

Influence of Resistance Exercise on Lean Body Mass in Aging Adults: A Meta-Analysis

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ABSTRACT

PETERSON, M. D., A. SEN, and P. M. GORDON. Influence of Resistance Exercise on Lean Body Mass in Aging Adults: A Meta-Analysis. *Med. Sci. Sports Exerc.*, Vol. 43, No. 2, pp. 249–258, 2011. **Purpose:** Sarcopenia plays a principal role in the pathogenesis of frailty and functional impairment that occur with aging. There are few published accounts that examine the overall benefit of resistance exercise (RE) for lean body mass (LBM) while considering a continuum of dosage schemes and/or age ranges. Therefore, the purpose of this meta-analysis was to determine the effects of RE on LBM in older men and women while taking these factors into consideration. **Methods:** This study followed the Preferred Reporting Items for Systematic Reviews and Meta-Analyses recommendations. Randomized controlled trials and randomized or nonrandomized studies among adults ≥ 50 yr were included. Heterogeneity between studies was assessed using the Cochran Q and the I^2 statistics, and publication bias was evaluated through physical inspection of funnel plots as well as formal rank-correlation statistics. Mixed-effects meta-regression was incorporated to assess the relationship between RE dosage and changes in LBM. **Results:** Data from 49 studies, representing a total of 1328 participants, were pooled using random-effect models. Results demonstrated a positive effect for LBM, and there was no evidence of publication bias. The Cochran Q statistic for heterogeneity was 497.8, which was significant ($P < 0.01$). Likewise, I^2 was equal to 84%, representing rejection of the null hypothesis of homogeneity. The weighted pooled estimate of mean LBM change was 1.1 kg (95% confidence interval = 0.9–1.2 kg). Meta-regression revealed that higher-volume interventions were associated ($\beta = 0.05$, $P < 0.01$) with significantly greater increases in LBM, whereas older individuals experienced less increase ($\beta = -0.03$, $P = 0.01$). **Conclusions:** RE is effective for eliciting gains in LBM among aging adults, particularly with higher-volume programs. Findings suggest that RE participation earlier in life may provide superior effectiveness. **Key Words:** STRENGTH TRAINING, SYSTEMATIC REVIEW, OLDER ADULTS, HYPERTROPHY

The term *sarcopenia* has emerged as a designation of nonspecific vulnerability to weakness, disability, comorbidity, and general diminished autonomy among older adults. Although a robust association exists between chronological age and virtually every symptom, aging *per se* is merely a crude proxy for determining sarcopenic risk. Complicating the ability to sufficiently diagnose early-onset vulnerability, declines are demonstrated to manifest as a gradual, often asymptomatic process for which the chief complaints rarely correspond with a subsequent diathesis for skeletal muscle atrophy. In particular, weakness and functional deficit have been considered hallmark pre-

dictors of age-related morbidity (12) and decreased autonomy. Moreover, age-related atrophy is often paralleled with increases in intermuscular adipose tissue infiltration and overall fat mass (i.e., “sarcopenic obesity”) (32,92), inflammation, metabolic syndrome, arterial stiffness, and glucose intolerance (36,93,94). Sarcopenia represents a complex phenotype of numerous interrelated pathologies, exposures, and behaviors, and thus failure to prevent its progression may significantly increase risk of frailty and mobility disability (11,58) and lead to losses of independence, increased health care costs, and overall reduced quality of life (30,59).

Although the mechanisms for muscular hypertrophy and strength are to some extent distinct, resistance exercise (RE) is considered to be the preferred approach to elicit these adaptations for healthy adults (5). At present, there is a great deal of variability in the dose–response relationship reported in the literature, and the specific efficacy of RE for muscular adaptation is inconsistent across investigations. Several studies have demonstrated similar hypertrophic responses between untrained, young- and middle-aged individuals and gender-matched elderly subjects (57,77,90), whereas others suggest greater adaptation among younger cohorts (65,105).

