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Research Article

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Posted Date: November 23rd, 2026

DOI: https://doi.org/10.21203/rs.3.rs-11342852/v1

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Additional Declarations: No competing interests reported.

Physical exercise and social isolation among older adults in China: a cross-sectional analysis of parallel indirect associations through depressive symptoms and social participation

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Abstract

Background Social isolation is an escalating concern for ageing populations, and physical exercise is widely promoted as a modifiable determinant of social connectedness. Whether depressive symptoms and social participation account for this association is unclear. Among adults aged 60 years or older, we estimated the exercise–isolation association and decomposed it into parallel indirect components through both.

Methods This study conducted a cross-sectional secondary analysis of the 2023 wave of the China Longitudinal Aging Social Survey (CLASS). After exclusions for ineligible age, missing survey weights, incomplete mediator information and missing covariates, 9,396 of 11,648 age-eligible respondents remained (451 communities, 28 provinces). Survey-weighted logistic regression with province fixed effects and community-clustered standard errors estimated the association between exercising at least monthly and social isolation. Because nested logistic coefficients are not directly comparable, the Karlson–Holm–Breen method decomposed the total association on a common scale, with 5,000 community-level bootstrap replications. Supplementary analyses comprised continuous isolation scores, alternative indicators, sample restrictions, multiple imputation, propensity-score weighting and tests for effect modification.

Results Monthly exercise was associated with lower odds of isolation (OR = 0.58; 95% CI, 0.48-0.71; standardized risk difference, -9.0 percentage points [95% CI, -12.2 to -5.8]). On the common scale, the indirect component through social participation was −0.029 (95% CI, −0.056 to −0.007) and that through depressive symptoms was −0.000 (95% CI, −0.009 to 0.005); the joint indirect component accounted for 5.4% (95% CI, 1.4% to 10.6%) of the total association. In exploratory analyses, effect modification by sex, urbanicity or age was non-significant after Holm correction.

Conclusions Physical exercise is associated with lower isolation risk, but only a small fraction of that association is accounted for by social participation, and neither candidate mediator admits a causal reading. Unresolved temporal ordering, missing-data selection, measurement constraints and incomplete sampling-design documentation jointly caution against a causal interpretation.

Keywords Social isolation; Physical exercise; Depressive symptoms; Social participation; Aged; China; Cross-sectional studies; Mediation analysis

Introduction

China's population is ageing rapidly: according to the 2025 Annual Bulletin on the Development of National Ageing Undertakings, adults aged 60 years or older accounted for 23.0% of the total population by the end of 2025 [1], a level consistent with moderate ageing and approaching advanced ageing. Within this demographic transition, social isolation among older adults has become an increasingly salient concern [2]. Conceptually, isolation denotes both an objective paucity of social relationships and a subjective perception that one's relational needs are unmet [3]. It is conventionally construed along two dimensions, objective and subjective [4], which together span a continuum of severity [5]. Social convoy theory provides the organising framework: individuals are surrounded by concentric layers of support, from intimate ties at the core to more peripheral contacts, and isolation risk rises when attrition in the inner layers is not offset by the recruitment of new ties [6]. The consequences are well documented: isolation has been consistently linked to mental illness [7], health decline [8] and premature mortality [9].

Physical exercise is a cornerstone of health promotion in later life, with well-documented benefits for physical functioning, cognitive decline and social ties [10]. These benefits are not confined to the individual: exercise also reshapes the social relationships in which older adults are embedded. Greater isolation and lower physical activity have each been associated with more severe depressive symptoms, with physical activity accounting for part of the isolation–depression association [11]. Note that this evidence positions physical activity as the mediator rather than the exposure—a reminder that the temporal ordering between exercise and isolation remains unsettled. In Chinese samples, exercise has been inversely associated with social exclusion both directly and, serially, through social participation and cognitive function, with social participation alone contributing close to 40% of the total association [12]. Taken together, these findings suggest that the exercise–isolation association is unlikely to be purely direct and may instead run, at least in part, through psychological and behavioural pathways.

We consider depressive symptoms first. That exercise alleviates depressive symptoms is well established. In studies of loneliness among older adults—loneliness being the subjective counterpart of isolation, related to but not interchangeable with it—differences in loneliness across levels of exercise are consistent with mediation via two routes, frailty and depression, with the latter contributing the larger share [13]. Exercise may thus bear on psychological well-being both directly and, by relieving the depressed mood that accompanies declining physical and social functioning, indirectly. Stronger evidence concerns the depression–isolation link itself, which appears mutually reinforcing: in a cross-lagged analysis of the English Longitudinal Study of Ageing, each predicted the other over time, and bidirectional Mendelian randomization supported causality in both directions [14]. Depression may therefore be either a consequence of isolation or a

psychological driver that sustains and deepens it. It is plausible that fewer depressive symptoms represent a psychological route through which exercise is linked to isolation risk. Whether this route carries appreciable weight in population-based samples remains an open question, and one that cross-sectional data such as ours are poorly placed to settle.

The second candidate mediator is social participation, which offers the most direct behavioural link between exercise and isolation. Most exercise is inherently social: group-based formats in particular bring participants together face-to-face at scheduled times and places, generating co-present contact that home-based health behaviours cannot readily reproduce. Among Chinese older adults, the association between social participation and quality of life is consistent with a serial chain running through loneliness, depression and anxiety, although its cross-sectional design precludes causal inference [15]; at the community level, group sports have also been positively associated with social integration, with self-efficacy as a mediator [16]. Neither study establishes temporal ordering. Taken together, these findings suggest that exercise may help older adults establish and maintain ties by building psychological resources and creating opportunities for contact. How much of the exercise–isolation association actually travels this route, however, remains unquantified on a comparable scale.

This evidence motivates both candidate mediators but leaves three gaps. First, most of this evidence derives from high-income countries, and most models test a single mediator at a time [17-19]; to our knowledge, none has entered depressive symptoms and social participation into one model and compared their relative contributions on a comparable scale. Second, studies handle complex survey design and the logit scale inconsistently: coefficients from nested logit models are not directly comparable—so the relative size of competing indirect components, and the share of the total association they explain, cannot be read off directly [20]. Third, null findings may be under-reported, which would inflate the apparent explanatory power of the existing evidence on the exercise–isolation association.

