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Optimism Versus Pessimism as Predictors of Physical Health: A Comprehensive Reanalysis of Dispositional Optimism Research

Michael F. Scheier and Joshua D. Swanson
Carnegie Mellon University

Meaghan A. Barlow
University of California, Berkeley

Joel B. Greenhouse
Carnegie Mellon University

Carsten Wrosch
Concordia University

Hilary A. Tindle
Vanderbilt University

(For the Optimism/Pessimism Meta-Analytic Consortium)

Prior research has related dispositional optimism to physical health. Traditionally, dispositional optimism is treated as a bipolar construct, anchored at one end by optimism and the other by pessimism. Optimism and pessimism, however, may not be diametrically opposed, but rather may reflect 2 independent, but related dimensions. This article reports a reanalysis of data from previously published studies on dispositional optimism. The reanalysis was designed to evaluate whether the presence of optimism or the absence of pessimism predicted positive physical health more strongly. Relevant literatures were screened for studies relating dispositional optimism to physical health. Authors of relevant studies were asked to join a consortium, the purpose of which was to reanalyze previously published data sets separating optimism and pessimism into distinguishable components. Ultimately, data were received from 61 separate samples (N = 221,133). Meta-analytic analysis of data in which optimism and pessimism were combined into an overall index (the typical procedure) revealed a significant positive association with an aggregated measure of physical health outcomes ($r = .026$, $p < .001$), as did meta-analytic analyses with the absence of pessimism ($r = .029$, $p < .001$) and the presence of optimism ($r = .011$, $p < .018$) separately. The effect size for pessimism was significantly larger than the effect size for optimism ($Z = -2.403$, $p < .02$). Thus, the absence of pessimism was more strongly related to positive health outcomes than was the presence of optimism. Implications of the findings for future research and clinical interventions are discussed.

Public Significance Statement
Prior research on dispositional optimism typically combines the presence of optimism and the absence of pessimism into an overall index. Prior research using this combined index suggests that dispositional optimism is associated with better physical health. The present reanalysis of existing data breaks apart the 2 components of dispositional optimism and suggests that the absence of pessimism is more strongly related to good physical health than is the presence of optimism.

Keywords: optimism, pessimism, physical health, meta-analytic methodology

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Michael F. Scheier and Joshua D. Swanson, Department of Psychology, Carnegie Mellon University; Meaghan A. Barlow, Department of Psychology, University of California, Berkeley; Joel B. Greenhouse, Department of Statistics and Data Science, Carnegie Mellon University; Carsten Wrosch, Department of Psychology, Concordia University; Hilary A. Tindle, School of Medicine, Vanderbilt University.

Members of the Optimism/Pessimism Meta-Analytic Consortium include: Diane Abatemarco, Wills Vision Center, Jefferson University; Cleopatra Abdou, Department of Psychology, University of Southern California; Kaleab Abebe, Division of General Internal Medicine, Center for Clinical Trials and Data Coordination, University of Pittsburgh School of Medicine; Nancy Adler, Center for Health and Community, University of California San Francisco; Amy L. Ai, College of Social Work, Florida State University; Ericha Anthony, Department of Family Medicine and Public Health, University of California San Diego; Elizabeth Barrett-Connor, Department of Family Medicine and Public Health, University of California San Diego; Scott Beach, Harvard Medical School and Massachusetts General Hospital; Theresa Beckie, College of Nursing, University of South Florida; Melanie Bell, Division of Epidemiology and Biostatistics, Mel and Enid Zuckerman

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Michael F. Scheier

Folk wisdom has long held that differences among people in optimism and pessimism are important to many aspects of daily living. In this case, folk wisdom seems to be right. Optimists have been documented to differ from pessimists in many important ways. They differ in how they approach and cope with the problems they confront (Nes & Segerstrom, 2006), the number and quality of relationships they form (Assad, Donnellan, & Conger, 2007; Brissette, Scheier, & Carver, 2002), and the quality of life they experience (Duffy, Bott, Allan, & Torrey, 2013; Segerstrom, Carver, & Scheier, 2017).