Notwithstanding the extensive support for RE among aging adults (i.e., “Evidence Category A”) (26), a systematic review to scrutinize treatment effects for lean body mass

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(LBM) across multiple training dosages and potential mediating variables is yet to be completed. To date, the most comprehensive reviews related to this topic have limited the analysis of LBM as a secondary outcome and/or have synthesized data from across combined cohorts of middle-aged and older adults (60–63). In such cases in which LBM was not the primary outcome, it is likely that many suitable studies would not have met inclusion criteria. Further, most meta-analytic evidence pertaining to RE for aging is specific to functional performance, disability, and/or strength outcomes (69,74,84,85,95). Those that have synthesized data for LBM have yielded conflicting results (60–63) and thus may obscure the true efficacy of RE to elicit adaptation. Such inconsistencies may also be due to variability in the control populations and/or differences in model covariates. It is certainly conceivable that RE would serve as a valuable preventive or treatment strategy in a clinical rehabilitation setting to promote increases in LBM. However, among aging adults, there are very few published accounts that examine the overall benefit of RE while considering a continuum of dosage schemes, treatment durations, and/or age ranges. Therefore, the purpose of this meta-analysis was to determine the effects of RE on LBM in older men and women, while taking these factors into consideration.

METHODS

This meta-analysis was conducted in accordance with the recommendations and criteria as outlined in the Preferred Reporting Items for Systematic Reviews and Meta-Analyses statement (80). The respective procedures that were incorporated during this meta-analysis, including the identification, screening, eligibility, and inclusion of studies, were all agreed upon between the authors in advance.

Types of Studies and Participants

Any randomized controlled trials (RCT) or clinical trials meeting the subsequent specifications were included. Non-randomized studies (non-RCT) that examined intervention treatments using stratified young versus older participants or aged men versus women were also eligible for inclusion in the analyses. Trials were included if the mean age of participants was older than 50 yr. Although sarcopenia for some individuals may occur before the fifth decade (73), most research pertaining to the treatment of sarcopenia or age-related general weakness is limited to older cutoffs (e.g., ≥ 50 yr). This *a priori* criterion for age was chosen to include the largest possible age continuum of “older” adults. The 2007 American College of Sports Medicine/American Heart Association joint recommendations for physical activity in older adults suggest that “old age” usually applies to individuals aged ≥ 65 yr but may also be relevant to adults aged ≥ 50 yr who have limitations that affect movement, fitness, or physical activity (82). Therefore, similar to previous reviews (68,69), inclusion of participants with a range in age

and health complications was critical to increase external validity and generalizability of results.

Types of Interventions and Outcome Measures

Studies with one or more cohorts participating in RE were eligible for inclusion in the analysis. RE programs ranged from in-home or senior-citizen community programs, group exercise programs, and/or individual personal training arrangements. RE was defined as a program that included specific training for the major muscle groups of the whole body (i.e., both upper and lower body; see file, SDC 1, Additional detail pertaining to the RE protocols that were considered for inclusion in the analysis; <http://links.lww.com/MSS/A38>).

Study inclusion was limited to three discrete measurements of LBM: 1) hydrodensitometry, 2) whole-body air plethysmography, and 3) dual energy x-ray absorptiometry. These tests were selected because of their documented validity and reliability for assessments as well as reported prevalence in the literature. Other methods of assessing LBM such as anthropometric estimates (e.g., circumference and/or skinfold measures), bioelectrical impedance analysis, or near-infrared interactance were not included in the analysis.

Search Strategy and Study Identification

Computerized searches of MEDLINE, EMBASE, PubMed, Web of Science, SPORTDiscus™, Evidence Based Medicine Reviews Multifile databases, and Digital Dissertations (accessed May, June, and July 2009) from their inception to July 2009 were undertaken. Hand searching of key exercise and sports science, geriatrics or gerontology, and medicine (i.e., general and internal) journals, reference lists, and other sources (e.g., exercise physiology texts and previous meta-analyses) was also undertaken. Studies published in foreign language journals were not included. Abstracts and citations from annual scientific conferences relating to exercise science or gerontology were not examined because of the paucity of requisite data. The preliminary search yielded more than 5000 relevant abstracts and citations. Full texts of more than 400 articles were obtained and examined by the primary reviewer (MP). (See file, SDC 2, Subsequent detail pertaining to the search strategy and study identification; <http://links.lww.com/MSS/A39>. Specific permutations of the text keyword combinations and Medical Subject Headings terms are provided.)