We addressed these gaps using data from the 2023 wave of the China Longitudinal Aging Social Survey (CLASS), and in so doing asked three questions. First, among adults aged 60 years or older, is exercising at least once a month associated with lower social isolation risk after adjustment for sociodemographic characteristics? Second, do depressive symptoms and social participation carry indirect components within that association, and how large are their relative contributions? Third, treated as exploratory and not accompanied by a formal hypothesis, does the association differ between family and friend networks, or across demographic subgroups? We tested three corresponding hypotheses: (H1) exercise is associated with lower isolation risk; (H2) part of this association operates indirectly through social participation; and (H3) part of it operates indirectly through depressive symptoms. Our design maps onto the three gaps: a large, geographically diverse sample of Chinese older adults addresses the first, decomposition on a comparable scale using the Karlson–Holm–Breen method with community-level bootstrap standard errors addresses the second, and reporting both indirect components regardless of statistical significance addresses the third. Because these data are cross-sectional, our aim is to estimate statistical associations and their robustness to alternative measurement, sampling and analytic choices, not to identify causal mediation [21,22].

Methods

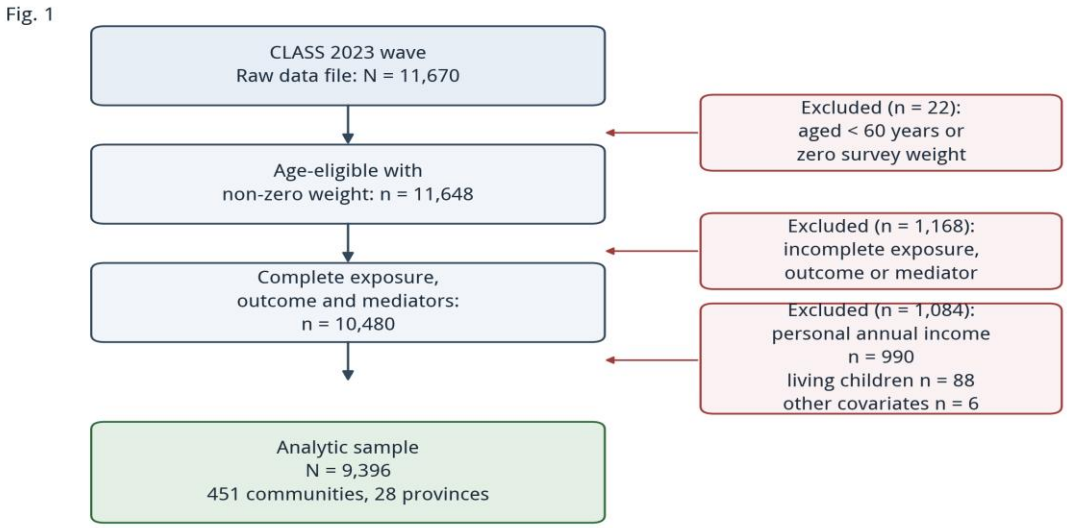
Study design and data source

This study is a cross-sectional secondary analysis of the 2023 wave of the China Longitudinal Aging Social Survey (CLASS), a nationally representative longitudinal survey of community-dwelling Chinese adults aged 60 years or older, conducted by the National Survey Research Center at Renmin University of China [23]. Baseline recruitment took place in 2014, with respondents followed up at approximately two-year intervals; the survey used a stratified, multistage probability-proportional-to-size sampling design and collects information on demographic characteristics, physical and mental health, socioeconomic status, social networks, psychological well-being and physical activity. Although CLASS is longitudinal, we analysed the 2023 wave as a cross-section. De-identified data were obtained through a formal application to the CLASS data repository under an approved data-use agreement. According to the CLASS project's official ethics notice, the project received approval from the Academic Committee of the School of Population and Health, Renmin University of China, and informed consent was obtained from all participants; this secondary analysis of de-identified data did not require separate ethical approval.CLASS has released five waves (2014-2023), and a longitudinal specification is in principle feasible. We nevertheless restricted the primary analysis to the 2023 wave for three reasons:(i) the research question concerns the share of a contemporaneous association that isattributable to two candidate pathways, an estimand that differs from a lagged longitudinal effect; (ii) the 2023 wave provides the most complete concurrent documentation of the sampling weights and of modules G2, D12, E2 and D14; and (iii) attrition across waves would replace the missing-data selection documented here with a more severe form of selection that we cannot characterise from the released files. We report the resulting temporal ambiguity as the first limitation and treat the longitudinal specification as a pre-specified next step rather than as an alternative we declined to attempt.

Participants

The 2023 raw data file contained 11,670 records. Of these, 11,648 respondents were aged 60 years or older and had a non-zero survey weight, and 10,480 had complete information on the exposure, the outcome and both mediators. We further excluded respondents with missing data on personal annual income (n = 990), number of living children (n = 88) and other covariates (n = 6), leaving an analytic sample of 9,396 adults from 451 communities in 28 provinces (Fig. 1). Missing depressive-symptom information alone affected 1,168 of the age-eligible respondents. Compared with included participants, the 2,252 age-eligible respondents excluded because of incomplete data had a higher weighted prevalence of social isolation (32.9% vs 25.1%), indicating that complete-case analysis under-represents isolated older adults. Because the direction of any resulting bias in the exercise–isolation association cannot be determined from these data, we applied survey weights throughout and repeated the analysis under multiple imputation.

Fig. 1 Sample selection



Measures

Outcome: social isolation

Social isolation was the outcome, assessed using the 6-item Lubben Social Network Scale (LSNS-6), administered as module D12 of the CLASS 2023 questionnaire [24]. Each item is scored from 0 to 5, giving a total score of 0–30. The scale captures the size of, and perceived access to, an older adult's network of family members and friends who are available for private discussion and who could provide help [25]. Consistent with convention, a total score below 12 was classified as social isolation [26]; we use this cut-point for epidemiological risk stratification only, and it is not a clinical diagnostic criterion. Items 1–3 constitute the family network subscale and items 4–6 the friend network subscale (range 0–15 each). On both subscales, higher scores denote larger and more accessible networks, and therefore less isolation, so that lower scores indicate greater isolation. In supplementary analyses we modelled the total score as a continuous outcome.

Exposure: physical exercise

Physical exercise was the exposure, derived from item G2 of the CLASS 2023 questionnaire, which asks how often the respondent engages in physical exercise. We dichotomised responses at once a month: exercising less often than once a month was coded 0 and all other frequencies 1, so that the exposed group comprises respondents exercising at least once a month. This cut-point was chosen to distinguish infrequent from more

frequent exercise; it does not indicate whether respondents met recommended levels of physical activity, and the reference group may therefore include occasional exercisers. Any resulting misclassification is likely to attenuate rather than inflate between-group differences. The valid frequency code 9 was retained in its ordinal position as the highest exercise frequency and was not treated as a missing-value code; the inability-to-answer code 9 in the depression module (E2) is a separate convention and was set to missing before scoring, as described below. We retained the original ordinal categories throughout and applied alternative cut-points in supplementary analyses. Throughout, we use the term physical exercise to refer to this item.

