

Completed education and reported fitness/leisure nonparticipation in 2022 among linked CFPS respondents reporting participation in 2020: a preregistered analysis

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Research Article

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Abstract

Background

Educational differences in fitness/leisure participation may reflect unequal access to time, resources, and supportive environments. Among adults reporting participation in 2020, we examined whether a 2022 report of no participation differed by completed education. The same-item responses were interpreted as a two-wave report pair, not exact cessation.

Methods

This preregistered observational analysis used linked 2020 and 2022 China Family Panel Studies data. Eligible respondents were aged ≥ 25 years, directly self-reported in both waves, had complete mandatory data, and reported any past-12-month fitness/leisure participation in 2020. Original, non-imputed completed education entered a prespecified linear model. An unweighted modified-Poisson model with covariance clustered by baseline primary sampling unit (PSU) standardized the probability of a 2022 “never” report at 6 and 12 education years. No population weighting or attrition correction was applied.

Results

Among 4,639 respondents in 176 PSUs, model-standardized probabilities of a 2022 “never” report were 47.2% (95% confidence interval [CI], 44.2%–50.1%) at 6 education years and 28.8% (95% CI, 26.9%–30.7%) at 12 years. The absolute probability difference (12 minus 6 years) was – 18.4 percentage points (95% CI, – 21.1 to – 15.6; $P < .001$), and the complementary probability ratio was 0.610 (95% CI, 0.569–0.654). Logistic-link and cluster/influence checks were directionally concordant. Five prespecified analyses stopped under the preregistered numerical rule; robustness to the age threshold, education functional form, baseline frequency, and four-state specification remains unestablished.

Conclusions

In this selected linked analytic sample, the model-standardized probability of reporting no fitness/leisure participation in 2022 was lower at 12 than at 6 completed education years. The finding supports population-representative investigation of modifiable barriers among adults with fewer completed years of education. The association is noncausal and not population-representative; the report pair does not measure total physical activity or establish exact cessation.

Background

Participation in fitness/leisure activity is a potentially modifiable health behavior, yet opportunities to participate are socially patterned. Discretionary time, safe spaces, facilities, social support, and other

resources are distributed unequally [1, 2]. These inequalities raise a longitudinal question that cross-sectional comparisons cannot answer: among people who initially report some participation, does later reported nonparticipation vary by educational position?

Completed education is one indicator of socioeconomic position; it is neither a complete measure of socioeconomic status nor a manipulable intervention. It may capture cohort-specific differences in access to schooling and credentials as well as material resources, occupational conditions, health knowledge, and social networks. Accordingly, our empirical contrast compares model-standardized predictions at 12 and 6 completed education years within a selected baseline-reporter sample. It should not be interpreted as the effect of assigning six additional years of education.

Longitudinal studies have reported education-related differences in leisure-time activity [2]. Previous CFPS analyses examined activity change across 2018–2020 using transition models that included education [3] and assessed opportunity constraints and subsequent weekly exercise participation across 2018–2022 [8]. The overall direction of the present question is therefore not unprecedented. Our analysis instead focuses on 2020 and 2022, when QP701N used the same wording, past-12-month recall period, response categories, and routing [5, 6]. Using the same item reduces one source of cross-wave incompatibility, although it does not establish measurement invariance across survey contexts.

We therefore asked whether, among respondents aged ≥ 25 years who directly self-reported in both waves and reported any past-12-month fitness/leisure participation in 2020, the model-standardized probability of a 2022 “never” report differed at 12 versus 6 original completed education years. This preregistered replication-extension uses a clearly defined risk set, an absolute probability contrast, and a same-item 2020–2022 window. Inference is explicitly limited to the eligible linked analytic sample.

Methods

Study design, data source, and registration

We conducted a nonrandomized, two-wave observational secondary analysis of the 2020 and 2022 CFPS. The analysis used the byte-verified canonical person files. We did not analyze 2016 or 2018 data and did not construct a 2018→2020 transition. CFPS documentation and questionnaires are publicly catalogued by the Institute of Social Science Survey; access to individual-level data is governed by the provider [4–7].

