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SYSTEMATIC REVIEW

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A systematic review and meta-analysis finds that psychological well-being predicts all-cause mortality independently of psychological ill-being

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Abstract

Background Does psychological well-being (e.g. life satisfaction, happiness, meaning in life) predict the length of a person's life, or are measures of well-being simply assessing the absence of ill-being (depression, loneliness, sadness) and are therefore redundant with ill-being measures? Individual studies have examined this question by measuring and controlling for both well-being and ill-being, but inconsistent findings have been reported.

Methods A systematic review and random-effects meta-analysis was conducted on the literature of prospective longitudinal studies linking all-cause mortality to well-being. Studies that controlled for ill-being were compared to studies that did not control for ill-being to identify the unique predictive ability of well-being. Separate effects were estimated for different well-being constructs (e.g., optimism) and study characteristics (e.g., patient population). Publication bias was assessed via sensitivity analyses such as removing outliers, trim-and-fill, and RoBMA.

Results A total of 99 prospective longitudinal studies predicted future all-cause mortality from current measures of well-being and controlled for concurrent measures of ill-being. Overall, well-being predicted significantly lower mortality rates when controlling for ill-being (Hazard Ratio [HR] = 0.86 [0.84-0.88]). This finding was apparent when well-being was operationalized with very different constructs and assessments, ranging from optimism to life satisfaction to perceived social support. The magnitude of this association was highly heterogeneous across studies (I^2~0.75) with the 95% Prediction Interval for the Hazard Ratio ranging from 0.72 to 1.04. Findings were robust to methods that adjusted effect sizes for the influence of outlying observations, publication bias, or substantial heterogeneity. Surprisingly, the average effect of well-being on mortality was not any stronger when studies failed to control for ill-being (HR = 0.90 [0.88-0.92]), which suggested that well-being predicted mortality independently of the association with an absence of ill-being.

Conclusion These results justify research seeking to evaluate the causal role of a broad range of well-being constructs on longevity because they imply that well-being is not simply a lack of ill-being, but more research is needed to identify the causal impacts and mechanisms of these associations.

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Keywords Well-being, Mortality, Statistical Methods, Survival, Health Behaviors, Theories of Well-being, Longevity

Background

When psychological factors help explain who lives, who dies, and when, then it demonstrates the value of a particular psychological theory to a broad audience of scholars, non-scholars, and policymakers seeking to understand one of humanity's most fundamental puzzles. Of course, people die for many reasons that are not psychological, but to the extent that individual's choices affect their health or exposure to life-threatening events—when diet affects the likelihood of fatal cardiac events, or when personal decisions lead to crime or risk-taking or even suicide—then constructs under the purview of psychologists may well be important in statistical models of individuals' longevity. For example, "deaths of despair," such as suicide, drug overdoses, and alcohol-related cirrhosis, are plausibly influenced by psychological constructs such as a person's life satisfaction [1]. People's psychological states and traits have been empirically related to a suite of behaviors—such as impulsive decision-making, poor diet or sleep, substance abuse, and criminal behaviors—that could influence longevity [2–4]. But how well understood are the life-or-death consequences of these psychological constructs?

An ongoing debate in epidemiology and psychology concerns the following question: Does human mortality depend on the *presence* of human happiness and flourishing, or merely on the *absence* of mental health symptoms or bad feelings? The answer to this question carries important implications for theory and practice, for example by informing whether finite clinical resources are better directed toward the reduction of mental health problems or the promotion of positive psychological states and characteristics. Most studies focus on whether psychological well-being (i.e. positive mental functioning such as happiness, self-esteem, a purpose in life, and so on [5]) predicts survival outright, but some measure its unique association with survival above and beyond measures of ill-being. Additionally, the literature varies widely in terms of constructs, populations, and study designs. This heterogeneity and lack of focus on study methodology has confused inferences about the unique associations between well-being, ill-being, and mortality.

Some prior research has asserted that measures of well-being are redundant with measures of ill-being [6]. According to this *redundant continuum model* of well-being, a participant's reported level of well-being simply reflects an absence of a corresponding level of ill-being. Some internal and construct validity analyses (e.g., confirmatory factor analysis [7]) have found that a well-being construct loaded onto a different factor than an ill-being construct, but according to the redundant model this is due to inconsequential unshared variation (e.g. cognitive demands of reverse-scored items [8]). In summary, the redundant model would lead to the prediction that there would be little, if any, unique predictive value of well-being measures once a model controlled for ill-being.

A competing prediction comes from the *separate continuum model.* This is the perspective that well-being and ill-being measures each capture different (and meaningful) variation in psychological states that could relate to longevity [7, 9]. For example, it is well-known that people who have experienced early childhood trauma (which can lead to depression or anxiety) can also have a sense of prosocial purpose to prevent such trauma for others, or to help counsel others who had similar experiences, even though ill-being symptoms remained. The depression or anxiety could result in earlier mortality, but the sense of purpose could have a corresponding effect on later mortality, perhaps by leading people to connect with other volunteers, thus reducing the risk of deaths of despair. Thus, the separate continuum model would lead to an empirical prediction that the association between well-being measures and mortality would be independent from the association between ill-being and mortality, even though ill-being and well-being were negatively correlated. Several foundational measures in positive psychology have been created with this perspective of well-being as positive mental health in mind [10, 11]. This is not to say that negative emotions and attitudes like loneliness and pessimism can be helpful in the short-term but that their long-term presence signals a lack of improvement in one's circumstances and one's psychological response to them.

This meta-analysis seeks to understand whether the predictive power of well-being for all-cause mortality is redundant with versus separate from the predictive power of ill-being. It does so as a systematic collection and statistical aggregation of the results of studies of well-being's statistical association with all-cause mortality that controlled for an ill-being construct. All-cause mortality is the outcome of interest because it is a clear, important, objective measurement, a common outcome in a diverse range of longitudinal studies, and the target of many behavioral health interventions, making it a helpful target for investigation in terms of statistical power. Previous meta-analyses of well-being and mortality have found high heterogeneity between studies that differed in the well-being construct of interest (as high as I^2 = 86% [12]), and therefore it was important in the present meta-analysis to examine variation in links to mortality across well-being constructs, via sensitivity analyses. Further, due to the long history of the "file-drawer problem" in psychology, in which only significant results are published, multiple methods were used to assess publication bias and sources of heterogeneity of effects [13].

Park and colleagues [14] pointed out that, though there are many longitudinal associations of well-being and mortality, few of them examine mediators. The preliminary consensus for these mediators appears to be that positive psychology constructs promote health behaviors and high-quality social supports, improving allostatic load and lowering inflammation [15]. Zhang and Han examined how physiological, behavioral, and social mediate the effect of positive affect on mortality as well as how stress moderates it [16]. The authors found evidence for both effects but could not aggregate the results meta-analytically. Identifying significant covariates that reduce the effect of well-being on mortality is an important step in finding potential mediators [17]. The present meta-analysis therefore examines potential mediators and moderators through meta-regression.

Why examine well-being adjusted by ill-being, and not vice versa? After all, ill-being has been shown to contribute to mortality [18]. First, ill-being has a more empirically-established relationship with mortality [19] and so researchers have been more skeptical of the links between well-being and mortality than they have of the relationship between ill-being and mortality. Positive psychology research on well-being, more broadly, has often been criticized for a lack of novelty compared to extant research on psychological malfunctioning [6]. No defense has yet overcome this critique by showing that the uncorrelated variation in well-being is related to mortality independently of ill-being [20]. Even though the evidence for the defense has been found in individual studies, their weight has not been brought to bear in the aggregate. Notably, the same criticism is rarely leveled at ill-being research. In a recent meta-analysis of 203 studies estimating the effect of mental disorder diagnoses on mortality, not one study noted that it adjusted for a well-being construct [18]. Meanwhile, about half of all studies of well-being and mortality also controlled for ill-being. Thus, in the present meta-analysis, there was far more evidence about the link between well-being and mortality, with and without controlling for ill-being, than vice versa. We nevertheless report a supplementary analysis of the more limited evidence base concerning ill-being's link to mortality with and without controlling for well-being.

Prior meta-analyses and reviews

Previous meta-analyses have found that well-being constructs such as optimism [21], purpose in life [22], positive affect [16], and well-being in general [13, 23] are associated with lower mortality. Although these meta-analyses included some studies that controlled for the effect of ill-being, these studies were not examined separately, nor where they statistically compared to those that did not control for ill-being. Zhang and Han [16] reported a Q-statistic for a test of heterogeneity between subgroups, and found that there were no significant differences in the magnitude of association between positive affect and mortality for studies that did versus did not control for negative affect. However, the authors did not report the magnitude of association for each subgroup of studies, and they did not assess the robustness of their finding.

Other meta-analyses and reviews have probed the importance of well-being on other health outcomes, some of which also mentioned controlling for ill-being. Boehm and Kubzansky [24] found that well-being is associated with less cardiovascular disease and mortality. The negative health behaviors of cigarette smoking and problematic drinking were inversely related to well-being, and health behaviors of physical activity, sleep quality, and healthy food consumption were positively related to well-being. The authors also found protective associations with atherosclerosis, heart rate variability, cardiac function, inflammation, and some measures of metabolic function. The authors mention that the effects on health were largely independent of ill-being, but there were too few studies for a quantitative, meta-analytic summary. A meta-analysis by Ngamaba and colleagues [25] found a moderate (r~.35) correlation between concurrent reports of physical health and well-being, highlighting the importance of longitudinal investigation to understand the causal pathway between the two. Lamers and colleagues [26] focused on recovery in medical patients in their meta-analysis of 17 studies, finding that well-being predicted prognosis and mortality. Optimism and hedonic well-being have shown opposite relationships to physiological stress recovery in terms of cardiac variables, but it was unclear how to connect these results to health and mortality, especially since optimism protected from cardiovascular disease [27, 28].