Mediators: depressive symptoms and social participation

Depressive symptoms were assessed with the 9-item short form of the Center for Epidemiologic Studies Depression Scale (CES-D) , administered as module E2 of the CLASS 2023 questionnaire. The short form was adapted from the 20-item CES-D developed by Radloff [27] and has been validated and widely used in Chinese older adults [28,29]. Respondents indicated, for each of nine symptom items, how often they had experienced that symptom in the past week, with response options "never", "sometimes" and "often" scored 1, 2 and 3. The three positively worded items—feeling good about life, being satisfied with one's living situation and having many pleasant things in life—were reverse-coded (4 minus the original score), yielding a total score of 9–27 in which higher scores indicate more severe depressive symptoms. Items that respondents could not answer (code 9) were set to missing before scoring. We retained item 8 at face value, as lack of things to do, and did not recast it as difficulty concentrating. The scale was used as a continuous measure of symptom severity; no cut-point was applied, and it does not constitute a diagnosis of depression.

Social participation was the sum of self-reported frequencies of seven activities over the past year, recorded in module D14. Each activity was scored from 0 (never) to 4 (very often), giving a total score of 0 – 28. The activities were community patrol, caring for other older adults or children, environmental protection, mediation of disputes, companionship services, skill-based voluntary service and youth education. This indicator captures organised, community-oriented activities and therefore represents a subset of social interaction rather than the totality of an individual's social life. In the analytic sample the distribution of this index was highly skewed (weighted mean 2.07; SD 4.04), so that the measure carries limited information at the individual level; we return to this point in the limitations.

Both mediators were entered simultaneously in a parallel-mediator model. Because the data are cross-sectional, we make no assumption about their temporal ordering relative to each other or to the exposure and outcome.

Covariates

Covariates were selected a priori on the basis of previous literature and directed acyclic graph reasoning, and comprised sex, age, educational attainment, marital status, hukou (household registration) type, urban or rural residence, personal annual income and number of living children; province was additionally entered as a set of fixed effects. Age was centred at 70 years and scaled by dividing by 10, and a quadratic term was included to

allow for non-linearity. Hukou was classified as agricultural or non-agricultural: respondents whose registration had been converted to the unified residence category were grouped with the non-agricultural category, whereas those who had converted from agricultural to non-agricultural status remained classified as agricultural. Urban or rural residence was defined by the type of area in which the interview took place, not by hukou. Because income was right-skewed and included a substantial number of zero values, it was transformed as ln(income + 1). Out-of-range and special non-response codes were set to missing. Respondents who reported never having given birth were assigned zero living children, consistent with the questionnaire skip pattern; missing values on this item in other cases were not imputed to zero.

A separate, health-adjusted model additionally included self-rated health (good versus less than good), the number of self-reported chronic conditions, and a functional limitation score derived from 11 items in module B4, computed as the sum minus 11 so that the minimum is zero; higher scores indicate more severe limitation. The incontinence item was retained in its original direction, without separate reverse-coding. These health indicators were entered in a separate model rather than in the main adjustment set because their temporal position relative to exercise and isolation is indeterminate: they may lie on the causal pathway, and adjusting for such variables can alter the estimand or introduce over-adjustment bias [30]. The analytic sample was restricted to respondents with complete data on the health indicators, so that models with and without these variables are fitted on identical samples and any difference between them reflects adjustment rather than sample composition.

Statistical analysis

Analyses were conducted in Python 3.13.5 (NumPy 2.3.5, pandas 2.2.3, SciPy, statsmodels 0.14.6) and used survey weights throughout: all models were fitted with the individual-level survey weight supplied in the CLASS 2023 individual release, applied as provided (rescaling the weight by a constant does not change the point estimates or the cluster-robust covariance, which we verified numerically). After descriptive summaries, we estimated the association between physical exercise and social isolation using survey-weighted logistic regression with province fixed effects and standard errors clustered at the community level. Because province fixed effects are included, the reported contrast is estimated within provinces rather than between them, and should be interpreted as a within-province comparison. Four models were fitted on the identical analytic sample: M1 adjusted for province fixed effects only; M2 additionally adjusted for the sociodemographic covariates listed above; M3 additionally adjusted for the health indicators (self-rated health, chronic conditions and functional limitation); and M4 additionally adjusted for depressive symptoms and social participation. M3 and M4 are parallel extensions of M2 rather than a cumulative sequence, so that M4 is not M3 with mediators added. Both are presented as sensitivity analyses rather than as part of the main adjustment sequence: the temporal position of the health indicators is indeterminate and they may lie on the causal pathway, and the mediators are entered to permit decomposition rather than to estimate an adjusted total effect.

Because coefficients from nested logistic models are not directly comparable, we applied the Karlson–Holm–Breen (KHB) method to decompose the total association on a comparable scale into direct and indirect components[20,31]. Because the correction is computed from the coefficients and residual variances of the reduced and full models, it remains computable under weighted pseudo-likelihood estimation; the combination of KHB with survey weights, province fixed effects and community-clustered errors is nevertheless not a standard application, and we return to this point in the Limitations. Confidence intervals for the indirect effects were obtained from a nonparametric cluster bootstrap that resampled communities with replacement within provinces (5,000 replications; percentile intervals). Family and friend network scores were decomposed using weighted linear models under the same resampling scheme, and the difference between the two total associations was taken directly from the bootstrap distribution. Robustness analyses comprised unweighted estimation, an expanded baseline sample obtained by relaxing the exclusion criteria, restrictions by age and by activities of daily living, multiple imputation of missing covariates (m = 20, combined using Rubin's rules) and propensity-score weighting. Effect modification by sex, urbanicity and age was tested with omnibus interaction terms, with Holm correction for multiple testing. All indirect components are interpreted as statistical decompositions rather than as identified causal mediation effects. The standardized risk difference was obtained by marginal standardization: predicted probabilities were computed for every respondent under infrequent exercise and under exercising at least monthly, averaged over the observed covariate distribution with survey weights, and differenced; confidence intervals were obtained by the delta method based on the cluster-robust variance. This study is reported in accordance with the STROBE statement for cross-sectional studies, and the completed checklist is submitted with the manuscript.

Results

Sample characteristics

The analytic sample comprised 9,396 respondents: 4,642 exercised less than once a month and 4,754 at least monthly. The weighted mean age was 69.6 years; 59.2% of respondents were men, and the weighted prevalence of social isolation was 25.1%. Mean social participation scores were higher in the at-least-monthly group than in the infrequent group (2.62 vs 1.47), as were mean LSNS-6 scores (14.87 vs 14.37), but these unadjusted comparisons do not estimate the association of interest because the two groups also differed in sociodemographic and economic characteristics. Covariate balance was acceptable for most characteristics ($|SMD| < 0.15$), but exceeded the conventional 0.1 threshold for non-agricultural hukou (SMD = 0.344), urban residence (0.246) and number of living children (-0.244); across all covariates and category levels the maximum absolute standardized mean difference was 0.437. Across all covariates and all category levels, the maximum absolute standardized mean difference was 0.437, which motivated the propensity-score weighting reported below (Table 1).

