeneous linear projections, rather than evidence that adding 10% meal improves gain by approximately 1% in a new formulation.

Feed conversion showed a similarly uncertain average. The pooled projected change was −1.15% per 10 percentage points, with a 95% confidence interval from −7.11% to +5.20%; lower FCR would indicate improved efficiency. Study-specific projections ranged from +0.95% through −0.85% to −4.04%, and I^2 was 88.64%. The favourable pooled sign consequently depends on averaging responses with different directions and magnitudes. Because neither an acceptable-loss margin nor a target formulation was specified, this interval cannot be translated into a statement that sunflower meal is safe, equivalent or economically preferable at a particular dose.

Feed intake produced a pooled projection of −0.25%, with a 95% confidence interval from −1.94% to +1.47%, and estimated I^2 of zero. This apparently more consistent slope does not mean that all intake responses were flat or alike: within-study deviations from linearity were appreciable in two experiments. A curve that rises at intermediate doses and returns toward its control can have a small linear projection, just as several differently shaped curves can share similar slopes. The intake result therefore shows why agreement in one summary coefficient must not replace inspection of the treatment means.

Table 2. Pooled linear projections per 10 percentage points of sunflower-meal inclusion.

Age (days)	Outcome	Change (%)	95% CI (%)	I^2 (%)
22–42	Body-weight gain	+1.04	−5.80 to +8.37	85.75
22–42	Feed intake	−0.25	−1.94 to +1.47	0.00
22–42	Feed conversion ratio	−1.15	−7.11 to +5.20	88.64
1–21	Body-weight gain	+0.85	−5.16 to +7.24	85.20
1–21	Feed intake	−0.49	−4.18 to +3.35	56.45
1–21	Feed conversion ratio	−0.88	−5.62 to +4.11	87.31

Original calculations from the extracted data. Each row pools three independent study slopes using REML and modified Knapp–Hartung confidence intervals (two degrees of freedom). Lower FCR is favourable. Age groups contain the same cohorts and are not independent; no age-interaction test was performed. Projections describe fitted trends, not observed contrasts at 10% inclusion.