Optimists and pessimists also differ in their physical health. For the past three decades, research on dispositional optimism and physical health has flourished. A *Google Scholar* search for "*dispositional optimism*" *and* "*physical health*" yields over 5,000 hits. Dispositional optimism predicts a number of short-term and long-term health outcomes, including rehospitalization after surgery (Scheier et al., 1999; Tindle et al., 2012), incident cardiovascular disease (Tindle et al., 2009), incident stroke (Kim, Park, & Peterson, 2011), and mortality (Kim et al., 2017; Tindle et al., 2009). It is also related to a number of biological markers tied to disease endpoints, including ambulatory blood pressure (Räikkönen et al., 1999) and cortisol secretion (Jobin, Wrosch, & Scheier, 2014), as well as levels of lipids (Boehm et al., 2013b) and antioxidants (Boehm et al., 2013a).

Although links between dispositional optimism and physical health now seem well-established (for a general quan-

College of Public Health, University of Arizona; David Bennett, Department of Psychiatry, College of Medicine, Drexel University; Paul Bennett, School of Nursing and Midwifery, Deakin University Burwood; Hasida Ben-Zur, School of Social Work, University of Haifa; Gunilla Berglund, Department of Psychology, Stockholm University; Elizabeth Blackburn, Department of Biochemistry and Biophysics, University of California San Francisco; Maria Bleil, School of Nursing, University of Washington; Julia Boehm, Department of Psychology, Chapman University; Jacky Boivin, School of Psychology, Cardiff University; David Boulifard, Department of Psychology, Rutgers, The State University of New Jersey; Jennifer Boylan, Health and Behavioral Sciences, University of Colorado, Denver; Phyllis Butow, Centre for Medical Psychology and Evidence-Based Decision-Making, School of Psychology, University of Sydney; Janet Catov, Department of Obstetrics, Gynecology and Reproductive Sciences and the Department of Epidemiology, University of Pittsburgh; Christopher Celano, Harvard Medical School and Massachusetts General Hospital; Sheldon Cohen, Department of Psychology, Carnegie Mellon University; Richard Contrada, Department of Psychology, Rutgers, The State University of New Jersey; Anna deFazio, Department of Obstetrics, Gynaecology and Neonatology, Centre for Cancer Research, University of Sydney; Firdaus Dhabhar, Department of Psychiatry and Behavioral Sciences and Department of Psychology, University of Miami; Alexandrina Dumitrescu, Institute of Clinical Dentistry, University of Tromsø; Meredith Duncan, School of Medicine, Vanderbilt University Medical Center; Chris Dunkel Schetter, Department of Psychology, University of California Los Angeles; Scott Dunsiger, Department of Behavioral and Social Sciences, School of Public Health, Brown University/Center for Behavioral and Preventive Medicine, The Miriam Hospital; Steriani Elavsky, Institute for Research on Children, Youth, and Family, Masaryk University; Ari Elliot, Department of Psychiatry, University of Rochester; Charles Emery, Department of Psychology, Ohio State University; Romano Endrighi, Center for Behavioral Science Research, Henry M Goldman School of Dental Medicine, Boston University; Elissa Epel, Department of Psychiatry, University of California San Francisco; Michael Frain, Department of Counselor Education, Florida Atlantic University; Matt Freiberg, School of Medicine, Vanderbilt University Medical Center; Hanna Gaggin, Harvard Medical School and Massachusetts General Hospital; Andrew Geers, Department of Psychology, University of Toledo; Marianne Geleijnse, Division of Human Nutrition and Health, Wageningen University & Research; Erik Giltay, Department of Psychiatry, Leiden University Medical Centre; Steven Gregorich, Department of Medicine, University of California San Francisco; Francine Grodstein, Channing Division of Network Medicine, Brigham and Women's Hospital; Kaitlin Hagan, Channing Division of Network Medicine, Brigham and Women's Hospital; Meredith Halling, Medical College of Wisconsin; China Harrison, School of Psychology, Cardiff University; Ana Progovac, Health Equity Research Laboratory, Department of Psychiatry, Cambridge Health Alliance; Brian Healy, Harvard Medical School and Massachusetts General Hospital; Bea Herbeck Belnap, Division of General Internal Medicine, Center for Behavioral Health and Smart Technology, University of Pittsburgh School of Medicine; Jukka Hintikka, Department of Psychiatry, University of Tampere; Yan Huang, Division of General Internal Medicine, Center for Research on Health Care Data Center, University of Pittsburgh; Jörg Huber, School of Health Sciences, University of Brighton; Jeff Huffman, Department of Psychiatry, Harvard Medical School; Violetta Jachimowicz, Department of Geriatrics, Medical University of Lodz and The President Stanisław Wojciechowski University School of Applied Sciences in Kalisz; Marta Jackowska, Psychology Department, Roehampton University; James Januzzi, Harvard Medical School and Massachusetts General Hospital; Olli Kampman, Faculty of Medicine and Health Technology, Tampere University; Tuomas Kerola, Päijät-Häme Central Hospital; Eric Kim,