Study Eligibility and Data Extraction

Each research article was expected to contain a supervised RE intervention and to include a detailed description of the requisite information in order to be considered for inclusion. A study (or cohort) was excluded if the intervention was specifically designed to treat a given disorder or disease, if subjects were administered anabolic hormone replacement therapy during the intervention, if subjects were not previously “untrained” (i.e., if subjects had been participating in RE within 6 months before the study), and/or if the subjects

or treatment did not meet inclusion criteria (e.g., subjects <50 yr included, RE intervention lasted less than 8 wk, RE was unsupervised, RE was not “whole-body” protocol, etc.). A specific coding tool was developed to record information pertaining to the study source, participants, experimental characteristics, and outcomes. The coding tool was derived from elements coded in previous meta-analyses of health behavior, physical activity, and exercise as well as suggestions from both meta-analysts and RE or physical activity experts. Although all eligible studies coded for data analyses in this investigation shared a common directive, several studies examined slightly different hypotheses. As an example, Hurlbut et al. (54) examined whether younger and older subjects experienced differential adaptation to RE. Thus, for studies that compared outcomes between young versus older participants, only data from the older participants were coded for analysis. For each included study, the author of correspondence was contacted to obtain any missing information or data. If authors could not be reached or if the data were no longer available, the trial was not included in the meta-analyses.

Intervention Operational Definitions

Volume. Volume of training referred to the total number of work sets performed per session (i.e., not including warm-up). Because the purpose of this analysis was to examine the effectiveness of whole-body RE on LBM, volume of training per whole body was considered to be an important variable. Therefore, training programs that isolated one part of the body or muscle group were not included in the analysis. There has been substantial debate concerning the appropriate operational definition of training volume within the RE literature, making this a difficult parameter to evaluate and to replicate in research. A widely accepted definition for this variable is volume load (VL), which takes into account the total number of performed sets, repetitions, and weight (kg) lifted (i.e., total repetitions [No.] \times external load [kg]). Although this is a readily used volume classification, published RE manuscripts do not generally include total VL as a prescription entity and/or do not provide ample data (i.e., exact number of repetitions performed and/or exact absolute load lifted) for a meta-analyst to compute total VL for each individual training intervention. Therefore, the total number of sets performed per whole body was considered to be an appropriate surrogate index of the absolute volume of physiologic stress.

Frequency. Training frequency was defined as the occurrence, per unit of time (e.g., calendar week), that a full-body RE regimen was completed. In some instances, interventions that incorporated higher-volume training were partitioned to accommodate greater overall time requirements. For example, full-body training, which is often prescribed 2–3 d \cdot wk⁻¹, may be divided into two upper-body and two lower-body training sessions per week (four total sessions). In this example, although weekly training took place for 4 d, the

frequency of training was still coded as 2 d (i.e., the full body was trained twice, in a given week).

Intensity. Intensity of training was defined as the percentage of one repetition maximum used for a given exercise. This operational definition for training intensity generates an objective, quantifiable unit, which is contrary to the more subjective measure of training fatigue or rating of perceived exertion.

Assessment of Reviewer Agreement

Interreviewer disagreements were resolved by consensus. The agreement rate before amending any such discrepancies was assessed using the kappa statistic (33) and determined to be 0.94. Two reviewers worked independently and screened titles and abstracts for eligibility. Potential references were retrieved in full text for evaluation against eligibility criteria. The kappa statistic was used to evaluate the chance-adjusted interreviewer agreement about study eligibility (i.e., eligible or not eligible). In the case of inadequate information contained in the manuscript, the lead reviewer (MP) sought clarification from study authors.

Tests for Publication Bias and Heterogeneity

To examine for evidence of publication bias, visual inspection of Begg’s funnel plots occurred (13). This process included the examination of scatter plots for LBM plotted against its SE. As a formal check for publication bias, the tests of Begg and Mazumdar (14) and of Egger et al. (35) were implemented. The Egger’s publication bias test quantifies the bias captured by the funnel plot, and more specifically, the standardized effect is regressed on precision (i.e., inverse of SE) (18). These formal statistics are used to evaluate the same assumption as the Begg’s test and may be incorporated as a cross check to the physical inspection of the scatter plots.

Heterogeneity between studies was assessed using the Cochran Q statistic (28). The Cochran Q uses the sum of squared deviations of the study-specific estimates derived from the pooled estimate and weights the contribution of each study. Probability values were obtained by comparing the Q statistic with a χ^2 distribution with $k - 1$ degrees of freedom, in which k represents the number of studies included. Because heterogeneity is to a certain extent inevitable in meta-analytic research, there is ample debate regarding the value of assigning statistical significance to this computation. Thus, we also incorporated the I^2 statistic, using the following equation:

$$I^2 = [(Q - df) / Q] \times 100\%$$

This procedure quantifies the proportion of variability in the trial results that are a function of heterogeneity rather than chance (48,49). With this method, I^2 ranges from 0% to 100% such that 0% reflects homogeneity and 100% is indicative of meaningful heterogeneity.