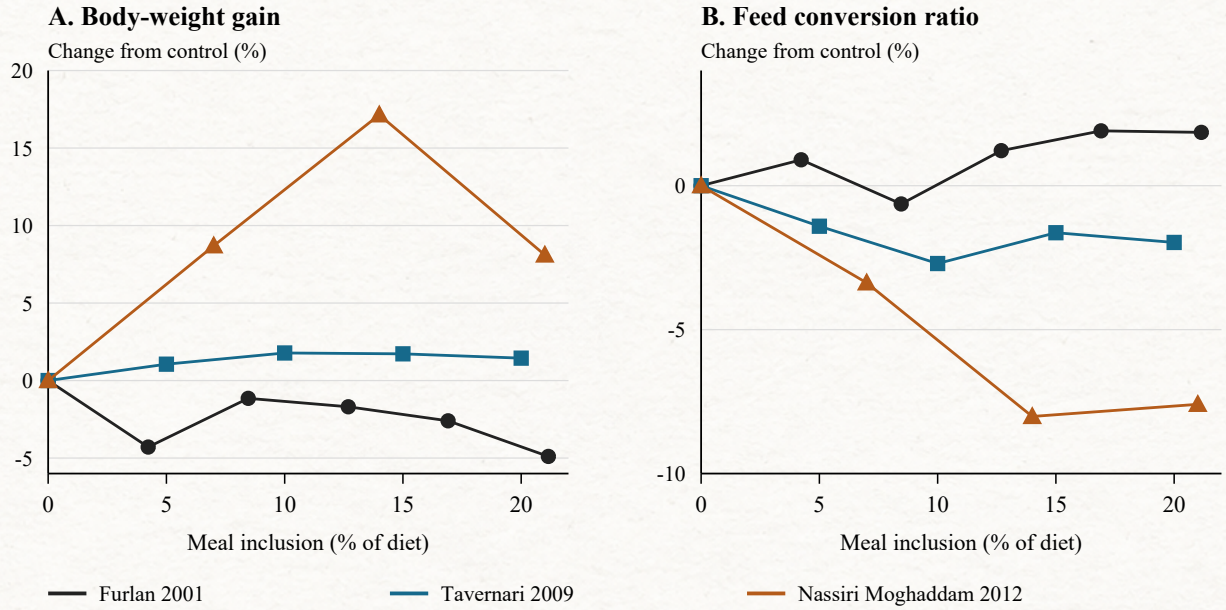


Figure 1. Observed days 22–42 treatment-mean changes relative to each study's control. Lines connect observed doses for visual guidance; they are not fitted biological curves. No confidence intervals are shown. Lower FCR denotes improved feed efficiency. Calculated from the original core treatment means.

3.3. Nonlinearity and sensitivity

The clearest departure from a simple linear gain response occurred in Nassiri Moghaddam et al. (2012), where the 14% treatment exceeded both the control and the 21% treatment. The calculated residual lack-of-fit statistic for the linear gain model was 18.66 on two residual degrees of freedom, conditional on the reported SEM. That pattern makes an interior response peak plausible within this particular experiment, but one observed peak does not identify a transferable optimum. Fitting a pooled quadratic or spline would assign biological importance to curvature primarily supported by one study and would conceal how little independent replication supports its location.

The generalised least-squares slopes also require a mathematical qualification. They are equivalent to weighted regressions of the arm log means with a study-specific intercept, and the shared-control covariance can yield a projected trend that differs from a simple average of treatment-to-control percentages. A small positive projection can therefore coexist with all observed treatment means lying below the control if the treated means rise across their dose range. This is a consequence of the chosen linear summary, not a data error. Presenting observed contrasts alongside slopes prevents a reader from mistaking a fitted coefficient for the

actual effect observed at an untested 10% inclusion.

Removing one study at a time changed the pooled gain projection from −0.35% to +2.34%, and the FCR projection from −2.28% to +0.09%. Every two-study modified Knapp–Hartung interval included both favourable and unfavourable changes, often over a very wide range. In particular, omitting Nassiri Moghaddam left estimates close to zero, but this does not vindicate the other two experiments as a definitive subset. The sensitivity demonstrates dependence on the recovered evidence and the imprecision of a two-study synthesis; it is not an invitation to choose whichever subset supports a preferred feeding recommendation.

Doubling or halving all assumed sampling variances did not create a precise conclusion. The gain projection ranged from +0.85% to +1.13% and the FCR projection from −0.95% to −1.24%, with intervals still spanning both directions. These are global variance sensitivities, not a full propagation of uncertainty about the reconstructed CVs, and they cannot correct reporting bias or an incorrect covariance assumption. Their contribution is narrower: the principal inference of uncertainty is not dependent on one arbitrary overall variance scale.

The separately fitted starter-period summaries were +0.85% for gain, −0.49% for intake and −0.88% for FCR per 10 percentage points, again with intervals crossing zero. Comparing those numbers directly with the grower estimates would be tempting, but the same pens contributed both periods and some inclusion schedules changed. Moreover, the two summaries may average different portions of a nonlinear response. These results support the statement that no clear age-specific pooled advantage was demonstrated in this dataset; they do not support a statistical claim that age has no modifying effect.

3.4. Evidence outside the core model

Several eligible reports expanded the biological scope without meeting the requirements of the core analysis. Pinheiro et al. (2002) compared different introduction histories, Mbukwane et al. (2022) reported cumulative outcomes under changing phase inclusions, and Gerzilov and Petrov (2022) studied high-protein meal in programmes whose reference group already contained sunflower meal. These designs address useful practical questions, but assigning a single final inclusion value to their entire growth record would change the intervention being analysed. They were consequently retained as evidence about feeding programmes and exposure history, with their inferential limits made explicit in Table 3.

Internal reporting issues were handled at the outcome level. In Nassiri Moghaddam, the extracted phase-specific means and SEM were directly readable, while discrepancies arose when phase totals were compared with cumulative outcomes; the phase observations were retained with an explicit sensitivity excluding the study. The W. A. G. de Araújo et al. (2014) report supplied a different age window and contained printed cell values that conflicted with its accompanying regression descriptions, so it was not used to enlarge the core model. This distinction does not imply that either experiment was improperly conducted. It explains why an independently verified table can still warrant author clarification before stronger quantitative conclusions are drawn.