The analysis was registered on the Open Science Framework (OSF; <https://doi.org/10.17605/OSF.IO/8ZXHS>; identifier 8ZXHS). Existing CFPS data had previously been accessed for result-blind aggregate feasibility work, but no substantive education–activity association or registered-model result had been estimated or viewed. Registration was therefore after result-blind feasibility work, not data-naïve. Before substantive estimation, the accepted OSF field text was concordant with a locally preserved, hash-bound statistical analysis plan (SAP; SHA-256 f93acb9c4707f31928ec5de353c67207d7055aec3b9d2d881bd9db8448794025). Because the supplied

OSF export did not provide separately retrievable attachment bytes, we do not claim byte identity for an OSF-hosted attachment. The displayed OSF timestamp lacked a time zone, so we did not independently convert it to UTC. The preserved project record documents evidence ingestion and concordance before the first substantive estimate (Supplementary Material).

Inferential target and survey design

The standardization population was the eligible linked sample with direct self-reports in both waves and complete data on all mandatory variables. Analyses were unweighted. Clustering the covariance by baseline PSU allowed outcomes within a PSU to contribute correlated score information; it did not alter point estimates, calibrate the sample, correct linkage or complete-case selection, or confer population representativeness. The provider variable $\subset \textit{population}$ was included as a fixed model covariate with six levels: Shanghai, Liaoning, Henan, Gansu, Guangdong, and other provinces/municipalities. These categories were neither reconstructed strata nor substitutes for survey weights. Because no validated longitudinal weight or prespecified observation model addressed the sequential selection processes, the analysis does not estimate population prevalence, population risk, a nationally representative association, or a national population effect.

Participants

The primary analytic sample included respondents who were aged ≥ 25 years in 2020, followed the direct-self pathway in both waves, had valid values for original completed education, age, the source gender field, sampling subpopulation, and baseline PSU, and reported a valid QP701N code of 1–7 in 2020. Respondents also had to link successfully to a valid direct-self QP701N response in 2022 and have complete data on all mandatory primary-model variables. The sole prespecified age-threshold sensitivity included respondents aged ≥ 18 years and used a separately prespecified and frozen age spline.

Fitness/leisure report pair

We limited the analysis to 2020–2022 because QP701N used the same wording, past-12-month recall period, response categories, and routing in both waves [5, 6]. The fitness/leisure item excluded walking or cycling undertaken solely for commuting; for respondents attending school, its programmed wording included physical-education or sports courses. We analyzed QP701N under the frozen direct-self routing rule. Valid codes 1–7 ranged from less than monthly to twice daily or more and denoted some reported participation; code 8 denoted “never.” QP702N was not used to derive duration, dose, moderate-to-vigorous physical activity, or guideline adherence.

The primary risk set comprised respondents with a 2020 participation report (codes 1–7). The binary outcome was a 2022 “never” report (coded 1) versus a 2022 participation report (coded 0). Thus, the outcome represents a literal two-wave report pair. It does not identify the timing of cessation,

uninterrupted participation between waves, objectively measured activity, total physical activity, or a clinical outcome.

Exposure and covariates

The exposure was the provider-constructed, non-imputed completed-education-years field (*cfps2020eduy*); use of *CFPS2020EDUY_IM* was prohibited. Education was centered at 9 years and divided by 3 for model fitting. The prespecified reporting contrast compared 12 with 6 completed years. This contrast compares two model values and does not represent an education intervention. Both anchors had to lie within the observed range, and every source sex/gender-by-sampling-subpopulation cell had to include at least 10 respondents within ± 1 year of each anchor. This outcome-blind support gate did not establish support across every joint education–age–covariate combination; dependence on functional form and joint support remains a limitation.

The primary model adjusted for baseline age, the source gender field, and sampling-subpopulation indicators. Age was represented by a natural cubic spline with boundary knots at 25 and 120 years and interior knots at 40, 55, and 70 years. Women (source code 0) were the reference category and men (code 1) the indicator; labels were verified against the canonical 2020 Stata value-label metadata. Shanghai was the reference sampling subpopulation, with Liaoning, Henan, Gansu, Guangdong, and other provinces/municipalities entered as indicators. The provider variable was retained as a prespecified design control, not treated as a reconstructed survey stratum.