Table 1 Descriptive statistics of the study variables

Characteristic	All (n = 9,396)	Less than monthly (n = 4,642)	At least monthly (n = 4,754)	SMD
Age, years	69.56 (5.01)	70.04 (5.04)	69.13 (4.93)	-0.183
Personal annual income, CNY	23474.52 (20361.46)	22562.58 (20528.44)	24311.86 (20170.64)	0.086
Log(personal annual income + 1)	9.56 (1.15)	9.50 (1.17)	9.61 (1.13)	0.100
Number of living children	2.03 (1.05)	2.16 (1.09)	1.91 (1.00)	-0.244
Number of chronic conditions	1.77 (1.44)	1.75 (1.50)	1.79 (1.38)	0.030
ADL score	11.25 (1.03)	11.28 (1.22)	11.23 (0.82)	-0.046
Depressive-symptom score	15.42 (3.37)	15.55 (3.40)	15.29 (3.33)	-0.078
Social-participation score	2.07 (4.04)	1.47 (3.45)	2.62 (4.45)	0.288
LSNS-6 score	14.63 (4.86)	14.37 (5.05)	14.87 (4.68)	0.103
Family-network score	7.66 (2.47)	7.55 (2.59)	7.75 (2.34)	0.082
Friend-network score	6.97 (2.89)	6.82 (2.94)	7.11 (2.84)	0.103
Men	4,854 (59.2%)	2,361 (59.0%)	2,493 (59.4%)	0.008
Married with spouse	7,745 (86.5%)	3,678 (84.2%)	4,067 (88.6%)	0.130
Non-agricultural hukou	3,798 (39.9%)	1,500 (31.2%)	2,298 (47.8%)	0.344
Urban / town residence	5,167 (53.8%)	2,290 (47.5%)	2,877 (59.6%)	0.246
Good self-rated health	5,249 (59.3%)	2,431 (56.4%)	2,818 (62.0%)	0.114
Social-isolation risk	2,398 (25.1%)	1,243 (27.1%)	1,155 (23.4%)	-0.085
Education: No literacy	1,713 (15.5%)	1,009 (18.1%)	704 (13.0%)	-0.141
Education: Private/literacy school	314 (2.6%)	152 (2.5%)	162 (2.8%)	0.014
Education: Primary school	3,668 (37.6%)	1,937 (40.8%)	1,731 (34.7%)	-0.124
Education: Junior middle school	2,553 (29.9%)	1,081 (26.7%)	1,472 (32.9%)	0.136
Education: High/technical school	948 (12.0%)	405 (10.6%)	543 (13.2%)	0.080
Education: Junior college	173 (2.1%)	50 (1.2%)	123 (3.0%)	0.128
Education: Bachelor or above	27 (0.3%)	8 (0.2%)	19 (0.4%)	0.045

Values for continuous variables are weighted means (SD); categorical cells are unweighted n (weighted %). SMD, standardized mean difference (at least monthly minus less than monthly). N = 9,396; n = 4,642 in the less-than-monthly group and n = 4,754 in the at-least-monthly group.

Internal consistency

Weighted Cronbach's α was 0.859 for the LSNS-6 (0.776 for the family and 0.821 for the friend subscale), 0.710 for depressive symptoms and 0.884 for social participation (Table 2). Internal consistency is adequate across all four measures. For the social participation index, however, a high α reflects the shared tendency to participate rather than the equivalence of the seven activities, which differ substantially in content; we therefore interpret the index as a summary count of activity frequency and breadth, not as a measure of a single underlying trait.

Table 2 Internal consistency

Scale	Items	Unweighted α	Weighted α
LSNS-6	6	0.861	0.859
Family network	3	0.779	0.776
Friend network	3	0.830	0.821
Depression (9 items)	9	0.701	0.710
Social participation (7 items)	7	0.884	0.884

Weighted Cronbach's α computed from the survey-weighted correlation matrix in the analytic sample (N = 9,396); unweighted α is reported in Additional file 1: Table S3. α is not presented as evidence of construct validity.

Measurement sensitivity check

A two-factor model fitted the LSNS-6 better than a one-factor model, although absolute fit remained poor (RMSEA = 0.155). Fit indices based on six items should be interpreted cautiously; we therefore retained the conventional total score and established cut-point for comparability with previous studies and report the family and friend subscales separately throughout. For the nine CES-D items, a one-factor model fitted poorly (CFI = 0.716; RMSEA = 0.137); separating positively worded affect items from negatively worded symptom items improved fit substantially (CFI = 0.978; RMSEA = 0.039). The two factors were only weakly correlated (r = 0.157). This structure reproduces the solution proposed by Radloff [27] and is consistent with subsequent evidence that positively worded items constitute a separate method factor [32]; we therefore regard it as a replication of the scale's known structure rather than as evidence of measurement failure. Nonetheless, the weak correlation implies that the total score combines two nearly orthogonal dimensions, which may attenuate associations involving depressive symptoms and should be borne in mind when interpreting the null indirect component through this mediator. The weighted α reported above (0.710) is close to the 0.72–0.78 reported for the same short form in Chinese older adults [28]. These analyses used weighted correlation matrices and treated ordered categorical items as continuous, without full complex-survey correction or ordinal threshold modelling, and should therefore be read as a measurement sensitivity diagnostic rather than a formal validation (Table 3).

Table 3 Descriptive factor fit check

Scale / structure	χ^2/df	CFI	RMSEA	SRMR
LSNS-6, one factor	3191.8 / 9	0.872	0.194	0.063
LSNS-6, two factors	1818.8 / 8	0.927	0.155	0.044
Depression (9 items), one factor	4781.3 / 27	0.716	0.137	0.108
Depression (9 items), two factors	395.1 / 26	0.978	0.039	0.027

Based on continuous-approximation likelihood checks on weighted correlation matrices; not an ordinal or fully survey-corrected confirmatory factor analysis. CFI, comparative fit index; RMSEA, root mean square error of approximation; SRMR, standardized root mean square residual.

Association between physical exercise and social isolation

In the fully adjusted model (M2), exercising at least once a month was associated with lower odds of social isolation (OR = 0.58; 95% CI, 0.48–0.71; P < 0.001). Standardized isolation probabilities were 30.17% under the infrequent-exercise assignment and 21.15% under the at-least-monthly assignment, a difference of −9.02 percentage points (95% CI, −12.24 to −5.80). These model-standardized probabilities differ from the unadjusted group percentages in Table 1 because they impose a common observed covariate distribution. Adding the health indicators (M3) strengthened the estimate slightly (OR = 0.57; 95% CI, 0.47–0.69; risk difference, −9.29 percentage points), whereas adding both mediators (M4) attenuated it slightly (OR = 0.59; 95% CI, 0.49–0.72; risk difference, −8.60 percentage points); direction and magnitude were otherwise materially unchanged, consistent with the small proportion of the total association accounted for by the indirect components reported below. These odds ratios are conditional odds contrasts and should not be read as relative-risk reductions.