Table 3. Selected eligible evidence retained outside the core dose model.

Report	Relevant contribution	Reason for separate treatment
Pinheiro et al. (2002)	Introduction at different ages	Cumulative outcomes and distinct exposure histories; late-phase variance cannot be recovered by subtracting SEMs.
Senkoylu & Dale (2006)	High-oil product; feed form	Mechanically extracted high-oil product; reported ± values do not identify SD versus SEM.
Raza et al. (2009)	Meal, fibre and enzyme formulations	Only whole-period performance recovered; dose and dietary fibre co-vary; numerical reporting questions remain.
L. F. Araújo et al. (2011)	Amino-acid formulation at 0/15%	Only one nonzero dose; FCR uncertainty absent and some composition entries need clarification.
Kalmendal et al. (2011)	High-fibre sunflower cake	Different product class and age window; verified performance table not recovered for pooling.
W. A. G. de Araújo et al. (2014)	Four meal doses with formulation treatments	Days 21–42; printed cells conflict with associated regression descriptions; possible overlapping report.
Horvatovic et al. (2015)	Enzymes, viscosity and staged doses	Dose changes between phases; marginal means and uncertainty convention require further clarification.
Mbukwane et al. (2022)	Enzymes and phased feeding	Eligible January publication; cumulative outcomes accompany a changing high-inclusion programme.
Gerzilov & Petrov (2022)	High-protein meal programmes	Eligible February publication; independent pen replication unestablished, FCR uncertainty absent, nonzero-meal reference.

This is an evidence-applicability table, not a complete screening flow diagram or a numerical quality ranking. A reason for exclusion from this model is not a judgement that the study has no scientific value.

The February 2022 high-protein study is particularly instructive because it appears, at first glance, to expand the evidence to much higher inclusion. Gerzilov and Petrov (2022) described seven groups of 30 birds, but did not establish independent replicate pens for each dietary programme, and its FCR table lacked a dispersion statistic. The body-weight SEM therefore could not safely be treated as a pen-level dietary-treatment SEM. The study remains relevant as an account of a feeding system, but incorporating its apparent precision into an inverse-variance model would risk giving individual birds the status of independently randomised diets.

4. Biological interpretation

4.1. Residual fibre as exposure and explanation

Residual fibre is central to interpreting sunflower meal because inclusion rate describes the quantity of an ingredient, whereas digestive responses arise from the properties of the resulting diet. A higher inclusion can increase structural carbohydrate intake while changing protein, fat and mineral supply simultaneously. Nevertheless, “fibre” is not a single analytical or physiological entity: crude fibre, neutral detergent fibre, acid detergent fibre and non-starch polysaccharides represent different fractions, and laboratory procedures influence their recovery (Van Soest et al., 1991). Consequently, a common numerical fibre scale cannot be created simply by relabelling these measurements. The useful synthesis is to retain analytical identity, distinguish ingredient from complete-diet concentrations, and treat unreported fibre as missing information rather than assigning a typical feed-table value that conceals variation among meal batches.

Structural fibre can support digestive function, making a uniformly adverse fibre hypothesis biologically incomplete. In young broilers, moderate oat-hull or sugar-beet-pulp supplementation improved performance and nutrient retention under particular combinations of basal diet and fat source (Jiménez-Moreno, González-Alvarado, González-Serrano, et al., 2009). A separate experiment found that additional hulls affected gizzard acidity and nutrient retention differently in rice- and

cornbased diets, with some benefits greater at younger ages (Jiménez-Moreno, GonzálezAlvarado, Lázaro, & Mateos, 2009). These interventions do not establish an equivalent response to sunflower meal, whose inclusion simultaneously replaces other nutrients. They instead show why the direction of a sunflowermeal slope may depend on the starting diet: correcting inadequate physical structure and imposing excessive nutrient dilution are plausible processes operating in different exposure ranges.