Re-analyses of existing studies of dispositional optimism led by Scheier and colleagues [29] have shown that both optimism and pessimism scores, rather than just both scores summed together, predicted future physical health, but independence between optimism and pessimism was not assessed. A preregistered meta-analysis for positive affect and chronic pain found an association in observational and intervention studies while controlling for negative affect, though this association was notably weaker than the one seen in measures of positive affect without controlling for negative affect [30].

These reviews have contributed to a focus on positive constructs in epidemiology, not just for their predictive power but for their ability to increase the number of outcomes of interest in epidemiology [31]. The robust evidence for the independent protective association between well-being and cardiovascular disease has also led the American Heart Association to explicitly support well-being interventions, though the evidence for independence was not clearly described in that statement [32]. Overall, in these reviews, the independent association of well-being with mortality is assumed but not robustly substantiated, especially outside of the special case of positive and negative affect.

Steptoe [33] addressed the independence of well-being and ill-being with mortality qualitatively. The author noted some studies that examined this issue, finding evidence for and against well-being's independence, specifically in longitudinal studies of mortality and heart disease. Steptoe additionally sought to explain the heterogeneity of these findings by noting possible publication bias and inconsistent statistical adjustment. Health problems may cause lower well-being and, in turn, death, and this pattern of causation might only be understood through adjustment and study design, which few studies implemented. Controlling for concurrent health could end up weakening the association between well-being and mortality because well-being might affect concurrent health. Similar arguments can be made for social, demographic, and health behavior covariates. Thus, detecting the association of well-being and mortality and possible causal pathways between them requires careful attention and selection of covariates.

Conceptual models for the effect of well-being on health draw on two basic pathways. The main effect model supports well-being having a direct relationship with physical functioning, leading to improved health; these pathways may differ from those that ill-being affects. The stress-buffering model augments the main effect model by emphasizing the effects of positive affect on psychosocial resources and on physiological stress, which in turn affect health behaviors and physiological resilience. Because of a lack of intervention studies on well-being with long-term follow-up, it is difficult to discern causality or support one model clearly over the other [34]. Examining covariates and moderators in their meta-analysis, Zhang and Han [16] did find support for both main effect and stress-buffering models of positive affect's influence on mortality in the studies that were available: positive affect may both increase physical functioning and reduce the lingering effects of negative emotions and stress.

Transparency and openness

As a secondary analysis of existing data, the present meta-analysis does not qualify as human subjects research, and therefore did not require IRB review. Analyses could not be pre-registered because they were conducted iteratively under the supervision of a committee of graduate advisors. Several steps were taken to reduce researcher degrees of freedom and "false positive" findings. First, data and analytic methods are openly available on the Open Science Framework (https://osf.io/s8xjq/?view_only=77768037aacb4801bdcb759b6e63f428). Second, a PRISMA checklist (presented in the Supplemental Materials) delineates the analyses that were planned versus exploratory. In short, all analyses in the Results section were planned and those in the Discussion section were exploratory. Directional hypotheses were not made beyond the hypothesis that well-being would have a statistical protective effect on mortality, controlling for ill-being. Finally, Type I error was mitigated by the successive use of disconfirmatory methods to account for reduce publication bias and heterogeneity.

Methods

Search process

Given that the question of the value of positive psychological constructs is largely about their difference from existing constructs, well-being is here investigated in its psychological context. Objective measures of well-being (like the number of social contacts one has in a week) and health-related measures of quality of life are clearly important, but their utility is not the question at hand. Similarly, ill-being is defined here in psychological rather than objective or medical terms.

Previous meta-analyses of well-being constrained their searches to specific constructs explicitly incorporated in formulations of psychological and subjective well-being (e.g., purpose in life and positive affect) and to broader evaluative, experienced, and eudaimonic conceptions [13]. This broader perspective is the focus of this review of the literature and the meta-analysis that follows. I also added explicitly perceived social support to this this because of its close relationships to evaluative well-being and feelings of warmth and gratitude.

Narrowing down search terms began with investigating the definitions of psychological and subjective well-being. Psychological well-being refers to the necessary building blocks of happiness and flourishing as defined by Ryff [10], building off of work by Jahoda [35] and her conception of ideal mental health. Psychological well-being includes things like purpose in life, mastery over one's environment, and personal growth. Subjective well-being, on the other hand, comes from a more descriptive tradition through Diener [36] where high positive affect, low negative affect, and high satisfaction in life are taken together as indicative of well-being; whatever the person is doing, it is working for them in their context. Many researchers use psychological and subjective well-being interchangeably, especially outside of positive psychology. Related to these are concepts like optimism, joy, and a sense of calm that have all traditionally been included in the wider umbrella of psychological and subjective well-being. These definitions, along with previous meta-analyses' search terms, guided the process of creating search terms for this meta-analysis. However, not every term was included because an exhaustive search of individual terms would be functionally impossible given the number of possible synonyms and redundant given the diminishing returns of adding each term when other terms would likely be included in the search. For example, "environmental mastery" is used rarely if at all without reference to "well-being;" it was more important to include words that are associated with well-being but used independently, like "subjective age" or "love."

We devised search terms more expansive than previous meta-analyses with similar targets for improved coverage of studies by including alternate terms and clinical psychological constructs. Like Martín-María and colleagues [13], we added more search terms related to psychological and evaluative well-being. We also added physiological measures (e.g., vagal tone) to the search terms in order to identify papers that might include psychological and subjective well-being measures but not describe them in the title or abstract. The precise search terms are in Supplemental Material 1. Included in these search terms is an updated search of the Chida and Steptoe [23] search terms from 2017 onward to supplement Martín-María and colleagues' [13] update of the same search.

We searched Pubmed, Web of Science Core Collection, and PsycINFO, yielding 10,303 individual papers in all on March 23, 2022. Previous meta-analyses of well-being constructs and mortality [13, 16, 21–23] were also examined for studies the search terms might have missed. There were no language restrictions in the search, and translated abstracts were captured in the process. Copies of non-English studies with approved translated abstracts were obtained in their original languages and translated using a combination of translation by fluent speakers and translation software.

For greater depth of search of literature originating outside of America and Europe, the KCI-Korean Journal Database and SCiELO Citation Index were also searched on Web of Science. In order to better capture gray literature, a Scopus search for conference papers, letters, book chapters, notes, short surveys, and editorials were included as well as a second Web of Science search across all databases for letters and meetings. Finally, a search of ProQuest Theses and Dissertations was also included to examine data from unpublished theses.

Selection criteria

The inclusion criteria beyond the search terms were as follows:

(1) Human population with a reported recruitment method and population.
(2) Longitudinal, prospective cohort design with a reported follow-up period.
(3) Published journal article or conference proceeding.
(4) Statistical analysis and reporting of an included psychological construct that primarily assesses the construct psychologically.
(5) All-cause mortality or survival as outcomes.

Exclusion criteria were as follows:

(1) Nonhuman populations.
(2) Follow-up period of less than one year.
(3) Reverse-coded clinical psychological or distress constructs.
(4) Retrospective cohort studies.
(5) Specific-cause mortality or composites of all-cause mortality and health events where all-cause mortality cannot be examined separately.
(6) Articles that would otherwise meet criteria but analyze the same central variable in a dataset that has been analyzed more recently with longer follow-up times.

Though most of these points are clear enough, point 4 in the inclusion criteria requires some explication. What is important in these measures is on the one focus on the construal process of one's situation or oneself and on the other the positive end of functioning rather than the absence of bad things.

This criterion led to the selection of various types of measures. First, obviously included were scales already existing in well-being research (e.g., the Satisfaction with Life Scale) or items that mentioned well-being concepts. Positively worded questions and subscales of clinical measures—like the positive affect questions on the CES-D—were also included so long as they were handled distinctly from ill-being measures. Unidimensional measures like the Life Orientation Test [37] for pessimism and optimism were included, though they are noted as such and interquantile hazard ratios are used when available to examine the optimistic end of the scale. Composited measures of different positive and ill-being constructs, however, were excluded.

Not included were signs and components of psychological distress, objective measures of life conditions, and mixtures of objective and psychological measures that usually comprise quality of life [38]. Psychological distress measures like depression scales are already often used as mortality indicators, and the incremental validity of psychological and subjective well-being over these measures is of interest here. Objective measures are indicators like income, number of social contacts, number of chronic diseases, and demographic factors that have also often been examined for their effects on mortality, but they are not themselves psychological or concerning well-being. Life events like marriage or divorce likely have some psychological impact but are not assessed in psychological terms and are therefore excluded; divorce can be a positive force in some people's lives. Similarly, perceived social support was included, but social support measured through structural (number of daily contacts) or functional (whether someone helps a participant with activities of daily living) means were not. Social support is also hypothesized as a mediator of the relationship between well-being and health outcomes [39], so the inclusion of social variables as covariates were also examined in the meta-regression.