In quantitative bias analysis for unmeasured confounding, the E-value was 2.84 for the point estimate and 2.19 for the confidence limit closest to the null. Because the outcome is common (weighted prevalence 25.1%), the odds ratio does not approximate a risk ratio; we therefore also applied the square-root approximation for converting the odds ratio to an approximate risk ratio, which yielded more conservative values of 1.95 and 1.67. An unmeasured confounder associated with both exercise and isolation by a risk ratio of at least 1.9 would therefore be required to explain away the observed association entirely. This threshold is moderate rather than high, and it neither rules out such confounding nor substitutes for a longitudinal design (Table 4). This threshold applies to the total exercise–isolation association and not to the indirect components, which are an order of magnitude smaller and were not appraised in this way.

Table 4 Physical exercise and social isolation

Model	OR (95% CI)	P	Pseudo R^2	Risk difference, percentage points	E-value (point estimate / upper CI)
M1	0.590 (0.486, 0.716)	<0.001	0.117	−8.81	2.78 / 2.14
M2	0.580 (0.477, 0.705)	<0.001	0.123	−9.02	2.84 / 2.19
M3	0.570 (0.468, 0.694)	<0.001	0.125	−9.29	2.90 / 2.24
M4	0.591 (0.487, 0.716)	<0.001	0.133	−8.60	2.77 / 2.14

M1, province fixed effects only; M2, M1 plus sociodemographic covariates; M3, M2 plus self-rated health, chronic conditions and functional limitation; M4, M2 plus depressive symptoms and social participation. M3 and M4 are parallel extensions of M2 rather than a cumulative sequence, both fitted on the identical analytic sample of 9,396. All models are survey-weighted with community-clustered confidence intervals. E-values are reported for the point estimate and for the confidence limit closest to the null.

Parallel indirect associations through depressive symptoms and social participation

Exercise was positively associated with social participation (adjusted coefficient 0.368; 95% CI, 0.073 to 0.663) but not with depressive symptoms (−0.031; 95% CI, −0.335 to 0.273), indicating that the null indirect component through depressive symptoms originates in the exercise–mediator relation itself rather than only in the final decomposition. On the common (log-odds) scale, KHB decomposition gave an indirect component of

−0.02948 through social participation (bootstrap 95% CI, −0.05646 to −0.00676) and of −0.00045 through depressive symptoms (95% CI, −0.00873 to 0.00538); the joint indirect component was −0.02992 (95% CI, −0.05765 to −0.00730), accounting for 5.38% (95% CI, 1.38% to 10.59%) of the total association. Both mediators together therefore account for only a small fraction of the association, and the remaining direct coefficient should not be reified as a 'pure' effect: it still comprises unmeasured pathways, residual confounding and measurement error. The direct coefficient was −0.52668 and the common-scale total was −0.55661; the latter differs from the raw reduced-model coefficient of −0.54474 because of rescaling, not because of inconsistency in the data. Obtaining 5,000 valid bootstrap replicates required 5,062 attempts, and the 62 non-estimable resamples were logged and replaced rather than discarded silently (Table 5, Fig. 2).

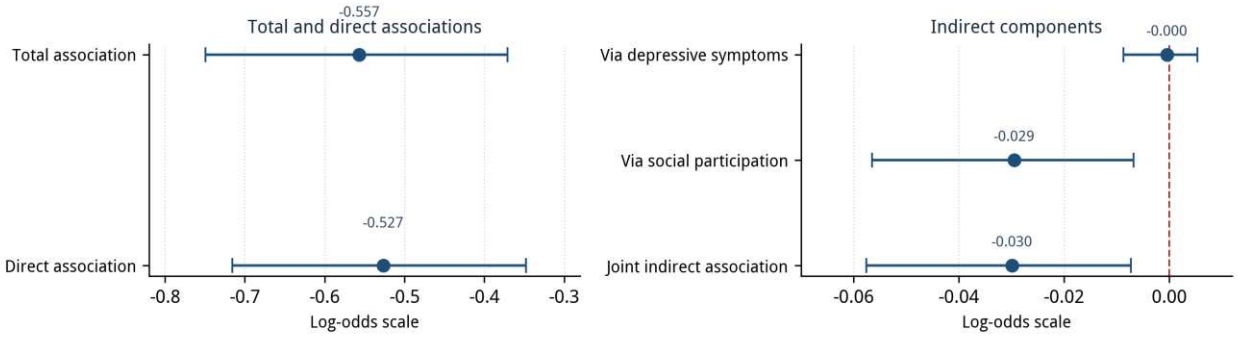
Table 5 Karlson–Holm–Breen parallel indirect associations

Component	Estimate	Bootstrap SE	95% bootstrap CI
Direct association	-0.5267	0.0938	-0.7158, -0.3476
Common-scale total	-0.5566	0.0955	-0.7494, -0.3712
Via depressive symptoms	-0.0004	0.0033	-0.0087, 0.0054
Via social participation	-0.0295	0.0126	-0.0565, -0.0068
Joint indirect association	-0.0299	0.0127	-0.0576, -0.0073
Joint indirect proportion, %	5.38	2.38	1.38, 10.59

Common logit scale, apart from the joint percentage; 5,000 valid percentile bootstrap estimates resampling communities within provinces.

Fig.2 Indirect associations on the common logit scale

Fig. 2



Points are point estimates; horizontal lines are 95% bootstrap confidence intervals (5,000 community-level replications, percentile method).

Left: total and direct associations. Right: indirect components through depressive symptoms and social participation and their joint value. Points are point estimates; horizontal lines are 95% bootstrap confidence intervals (5,000 community-level replications, percentile method). The interval for the depressive-symptom component includes zero; the intervals for social participation and for the joint indirect association exclude zero.

Four sensitivity checks, each with 2,000 bootstrap replications, additionally adjusted for health indicators; removed the loneliness item from the depression scale; removed both the loneliness and 'nothing to do' items together with the companionship item from the participation index; and replaced the frequency sum with the count of activity types. The first of these was motivated by the conceptual overlap between loneliness and the isolation outcome. The indirect component through social participation was unchanged in direction and remained statistically significant in all four, whereas that through depressive symptoms remained null. These checks address content overlap and indicator specification; the abbreviated scales are diagnostic variants and are not proposed as independently validated instruments.

Comparison of family and friend networks

Because higher LSNS-6 scores denote larger and more accessible networks, positive coefficients indicate richer rather than more impaired network resources. Total associations with exercise were positive for both the family network (0.614; 95% CI, 0.436 to 0.782) and the friend network (0.779; 95% CI, 0.554 to 0.997), and indirect component through social participation was positive in both (Table 6).