Gizzard responses can help distinguish these processes, but organ size should not be treated as proof of improved feed utilisation. The gizzard contributes to particle reduction, digestive mixing and regulation of passage, and responds to dietary structure (Svihus, 2011). Yet an increase in relative gizzard weight may reflect increased muscular work, altered body weight, or both; it does not reveal whether absorbed nutrients subsequently support growth. Likewise, changes in intestinal morphology need interpretation alongside intake, digestibility and performance rather than an automatic designation as beneficial or harmful. A mechanistic account becomes persuasive when several measurements form a coherent sequence. Without those measurements, the meta-analysis can identify a performance association consistent with structural effects, while leaving the actual digestive pathway unresolved.

4.2. Processing and the nutrient package

Processing changes the meaning of a sunflower-meal dose by changing what accompanies its protein. Dehulling can reduce the proportion of hull material, while extraction determines how much residual oil remains. These changes suggest that a high-protein meal may permit greater dietary inclusion than a more fibrous product. However, a protein concentration is an incomplete specification of processing quality, and products described as meal, cake or expeller are not necessarily nutritionally interchangeable. Kalmendal et al. (2011), for example, studied high-fibre sunflower cake, whereas Senkoylu and Dale (2006) evaluated a high-oil extraction product. Combining such products without description would convert processing differences into unexplained dose heterogeneity. The primary inference should therefore refer to the documented product class, with broader sunflower coproduct comparisons presented as a separate question.

Residual oil can modify a practical response even when fibre remains substantial. The high-oil product investigated by Senkoylu and Dale (2006) illustrates why sunflower ingredients cannot be ordered nutritionally by fibre concentration alone. Nevertheless, residual oil is not a universal correction for fibre-related constraints: the complete diet must still provide suitable amino-acid concentrations, intake capacity and physical form. When additional oil is used to balance a fibrous diet, the resulting performance reflects the supplemented formulation rather than the intrinsic feeding value of the meal in isolation. Thus, oil should be recorded both in the meal and in the formulated diet. A favourable response in an energy-balanced treatment supports that formulation strategy; it does not establish that the same inclusion would perform similarly without the compensating ingredients.

Protein replacement also requires attention to digestible amino acids rather than crude protein alone. Villamide and San Juan (1998) showed that sunflower-meal composition was associated with metabolizable energy and amino-acid utilisation in adult-cockerel assays, with differences in digestible lysine contribution among the tested meals. These findings support batch characterisation, but adult-rooster true digestibility and energy measurements are not identical to ileal digestibility or apparent energy values in growing broilers. Nor does equal dietary crude protein ensure equal available lysine or a balanced supply of other indispensable amino acids. The defensible synthesis is to distinguish measured values from formulation assumptions and to interpret a dose response against the amino-acid system actually used. Otherwise, a shortage associated with substitution may be misidentified as a direct fibre effect.

4.3. Bird age and the timing of exposure

Age is a credible modifier because digestive function changes during early growth. Noy and Sklan (1995) documented age-related changes in enzyme secretion, passage and

nutrient digestion during the first three weeks after hatch. Yet developmental change does not imply a simple rule that fibre is always more detrimental in younger birds; the hull-supplementation experiments discussed above demonstrate conditional benefits in young chicks. Age therefore changes both the potential constraint and the opportunity for a response. A young bird may be sensitive to an inadequately balanced, bulky diet while still benefiting from suitable structural material. The relevant hypothesis concerns an interaction among age, formulation and fibre characteristics, rather than a universal maturity threshold beyond which any sunflower-meal inclusion becomes acceptable.

The age recorded for an outcome is also a property of the exposure design. A measurement at market age may summarise continuous feeding from hatch, supplementation beginning in the grower phase, or recovery after an earlier treatment. These histories can produce the same endpoint age but represent different biological questions. Conversely, a cumulative growth response can mask opposing changes during separate phases. The synthesis should therefore retain starting age, ending age and treatment duration, and identify whether outcomes are phase-specific or cumulative. Repeated observations from the same pens cannot be counted as independent demonstrations of an age effect. Where exposure histories are too heterogeneous or covariance is unavailable, age-stratified description is more informative than a coefficient whose apparent precision depends on treating correlated records as new experiments.