Primary model and estimands

We fitted an unweighted modified-Poisson generalized linear model with a log link [9]:

$$\log E(Y_i | E_i, A_i, G_i, S_i) = \beta_0 + \beta E(E_i - 9)/3 + fA(A_i) + \beta GG_i + \gamma_{S_i}$$

Covariance was estimated with a CR1 sandwich estimator clustered by baseline PSU, using t inference with $G - 1 = 175$ cluster degrees of freedom. For education value e , fitted probabilities were standardized over the empirical covariate distribution: $\bar{p}(e) = (1/N) \sum_i \hat{m}(e, A_i, G_i, S_i)$. The primary estimand was the standardized probability difference, $RD_{12-6} = \bar{p}(12) - \bar{p}(6)$; the standardized probability ratio, $RR_{12/6} = \bar{p}(12) / \bar{p}(6)$, was complementary. Exponentiating the education coefficient gives a conditional ratio per 3 additional education years. Under the no-interaction log-link model, the 12-versus-6 ratio equals $\exp(2\beta E)$; the standardized probabilities and RD, but not this ratio, depend on the selected sample's empirical covariate distribution. Delta-method gradients retained covariance between the two anchors, using clustered CR1 covariance and t critical values. Two-sided P values were calculated for the prespecified contrasts and coefficient. The RD confidence interval was also compared

descriptively with a prespecified ± 0.02 zone; this was neither a formal equivalence test nor a validated policy threshold.

Secondary and sensitivity analyses

The prespecified secondary outcome comprised four report-pair states: participation→participation (reference), participation→never, never→participation, and never→never. A multinomial logit model was to use the primary covariates and clustered covariance. Componentwise standardized differences were to receive familywise 95% intervals from a single-step maximum-|t| procedure based on 100,000 multivariate-normal draws with seed 20260830.

Prespecified sensitivity analyses included an age threshold of ≥ 18 years, with spline boundaries at 18 and 120 and interior knots at 30, 45, 60, and 75 years; education categories (0–6, 7–9, 10–12, and ≥ 13 years); an education natural cubic spline with boundaries at 0 and 23 and interior knots at 6, 9, and 12 years; adjustment for baseline QP701N frequency; logistic regression; leave-one-PSU-out diagnostics; and a fixed-influence, studentized Rademacher wild-cluster procedure with 9,999 draws and seed 20260830. There was one primary estimand. The RR was complementary, and completed sensitivities were supportive rather than co-primary; their descriptive P values were not multiplicity-adjusted. The maximum-|t| procedure was not run because the four-state model met a preregistered stopping criterion before fitting.

Missing data and stopping rules

Analyses required complete observation on all mandatory variables. We did not impute the exposure, outcome, activity state, age eligibility, direct/proxy status, source gender field, sampling-subpopulation indicator, PSU, linkage key, or any other mandatory field. Multiple imputation by chained equations (MICE), inverse-probability-of-observation weighting, survey weighting, missing-not-at-random analysis, and new attrition correction were not used.

Prespecified stopping criteria included analytic $N < 500$; a primary outcome state with fewer than 100 respondents; an education category with fewer than 50; fewer than 30 contributing PSUs or cluster degrees of freedom < 30 ; failure of local support, rank, convergence, clustered covariance, privacy, or standardized-probability checks; material optimizer dependence; and a column-scaled design-matrix condition number ≥ 100 . The condition number was calculated after dividing each design-matrix column by its Euclidean norm, and the threshold was fixed before substantive results were estimated. The four-state model had additional state, joint-cell, and separation criteria. A sensitivity model could not replace a stopped primary model.

Reproducibility, implementation corrections, and disclosure control

Automated tests and role-separated AI-assisted review supported implementation quality assurance; they were not external human peer review and did not replace human scientific accountability. A second construction implementation reproduced eligibility and key derived states before modeling. After unblinding, corrections were limited to coding, implementation, lineage, or preregistration-compliance errors. Run 1 incorrectly fitted and serialized five branches that should have stopped under the condition-number rule. Those values remain quarantined for audit and are excluded from scientific reporting; the final compliant pipeline stops the branches before scientific result serialization. No correction changed the population, exposure, outcome, estimand, covariates, anchors, or primary model, and released primary, logistic, and cluster results were unchanged. The eight post-unblinding ledger entries concerned implementation or preregistration compliance, not scientific-design deviations, and no stopped analysis was replaced.

The final pipeline passed 31 automated tests and completed a deterministic clean rerun from separately reconstructed governed inputs using the same code and locked environment. Primary and clean-run result objects were byte-identical. The minimum disclosure threshold was 10. No person identifiers, PSU identifiers, row-level records, or small cells were exported. A post-freeze descriptive aggregate artifact used only the hash-matched frozen analytic dataset, produced no exposure–outcome cross-tabulation or new inferential result, and is bound by SHA-256

29682a704160b4862a620b6fed3e992d75c6e076977d97d55a24ca344529e917. OpenAI GPT-based Codex/ChatGPT systems were used to assist with code scaffolding, role-separated internal quality assurance, document formatting, manuscript DOCX formatting and layout editing, figure preparation, and language editing.