Finally, quality of life questionnaires are generally outside the margin of well-being measures, but each measure required item analysis to determine how positive and psychological the measures were. The positive psychological component of quality of life can sometimes be separated for analysis on its own and is done in this meta-analysis when possible. Purely health-related or functional quality of life measures were excluded. This distinction can get tricky: for example, the Getting out of Bed Scale is described by Dumontier and colleagues [40] as being a health-related quality of life scale when its item content is about meaning in life and optimism. For this reason, all quality of life scales were screened in as abstracts and, like all scales, examined for item content during the data extraction process.

Though we included other meta-analyses in the search, not all of the studies included in them met our inclusion criteria. Most illustrative is Lang and colleagues' [41] work on individuals' prediction of their life satisfaction five years in the future. The authors examined this construct on the level of how accurate the prediction was: they found that unrealistically optimistic predictions also predicted early death (HR = 1.10, 95%CI: [1.04–1.17]). Though the prognostic indicator uses life satisfaction in its definition, this accuracy check is more a measure of a person's ability to predict their future, which may rely on cognitive or clinical factors, than a well-being construct. The authors did not perform a hazard analysis on current life satisfaction or on predicted life satisfaction itself as optimism, so those measurements could not be extracted. For these reasons, the study did not merit inclusion in this meta-analysis, though it did for Martín-María et al. [13].

Constructs were placed within subgroups by how they explicitly or implicitly connected to a subgroup, though usually constructs like life satisfaction and optimism were explicitly named in the articles. Because of similar item content, happiness and positive affect were combined into a single group, as were meaning in life and purpose in life. Authors of other meta-analyses grouped them differently, such as Zhang and Han's [16] broad definition of positive affect. Some constructs did not fit into a larger subgroup and were only considered in the overall meta-analysis of well-being constructs.

Data collection

All abstracts were screened by both an author (MG) and by two researchers, one who did approximately 75% and one who did the remaining 25% so that every abstract was examined at least twice. Disagreements (~3.5% of the total) were resolved mutually through discussion and majority vote, nearly all of them being removed from consideration. However, given the high fraction of excluded abstracts, inter-rater reliability was $\kappa = 0.46$, which corresponds to moderate reliability.

We examined the full texts of the remaining studies for exclusion criteria before data extraction. Mostly, this revealed that some initially screened studies purported to study well-being did not include well-being constructs in their analysis. They were therefore removed.

We extracted data from the studies using the CHARMS-PF protocol [42]. When studies reported multiple follow-up times, we used only the longest one. When studies reported multiple included predictors, we extracted all of them and included in the meta-analysis unless one was a composite measure of the others. In that case we excluded the composite. When studies report multiple handlings of a single predictor, we used the one with the largest number of quantiles or a continuous/standardized version. When studies used multiple adjusted models, we used the model with the most covariates. When predictors were handled as continuous measures but not standardized, we standardized them by using the standard deviation of the scale or other types of distributions reported in the data. When the standard deviation was not available, we used the reported ratio as-is and noted as a continuous measure with the range of the scale. When studies handled predictors in quantiles, the number of quantiles was noted for meta-regression purposes, and we only used the ratio of the top quantile to the bottom quantile. However, if the predictor was measured with a scale that included a clinically relevant range—like an optimism scale where low scores indicate not low optimism but extreme pessimism—we recorded the interquartile ratio between the top quantile and the middle quantile or the quantile immediately above the midpoint, depending on whether the number of quantiles was even or odd. We did this to ensure that extracted effect sizes would represent comparisons within well-being rather than between ill-being and well-being.

In order to assess data extraction quality, a trained research assistant extracted data for a random selection of 15 papers approved through screening. This research assistant came to nearly identical results for sample descriptions, analysis methods, subgroup labels, and hazard ratios, among the other CHARMS-PF components. However, presentations of hazard ratios in tables and papers were sometimes unclear as to what covariates were being used, and the word "independent" was sometimes used to mean a bivariate association rather than the partial association after adjustment that is of interest for this meta-analysis. Because of this, an author (MG) re-examined every measurement that appeared to be adjusted for ill-being and re-categorized them as unadjusted as necessary, which led to the re-categorization of 7 measurements. The assistant agreed on these re-categorizations.

Hazard ratios are the focus of this meta-analysis, and we examined most relative risk and odds ratios separately. The rationale behind this division is that the two groups of ratios describe different things. Hazard ratios are comparisons of log-linear slopes that approximate actual mortality phenomena, and the other two are ratios of survivors at study end points. Relative risk and odds ratio at study endpoints are dubious in a long-term study of mortality: all participants will eventually die, no matter the group. On the other hand, event timing is built into the hazard ratio, and it is useful even if all participants die at the end of a study. Because many researchers appear unaware of the differences between relative risk ratios and hazard ratios, many studies present relative risk ratios even though studies use proportional hazards models to derive them. Depending on the characteristics of the studies, including relative risk ratios could inflate or deflate the apparent protective association. For this meta-analysis, we examined each paper for the specific model being used and, in the case of a proportional hazards model, assumed that was labeled in the study as a relative risk ratio was actually a hazard ratio. However, models like accelerated failure time models use relative risk rather than hazard ratios but utilize survival time data and yield results very similar to proportional hazards models [43]. Because of this, relative risk ratios from accelerated failure time models and other time-varying models are included with hazard ratios in the meta-analysis.

As for true relative risk and odds ratios, they were separated from the analysis once it was established that they could not be compared to hazard ratios. Since relative risk and odds ratios are comparable but not equal, relative risk was estimated from odds ratios using the total fraction of events (deaths) in a study, a conservative estimate. Though a truly equivalent conversion can only be made using the number of deaths in and sizes of each group being compared, continuous measurements and incomplete reporting make this method unfeasible.

Recent guidance has suggested to eschew the term study quality in favor of those focusing on risk of bias [44]. Risk of bias was determined using the Q-Coh tool [45]. Q-Coh examines a study's population selection, methodological confounds, exposure ascertainment, and outcome ascertainment. In comparison with other risk of bias tools, it has relatively high inter-rater reliability and is slightly more flexible than Cochrane's ROBINS-I instrument since it is amenable to non-intervention studies [46]. The Newcastle-Ottawa Scale was also considered, but Losilla and colleagues [46] found that it tends toward worse inter-rater reliability and to laxer standards of risk of bias. Q-Coh can provide a numerical score in the end, though collinearity among the instrument's questions necessitate a categorical assessment of low, moderate, or high risk of bias. We examined low risk of bias studies as a subgroup and use the numerical Q-Coh score in a meta-regression.

Given the importance of covariates in determining the independent association of a psychological construct and mortality, we also assessed the extent of study's control of demographics, health, and health behaviors in the meta-regression.

Statistical analysis

Ratios and their 95% CI were natural log-transformed before being entered into meta-analysis in order to gain the roughly normal distribution of values necessary for a meta-analysis. Using the statsmodels.meta_analysis package in python [47], random-effects models and inverse-variance methods were employed to combine study results. I chose random-effects models over fixed-effects models because of the methodological heterogeneity in populations, measures, and follow-up times in the included studies. Given that previous meta-analyses have had high statistical heterogeneity, we employed the weighted least-squares method (equivalent to the Hartung-Knapp-Sidik-Jonkman (HKSJ) adjustment) for comparison to the pure random-effects results. The HKSJ adjustment is most commonly implemented when there are few studies in a meta-analysis, but it generally reduces Type I error [48].

The above meta-analyses were performed for each of the following predetermined constructs, subconstructs, and subgroups of studies: all well-being constructs together, well-being constructs without social support, social support, optimism, purpose in life, positive affect, life satisfaction, studies on medical patients, studies on older people (mean age at start of 65 or above), studies with low risk of bias, and ill-being constructs. These were functionally mixed-effects models with adjustment of ill-being as a fixed effect and variance within subgroups as a random effect.

Heterogeneity was quantified using the I^2 statistic and τ. Meta-analyses with I^2 values of 0-0.25 are interpreted as low heterogeneity, 0.25-0.5 as medium heterogeneity, and above 0.5 as high heterogeneity [49]. However, I^2

does not describe the width of the prediction interval; τ is necessary for that [50]. Because τ is expressed in log-scale, the 95% prediction interval for the mean hazard ratio is derived from τ and reported alongside I^2.

It was also important to examine the strength of the independent association between well-being and mortality relative to relevant benchmarks of well-being and ill-being. We performed Z-tests comparing the pooled hazard ratios of studies that adjusted for ill-being and of those that did not adjust. This estimated the effect of adjustment—or how much of the association with mortality is shared between well-being and ill-being—though it carries the significant caveat that the studies in each group are not necessarily equivalent. (To examine possible differences, Z-tests were performed on the study characteristics of each subgroup). To compare the strength of well-being and ill-being, Z-tests were also performed between measures of adjusted well-being constructs and of ill-being constructs (all of which are adjusted) to compare their absolute effect sizes. These tests were between groups of studies but also within studies. We considered using equivalence testing because it can make a meta-analytic statement of significant equivalence rather than of nonsignificant difference. However, equivalence testing is difficult to do in meta-analysis of subgroups because it is designed for subgroup comparisons made within studies rather than between studies.