Older-bird tolerance should consequently be expressed as a conditional observation rather than a general feeding recommendation. Grower-phase studies can show how birds of a specified age responded to their tested formulations, but they cannot establish the starter response that the same product would have produced. Comparisons with separate starter experiments also introduce differences in genotype, nutrient requirements, management and meal quality. Even a consistent contrast between age groups would therefore require cautious causal interpretation. For the exploratory synthesis, age supplies a framework for judging transferability and defining the population to which the estimates apply. Estimating a numerical age interaction should await sufficient independent studies with overlapping meal characteristics and inclusion ranges, rather than relying on the number of treatment means available.

4.4. Enzymes as conditional interventions

Exogenous enzymes offer a plausible means of changing the utilisation of sunflower-containing diets, but their expected action must be linked to the substrate. A carbohydrase mixture and a phytase-containing mixture address different aspects of feed utilisation; neither can be assumed to remove every constraint associated with hull-rich material. Mushtaq et al. (2009) explicitly varied sunflower-meal inclusion, digestible lysine concentration and an enzyme combination, illustrating that enzyme responses belong within a formulation context. However, a response to a mixture does not identify which activity was responsible or whether the benefit arose from sunflower components rather than other ingredients. Enzyme treatment is consequently a defined co-intervention, not a generic correction factor that can be applied to every sunflower-meal dose.

Different outcomes can also respond differently to the same enzyme treatment. Tavernari et al. (2008) reported improved apparent phosphorus and calcium metabolizability with an enzyme complex without a significant improvement in dietary nitrogen-corrected apparent metabolizable energy. This separation cautions against translating evidence for mineral release directly into an assumed gain in growth or feed conversion. Conversely, an unchanged performance outcome does not demonstrate complete biochemical inactivity, particularly when the basal diet already supplies adequate nutrients. The interpretation should connect the claimed mechanism to the measured response: nutrient availability, intake and growth are related but distinct endpoints. Such distinctions explain how apparently conflicting enzyme findings can coexist without requiring either a universal efficacy claim or a universal dismissal.

Enzyme formulation credits further determine the comparison being made. Adding an enzyme to an unchanged adequate diet evaluates a different question from using an enzyme to compensate for deliberately reduced

nutrient specifications. In the second design, a satisfactory response may represent successful replacement of nutrients that were removed, rather than superiority over an adequate unsupplemented diet. Failure to distinguish these comparisons can obscure both efficacy and economic relevance. Extraction should therefore preserve enzyme activities, inclusion, diet formulation strategy and, when reported, recovered activity after feed manufacture. Where these details are missing, subgroup labels such as “with enzyme” cannot adequately represent the intervention. A parsimonious synthesis should describe this uncertainty explicitly instead of allowing a heterogeneous enzyme indicator to absorb unexplained dietary differences.

4.5. Causal interpretation and practical transfer

A sunflower-meal dose slope is most directly interpreted as the response to a sequence of formulated diets. Within a randomised experiment, that sequence can support a causal comparison of the assigned diets, subject to the study's design and conduct. It does not isolate the causal contribution of fibre, lysine, residual oil or pellet structure when these change together. Across experiments, the distinction becomes sharper because meal composition may correlate with country, processing technology and experimental period. The present mechanistic interpretation therefore concerns explanations consistent with the observed dietary responses, rather than effects independently estimated for every proposed pathway. This boundary preserves practical value: a formulated-diet effect can be useful for feed decisions even when the contribution of each constituent remains unidentified.

5. Implications for formulation, interpretation and future research

5.1. What an inclusion recommendation would need to specify

An inclusion recommendation becomes useful when it identifies the product, diet, birds, outcome and acceptable degree of uncertainty. A statement such as “use up to 20%” leaves all five unresolved: it could concern a low-oil fibrous meal, a dehulled high-protein fraction or a mechanically extracted cake, and it could refer to gain, FCR or merely a nonsignificant test. Yet refusing all practical interpretation would also waste the experiments. The defensible recommendation is conditional: evaluate the proposed inclusion within a nutrient-balanced formulation using measured meal composition, then judge the resulting performance against a clearly stated production objective.