SD

Treatment effects for LBM were calculated for each study following the extraction or coding of change scores and SD. Specifically, the SD of change was needed to calculate the effect size, and for many of the studies, this value was not reported. Rather, the majority of studies obtained for this analysis included the SD for the baseline and postintervention LBM outcomes, or in many cases the SEM. If the study reported exact *P* values for the change in LBM outcome, the SD of change was computed. However, for those studies that did not report exact *P* values, the SD of change was calculated using the baseline and the postintervention SD as well as the within-participant bivariate correlation of LBM measures using the following equation:

$$SD_{\text{change}} = \sqrt{[(SD_{\text{pre}})^2 + (SD_{\text{post}})^2 - 2 \times \text{corr}(\text{pre}, \text{post}) \times SD_{\text{pre}} \times SD_{\text{post}}]}$$

For every article included, authors were contacted in an effort to retrieve the SD change outcomes, the raw data for the calculation of the within-participant baseline and post-intervention LBM correlations, or the specific and respective *r* (correlation) values. If an author could not be reached, an assumption was made that the bivariate correlation was similar across interventions. This strategy, which has been recommended by Follmann et al. (38) and previously used in published meta-analyses (88), allows for the computation of effect sizes for all cohorts included in a review. A within-participant correlation of *r* = 0.96 was used, which was derived through exact calculation from 16 of the included cohorts (44,46,51,53,56,72,87,89,96).

Effect Sizes

The analysis of pooled data was conducted with a fixed- and random-effects model. Although there is debate regarding the appropriate use of a fixed- or random-effects model when calculating summary estimates (47), these models generally provide similar estimates unless heterogeneity is present among the studies. In the context of analyzing RE interventions for LBM, we decided that a random-effects model was a more suitable method as it decreased the risk of a type I error and because it assumed variability among included trials. A forest plot was generated to illustrate the study-specific effect sizes along with 95% confidence intervals (CI). Combining estimates then allowed for the assessment of a pooled effect, as has been previously described (88), in which the reciprocal of the sum of two variances was accounted for including 1) the estimated variance associated with the study and 2) the estimated component of variance due to variation between studies. In each study, the effect size for the intervention was calculated by the difference between the means of the posttest and pretest at the end of the intervention. The study-specific weights were derived as the inverse of the square of the respective SE. All statistical analyses were performed using STATA 10.0 (StataCorp LP, College Station,

TX), MINITAB 14.0 (Minitab Inc., State College, PA), and SAS 9.2 (SAS Institute Inc., Cary, NC).

Meta-Regression and Influence Analysis

A weighted, subgroup multiple meta-regression was performed with a mixed-effects model (specified *a priori*) to examine the association between age, study duration, resistance training variables (i.e., training intensity, volume, frequency), type of LBM assessment, and study design (i.e., RCT vs non-RCT) with changes in LBM. For this analysis, each of the aforementioned potential predictors was entered into a model as fixed effects, and the subsequent assessment was considered to be a random effect.

To assess the influence of individual studies on the overall estimate of LBM change, we conducted an influence analysis. Using this analysis, the estimates were computed, omitting a single study in each cycle.

RESULTS

The flow of article search and selection, from “potentially relevant” to final inclusion, is depicted in Figure 1.

Study characteristics. Of the 5011 references screened, 49 studies with 81 cohorts were deemed eligible according to the inclusion criteria. Of the included articles, the publication dates ranged from 1990 to 2009. Thirty-seven percent of the studies included random assignment of treatment conditions as well as control groups (RCT) (1,2,15,17,19,20,24,25,34,37,46,53,56,72,76,96,98,100,102). The remaining studies were classified as non-RCT (6,9,10,21–23,27,31,39–41,44,51,52,54–56,64,70,71,75,79,81,86,87,89–91,97,99,107), of which six studies assessed a single cohort of older men (21,23,27,64,79,86), four studies assessed a single cohort of older women (41,56,91,99), nine studies compared the effects of training on men versus women (6,10,31,39,51,75,81,97,107), eight studies assessed the effects of combined older men and women (9,22,40,44,52,55,87,89), and four studies assessed four groups, including young or middle-aged women, young or middle-aged men, older women, and older men (54,70,71,90).