Table 1 Comparing characteristics of well-being construct hazard ratio reports between IB-adjusted and IB-unadjusted studies

Characteristic	Value in IB-adjusted group (k = 99)	Value in IB-unadjusted group (k = 81)	Test for difference (*p*)
Gender *(k)*			
- Men only	3	8	0.07
- Women only	7	7	0.78
Follow-up length (M [SD])	11.9 [8.7]	12.8 [9.5]	0.49
log(sample size) (M [SD])	7.9 [1.4]	7.4 [1.5]	**0.03**
Mortality fraction (M [SD])	0.25 [0.20]	0.37 [0.28]	**0.007**
Population *(k)*			
- Older people	35	40	0.07
- Patients	17	10	0.41
- Representative sample	34	27	1.00
Constructs (*k*)			
- Optimism	16	20	0.19
- Life satisfaction	20	19	0.72
- Purpose in life	10	2	0.07
- Positive affect	18	13	0.84
- General well-being	15	6	0.16
- Social support	14	7	0.35
Handling *(k)*			
- Dichotomized	24	15	0.37
- Tertiles	18	16	0.85
- Quartiles	10	5	0.42
- Quintiles	1	2	0.59
- Continuous	7	8	0.59
- Standardized	39	27	0.44
Q-Coh Risk of Bias *(k)*			
- Low Risk	11	12	0.51
- Moderate Risk	48	26	**0.03**
- High Risk	40	43	0.10

"General well-being" refers to measures of broad, overall construals of psychological or subjective well-being. *p*-values for frequency data are calculated using Fisher's exact test, while *p*-values for paired means and standard deviations are calculated using an unpaired two-sample t-test, except for mortality fraction comparison, which was done using a Mann-Whitney U-test

IB Ill-being

Bolded values indicate significant difference at $p < 0.05$

We implemented a meta-regression for all descriptive study characteristics in well-being construct measurements using the PYMARE python package [51]. Bivariate regressions were performed for all summary statistics and population- and handling-based characteristics presented in Table 1 as well as adjustment category, adjustment quality, and risk of bias score. Parallel analyses were also performed for ill-being construct measurements. We included variables in the multivariate meta-regressions if they showed $p<.15$ significance levels in bivariate meta-regressions. In the case of control variables—demographics, health, health behaviors, social variables, other well-being constructs, and ill-being constructs—a significant positive meta-regression slope implies that the control variable shares some empirical connection to well-being as a strong covariate or a mediator of its relationship with mortality. A negative slope is an enhancement of the effect, and it may indicate that the control variable is a suppressor variable, revealing a stronger relationship between underlying well-being construct and protective effect. Effects for other types of covariates imply that they may be moderators of the well-being effect.

Because most methods of estimating publication bias have some theoretical or practical drawbacks, we used multiple methods to estimate biases in the reported measurements and the true effects of the well-being constructs themselves. Funnel plots and Egger's regressions tested for potential publication bias. The Egger's test examines the significance of the correlation between effect size and standard error; a significant non-zero slope indicates that effect sizes and standard errors are not drawn randomly from around the average effect size [52]. The trim-and-fill method was used to correct for publication bias and provide an estimate for an unbiased pooled hazard ratio [53]. Estimators use the difference in total ranks on either side of the mean effect size to approximate how many measurements *k* are "missing" on the other side of the funnel plot. The *k* largest effect sizes are removed from the over-full side of the funnel plot, the mean effect size is estimated again. This process is repeated until *k* is stable or the maximum number of iterations is reached. To "fill" the other side of the funnel plot with studies, the *k* largest effect sizes are reflected across the mean effect size estimated in the previous step, and the standard error is conserved. The mean effect size and confidence intervals are then calculated again. Though this process does not maximize the effect of the filled studies on the mean effect and the confidence interval, it maximizes the effect sizes of the missing studies within the bounds of the dataset. The average of the L_0 and Q_0 estimators was used to estimate how many studies were missing when these conservative estimators disagreed [54].

A robust Bayesian meta-analysis (RoBMA) was also performed in R to mitigate publication bias [55]. RoBMA was created to combine *p*-value selection corrections and corrections for small study effects. 36 models with different priors about effects, heterogeneity, and bias are simulated, fit to the dataset, and finally weighted in an ensemble based on their likelihood given the data. RoBMA shows superior removal of publication bias and recovery of true effects compared to other tools in real and simulated datasets method [56]. The downside of RoBMA is that it is less easily interpretable than the trim-and-fill method, which had worse but acceptable performance in comparative tests. Additionally, meta-analyses with few studies are not well-constrained using this technique. Bayes factors are calculated each for effect/no effect, heterogeneity/no heterogeneity, and publication bias/no publication bias, and effect sizes and 95% credible intervals are calculated through model averaging. Bayes factors are reported when illustrative, but only the estimated effect sizes and credible intervals are reported for all groups.

We also estimated outliers by testing whether an individual study's confidence interval was entirely outside the pooled confidence interval. We repeated the overall and construct-specific meta-analysis and adjusted/unadjusted comparison after removing outliers in case any of these measurements had an outsized influence on the pooled effect size.

Results

Descriptive characteristics

Search terms retrieved 11,604 studies, which led to the inclusion of 94 new studies. 54 studies were included from previous meta-analyses. In all, this amounted to 148 studies with 260 data points. Of these, 224 were hazard ratios or equivalent measurements: 180 of well-being constructs, and 44 of ill-being constructs from studies that controlled for a well-being construct. 99 well-being measurements were IB-adjusted (i.e., controlled for ill-being), and 81 were IB-unadjusted. All other measurements were converted to relative risk ratios and analyzed separately. Reasons for exclusion during the full-text screening phase included having no mortality data, no analysis of well-being constructs, follow-up being less than a year, incomplete reporting, specific-cause mortality, retrospective methods, and unretrievable reporting. This process is explained in further detail in the PRISMA flow diagram in Fig. 1.

Descriptive characteristics of the overall sample of well-being measurements are presented in Table 1. The average study followed around 2000 participants for 12.3 years. On average, about one third of participants died during a study's follow-up period. Only about 13% of all studies had a low risk of bias using the Q-Coh tool. Sample size was roughly normally distributed in log-space rather than linear space, and so the log of sample size was used in all covariation analyses. No other continuous distribution was so non-normally distributed.

Though IB-adjusted measurements are the focus of this meta-analysis, they must be compared to the IB-unadjusted measurements. In terms of study characteristics, IB-adjusted studies and IB-unadjusted studies were similar. However, IB-unadjusted studies tended to be smaller, have a higher death fraction, and have more studies with moderate risk of bias.

Optimism, life satisfaction, positive affect, perceived social support, general well-being (broad measurements of various psychological and subjective well-being components lumped together), and purpose in life were the most common constructs measured. Each of these constructs were large enough for meta-analyses within IB-adjusted and IB-unadjusted groups.

A majority of measurements (99/180) controlled for an ill-being construct. Depressive symptoms and depressive disorder status were by far the most common ill-being construct being controlled for (k = 66). The prevalence of this control makes sense because many researchers consider depression emblematic of low well-being [57]. Negative affect/emotion (k = 22) and loneliness (k = 8) were also relatively common adjustments.

Meta-analyses

In a random-effects meta-analysis, well-being was strongly associated with protection from all-cause mortality even when controlling for ill-being. These results for IB-adjusted and IB-unadjusted subgroups are summarized in Table 2. The pooled IB-adjusted hazard ratio over all studies controlled for ill-being was 0.86 (95% CI: [0.84-0.88]). In other words, people one standard deviation above the average on well-being die at a 14% slower rate than those with average well-being. This association remained significant with the HKSJ adjustment, which is used to counter bias in small sample sizes (95% CI: [0.84-0.88]). The predictive ability of well-being did not change when removing social support measurements.