The friend-minus-family difference in the total association was 0.164 (bootstrap 95% CI, −0.005 to 0.327); because this interval includes zero, the friend network cannot be said to be more strongly associated with exercise overall. The corresponding difference in the joint indirect component was 0.019 (95% CI, 0.005 to 0.036), which excludes zero but is an order of magnitude smaller than the difference in total associations. This apparent discrepancy reflects a difference in precision rather than in substantive magnitude, and both estimates are small relative to the total associations from which they are derived. This comparison was exploratory; it neither substitutes for the comparison of total associations nor supports any general claim that friend ties matter more than family ties in this setting.

Table 6 Decomposition of family and friend networks

Component	Family	Friends
Total association	0.6145 (0.4359, 0.7823)	0.7789 (0.5540, 0.9969)
Direct association	0.5883 (0.4124, 0.7527)	0.7336 (0.5136, 0.9487)
Depressive-symptom pathway	0.0004 (-0.0048, 0.0082)	0.0009 (-0.0082, 0.0137)
Participation pathway	0.0258 (0.0065, 0.0466)	0.0443 (0.0112, 0.0775)
Friends − family: total contrast	0.1644 (-0.0051, 0.3267)	—
Friends − family: joint indirect contrast	0.0191 (0.0047, 0.0356)	—

Outcome is the 0–15 subscale score in the same direction (higher = richer network); linear models with 5,000 shared bootstrap draws. The final two rows are friends-minus-family contrasts.

Robustness, missing data and propensity weighting

The association retained its direction in unweighted analysis; in an expanded sample that did not require complete mediator data (N = 10,393); among respondents aged 60–79 years; among those without functional limitation in module B4; and when clustering was instead defined at a higher level of aggregation, by the shared prefix of the community code, as a sensitivity check on the clustering definition. Under multiple

imputation of missing covariates, the odds ratio attenuated but remained in the same direction (OR = 0.651; 95% CI, 0.541 to 0.784); this attenuation is consistent with the higher isolation prevalence among excluded respondents reported above, suggesting that complete-case analysis slightly overstated the protective association. Propensity-score weighting gave a risk difference of −9.19 percentage points (bootstrap 95% CI, −12.01 to −6.47), close to the main estimate of −9.02 percentage points. After weighting, the maximum absolute standardized mean difference fell from 0.437 to 0.051. Kish effective sample sizes were 2,794 and 1,250 in the two exposure groups (approximately 4,044 in total, compared with 9,396 unweighted), indicating a loss of precision from weighting rather than any exclusion of respondents. Propensity scores ranged from 0.0038 to 0.9537, with 291 respondents outside the 0.01–0.99 range; under restricted overlap (N = 9,105), the risk difference was −9.24 percentage points (95% CI, −12.45 to −6.54). All three estimates lie between −9.0 and −9.3 percentage points with substantially overlapping confidence intervals; the restricted-overlap estimate is similar in magnitude but applies to a more narrowly defined population and is therefore presented as exploratory (Table 7).

Table 7 Main sample and missing-data sensitivity

Specification	N	OR (95% CI)
Unweighted	9396	0.602 (0.498, 0.727)
Broader baseline sample	10393	0.573 (0.472, 0.695)
Age 60–79	8380	0.578 (0.474, 0.704)
No ADL limitations	8100	0.603 (0.491, 0.739)
Covariate multiple imputation	10474	0.651 (0.541, 0.784)

Multiple imputation of covariates only; 20 imputations; no imputation of depressive symptoms.

Demographic interactions and appraisal of hypotheses

In exploratory analyses, omnibus tests for effect modification gave uncorrected P values of 0.032 for age, 0.388 for sex and 0.089 for urbanicity; after Holm correction these were 0.097, 0.388 and 0.178. No interaction therefore remained statistically significant after correction for multiple testing. The age interaction was the only one nominally significant before correction, and this should not be interpreted as evidence of heterogeneity; within-subgroup significance cannot in any case establish differences between subgroups. Taken together, H1 was supported: exercising at least once a month was associated with lower social isolation. H2 was supported in the statistical sense but only weakly in substantive terms: the indirect component through social participation was detectable and its confidence interval excluded zero, yet it accounted for just 5.4% of the total association. H3 was not supported: the indirect component through depressive symptoms was close to zero and its interval included zero (Table 8).

Table 8 Omnibus interaction tests

Grouping	Wald χ^2	df	Raw P	Holm-corrected P
Sex	0.745	1	0.388	0.388
Urban / rural	2.889	1	0.089	0.178

Age group	6.859	2	0.032	0.097

The three omnibus tests form one correction family; significance within subgroups cannot be used to judge differences between groups.

Discussion

Principal findings

Physical exercise was associated with lower social isolation among older adults, and the association was stable in direction and magnitude across sensitivity analyses spanning alternative model specifications, weighting strategies, sample restrictions and approaches to missing covariate data. The parallel decomposition yielded two contrasting results: the indirect component through social participation was statistically detectable but very small, whereas that through depressive symptoms was not. The contribution of this study therefore lies less in confirming a mechanism than in quantifying how little of the exercise–isolation association is accounted for by two commonly invoked pathways, and in reporting the null result for one of them explicitly.

Contribution relative to existing evidence

The principal contribution of this study is not the identification of a new mechanism but a quantitative bound on two mechanisms that are frequently invoked. Three aspects deserve emphasis. First, by entering depressive symptoms and social participation into a single model and decomposing both on a common scale, we obtain estimates that are comparable with one another and, in principle, with any previous study using the same decomposition. Second, the mediated proportions reported in this literature have generally not been corrected for the change of scale that accompanies the introduction of a mediator into a logistic model, a problem that tends to inflate them; the joint proportion of 5.4% should therefore be read as an upper bound under our specification rather than as a direct replication of earlier figures. Third, we report the null component through depressive symptoms in full rather than dropping it, which addresses directly the concern set out in the Background that null findings are under-reported and that the explanatory power of the exercise–isolation association has consequently been overstated. A bounded and partly null result of this kind is informative for intervention design: it indicates that promoting social participation alone would be unlikely to reproduce more than a small part of the association observed here.

Our findings agree in direction with evidence that physical activity is indirectly associated with psychosocial outcomes in older adults through psychological and behavioural links [33], although outcomes and sampling designs differ substantially and effect sizes should not be compared directly. We use physical exercise throughout to refer to our exposure; where cited studies measured physical activity more broadly, we retain their terminology. Unlike studies of loneliness, in which depressive symptoms appear to carry part of the association with physical activity [13], we found no appreciable contribution of this pathway. One possible explanation is that loneliness is a subjective experience that overlaps substantially with emotional state, whereas the LSNS-6 indexes the number of relationships available for discussion and support—a structural indicator with less shared variance with emotional symptoms [34].