Subject characteristics. Data on 1328 subjects were included in the analysis (Supplementary Table 1; <http://links.lww.com/MSS/A40>). The age range for subjects was between 50 and 83, with the mean age of the subjects in the majority of studies falling between 60 and 75 (mean = 65.5 ± 6.5 yr). A large percentage of the assigned cohorts consisted of male and female combined groups (30 cohorts) (9,15,17,19,22,24,25,34,40,44,46,52,53,55,71,72,87,89,96,101,102), with the remaining distributed in male-only (23 cohorts) (1,6,10,21,23,27,31,39,51,54,64,70,75,76,79,81,86,90,97,100,107) and/or female-only (28 cohorts) (1,2,6,10,20,31,37,39,41,51,54,56,70,75,76,81,90,91,97–99,107) cohorts.

Treatment characteristics. Length of training ranged from 10 to 52 wk (mean duration = 20.5 ± 9.1 wk), frequency

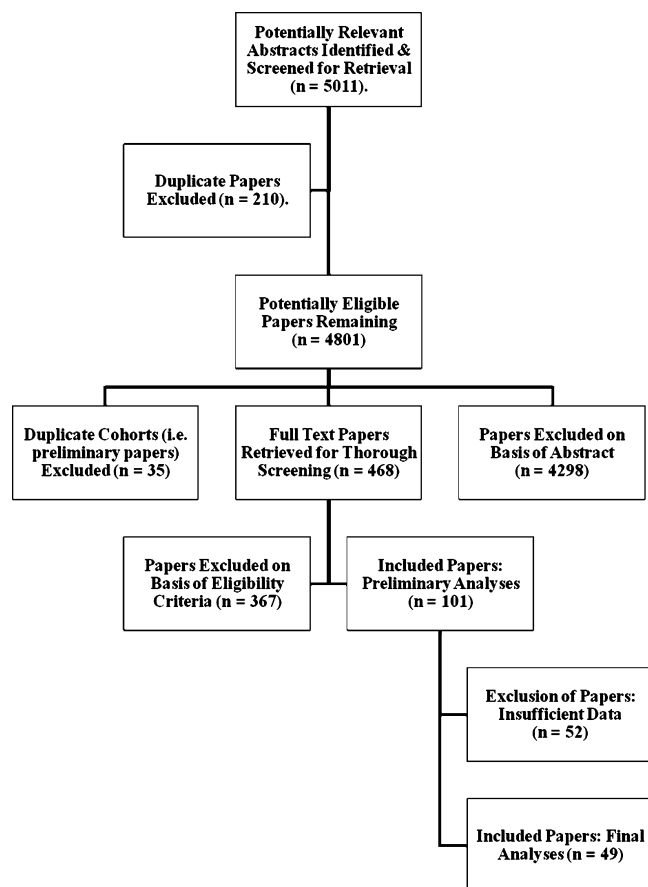


FIGURE 1—Flow of articles through the review process.

from two to three times per week (mean = 2.8 ± 0.4 d·wk⁻¹), and intensity from 50% to 80% of one repetition maximum (mean = $74.6\% \pm 6.9\%$). The number of sets per exercise session ranged from 7 to 39 (i.e., per the full-body program; mean = 20 ± 7.3 sets), whereas the number of exercises performed ranged from 5 to 16 (mean = 8.3 ± 2.1 resistance training exercises). The within-group number of repetitions performed for each set ranged between 2 and 20 (mean = 10 repetitions), whereas the rest period between sets ranged from 60 to 360 s (mean = 110 s) (see Table 1 of SDC 3, Specific detail pertaining to the important characteristics of each study included in the analysis; <http://links.lww.com/MSS/A40>). Compliance, defined as the percentage of exercise sessions attended, ranged from 80% to 100% (mean = $87.2\% \pm 3.4\%$).

Publication bias and heterogeneity. The tests used to inspect for evidence of publication bias revealed no such bias. Examination of the Begg's funnel plots (Fig. 2) demonstrated considerable symmetry, suggesting that there was no significant publication bias. Results from Begg's rank-correlation ($P = 0.10$) and Egger's ($P = 0.26$) tests further confirmed no evidence of publication bias.