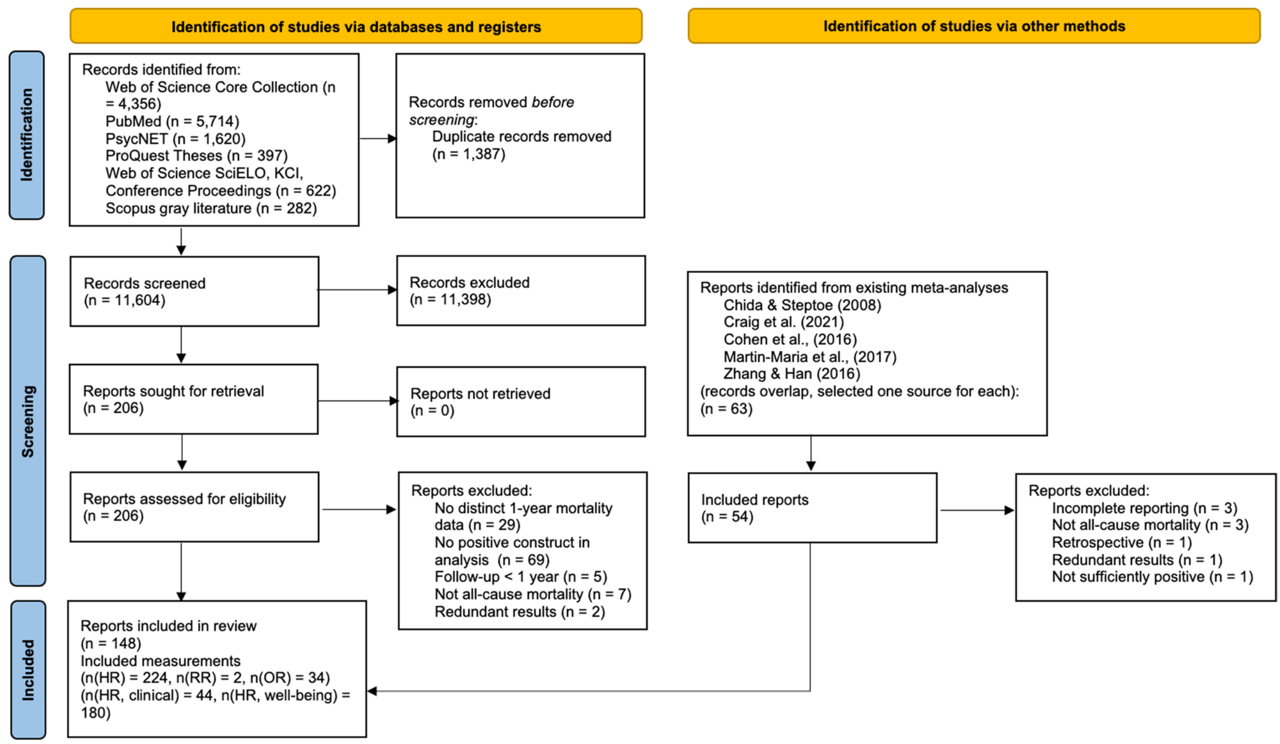

Fig. 1 PRISMA diagram for measurement identification and extraction. *Note*. Format adapted from Page et al. (2021)

Table 2 Comparison of construct-based meta-analyses for well-being measurements that adjust and do not adjust for opposing ill-being constructs

Variable	Adjusted				Unadjusted				Z-test for difference	
	HR	CI	CI_{HKSJ}	k	HR	CI	CI_{HKSJ}	k	*p*	p_{HKSJ}
well-being construct	0.86	(0.84–0.88)	(0.84–0.88)	99	0.90	(0.88–0.92)	(0.88–0.93)	81	**0.01**	**0.03**
WB-SS	0.86	(0.84–0.88)	(0.83–0.88)	85	0.90	(0.88–0.92)	(0.87–0.92)	74	**0.02**	**0.05**
optimism	0.87	(0.84–0.90)	(0.84–0.90)	16	0.90	(0.86–0.93)	(0.86–0.94)	20	0.30	0.34
life satisfaction	0.82	(0.76–0.88)	(0.76–0.88)	20	0.92	(0.89–0.95)	(0.87–0.96)	19	**< 0.01**	**0.01**
positive affect	0.92	(0.89–0.96)	(0.89–0.96)	18	0.97	(0.93–1.02)	(0.91–1.03)	13	0.09	0.17
general well-being	0.82	(0.80–0.87)	(0.75–0.89)	15	0.84	(0.78–0.89)	(0.78–0.89)	6	0.67	0.67
social support	0.87	(0.79–0.97)	(0.78–0.98)	14	0.95	(0.87–1.05)	(0.85–1.07)	7	0.23	0.30
purpose in life	0.87	(0.80–0.94)	(0.78–0.97)	10	*	*	*	2	*	*

WB-SS Well-being constructs without social support, *HR* Hazard ratio, *CI* 95% confidence interval, CI_{HKSJ} 95% confidence interval with HKSJ adjustment, p_{HKSJ} = *p*-value with HKSJ adjustment; "general well-being" refers to measures of broad, overall construals of psychological or subjective well-being

Bolded comparisons are significant at the *p* = .05 threshold. Asterisks indicate a subgroup that was too small for a meta-analysis

Pooled hazard ratios for optimism (HR = 0.87, 95% CI: [0.84-0.90]), life satisfaction (HR = 0.82, 95% CI: [0.76-0.88]), positive affect (HR = 0.92, 95% CI: [0.89-0.96]), general well-being (HR = 0.82, 95% CI: [0.80-0.87]), and purpose in life (HR = 0.87, 95% CI: [0.80-0.94]) were all significant among studies that controlled for an ill-being construct. Perceived social support was also significantly associated with protection from mortality (HR = 0.87, 95% CI: [0.79-0.97]).

Hazard ratios were also significant across different populations and study variables. Results for IB-adjusted and IB-unadjusted subgroups are summarized in Table 3. Samples focused on older people (HR = 0.83, 95% CI: [0.80-0.87]), on patients with serious medical conditions (HR = 0.84, 95% CI: [0.77-0.92]), and on representative samples in large populations (HR = 0.89, 95% CI: [0.86-0.92]) were all significant. Importantly, studies with low risk of bias that adjusted for ill-being still showed an overall protective association (HR = 0.91, 95% CI: [0.83-0.98]).

Heterogeneity was high (I^2~0.75, 95% PI: [0.72–1.04]) among all IB-adjusted measurements of constructs. In fact, heterogeneity was high for all subgroup measurements except for optimism (I^2=0.32, 95% PI: [0.81–0.94])

Table 3 Comparison of population and handling-based meta-analyses for well-being measurements that adjust and do not adjust for ill-being constructs

Variable	Adjusted				Unadjusted				Z-test for difference	
	HR	CI	CI_{HKSJ}	k	HR	CI	CI_{HKSJ}	k	*p*	p_{HKSJ}
older people	0.83	(0.80–0.87)	(0.78–0.88)	35	0.90	(0.87–0.92)	(0.87–0.93)	37	**< 0.01**	**0.03**
patients	0.84	(0.77–0.92)	(0.77–0.92)	17	0.79	(0.70–0.90)	(0.70–0.91)	9	0.43	0.43
representative	0.89	(0.86–0.92)	(0.85–0.93)	34	0.94	(0.91–0.96)	(0.90–0.97)	26	**0.03**	0.08
dichotomized	0.81	(0.76–0.87)	(0.75–0.89)	24	0.94	(0.90–0.98)	(0.89-1.00)	14	**< 0.01**	**< 0.01**
tertiles	0.78	(0.69–0.86)	(0.71–0.85)	18	0.91	(0.83-1.00)	(0.80–1.03)	15	**0.03**	**0.04**
quartiles	0.77	(0.69–0.86)	(0.67–0.87)	10	*	*	*	5	*	*
continuous	0.94	(0.85–1.03)	(0.85–1.03)	7	0.96	(0.94–0.99)	(0.93–0.99)	6	0.58	0.59
standardized	0.89	(0.87–0.92)	(0.86–0.92)	39	0.90	(0.87–0.92)	(0.86–0.93)	26	0.94	0.95
low risk of bias	0.91	(0.83–0.98)	(0.83–0.99)	11	0.95	(0.91–0.99)	(0.89–1.01)	12	0.33	0.41
one question	0.83	(0.75–0.92)	(0.75–0.93)	8	0.90	(0.84–0.97)	(0.83–0.98)	17	0.19	0.24

HR Hazard ratio, *CI* 95% confidence interval, CI_{HKSJ} 95% confidence interval with HKSJ adjustment, p_{HKSJ} p-value with HKSJ adjustment

Bolded comparisons are significant at the p =0.05 threshold

Asterisks indicate a subgroup that was too small for a meta-analysis

Table 4 Meta-analysis of IB-adjusted well-being measurements, without outliers

construct	HR	CI	CI_{HKSJ}	k	I^2 (%)	95% PI
well-being construct	0.87	(0.86–0.89)	(0.82–0.93)	73	19	(0.82–0.93)
WB-SS	0.87	(0.86–0.90)	(0.82–0.93)	56	22	(0.82–0.93)
optimism	0.87	(0.84–0.90)	(0.81–0.94)	16	32	(0.81–0.94)
life satisfaction	0.84	(0.81–0.88)	(0.77–0.92)	13	27	(0.77–0.92)
positive affect	0.93	(0.89–0.96)	(0.86–0.99)	13	24	(0.86–0.99)
general well-being	0.81	(0.74–0.89)	(0.68–0.97)	11	33	(0.68–0.97)
social support	0.89	(0.82–0.97)	(0.80–0.99)	11	6	(0.80–0.99)
purpose in life	0.88	(0.84–0.92)	(0.84–0.92)	7	24	(0.82–0.94)

WB-SS Well-being constructs without social support, *HR* Hazard ratio, *CI* 95% confidence interval, CI_{HKSJ} 95% confidence interval with HKSJ adjustment; "general well-being" refers to measures of broad, overall construals of psychological or subjective well-being

and for one-item assessments (I^2=0.27, 95% PI: [0.70–0.99]). These were also the only groups where the prediction intervals of the hazard ratios lie below 1, indicating that well-being was protective in nearly all samples.

Dealing with heterogeneity and publication bias

Outliers

The results for well-being measurements without outliers are presented in Table 4. Heterogeneity dropped sharply in all subgroups, but the meta-analytic association of all IB-adjusted measurements of well-being constructs with protection from mortality was robust after the removal of outliers (HR=0.87, 95% CI: [0.86-0.89]). With the drop in heterogeneity, the prediction interval of all adjusted measurements was significant (I^2=0.18, 95% PI: [0.82-0.93]), indicating a protective association across samples despite different characteristics. All subgroups analyses that were significantly associated with protection from mortality in the Tables 2 and 3 remained associated with protection after removing outliers, except for the low risk of bias subgroup (HR = 0.94, 95% CI: [0.86-1.02]).