The present synthesis does not establish a maximum inclusion rate, but it does identify why a universal ceiling would be misleading. The core experiments covered overlapping doses and similar meal crude-fibre percentages, yet their growth responses diverged. That discrepancy means that dose alone leaves important variation unexplained; it does not prove that one unmeasured factor, such as lysine or pellet quality, is responsible. A formulation decision should therefore treat the reported experimental doses as tested conditions to investigate, rather than as guaranteed boundaries separating acceptable and unacceptable performance.

Feed cost per kilogram of live-weight gain provides a more useful economic target than ingredient price alone. In a simplified calculation, diet cost multiplied by FCR approximates feed cost per unit gain, so cheaper feed can offset some deterioration in feed conversion. However, that calculation omits mortality, growth rate, housing occupancy, processing yield and the cost of reformulating energy and amino acids. Consequently, an economically attractive meal inclusion must be assessed with the whole production system and local prices; the current biological meta-analysis supplies neither a contemporary price comparison nor an economic optimum.

The same discipline applies to environmental claims. Using a coproduct can appear to improve resource use, especially if it displaces an ingredient associated with substantial upstream burdens. Nevertheless, environmental outcomes depend on allocation between sunflower oil and meal, transport, added oil and amino acids, feed conversion, manure production and the alternative use of the coproduct. No life-cycle dataset was assembled here, and a small change in FCR cannot by itself establish a change in total environmental impact. The nutritional results can inform a later environmental assessment, but they do not warrant an unmeasured sustainability benefit.

An operational decision can still be structured without pretending that the literature supplies a universal threshold. A feed mill can define the meal lot chemically, formulate several feasible inclusions, and compare their expected nutrient adequacy and ingredient costs before testing a limited set under its own conditions. The limitation is that laboratory composition alone does not capture every physical or biological property of the meal. For that reason, analytical quality control and replicated feeding validation should be viewed as complementary stages: one establishes what enters the feed, while the other measures the consequences in the intended birds and production system.

5.2. Why residual fibre was not an estimable independent moderator

A central result is the lack of independent information about fibre, even though fibre is reported in all three core meal descriptions. Within a study using one batch, multiplying inclusion by that batch's fibre concentration generates a variable exactly proportional to inclusion. A regression containing both quantities cannot distinguish their effects within that study. Differences among batches could, in principle, identify modification, but the present batch range is narrow and coincides with many other study differences. The analysis therefore identifies a structural limitation of the evidence, not evidence that fibre is irrelevant.

This limitation would persist even if a statistical program returned numerical coefficients. With three studies, a coefficient for meal fibre would largely compare one experimental setting with another, incorporating differences in genotype, country, protein balance, added oil, housing and measurement practice. A stable-looking regression line could consequently reflect those co-varying features. The appropriate interpretation of any such coefficient would be an ecological association requiring further validation, not a causal estimate of the effect of removing hulls. Recognising that boundary is more scientifically informative than attaching a precise fibre threshold to an underidentified model.

Complete-diet fibre offers a related but different exposure. It is closer to the bird's consumed mixture, yet it is also a result of formulation choices and ingredient displacement. Adjusting for it statistically may remove part of the very intervention that the total-diet question seeks to measure, while leaving other compositional changes unaccounted for. The analysis must therefore decide whether it aims to estimate the practical effect of replacing ingredients or the mechanistic effect of fibre at fixed nutrient supply. Those are legitimate but distinct estimands, and one regression cannot silently answer both.

5.3. Designs that could separate dose, fibre and age

A more informative experiment would compare at least two chemically characterised meal fractions across overlapping inclusion levels within the same experimental setting. Using material derived from a common seed source would help reduce unrelated batch variation, while explicit measurements of residual oil, protein, amino acids and fibre fractions would describe the resulting products. Even this design would not isolate fibre perfectly because dehulling changes several nutrients simultaneously. Its advantage would be to make those changes visible and to support a planned comparison between total product effects and hypotheses about the mechanisms responsible.