Results

Sample construction and characteristics

The 2020 and 2022 person files contained 28,530 and 27,001 records, respectively; valid one-to-one linkage yielded 19,471 records. Applying the age ≥ 25 years, direct-self, complete mandatory-data, and valid QP701N requirements in both waves produced the nested 12,651-person sample eligible for the four-state analysis. Component-specific exclusion counts were not retained in the frozen aggregate and were not reconstructed after results were available. Restricting this set to respondents with a 2020 participation report yielded the primary sample of 4,639 respondents in 176 PSUs: 3,102 reported participation in both waves, and 1,537 reported participation in 2020 followed by “never” in 2022 (Table 1; Fig. 1).

The primary sample had a mean age of 47.0 years (SD, 14.6) and a median age of 47 years (interquartile range, 34–58). Mean completed education was 10.8 years (SD, 4.5), with a median of 12 years (interquartile range, 9–15). The sample included 2,463 men (53.1%) and 2,176 women (46.9%); all six sampling-subpopulation categories were represented. These summaries came from the frozen

aggregate reporting artifact, and every released count was at least 10. The provider’s source label and value labels were retained for the source gender field.

Table 1
Characteristics of the primary analytic sample

Characteristic	Value
Primary analytic sample, N	4 639
Baseline PSUs, n	176
Participation→participation, n (%)	3 102 (66.9)
Participation→never, n (%)	1 537 (33.1)
Age, mean (SD), years	47.0 (14.6)
Age, median (IQR), years	47 (34–58)
Education, mean (SD), years	10.8 (4.5)
Education, median (IQR), years	12 (9–15)
Men, n (%)	2 463 (53.1)
Women, n (%)	2 176 (46.9)
Age range, years	25–87
Education range, years	0–23
Shanghai sampling subpopulation, n (%)	327 (7.0)
Liaoning sampling subpopulation, n (%)	435 (9.4)
Henan sampling subpopulation, n (%)	587 (12.7)
Gansu sampling subpopulation, n (%)	597 (12.9)
Guangdong sampling subpopulation, n (%)	377 (8.1)
Other provinces/municipalities, n (%)	2 316 (49.9)
Notes: The primary analytic sample is the baseline-reporter risk set and differs from the complete 12,651-person two-wave sample eligible for the four-state analysis. Values were obtained from the frozen aggregate artifact (SHA-256 29682a...529e917). The minimum released cell count was 10. IQR, interquartile range; PSU, primary sampling unit. Counts and percentages are unweighted summaries of the analytic sample.	

Primary association

The primary model passed all prespecified entry and numerical criteria: rank 12/12, scaled condition number 97.26, 176 contributing PSUs, and 175 cluster degrees of freedom. Standardized to the covariate

distribution of the primary sample, the modeled probability of a 2022 “never” report was 0.472 (95% CI, 0.442–0.501) at 6 education years and 0.288 (95% CI, 0.269–0.307) at 12 years (Table 2; Fig. 2).

The primary standardized probability difference for 12 versus 6 education years was – 0.184 (95% CI, – 0.211 to – 0.156; $P<.001$). The complementary standardized probability ratio was 0.610 (95% CI, 0.569–0.654; $P<.001$). The conditional model-based ratio per 3 additional education years was 0.781 (95% CI, 0.754–0.809); this parameter is distinct from the standardized 12-versus-6 ratio. The full confidence interval for the probability difference lay below – 0.02.

Table 2
Primary standardized education contrast

Quantity	Estimate	95% CI	<i>P</i> value
Probability at 6 education years	0.4716	0.4424 to 0.5007	—
Probability at 12 education years	0.2876	0.2686 to 0.3066	—
RD, 12 minus 6 years	–0.1839	–0.2115 to – 0.1564	< .001
RR, 12 divided by 6 years	0.6099	0.5690 to 0.6539	< .001
Notes: Estimates are from the unweighted modified-Poisson model with CR1 covariance clustered by baseline PSU and 175 cluster degrees of freedom. Probabilities were standardized to the covariate distribution of the primary analytic sample. Probabilities at the two education anchors were not tested against zero. RD was the primary estimand; RR was complementary.			