The Cochran Q statistic for heterogeneity was 497.8. On the basis of a chi squared (χ^2) with 80 degrees of freedom, this was significant ($P < 0.01$). Further, I^2 was 84% indicating rejection of the null hypothesis of homogeneity (49).

Intervention effect. Many trials reported data from more than a single cohort (Table 1 of SDC 3; <http://links.lww.com/MSS/A40>). The pooled estimate of LBM change from baseline to postintervention, combining data from 81 treatment cohorts (49 studies), was 1.1 kg (95% CI = 0.9–1.2 kg; $P < 0.001$). A forest plot of the main effects for LBM as well as the CI for all 81 cohorts is provided in Figure 3. Results from the influence analysis demonstrated that removal or omission of any individual study did not alter the summary effects and 95% CI.

Meta-regression. By using multiple meta-regression, a strong linear association was determined between the volume of training (i.e., controlling for age, study duration, type of LBM assessment, study design, gender, training intensity, and frequency) and the magnitude of the LBM change ($\beta = 0.05$, $P < 0.01$), with higher-volume interventions being associated with greater LBM increases (Fig. 4). Further, age was associated with LBM increase ($\beta = -0.03$, $P = 0.01$), such that older men and women experienced less gain. Gender, intervention duration, study design, and differences in training intensity and frequency were not significantly associated with changes in LBM ($P > 0.05$).

DISCUSSION

Data derived from 49 studies and 81 cohorts verified a robust association between full-body RE and increased LBM. Analysis revealed that after an average of 20.5 wk of RE, aging men and women experienced a significant main effect equal to a 1.1-kg increase in LBM. These findings bear clinical significance, given the exaggerated rate of skeletal muscle atrophy that occurs among sedentary individuals after the age of 50 yr (78). Since these declines are known to precipitate a heightened risk for functional disabilities, including deficits in strength, gait, mobility, and essential activities of daily living (78), preservation or increases in lean muscle mass through RE may serve as a powerful treatment or preventive strategy.

The examination in LBM is a readily attainable and usable parameter related to pathology as well as a practical surrogate predictor of weakness and/or functional deficit. However, the net change of LBM is not a sufficient index to quantify hypertrophic or atrophic alterations of skeletal muscle, particularly over the short term. Measurement of LBM provides no specific detail regarding changes in single fiber cross-sectional area, muscle volume, satellite cell concentration or differentiation, or architectural modifications (e.g., changes in pennation angle and sarcomere length), all of which represent vital morphological characteristics associated with muscular functional capacity. For this analysis, it is plausible that such biomolecular changes would not be adequately reflected through a gross aggregate such as LBM. Case in point, numerous studies have confirmed significant hypertrophic and architectural responses among aging men and women following short bouts of

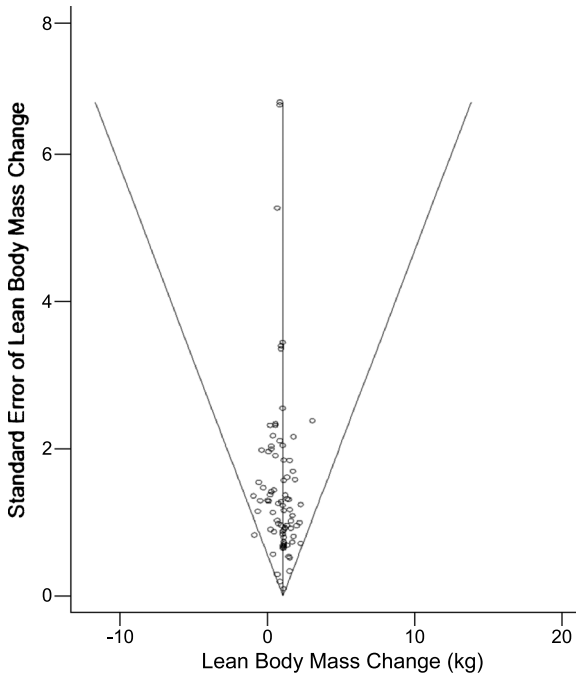


FIGURE 2—Begg’s funnel plot for LBM, with 95% CI. No evidence of publication bias was detected.

RE, despite negligible or nonsignificant changes in LBM (42,43,45). Certainly, these data, which are generally ascertained through histochemical and/or precise imaging techniques, provide superior mechanistic indices of adaptation. However, at present, the collection of such data is not considered to be practical in a clinical setting. Therefore, the measurement of LBM may continue to serve as a viable proxy for diagnosis and/or observation of outcomes within longitudinal interventions or translational directives.