Egger's regression and trim-and-fill method

The Egger's linear regression test shows a small but nevertheless statistically significant relationship between effect size and the standard error of the effect size in the IB-adjusted group, which is usually interpreted as the presence of publication bias (see Fig. 2a). The trim-and-fill technique estimated this bias as eight "missing" IB-adjusted measurements that were not published. Results that corrected for potential publication bias are presented in Table 5. After including these "missing" studies, the overall hazard ratio for studies adjusting for ill-being changed little and remained significantly protective (HR = 0.88, 95% CI: [0.85-0.90]). Including estimated publication bias did not change the significance of any construct or study variable's protective effect in Tables 2 and 3. An illustration of how adding the missing studies removes the relationship between association strength and its standard error is included in Fig. 2.

RoBMA

The robust Bayesian meta-analysis (RoBMA) is an ensemble of 36 models of publication bias, effect, and heterogeneity that are weighed in a sum based on how

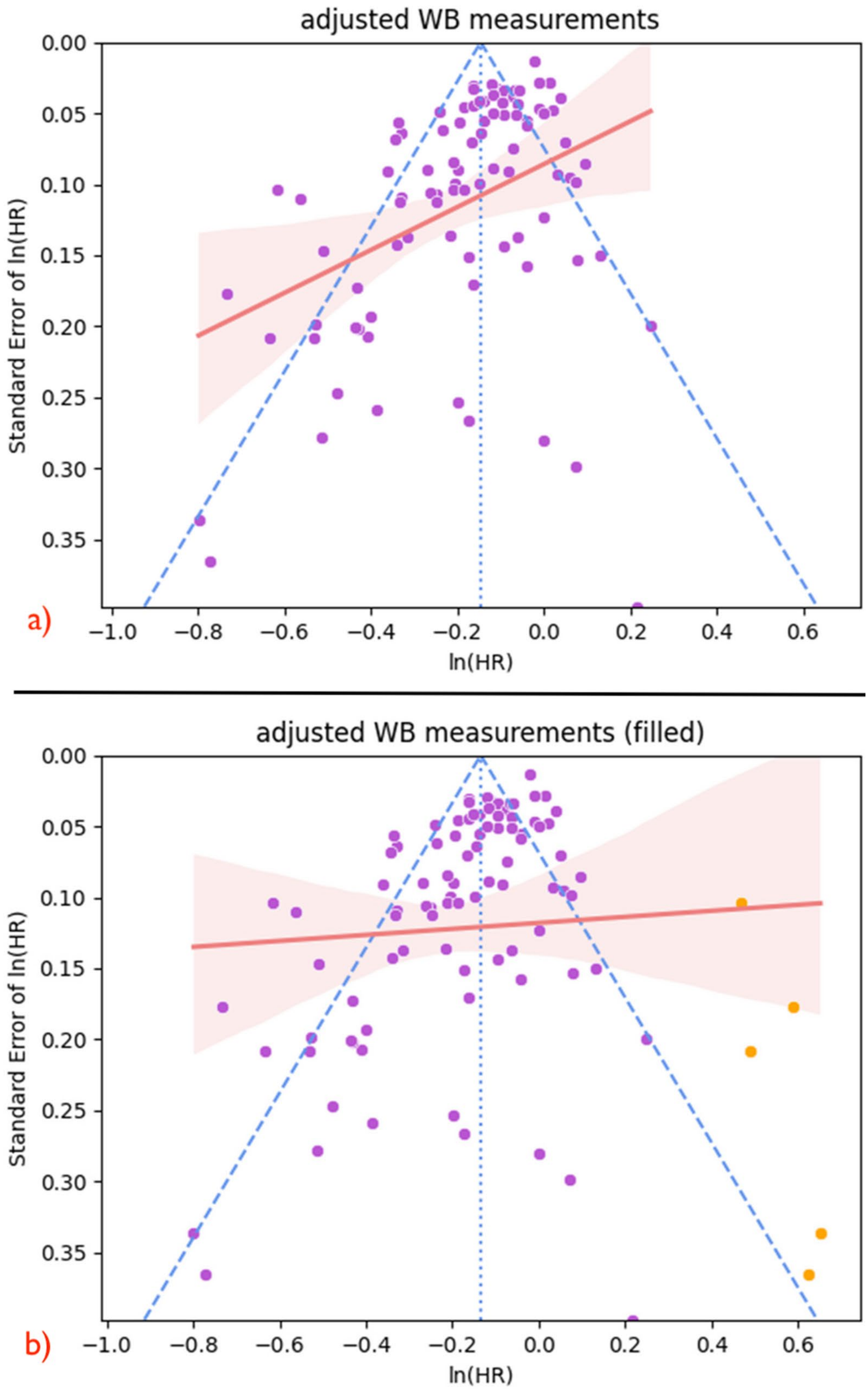

Fig. 2 Funnel plot and trim-and-fill estimate for adjusted well-being constructs with Egger's regression lines for illustration. *Note*. Red line represents the line of best fit for Egger's regression before (**a**) and after (**b**) adding filled-in studies. Shaded area represents 95% confidence interval on regression line. A null slope indicates a lack of relationship between effect size and measurement precision, which is in turn interpreted as a lack of publication bias. Blue dotted line represents the best estimate of the real studies are in purple, and "filled" studies are in orange

Table 5 Meta-analysis of IB-adjusted well-being measurements, correcting for estimated bias using the trim-and-fill technique

construct	HR	CI	CI_{HKSJ}	inferred studies (k)	original studies (k)
well-being construct	0.88	(0.85–0.90)	(0.84–0.91)	7	99
WB-SS	0.88	(0.86–0.90)	(0.84–0.91)	7	85
optimism	0.90	(0.86–0.94)	(0.85–0.94)	4	16
life satisfaction	0.82	(0.77–0.88)	(0.76–0.89)	1	20
positive affect	0.94	(0.90–0.98)	(0.89–0.99)	3	18
general well-being	0.85	(0.78–0.93)	(0.76–0.95)	2	15
social support	0.89	(0.80-1.00)	(0.78–1.01)	1	14
purpose in life	0.82	(0.74–0.90)	(0.70–0.95)	1	10

WB-SS Well-being constructs without social support, *HR* Hazard ratio, *CI* 95% confidence interval, CI_{HKSJ} 95% confidence interval with HKSJ adjustment; "general well-being" refers to measures of broad, overall construals of psychological or subjective well-being

Table 6 Meta-analysis of IB-adjusted well-being constructs, correcting for estimated bias using the RoBMA technique

construct	HR	HDI	number of studies
well-being construct	0.93	(0.90–0.95)	99
WB-SS	0.92	(0.90–0.94)	79
optimism	0.94	(0.92–0.96)	16
life satisfaction	0.90	(0.85–0.95)	20
positive affect	0.96	(0.93-1)	18
general well-being	0.89	(0.84-1)	15
social support	0.99	(0.91-1)	14
purpose in life	0.90	(0.79-1)	10
older people	0.93	(0.90–0.96)	35
patients	0.91	(0.85-1)	17
representative	0.96	(0.94–0.97)	34
dichotomized	0.92	(0.85–0.95)	24
tertiles	0.87	(0.82–0.93)	18
quartiles	0.87	(0.79-1)	10
continuous	0.99	(0.93-1)	7
standardized	0.94	(0.92–0.96)	39
low risk of bias	0.99	(0.92-1)	11
one question	0.91	(0.85-1)	8

WB-SS Well-being constructs without social support, *HR* Hazard ratio, *HDI* 95% highest density interval, "general well-being" refers to measures of broad, overall construals of psychological or subjective well-being

they fit the data in a meta-analysis [55]. For instance, some models include no effect and some bias, and these models will not be strongly weighted if the distribution of effect sizes in the meta-analysis does not conform to this prior. The RoBMA technique showed some meaningful differences from the traditional meta-analysis results. The RoBMA results are summarized in Table 6. The overall IB-adjusted meta-analysis showed abundant evidence for a true association (Bayes Factor ratio comparing evidence of an association to the evidence of no association = 5.1 $\times$ 10^{13}) and for heterogeneity (BF = 7.2 $\times$ 10^{19}) but with minimal evidence for publication bias (BF = 1.79). The ensemble of models in RoBMA found a protective credible interval centered at HR = 0.93 (95% HDI: [0.90-0.95]). There was also evidence for a protective association in optimism (HR = 0.94, 95% HDI: [0.92-0.96]) and life satisfaction (HR = 0.90, 95% HDI: [0.85-0.95]). Studies on older people (HR = 0.93, 95% HDI: [0.90-0.96]), in representative samples (HR = 0.96, 95% HDI: [0.94-0.97]), and studies using standardized, continuous measurements of well-being (HR = 0.94, 95% HDI: [0.92-0.96]) were all significantly associated with protection. Because of the much more conservative methods and assumptions in RoBMA, these are more underpowered estimates than the corresponding ones in Table 2 [55].

Multiple individual constructs that were significant in the traditional random-effects meta-analysis—positive affect, general well-being, social support, and purpose in life —have credible RoBMA intervals that end at HR = 1, a null effect. That is, there was no evidence of greater mortality risk for these subgroups. Because half of the models averaged in each ensemble assume no effect at all, there was still evidence of null effects. None of the constructs showed evidence of bias (all BF < 1). Individual construct analyses may have been too heterogenous and underpowered for RoBMA [55].