Completed and stopped sensitivity analyses

The logistic-link sensitivity produced a standardized probability difference of – 0.189 (95% CI, – 0.217 to – 0.162) and a probability ratio of 0.605 (95% CI, 0.566–0.648). The prespecified fixed-influence, studentized Rademacher wild-cluster procedure (9,999 draws; minimum nonzero Monte Carlo $P=.0001$) gave an interval of – 0.211 to – 0.156 and $P=.0001$ for the primary difference. All 176 leave-one-PSU-out refits were valid; the maximum absolute change in the difference was 0.0044, the 95th percentile was 0.0022, and no sign reversal occurred. These checks address link-family and cluster/influence uncertainty, not selection, education functional form, baseline-frequency composition, or population transportability.

Five analyses met the prespecified scaled-condition-number stopping rule: age ≥ 18 years (243.62), education categories (103.11), education spline (206.51), baseline-frequency adjustment (105.52), and the four-state multinomial model (100.83). In the final compliant run, these branches stopped before scientific result serialization. Their disposition is not a null finding; no four-state component probabilities or maximum-|t| intervals were interpreted (Table 3; Supplementary Figure S2).

Table 3
Disposition of prespecified sensitivity analyses

Analysis	Result or diagnostic	Disposition
Logistic-link RD	-0.1894 (95% CI, - 0.2168 to - 0.1621)	Completed
Wild-cluster interval for primary RD	-0.2114 to - 0.1565; $P=.0001$	Completed
Leave-one-PSU-out	176/176 valid; no sign reversal	Completed
Age ≥ 18	condition number 243.623	Stopped
Education categories	condition number 103.108	Stopped
Education spline	condition number 206.506	Stopped
Baseline frequency adjustment	condition number 105.524	Stopped
Four-state multinomial	condition number 100.829	Stopped
Notes: The prespecified stopping threshold was a condition number ≥ 100 . Stopped analyses yielded no released scientific estimates and should not be interpreted as null findings.		

Discussion

Principal findings

Among selected linked respondents who reported fitness/leisure participation in 2020, the model-standardized probability of a 2022 “never” report was 18.4 percentage points lower at 12 than at 6 completed education years (95% CI, 15.6 to 21.1 points lower). Logistic-link, wild-cluster, and leave-one-PSU-out checks were directionally concordant. Prespecified analyses of the age threshold, education categories, education spline, baseline frequency, and four-state outcome stopped under the numerical rule; robustness across those dimensions therefore remains unknown.

The full interval lay below the prespecified - 0.02 descriptive boundary. This comparison was not a formal equivalence test and did not define a policy threshold. The estimate is an association under the prespecified linear model in a selected sample; it is neither an effect of education nor evidence that assigning additional schooling would prevent later reported nonparticipation.

Comparison with previous literature

The direction of the association is consistent with longitudinal evidence that leisure-time inactivity is more common at lower educational levels [2] and with earlier CFPS analyses that included education in activity-transition models [3]. Zhang and Li assessed opportunity constraints and subsequent weekly exercise participation using CFPS 2018–2022 [8]. The present study differs by defining a 2020 baseline-participant risk set, using a 2022 “never” report, prespecifying a standardized 12-versus-6-year absolute contrast, and restricting analysis to waves with the same QP701N instrument. It is therefore a

replication-extension rather than the first evidence of education-related differences in later activity reporting.

Interpretation and possible social pathways

Completed education may capture cohort-specific schooling opportunities, material resources, working conditions, discretionary time, access to facilities and supportive environments, health information, and social networks. These are plausible explanations derived from social-epidemiologic theory, not mechanisms estimated in this analysis. Early-life socioeconomic circumstances, cohort, geography, health, occupation, and other unmeasured factors may confound the association. Education-related differences in understanding, recall, or socially desirable reporting may also contribute.

Public-health implications

This selected-sample finding supports population-representative monitoring to determine whether the education-related difference generalizes beyond the analytic sample. It also supports prospectively designed research on modifiable barriers among adults with fewer completed years of education. The analysis does not estimate the magnitude of a national disparity, identify a barrier to target, quantify preventable cases, or support an education intervention. Because occupational, transport, household, and total activity were not measured, the finding should not be generalized to total physical activity.

Strengths

Strengths include use of verified canonical data sources, result-blind registration after feasibility work, a prespecified selected-sample estimand, independent premodel construction checks, covariance clustered by baseline PSU, reporting on both absolute and relative scales, transparent numerical stopping, a complete correction ledger, and a deterministic byte-identical rerun in a locked environment. The shared QP701N wording, recall period, response categories, and routing in 2020 and 2022 reduce one source of cross-wave incompatibility.