Age should be investigated by distinguishing chronological stage from exposure duration. One possible design would compare birds continuing an assigned meal diet with birds first introduced to the same diet at a later stage, while maintaining contemporary controls and measuring phase-specific outcomes. Such a design is more demanding than recording final body weight, and carry-over effects would still require analysis. Nevertheless, it could distinguish an apparent improvement due to digestive maturity from one due to prior adaptation, or identify whether early growth differences influence later intake and efficiency. The current literature cannot make those distinctions reliably from cumulative means alone.

Replicate allocation should be based on the pen-level effect the experiment intends to detect. Adding more birds to an existing pen can improve the stability of a pen mean, but it does not add an independent dietary replicate.

Conversely, increasing the number of pens can provide information about between-pen variability even when total bird numbers change little. A future trial should therefore justify its number of independent pens, record allocation and blocking procedures, and report the analysis unit explicitly. This would improve both the original inference and the usefulness of the experiment in later synthesis.

Reporting should allow another investigator to reconstruct the main comparisons without guessing. At minimum, each phase needs the number of pens analysed, actual dietary inclusion, arm means, a clearly defined SD or SEM, mortality handling and the distinction between calculated and analysed nutrient composition. Repeated phases also need sufficient covariance information or accessible pen-level records to support age comparisons. This requirement is more demanding than listing significance letters, but the additional information is modest compared with the cost of a feeding experiment. It converts a locally interpretable paper into reusable quantitative evidence.

5.4. Requirements for a stronger meta-analysis

The most immediate extension is systematic retrieval of the missing eligible full texts and clarification of ambiguous tables. In particular, the verified bibliographic records for Musharaf (1991) and Rama Rao et al. (2006) identify potentially relevant experiments whose full treatment data were not recovered for the present analysis. Their absence may affect the pooled result, but an abstract cannot establish the direction or size of that effect. Obtaining actual inclusion rates, performance means, pen counts and uncertainty from those reports is preferable to extracting approximate values from secondary summaries or treating replacement percentages as dietary dose.

A larger synthesis should preserve an evidence hierarchy rather than merely accumulating rows. Clearly defined, independently replicated meal trials with recoverable variance could form the principal dataset; products with different residual oil or uncertain replication could be analysed separately, and incomplete reports could remain in the narrative evidence map. The limitation is that excluding poorly reported experiments may itself induce selection bias. A transparent analysis should therefore show which evidence is omitted and why, and explore whether conclusions differ when credible alternative variance or product classifications are applied.

With enough independent studies and overlap in exposure, a multilevel dose–response model could examine nonlinear inclusion, meal-fibre modification and age. However, the necessary sample size is determined by the distribution of studies across combinations of those moderators, not by the total number of birds or treatment contrasts. Ten studies all using similar fibre at the same age would still provide little evidence for a fibre-by-age interaction. Model complexity should therefore follow an explicit assessment of covariate coverage, identifiability and sensitivity, with simple summaries retained when the richer model is not supported.

5.5. Strengths and limitations of the present synthesis

The principal strength is traceability from source table to extraction, effect calculation and interpretation. The analysis distinguishes dietary inclusion from protein replacement, accounts for a shared control, uses pens rather than birds for precision, and exposes the uncertainty associated with a small number of experiments. These safeguards do not make the evidence complete. They make it possible to see exactly which conclusion follows from the recovered data and which would require additional assumptions, thereby supporting correction and extension instead of presenting an apparently definitive but irreproducible pooled effect.

The principal limitations are targeted rather than exhaustive retrieval, only three core experiments, conditional reconstruction of two studies' variances, nonlinearity, heterogeneous formulations and incomplete reporting of composition and covariance. The common age window improves comparability but narrows the quantitative question and does not remove dietary history. Because these restrictions were established during evidence retrieval rather than through a prospectively registered protocol, selective analytical choices remain a concern. The raw data, stated exclusions and sensitivity results reduce that concern but cannot eliminate it; the findings should be treated as a transparent

exploratory analysis awaiting a broader evidence base.