The joint indirect proportion of 5.4% is an order of magnitude below the figures commonly reported in this literature, and two comparisons illustrate the gap. First, a study of social exclusion among Chinese older adults reported that social participation alone contributed close to 40% of the total association, using a serial-mediation specification without correction for the change of scale that accompanies the introduction of a mediator into a logistic model [12]; because coefficients from nested logistic models are not directly comparable, that proportion is likely to be overstated, which is precisely the problem the KHB decomposition is designed to address [20,31]. Second, a recent cross-sectional study of loneliness reported that three indirect pathways together accounted for 58.6% of the total association [13]. Scale correction does not apply there, because the outcome was continuous; the difference is more likely to arise from the outcome and from the design. Loneliness is a subjective state that shares variance with emotional symptoms, whereas the LSNS-6 indexes the number of relationships available for discussion and support [31], and that model did not include province fixed effects or community-level clustering, both of which absorb shared community-level variance in our specification. A further consideration applies to both comparisons: the numerator and denominator of a mediated proportion are defined by the fitted model, so proportions taken from different specifications are not estimates of a common quantity. We therefore read the contrast as evidence that the indirect component is smaller than this literature suggests, and as an indication of the directions in which earlier estimates may be inflated, rather than as a like-for-like replication that we have failed to reproduce.

A related pattern in Additional file 1: Table S7 deserves mention rather than omission: the odds ratio for the highest exercise-frequency category (G2 = 9) was closer to the null than that for the intermediate categories (G2 = 2–8). We do not read these contrasts as a dose–response relation. The pattern is consistent with reverse causation, in that respondents who exercise most frequently may do so in response to health problems that also restrict social contact, and with compositional differences between the highest-frequency group and other exercisers. Because the ordering of adjacent frequency codes cannot be verified from the questionnaire and the ordinal contrast is reported as a diagnostic, we report the pattern but do not model it; it does not affect the primary monthly contrast.

A further consideration is internal to our own measurement: the near-orthogonality of the positive-affect and negative-symptom factors of the CES-D implies that the total score combines two distinct dimensions, which would attenuate any association involving depressive symptoms. Finally, unlike the bidirectional study cited above, our design cannot distinguish whether depression is an antecedent, a consequence, or both; this is precisely why we describe the depression pathway as unsupported rather than as playing no role.

Our results are consistent with theoretical accounts in which both physical exercise and social participation create opportunities for interpersonal contact. Social integration theory holds that repeated joint activities give individuals access to companionship, support and informational resources [35]. We did not observe these processes unfold over time, however, and three caveats follow.

First, the reverse direction is equally plausible: respondents with richer social networks may be more likely to take part in both exercise and community activities. Because the exposure and the mediator were measured

in the same wave, and given the E-value of 1.9 reported above, a modest degree of reverse causation or unmeasured confounding could account for the indirect component. Second, we did not measure community-level access to sports facilities, the density of community organisations or features of the neighbourhood environment, each of which could influence exercise and network resources alike and thus confound the association. Third, social participation and physical exercise covary closely, and this design cannot distinguish participation as a mediator from participation as the social expression of being physically active. Our sensitivity check removing the companionship item speaks directly to this concern: the indirect component persisted, suggesting that the pattern is not driven solely by the overlap between participation and the social character of exercise itself.

The observed indirect component should therefore be read as an empirical pattern consistent with a behavioural interpretation, not as grounds for expecting that increasing participation would transmit the effect of an exercise intervention. Although the indirect component through social participation was statistically significant, it would be misleading to treat it as the principal explanation of the overall association, since the two mediators together accounted for only a small fraction of it. The practical implication is that interventions aimed solely at increasing social participation would be unlikely to reproduce more than a small part of the association observed here. Nor should the residual direct coefficient be reified as a purely physiological or otherwise unmediated causal effect, since it still comprises unmeasured pathways, residual confounding and measurement error.

Several factors may account for the absence of an indirect association through depressive symptoms. The first and third are speculative; the second is supported by the factor analysis reported above. First, our exposure contrast was broad: it captured low-frequency participation rather than an objective dose of exercise, and such a contrast may be too weak to produce detectable emotional change. The same contrast did, however, yield a detectable indirect component through social participation, which argues against exposure measurement as the sole explanation and points instead towards the measurement of depressive symptoms. Second, the depression measure combines positively worded affect items and negatively worded symptom items, and our factor analysis indicated that a single dimension does not adequately describe its structure. Third, the outcome was a structural indicator of network resources rather than subjective loneliness or clinical depression, and is inherently less closely related to emotional symptoms. Together, these measurement and construct differences may explain why our results diverge from studies linking activity levels to depressive symptoms, but they do not establish why the pathway was null. Indeed, the adjusted association between exercise and depressive symptoms was already close to zero in our data, indicating that the null result originates in the exposure–mediator relation rather than in the decomposition itself.

Removing the loneliness and "nothing to do" items did not alter this conclusion, which argues against the concern that content overlap either produced or masked an association. We therefore interpret the null result as indicating uncertainty about the proposed mechanism, not its absence. It should not be taken as evidence that

emotional processes are unimportant in other populations or in longitudinal settings; nor, conversely, does a null result of this kind justify post hoc searches for alternative variables until one reaches significance.

Network-specific findings and implications for practice

Both network scores were positively associated with exercise, as reported above. Social convoy theory holds that family ties are structurally given and relatively stable, whereas friend ties depend on specific settings and require active maintenance [36]. Exercise may provide precisely such a setting for non-kin contact, so a stronger association with the friend network would be expected on theoretical grounds. Although the point estimate was higher for the friend network, the direct comparison between the two total associations was not statistically significant, and we therefore cannot conclude that exercise is more closely associated with friend ties. The friend-minus-family difference in the indirect component was positive but small in absolute terms: it is suggestive of the hypothesis linking exercise settings to non-kin relationships, but as an exploratory comparison it cannot establish a ranking of network benefits.

Where dose and access barriers are low, structuring exercise as a social activity—community-organised group exercise, fitness trails, and fixed-time, fixed-place group activities such as square dancing, a form of public group exercise widespread among middle-aged and older Chinese women—is more likely than simply raising individual exercise frequency to address physical activity and social connectedness at the same time. Community-based sports organisations can thus be viewed as dual-purpose intervention vehicles, addressing physical activity and social connectedness within a single setting. This is consistent with evidence on socially oriented physical exercise in middle-aged and older adults [12]. These suggestions follow from theory and from the direction of our estimates rather than from statistically robust evidence of differential benefit, and should be read as hypotheses for intervention studies to test rather than as recommendations. Our estimates remain cross-sectional and cannot be read as the expected benefit of any specific programme; intervention design should pair encouragement to participate with measurement of the actual opportunities for social interaction, individual preferences and environmental access that such programmes afford.