Limitations

First, inference is restricted to respondents who reported participation in 2020, linked successfully, directly self-reported in both waves, and had complete mandatory data. Education may be related to each selection process. Conditioning on these processes could attenuate, amplify, or otherwise change the observed contrast, and neither the direction nor magnitude of that change is identified. The unweighted analysis therefore does not estimate national prevalence, population risk, or a national education gradient. PSU clustering affects covariance estimation only; it neither corrects selection nor creates representativeness.

Second, QP701N is a self-reported, past-12-month fitness/leisure frequency item. Baseline codes 1–7 range from less than monthly to twice daily or more; because the frequency-adjusted analysis stopped, differences in baseline-frequency composition remain unresolved. For respondents attending school, the item included physical-education or sports courses. Two reports cannot distinguish intermittent activity,

reporting error, persistent within-year nonparticipation, or the timing of change. Identical wording does not establish measurement invariance across survey mode, season, pandemic context, or respondent interpretation.

Third, complete observation and successful linkage do not remove selection bias, and no new attrition correction was prespecified. Fourth, adjustment was limited to age, the source gender field, and sampling-subpopulation indicators, leaving substantial potential for residual and unmeasured confounding. Fifth, the 2020 and 2022 recall periods covered different phases of pandemic disruption and recovery. Sixth, the primary condition number was close to the prespecified threshold, and five substantively important analyses stopped. Completed link and cluster/influence checks therefore should not be interpreted as broad robustness. Finally, the contribution is deliberately incremental: a transparent replication-extension rather than a causal or population-inference design.

Conclusions

Among selected linked CFPS respondents who reported fitness/leisure participation in 2020, the model-standardized probability of a 2022 “never” report was lower at 12 than at 6 completed education years. The 18.4-percentage-point difference was precise under the primary specification and directionally concordant in completed link and cluster/influence checks, but important prespecified robustness analyses were unavailable. This bounded, noncausal replication-extension applies to the observed analytic sample; it does not establish a national education gradient, a difference in total physical activity, or an exact cessation event.

Abbreviations

CFPS	
China Family Panel Studies	
CI	
confidence interval	
CR1	
small-sample-adjusted cluster-robust covariance estimator	
IQR	
interquartile range	
MICE	
multiple imputation by chained equations	
MNAR	
missing not at random	
OSF	
Open Science Framework	
PSU	
primary sampling unit	

QA
quality assurance
RD
standardized probability difference (risk difference)
RR
standardized probability ratio (risk ratio)
SAP
statistical analysis plan.

Declarations

Ethics approval and consent to participate

The China Family Panel Studies (CFPS) was reviewed and approved by the Peking University Biomedical Institutional Review Board (IRB00001052-14010). CFPS obtains informed consent from respondents before each wave of data collection. The present study was a secondary analysis of existing CFPS data and involved no new participant recruitment or primary data collection.

Consent for publication

Not applicable.

Competing interests

The authors declare that they have no competing interests.

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Author Contribution

ZT conceptualized the study, developed the methodology, curated and analyzed the data, implemented the analytical workflow, conducted validation and visualization, and drafted the manuscript. RL contributed to study conceptualization and methodology, supervised the study, acquired funding, administered the project, contributed to validation, and critically revised the manuscript. Both authors read and approved the final manuscript.