Comparing IB-adjusted and IB-unadjusted studies

Between IB-adjusted and IB-unadjusted groups—measurements that controlled for ill-being and those that didn't—there were some significant differences. As seen in Table 2, IB-adjusted hazard ratios of all well-being constructs and of life satisfaction were significantly *more* protective than IB-unadjusted measurements of the same (p = .01 and p < .01, respectively). IB-adjusted measurements are also more protective that IB-unadjusted ones in populations of older people, in representative samples, and when using dichotomized measures (p < .01, p = .03, and p < .01).

If one assumes that well-being and ill-being share some association with mortality, then controlling for one should reduce the association strength of the other; instead, the opposite is seen here. Because these groups of measurements come from different studies rather than the same one, the explanation probably lies in differences between the groups of studies. As seen in Table 1, there were some minor systematic differences between adjusted and unadjusted studies, but cutting across these differences or controlling for them failed to account for

Table 7 Meta-regression results over all well-being construct measurements

Variable	Bivariate		Multivariate	
	β	p	β	p
follow-up length	**0.003**	**0.007**	0.002	0.07
log(sample size)	0.012	0.10	**0.019**	**0.01**
patients	-0.05	0.13	0.015	0.69
representative	**0.057**	**0.007**	0.037	0.10
continuous	**0.082**	**0.01**	0.026	0.44
quartiles	**-0.12**	**<0.001**	**-0.13**	**<0.001**
OWB	0.046	0.06	**0.051**	**0.05**
social	-0.058	0.06	-0.036	0.25
IB-adjusted	-0.034	0.10	-0.033	0.12

IB Ill-being. Only bivariate regressions with coefficients $p < 0.15$ were included in the multivariate regression. Bivariate regressions with $p > 0.15$ include Q-Coh numeric total, adjustment quality, publication year, standardized handling, interquantile HR handling, men-only population, women-only population, patient population, tertile handling, quintile handling, controlling for demographics, controlling for health, controlling for health behaviors, and controlling for social variables. *OWB* = controlling for secondary well-being construct. Note that positive coefficients *decrease* and negative coefficients *increase* the protective effect of well-being

Bolded values indicate significant coefficient strength $p < 0.05$

the difference in hazard ratios between IB-adjusted and IB-unadjusted studies. These and other possible explanations are further discussed in the discussion section "Limited explanation of heterogeneity" and the supplemental materials section "Explaining adjustment differences."

Comparing ill-being and well-being

The adjusted ill-being constructs that were reported alongside IB-adjusted well-being measurements form a useful benchmark for the protective power of well-being. Ill-being constructs were significantly predictive of mortality (HR = 1.05, 95% CI: [1.02–1.08], $k = 44$), but IB-adjusted well-being constructs were more powerful than them in a Z-test comparison of pooled effect sizes ($p < .001$). There was no overall significant strength difference between the ill-being measurements and the smaller group of IB-adjusted well-being measurements that were taken from the same papers ($p = .16$). However, when examining all the *within-study* differences in strength between ill-being measurements and IB-adjusted well-being measurements, well-being measurements were marginally more powerful in a random-effects meta-analysis (pooled ln(HR) difference = 0.042, 95% CI: [0.009-0.075], $k = 68$).

Meta-regressions

Meta-regressions were performed over all well-being measurements for all characteristics presented in Table 7. Candidate covariates that passed the $p < .15$ threshold in bivariate metaregressions were follow-up length, log(sample size), patient population, representative samples, continuous handling, quartile handling, adjusting for social variables, adjusting for a secondary well-being construct, and adjusting for an ill-being construct. When all of them were included in a multivariate meta-regression, only log(sample size), quartile handling, and controlling for a secondary well-being construct were significant ($p = .01$, $p < .001$, $p = .05$, respectively).

The same meta-regression performed over the subset of IB-adjusted measurements showed significant bivariate associations for follow-up time, standardized handling, tertile handling, quartile handling, and controlling for a secondary well-being construct, but no study variables were significant in the multivariate meta-regression.

The location of the sample (Western Europe, Eastern Europe, North America, East Asia, Middle East) did not appear under the $p < .15$ threshold for the meta-regression. A comparison between studies done in and outside the US was nonsignificant ($p = .70$). However, pairwise comparisons between all locations show significant differences between studies in Eastern European ($k = 5$) and Middle Eastern ($k = 5$) samples have *stronger* associations with protection effects than those in North American samples ($k = 65$; $p = .04$ in both cases).

Publication year did not appear under the threshold for the meta-regression either. In order to further examine publication year effects, studies published before 2012 were compared to those published in 2012 and after. No significant difference was found ($p = .18$).

RR/OR results in brief

The pooled relative risk from IB-adjusted well-being risk ratios and odds ratios was not significantly protective from mortality (RR = 0.92, 95%CI: [0.82-1.03], $k = 12$), and neither were the IB-unadjusted risk ratios (RR = 0.92, 95%CI: [0.85 – 1.00], $k = 11$). The IB-adjusted and IB-unadjusted pooled estimates were not statistically different ($p = .97$). Heterogeneity was very high in both the adjusted and unadjusted groups ($I^2 = 75\%$, 95% PI: [0.76-1.13]; $I^2 = 93\%$, 95% PI: [0.75-1.12], respectively). Power may have been an issue, since a meta-analysis of all RR/OR measurements together, regardless of adjustment, indicated a weak protective effect (RR = 0.93, 95% CI: [0.88-0.98], k = 23).

Discussion

Well-being remains associated with protection from mortality after adjustment

The purpose of this meta-analysis was to test whether the association between well-being and all-cause mortality in longitudinal studies is independent from the association between ill-being and all-cause mortality. On this count, the random-effects meta-analysis found that well-being is significantly associated with lower mortality in studies that simultaneously control for ill-being. This association was significant for the individual well-being constructs of optimism, life satisfaction, positive affect, perceived social support, and purpose in life. It was also significant for broad index measures of psychological and subjective well-being. The overall association of IB-adjusted well-being measurements with lower mortality remained significant in patient populations, elderly populations, and representative samples. No statistical handling method nullified this effect.

Meta-regressions did not locate significant differences in the association across most study variables. Larger sample sizes reduced the protective effect, and quartile handling enhanced it according to the multivariate analysis.

Additionally, publication bias was present according to the Egger's regression and trim-and-fill method, but neither removing outliers, adding inferred "missing" studies, nor averaging across many possible models of heterogeneity and bias removed the protective association of IB-adjusted well-being measurements. The RoBMA method did not find evidence of publication bias found a significant overall association when removing estimated heterogeneity. Removing outliers substantially reduced heterogeneity, though overall heterogeneity indicated that not all measures of well-being are associated with protection across contexts.

The robustness of well-being's association with protection from mortality is the central finding of this meta-analysis. This meta-analysis replicates and extends findings of previous meta-analyses of well-being and mortality that included fewer studies and did not focus on the statistical control of ill-being [13]. The strength of the association between mortality and well-being in this meta-analysis is similar to those, even though ill-being is being controlled for. In terms of magnitude, the effect independently associated with a standard deviation increase or decrease on well-being is about half as influential on mortality as a clinical depression diagnosis is [58]. As a practical reference using death probabilities from a 2020 United States Social Security Administration actuarial life table (https://www.ssa.gov/oact/STATS/table4c6.html), a standard deviation increase on well-being corresponds to just under two years of more life for both men and women.

The implications of this meta-analysis as well as the factors limiting its interpretation are discussed in the following sections in order to properly contextualize the results.

Heterogeneity and homogeneity limit generalization

However interesting the associations are between well-being and mortality, even after accounting for risk of bias, the studies themselves are too similar *and* too different from each other to infer general patterns from.

On the one hand, the studies may have been too different in terms of study characteristics for covariates to be significant in meta-regressions: though some associations are clearly significant in nearly all populations, many well-being constructs have 95% prediction intervals that cover a null hazard ratio, indicating that well-being may not be protective in all circumstances. Heterogeneity in association strength could not be ascribed to specific differences in study characteristics, limiting our inferences about what is driving the association.

On the other hand, the studies may have been too similar on how they statistically adjusted their well-being measurements: Most studies controlled for health and demographic variables, so one could only compare them to the very few studies that did not control for them, leading to low statistical power in hypothesis testing.

A few comments can be made. Adjustment for ill-being did not emerge as a significant covariate in the meta-regression, even though it was a moderately-powered comparison. Meanwhile, controlling for a secondary well-being construct did emerge as a significant covariate at the marginal p-value of 0.05: conceptually and statistically, adjusting for a related well-being construct reduces the strength of the association between the primary well-being construct and mortality. One can simply infer that the two well-being constructs are similar enough to share the association, but well-being and ill-being may not be.

Comparisons *within* the sample of IB-adjusted studies were interpretable but inconclusive. No construct association was significantly stronger or weaker than the overall association, and no handling method or population was significantly different either.

Z-tests *across* IB-adjusted and IB-unadjusted subgroups showed an increase in the protective association for life satisfaction and for positive constructs in general. Though a decrease would be easily understandable, it is harder to explain how IB-adjusted studies report *greater* protection than IB-unadjusted studies.