Results from the meta-regression identified volume of training as a significant predictor of LBM. These findings suggest that higher volumes of RE are associated with greater increases in LBM. Although volume ranged from 7

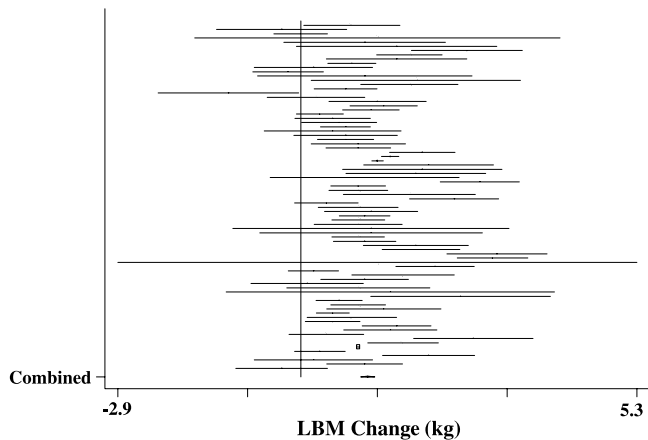


FIGURE 3—Forest plot of effect sizes and 95% CI for all 83 cohorts (50 studies) representing LBM on the basis of the fixed-effects meta-analysis results.

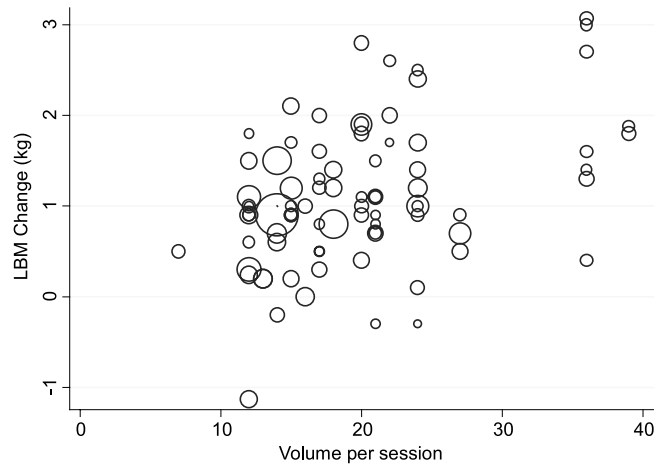


FIGURE 4—LBM change by training volume (sets per session), weighted by number of subjects in the study.

to 39 total sets per session (for whole-body RE), the majority of included studies conformed to the original American College of Sports Medicine recommendations for RE in older adults (i.e., 8–10 exercises for one to two sets of full-body RE) (3). Subsequent results from the meta-regression did not identify any significant relationships between program duration, intensity, or frequency and subsequent changes in LBM. It is conceivable that the overall lack of variability in training regimens across program models may have confounded these results. However, of the various RE prescription components, volume of RE has received the greatest attention concerning effectiveness and risk-reward. In essence, if the effort-to-benefit ratio does not warrant progression in volume to accommodate increases in muscular adaptation, then the opportunity cost of performing higher-dosage RE over other functional directives, for example, balance exercise, cardiovascular or aerobic exercise, flexibility or stretching, etc., would not be justified. This is the first comprehensive, meta-analysis to confirm a significant positive association between RE volume and LBM in aging men and women. Single-set and/or fixed-volume RE programs may no longer be considered sufficient for individuals seeking progressive adaptations in LBM.

Current exercise recommendations are dissimilar for young and middle-aged healthy adults (3,66) as compared with those for elderly populations (4,26,83), particularly for RE prescription. The majority of studies and subsequent recommendations for young, healthy adults have incorporated models of periodization to promote enhanced adaptation of muscle mass and fitness, whereas no such suggestions have been endorsed for the aging population. Periodization schemes call for a multidimensional approach to progressing and alternating training variables and subsequent recovery over set blocks of time, as well as systematic “nonlinear” manipulation of dosages (i.e., “daily undulating periodization”) to accommodate adaptations in muscular fitness (5). On the basis of current data, it appears that to facilitate progressive adaptation in LBM, it is necessary

to increase the prescription dosage as individuals become more familiarized with training. In essence, merely increasing training load over time may not be sufficient beyond a certain point because this represents an inevitable reliance on the same relative intensity. Thus, progression models for resistance training among older adults should include a systematic manipulation of volume to accommodate chronic adaptation in LBM (85).