6. Conclusions

The verified days 22–42 evidence does not support a single reliable direction of performance change as sunflower-meal inclusion rises. The pooled linear projections for growth and FCR were small relative to their uncertainty, and the underlying studies differed substantially. This is not proof that inclusion has no effect: the intervals permit practically important changes, and individual dose curves contain responses obscured by an average slope. The appropriate conclusion is that these experiments do not establish a universal inclusion ceiling, a pooled optimum or equivalence to the control diet.

Residual fibre and bird age remain central to the nutritional question, but their independent effects were not identified by this dataset. Meal fibre was narrowly distributed and inseparable from dose within a batch, while phase comparisons were entangled with repeated pens, prior exposure and reformulation. The synthesis therefore supports product-specific characterisation, careful balancing of the complete diet and replicated evaluation at the intended age. A stronger quantitative recommendation will require independent variation in meal quality and age, clearer reporting and fuller recovery of the eligible literature, rather than a more elaborate regression applied to the same limited information.

Data availability

The supplementary material contains the extracted core treatment data, source locations, calculation methods, numerical results and sensitivity analyses. These materials permit recalculation and scrutiny, but they do not replace the original articles or provide unreported pen-level observations. Any future correction or additional study should therefore be linked to its primary source and analysed as an explicit update, preserving the distinction between extracted facts and reconstructed statistical assumptions.

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Соняшниковий шрот у раціонах курчат-бройлерів: метааналіз залежності «доза–відповідь» з урахуванням рівня введення, залишкової клітковини та віку птиці

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Соняшниковий шрот дає змогу урізноманітнити джерела протеїну в раціонах курчат-бройлерів, проте рекомендації у вигляді єдиної максимальної норми введення не враховують відмінностей у складі шроту, рецептурі раціону та попередній годівлі. У пошуковому метааналізі залежності «доза–відповідь» використано дані про продуктивність курчат-бройлерів за 22–42-й дні життя з трьох незалежних експериментів. П'ятнадцять варіантів годівлі, що охоплювали 80 загонів і 2176 початково розподілених птахів, дали 12 порівнянь раціонів із соняшниковим шротом із контрольними раціонами. Корельовані логарифми відношень середніх значень узагальнювали за допомогою коефіцієнтів нахилу залежності від дози, оцінених у межах кожного дослідження методом узагальнених найменших квадратів, і об'єднували методом обмеженої максимальної правдоподібності з модифікованими довірчими інтервалами Кнаппа–Гартунга. Дисперсії у двох дослідженнях відновлювали за опублікованими коефіцієнтами варіації на основі явно зазначеного припущення щодо залишкової дисперсії. Об'єднані лінійні проєкції на кожні 10 відсоткових пунктів введення відповідали змінам +1,04% для приросту живої маси (95% довірчий інтервал від −5,80% до +8,37%), −0,25% для споживання корму (від −1,94% до +1,47%) та −1,15% для коефіцієнта конверсії корму (від −7,11% до +5,20%). Коефіцієнти нахилу для приросту маси та конверсії корму були неоднорідними, а деякі залежності в межах окремих досліджень — нелінійними. Ці результати не доводять еквівалентності та не визначають оптимуму. Опублікований вміст сирої клітковини у шроті перебував у вузькому діапазоні, приблизно 21–22%, тоді як у межах окремих експериментів дозу шроту та кількість клітковини, внесеної з ним, неможливо було відокремити. Повторні спостереження у стартовий період і зміни програм годівлі також унеможливили незалежне оцінювання модифікувального впливу віку. Отже, дані підтримують складання раціонів з урахуванням конкретного складу продукту та явне врахування попередньої годівлі, а не встановлення універсальної максимальної норми введення. Цей аналіз є відтворюваним кількісним синтезом, обмеженим обсягом знайдених даних, а не вичерпним систематичним оглядом чи валідованою прогнозною моделлю з трьома модераторами.

Ключові слова: *курчата-бройлери; соняшниковий шрот; залежність «доза–відповідь»; сира клітковина; конверсія корму; заміна інгредієнтів раціону; метааналіз*