No aspect of the studies—such as risk of bias or broken statistical assumptions—could explain this pattern. Suppressor effects are possible, but in simulations these effects are washed out in the meta-analysis (see "Explaining adjustment difference" and "Suppressor example" in the Supplemental Materials). Therefore, other reasons must be considered if this effect is real rather than random. The apparent suppression is likely due to undiscovered, confounding systematic differences between IB-adjusted and IB-unadjusted studies, perhaps related to the subtler effects of study quality, well-being measures, and choice of control variables.

The importance of picking the right well-being measures and control variables can be seen in the case of Liu and colleagues' [59] study of over 700,000 women—nearly half of the total measurements in this meta-analysis—that used the single question "How often do you feel happy?" on a four-point Likert scale to measure happiness. Diener and colleagues [60] argued that single questions like this do not parsimoniously capture well-being but rather measure only part of well-being as well as other self-concept factors. Kubzansky and colleagues [61] noted that the happiness question was a significant predictor of mortality independent of various behavioral and social covariates until a self-rated health question was added to covariates. The authors argued that, as a global statement of subjective experience, self-rated health questions are correlated with happiness more strongly than with objective measures of health. Therefore, controlling for self-rated health would also be controlling for happiness, which would obviously make measuring the effect of happiness impossible. Many studies of positive constructs may be inadvertently controlling for positive constructs. One important caveat is that, in this meta-analysis, using self-rated health was not a covariate of the association strength (p = .79). Nevertheless, heterogeneity of covariates may be too high to adequately probe the effect of self-rated health between studies.

Theoretical implications

This meta-analysis supports the "separate" model of well-being and ill-being over the "redundant" model, at least in how they are measured. That is to say that well-being and ill-being measures examine different ranges of psychological functioning; measurements of well-being and ill-being at one point in time have independent associations with mortality over the course of years. One may argue that observed differences between well-being and ill-being may be due to desirable responding and response styles to positively and negatively-worded questions [62, 63]. However, even if these differences between ill-being and well-being are chalked up to artifacts, these artifacts have a material effect on mortality. Whatever it is that makes people answer well-being and ill-being questions differently, it matters and likely relates to some set of life skills or traits.

It remains to be seen how separate well-being and ill-being are. Well-being and ill-being could exist on different parts of the same spectrum of functioning or they could represent different axes of functioning: A person might appear in this analysis to exist somewhere along the spectrum between well-being and ill-being only because this analysis forces two partially correlated factors to collapse into one dimension when measuring a single outcome. There is substantial evidence in the literature against single-continuum interpretation of these findings and for this "oblique two-factor" interpretation. For instance, positive and negative affect are separate constructs rather than opposite ends of a spectrum [64, 65]. Similarly, mixed affective states have routinely been reported as distinct from neutral affective states, indicating that positive and negative affect do not merely cancel each other out in the way that a bipolar model would imply [66]. Behavioral psychology and neuroscience have found qualitative differences in concurrent reinforcement and punishment learning [67], in positive and negative valence systems [68, 69], and in the ingredients of the approach-avoidance conflict [70]. Positive feelings and constructive behavior arises from neural circuits that are significantly different from their negative and maladaptive counterparts.

In a latent profile analysis, Zhao and Tay [71] found that the majority of their findings reflected a single-continuum, but those in their high ill-being profiles still had significant variation in well-being components, indicating some variation can't be captured by such a model. This variation implies some independence for well-being, and the variation might be meaningful in terms of predicting life outcomes longitudinally. A single continuum may be an artifact as well, since assuming bipolarity at the stage of scale construction can also ensure bipolarity in findings [7].

More precise measurements and study designs than those available in this meta-analysis are necessary to probe this question. It remains to be seen whether these momentary differences matter longitudinally: the dimensions may collapse into one as people make decisions through their lives to maintain emotional homeostasis and approach their goals. It may also be necessary to measure multiple outcomes to sufficiently test the separate continuum vs. the oblique two-factor solution. Statistical properties like heteroscedasticity between well-being and ill-being may also be informative.

Practical implications

Finding the statistical difference between well-being and ill-being was relatively simple; investigating causality and mechanisms of this difference is much harder. Nevertheless, this meta-analysis further prompts their investigation.

As of now, it is unclear whether well-being interventions may increase longevity; some genetic or environmental factor might increase both well-being and longevity. Few studies have examined the health and behavioral outcomes of positive psychology interventions. A systematic review of positive psychological interventions for breast cancer patients [72] found no physical health outcomes reported. A brief psychological intervention incorporating positive psychology has been shown to reduce mortality in problem drinkers [73]. Kushlev and colleagues [74] found that a positive psychology intervention reduced the number of sick days taken in a community-sample RCT, which may translate to physical health improvements. More longitudinal research should attempt to link length and quality of life in positive psychology interventions.

This research may also better elucidate *how* well-being leads to longevity. This meta-analysis gives few answers or paths to answers because of the high heterogeneity of studies. Theory-driven longitudinal studies about well-being and ill-being constructs with physiological data at multiple time points could assess potential mediational and physiological pathways from well-being to survival. They will also help isolate the components of well-being that drive that survival.

Because the association between well-being and longevity is robust to adjustment for health, health behaviors, social variables, and demographics in this meta-analysis, well-being intervention are possible complements to the health behavior promotion approaches that have long been present in primary care settings [75–77].

Limitations

The conclusions and implications of this meta-analysis are curbed by its limitations: heterogeneity, publication bias, and study quality. Heterogeneity is a given in most meta-analyses because they aggregate studies with a variety of study variables, all of which limit specificity. Other meta-analyses of well-being and mortality have similar heterogeneity to this one [13]. Heterogeneity reduces our ability to detect things like covariation and moderation, especially without theory-driven hypotheses to limit the number of statistical tests.

Heterogeneity is very important in terms of the different statistical control variables that studies use. We attempted to study heterogeneity on a between-study level, but it may be much more worthwhile to pry apart the heterogeneity caused by the stagewise addition of different variables in the proportional hazards regressions within studies. One may examine the before-and-after effects of, say, adding health variables in each study. Doing so may further decrease heterogeneity. This method is not a silver bullet, however, because the stages may be in different order and be composed of different groupings of variables depending on the study, so significant heterogeneity would persist. Even having full datasets of the included studies available for re-analysis would still present heterogeneous sets of variables. Study characteristics like population cannot be studied on a within-study level either.

The specific pair of well-being and ill-being constructs being measured may also matter more than this meta-analysis accounted for. Some ill-being measures may be broader or more specific, leading to different amounts of overlap with different constructs.

Publication bias was present according to some methods, and, even though it was generously estimated, it may be larger than was estimated by the multiple methods I employed. Paired with this is the large number of studies with moderate to high levels of risk of bias as measured by the Q-Coh protocol. Q-Coh is more likely to find bias than other, more common instruments like the Newcastle-Ottawa Scale [46], and I made attempts to account for bias with multiple methods, but the high risk of bias in most of these studies shouldn't be ignored. Given that I am a single rater who employed personal heuristics and algorithms to arise at systematic ratings of studies, it is also possible that the way we applied the Q-Coh protocol does not completely directly map onto the original intent of each of its points. RoBMA also attempts to estimate and remove publication bias, but this is only an estimate. There were even outright mistakes in some publications where the authors reports controlling or not controlling for ill-being but clearly did so when examining relevant tables; such mistakes may be subtle or unimportant for the premise of the study at-hand, but they are vital for this analysis, and it may be impossible to detect such mistakes without a review of each study's analysis code, which is itself impossible in some cases. Enough bias and low-quality studies can lead to erroneous conclusions.

Finally, despite efforts for breadth in the search process, the specific search terms and methods used may have missed portions of the literature. Cursory sensitivity analyses show that several studies would have to be missing to nullify the statistical associations found, but this is still possible. Other fields may use other terms, and certainly other languages do. Attempts were made in the search process to find translated abstracts, but untranslated abstracts within searches would have been missed; only a handful of studies were written in other languages, and only one qualified for inclusion. The gray literature searches found limited conference proceedings, theses, and others; more are likely out there but are difficult to recover efficiently.

Conclusion

The effects of well-being and ill-being have not been clearly separated in prior studies and meta-analyses of all-cause mortality, leading to assumptions about the superfluousness of measuring well-being. This meta-analysis demonstrated that such an assumption is misguided, showing that measures of well-being and specific well-being constructs are robustly associated with protection from all-cause mortality even when controlling for ill-being. Publication bias, risk of bias, and outliers could not explain this association, but heterogeneity of studies limited inference and generalization. Future work should examine the causal effects of well-being interventions on longevity as well as the biopsychosocial mediators that generate the association between well-being and survival.

Supplementary Information

The online version contains supplementary material available at https://doi.org/10.1186/s12889-026-27188-5.

Supplementary Material 1.

Supplementary Material 2.

Supplementary Material 3.

Supplementary Material 4.

Supplementary Material 5.

Supplementary Material 6.

Supplementary Material 7.

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Authors' contributions

MG wrote and reviewed the manuscript, wrote the methods and analyses, and prepared all tables and figures. DY wrote and reviewed the manuscript and reviewed the methods and analyses. One non-author contributor noted in the acknowledgements reviewed the manuscript and made many comments on it.

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Data availability

Information on how to find the data is provided within the manuscript. It is also found on this repository: https://osf.io/s8xjq/?view_only=77768037aacb4801bdcb759b6e63f428.

Declarations

Competing interests

The authors declare no competing interests.

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