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Data Availability

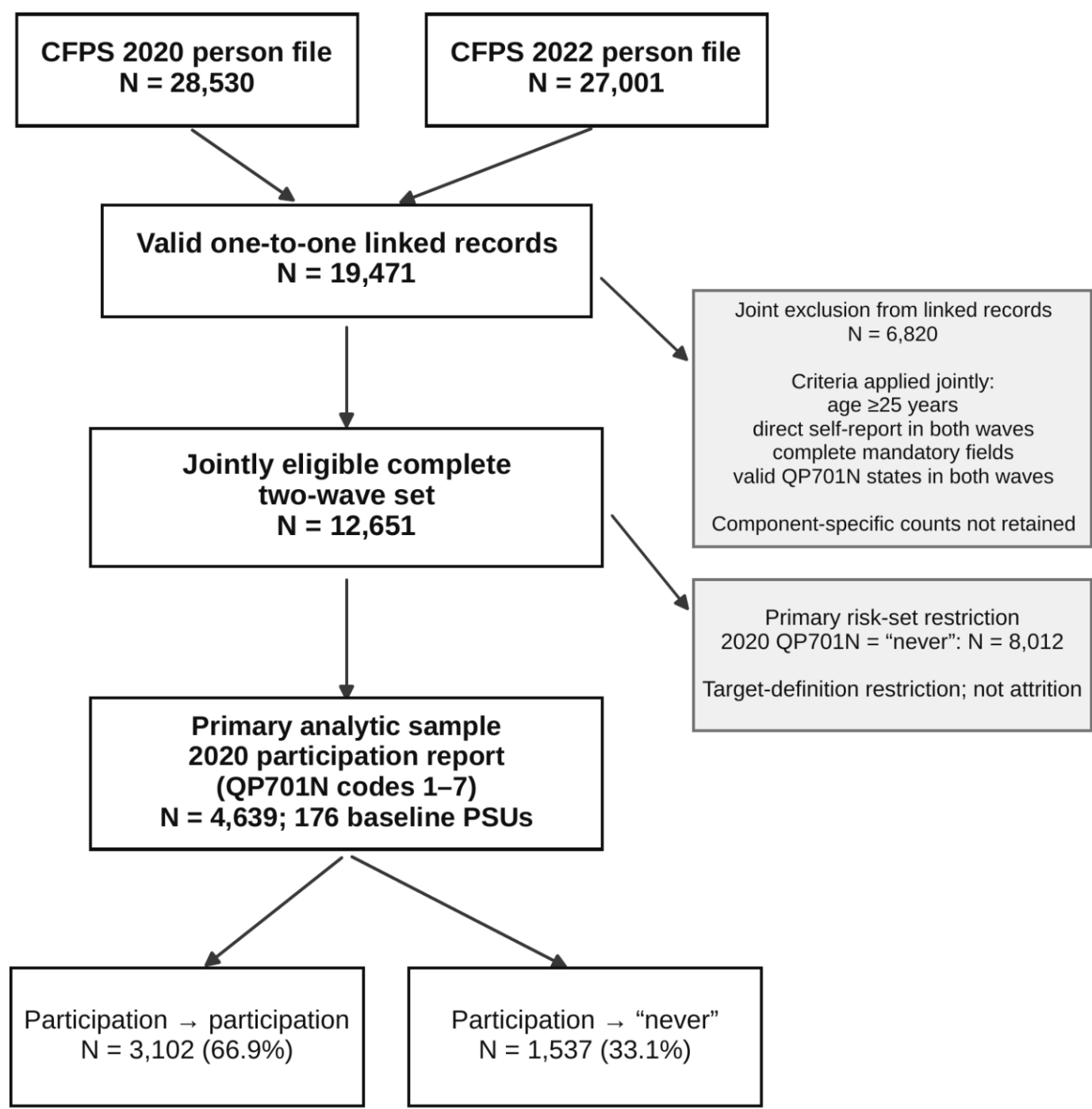
The CFPS microdata analyzed in this study are available to eligible researchers through the official China Family Panel Studies data platform [4], subject to the provider's registration and data-use requirements. Under CFPS data-sharing rules, the authors are not permitted to redistribute CFPS microdata, including modified or derived microdata, on journal websites or unauthorized third-party platforms. Public-safe reproducibility materials generated for this study—including the final construction and analysis code, tests, environment specification, aggregate reporting outputs, correction ledger, and manifests—contain no CFPS microdata or participant/PSU identifiers and are available from the corresponding author on reasonable request.

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Figures



Frozen, unweighted analytic-sample counts. The 19,471 → 12,651 exclusion components were not reconstructed.

Figure 1

Study flow and construction of the primary analytic sample. The 2020 and 2022 person files contained 28,530 and 27,001 records, respectively; valid one-to-one linkage yielded 19,471 records. Age ≥ 25 years, direct self-report in both waves, complete mandatory fields, and valid QP701N states in both waves were applied jointly, yielding 12,651 eligible complete two-wave records. Component-specific counts within this joint gate were not retained and were not reconstructed. Restriction to a 2020 participation report (QP701N codes 1–7) yielded the primary analytic sample of 4,639 respondents across 176 baseline PSUs; the 8,012 baseline “never” reports are shown as a risk-set definition rather than attrition. The primary sample comprised 3,102 participation→participation and 1,537 participation→“never” report pairs. Counts are unweighted. PSU, primary sampling unit.