Two implications follow for programme design. First, because the indirect component through social participation is small, interventions that add a social component to an exercise programme should not be expected to yield network benefits proportional to the increase in participation; the opportunities for contact that a programme actually affords should be measured directly rather than inferred from attendance. Second, our estimates describe differences between groups at a single point in time and cannot be read as the expected response to a programme; they can justify a trial of socially structured exercise but cannot be used to predict its effect size.

Strengths and limitations

Strengths

Strengths include a large sample spanning 28 provinces; the explicit retention of alternative definitions and null results, so that the null depression pathway, the null network comparison and the non-significant

interactions are all reported regardless of significance; item-level coding checks ensuring that all nested models were fitted to an identical analytic sample; decomposition on a comparable scale rather than direct comparison of nested logistic coefficients; a common cluster bootstrap scheme for the family and friend comparisons; and reporting of failed bootstrap replications (1.2% did not converge). Agreement between separately fitted models and invariance to rescaling of the survey weights indicate stable computation, although these checks concern numerical implementation and do not validate the substantive assumptions.

Limitations

First, the data are cross-sectional. Exercise frequency (past year), depressive symptoms (past week) and network resources refer to different recall windows, so neither reverse causation nor unmeasured confounding can be excluded; cross-sectional decomposition may also diverge from the longitudinal processes it is intended to represent [37]. The parallel-mediator specification is therefore best regarded as an analytic framework, not as evidence that the two mediators are causally independent or that any particular temporal sequence holds.

Second, the mediated proportions reported in this literature have generally not been corrected for the change of scale that accompanies the introduction of a mediator into a logistic model, a problem that tends to inflate them, and have rarely been estimated with province fixed effects and community-level clustering; the joint proportion of 5.4% should therefore be read as an upper bound under our specification rather than as a direct replication of earlier figures.

Third, measurement was self-reported and coarse. The monthly threshold does not capture adherence to recommended activity levels, and the ordered frequency categories carry no information on duration or intensity, and the instrument does not distinguish group-based from solitary exercise—a distinction that is central to the social-contact mechanism we invoke, and one that our data cannot address. Measurement error in a coarse exposure of this kind can additionally bias decomposition estimates [38]. Respondents aged 80 years or older comprised approximately 11% of the analytic sample (about 1,016 of 9,396, derived from the age-restricted sample reported in Additional file 1: Table S3); because network attrition and the feasibility of exercise both change at advanced ages, the LSNS-6 cut-point and the monthly exercise contrast may not behave identically in this group, which our sample does not allow us to examine separately. The LSNS-6 cut-point identifies a screening state rather than a diagnosis, and module D14 covers selected organised activities rather than social participation in general.the resulting index was highly skewed (weighted mean 2.07, SD 4.04, on a 0–28 range), so its information content at the individual level is limited and any association involving it is likely to be attenuated. Exploratory factor analysis did not reach conventional fit thresholds for the LSNS-6 (RMSEA = 0.155), although it fitted well for the two-factor CES-D structure (RMSEA = 0.039); we retained the original scoring for transparency and comparability, not because these problems were resolved.

Fourth, documentation of the sampling design was incomplete. The 451 communities need not correspond to the original primary sampling units, and neither province-stratified resampling nor alternative clustering

defined by code prefixes can compensate for that missing information; variance estimates and population inferences should therefore be read with corresponding caution.

Fifth, propensity scores address observed imbalance only. The weighted outcome model does not by itself license causal interpretation or claims of double robustness, and the effective sample size fell substantially after weighting; we therefore treat this analysis as exploratory.

Sixth, the network comparisons and interaction tests involve multiple comparisons. Because no interaction survived Holm correction, we make no subgroup-specific recommendations.

Conclusions

Among older adults in China, exercising at least once a month was associated with lower social isolation, and this association was stable across multiple sensitivity analyses. Decomposition of the total association showed detectable but very small indirect component through social participation, whereas the indirect component through depressive symptoms was null; we report this null result in full rather than selectively. Both network dimensions were positively associated with exercise, but their relative contributions remain unresolved. These findings provide bounded descriptive evidence rather than evidence of causal mediation, and the null component is reported in full regardless of significance. Longitudinal and intervention studies are needed to establish whether these pathways are causal and, if so, to identify the dose and format of exercise that best combines physical activity with opportunities for social contact.

List of abbreviations

ADL: activities of daily living;

CES-D: Center for Epidemiologic Studies Depression Scale

CFI: comparative fit index

CI: confidence interval

CLASS: China Longitudinal Aging Social Survey

KHB: Karlson–Holm–Breen

LSNS-6: 6-item Lubben Social Network Scale

OR: odds ratio

RMSEA: root mean square error of approximation

SMD: standardized mean difference

SRMR: standardized root mean square residual.

Declarations

Ethics approval and consent to participate

This study utilized data from the Chinese Longitudinal Aging Social Survey (CLASS). According to the CLASS project's official ethics notice, the CLASS project received approval from the Academic Committee of the School of Population and Health, Renmin University of China, and informed consent was obtained from all participants. The present study is a secondary analysis of de-identified data obtained through a formal

application to the CLASS data repository under an approved data-use agreement; it involved no new participant recruitment and no new human subject research, and therefore did not require separate ethical approval. The research was conducted in accordance with the Declaration of Helsinki.

Clinical trial number

Not applicable.

Consent for publication

Not applicable.

Availability of data and materials

The CLASS 2023 individual-level data analysed in this study are not publicly available. De-identified data were obtained through a formal application to the CLASS data repository and may be requested from the Institute of Gerontology, Renmin University of China (http://class.ruc.edu.cn/index.htm), subject to an approved data-use agreement; individual records cannot be redistributed by the authors under the terms of that agreement. Aggregate results and the executable analysis code supporting the findings are available from the corresponding author on reasonable request.

Competing interests

The authors declare no competing interests.

Funding

This work was supported by Social Science Fund of Fujian Province:[grant no FJ2025C131].

This work was supported by Startup Fund for Advanced Talents of Ningde Normal University: [grant no 2023Y18].

Authors' contributions

Xiao-e Chu contributed to data acquisition, and to drafting and editing the manuscript. Ning Ji contributed to data analysis, and to revising the manuscript. Both authors read and approved the final manuscript, and no other person made a substantial contribution to the paper.

Acknowledgements

The authors thank the Institute of Gerontology, Renmin University of China, for providing the CLASS data, and all respondents and interviewers who participated in the survey.

Declaration of AI and AI-assisted technologies in the writing process

The authors did not use generative AI or AI-assisted technologies in the writing of this manuscript; all text was drafted, reviewed and edited by the authors, who take full responsibility for its content.

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