Meta-regression also identified a negative association between age and LBM main effects. Therefore, although significant hypertrophy is possible in the “oldest old” (67), it may be expected that the benefits of early RE participation will translate to superior increases in LBM and thus preservation of muscle function and instrumental activities of daily living, prevention of disability, and maintenance of independence. However, as a cautionary statement, it should be noted that the majority of these current data were derived from healthy older adults. Indeed, more research is warranted to ascertain the influence of RE for specific disease outcomes and across a broad spectrum of physical abilities or limitations. Previous research pertaining to efficacy of RE on hypertrophy among aging adults has demonstrated that despite a diminished preexercise rate (7,103,104,106), significant increases in protein synthesis are indeed possible through higher-volume, progressive RE (90,108). Although current data are reflective of this phenomenon, additional research is warranted to investigate the dose-response relationship between RE and adaptation potential for fiber-specific hypertrophy, muscle quality, LBM, and changes in protein synthesis across a continuum of ages. To date, the most comprehensive review on RE for the primary outcome of “muscle pathology” (i.e., cross-sectional area, fiber area, or mass, as derived by imaging techniques) has suggested moderate to large (0.5–0.79) effect sizes for muscle hypertrophy (type II fiber area, $d = 0.71$) (50).

Previous published meta-analyses, which have included LBM as a secondary outcome, were conducted on adults of various age ranges and have demonstrated inconsistent findings. Specifically, effect size data vary from nonsignificant differences (treatment – control) in men (63) and premenopausal women (61) to significant differences of 1–4 kg (60,62). For the current analysis, we included 32 studies that were not RCT designs, and thus it was impossible to ascertain a treatment-control effect size calculation. Rather, we examined effect sizes from preintervention to postintervention. Many meta-analyses use only RCT. However, there have been several recent reviews that have demonstrated no differences in effect sizes between studies with different designs (16,29), and there is ample debate regarding the value of this quality indicator for meta-analysis study inclusion (8).

Further, regarding the issue of assessing indicators of study “quality,” no acceptable scale currently exists for examining the quality of RE intervention research. According to the recently updated Cochrane Collaboration guidelines for systematic reviews, there is a general recommendation against the use of quality scales in such situations because

of an overall lack of supporting evidence and validity (47). Therefore, subanalysis for overall study quality was not carried out for the current investigation, although previous reviews have reported an overall lack of quality among the majority of resistance training literature for older adults (69). As has been suggested in these reviews, future RE research should be designed to accommodate increased internal validity and to include such features as intention to treat analysis, blinded assessors, attention control groups, and concealed randomization. The gradual acceptance of RE as a viable preventive or treatment strategy will inevitably lead to larger samples from which to recruit, and ultimately to higher-quality RCTs. Therefore, it may be necessary to eventually conceptualize a specific quality rating scale for RE interventions because such a scale would be valuable for future refinement of evidence-based RE recommendations.

As with all meta-analyses, a limitation to the generalizability of findings is that data do not infer a causal effect. Because sarcopenia is a collection of interrelated deteriorations that occurs on a gradual basis during the aging progression, the capacity to increase LBM through participation in RE represents an effective preventive strategy to complement other behavioral interventions. However, because sarcopenia is strongly related to muscular weakness, dysfunction, and disease comorbidity, certainly more translational studies are warranted to directly examine treatment options for these consequences. Nevertheless, we have also identified through meta-analyses that RE in older adults has significant value for strength outcomes (85). Collectively, these findings support the efficacy of RE as an important public health directive that may positively influence quality of life and independence for elderly individuals.

CONCLUSIONS

The results of the current meta-analysis suggest that RE elicits an approximate 1-kg increase in LBM among older adults. Although modest compared with the expected adaptation with healthy young adults, this increase is in contrast to the 0.18-kg annual decline that may occur (78) through sedentary lifestyles, beyond 50 yr of age. Moreover, volume of training and age of participation are important determinants of effectiveness, suggesting that higher dosages result in greater adaptive response, and that aging individuals should consider starting a regimen of RE as early as possible to optimize results. These findings expand upon current recommendations, which merely suggest an increase in training load, to accommodate fitness improvements.

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