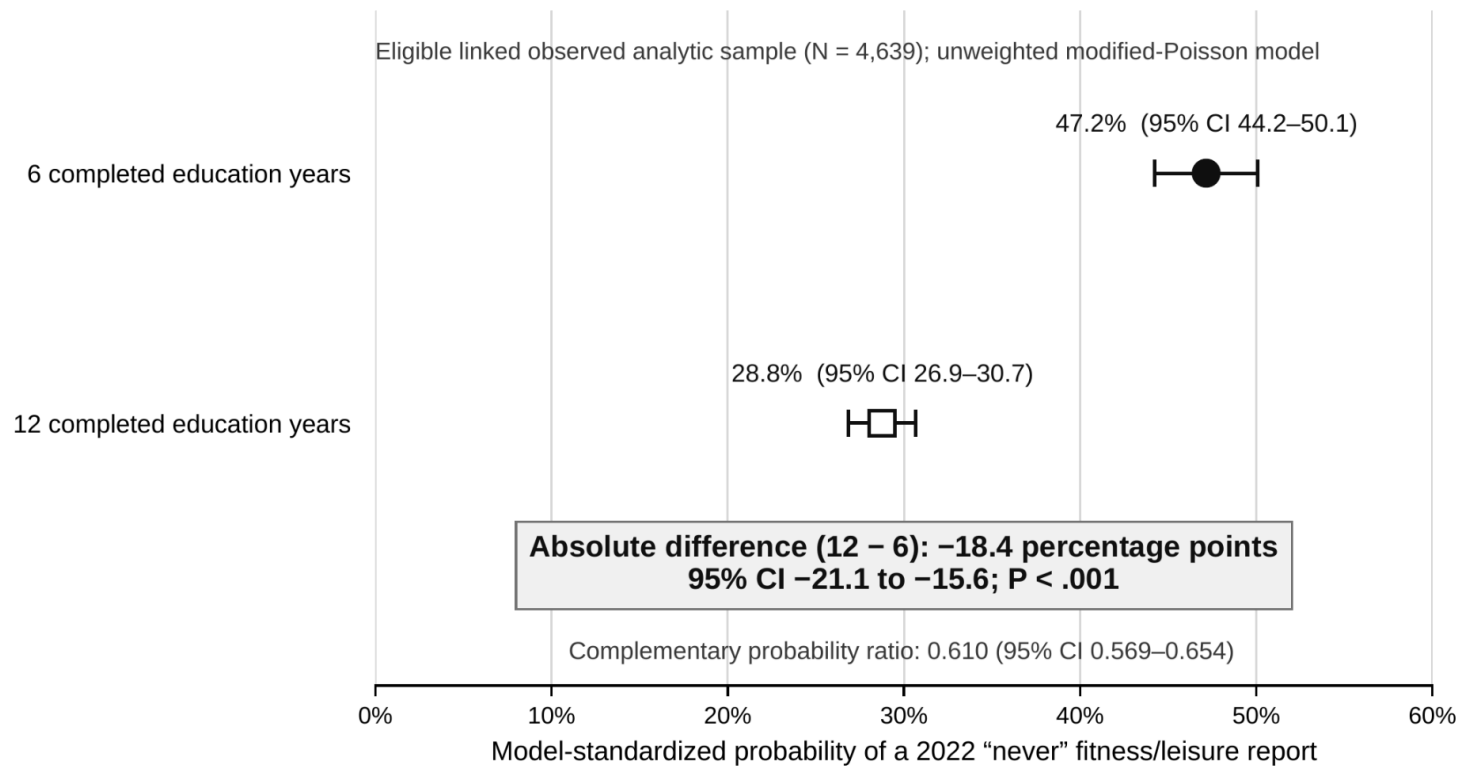


Figure 2

Model-standardized probability of a 2022 “never” report at 6 and 12 education years. Points show probabilities predicted by the unweighted modified-Poisson model and standardized to the covariate distribution of the eligible linked observed analytic sample (N=4,639); error bars are 95% confidence intervals based on CR1 covariance clustered by baseline PSU with 175 cluster degrees of freedom. The probability was 47.2% (95% CI, 44.2%–50.1%) at 6 completed education years and 28.8% (26.9%–30.7%) at 12 years. The primary absolute probability difference (12–6) was –18.4 percentage points (95% CI, –21.1 to –15.6; P<.001); the complementary probability ratio was 0.610 (95% CI, 0.569–0.654). These are model-standardized values, not raw education-group proportions. The figure does not establish linearity between the anchors, causality, national representativeness, total physical activity, or exact cessation.

Supplementary Files

This is a list of supplementary files associated with this preprint. Click to download.

- [SupplementaryFigureS2SensitivitySummary.pdf](#)
- [SupplementaryFigureS1MeasurementRegimeComparisonORIGINAL.png](#)