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Joshua D. Swanson

titative review see, Rasmussen, Scheier, & Greenhouse, 2009; for a general qualitative review see, Boehm & Kubzansky, 2012), how best to construe the construct of optimism has proven more controversial. Most of the research that has been conducted on dispositional optimism treats the variable as bipolar in nature, anchored at one end by optimism (the generalized expectancy that favorable outcomes will occur in the future) and at the other end by pessimism (the generalized expectancy that unfavorable outcomes will occur in the future). According to this view, as someone moves away from optimism that person necessarily moves more toward pessimism.

This prevailing view has emerged in part because of the way in which the scales used to measure dispositional optimism are scored. The two most widely used scales to measure dispositional optimism are the Life Orientation Test (LOT; Scheier & Carver, 1985) and the Life Orientation Test–Revised (LOT-R; Scheier, Carver, & Bridges, 1994). Each of these scales contains two sets of items. Items from one set are framed in a positive way (assessing the affirmation of optimism or not), and items from the second set are framed in a negative way (assessing the affirmation of pessimism or not). Typically, the negatively framed items are reverse coded and then added to the positively framed items to produce on overall scale score.

Some researchers have questioned the validity of this "bipolar" point of view. Interest in the question arose after several factor analyses suggested that a two-factor model of the items on the LOT and LOT-R fit the data better than did a model with a one-factor solution (e.g., Chang & McBride-Chang, 1996; Hjelle, Belongia, & Nesser, 1996). In these analyses, items assessing expectations for positive out-

Department of Social and Behavioral Science, Harvard T.H. Chan School of Public Health; Yojin Kim, Department of Social Welfare, Hallym University; Jennifer Knight, Department of Psychiatry and Behavioral Medicine, Medical College of Wisconsin; Barna Konkolÿ Thege, Waypoint Research Institute, Waypoint Centre for Mental Health Care, Penetanguishene and Department of Psychiatry, University of Toronto; Tomasz Kostka, Department of Geriatrics, Medical University of Lodz, and Healthy Ageing Research Centre (HARC), Central Veterans' Hospital; Donna Kritz-Silverstein, Department of Family Medicine and Public Health, University of California San Diego; Daan Kromhout, Department of Epidemiology, University Medical Center Groningen; Markku Kuappi, Rheumatology and Internal Medicine, University of Tampere; Laura Kubzansky, Department of Social and Behavioral Sciences, Harvard T.H. Chan School of Public Health; Andreas Kueffer, Department of Psychiatry, University of California San Francisco; Lewis Kuller, Department of Epidemiology, University of Pittsburgh; Julian Lai, Department of Social and Behavioural Sciences, City University of Hong Kong; Wendy Lam, Centre for Psycho-Oncology Research and Training, School of Public Health, University of Hong Kong; Deborah Lancastle, Faculty of Life Sciences and Education, University of South Wales; Joseph Landers, QIMR Berghofer Medical Research Institute; Gwen Latendresse, College of Nursing, University of Utah; Chengwei Li, Department of Epidemiology, University of Michigan School of Public Health; Jue Lin, Department of Biochemistry and Biophysics, University of California San Francisco; Lynda Lisabeth, Department of Epidemiology, University of Michigan School of Public Health, and Stroke Program, University of Michigan Medical School; Carissa Low, Department of Medicine, Division of Hematology/Oncology, University of Pittsburgh; Mark Lumley, Department of Psychology, Wayne State University; Nicholas Martin, QIMR Berghofer Medical Research Institute; Sheila McDonald, Department of Paediatrics, University of Calgary; Joel Milam, Institute for Health Promotion and Disease Prevention Research, Department of Preventive Medicine, Keck School of Medicine, University of Southern California; Mary Minton, College of Nursing, South Dakota State University; Paula Mommersteeg, Department of Medical and Clinical Psychology, and Center of Research on Psychology in Somatic Diseases, Tilburg University; Lewis Morgenstern, Department of Epidemiology, University of Michigan School of Public Health, and Stroke Program, University of Michigan Medical School; Miriam Mosing, Department of Neuroscience, Karolinska Institute; Cheryl Moyer, Learning Health Sciences, and Obstetrics and Gynecology, University of Michigan; Aoife O'Donovan, Department of Psychiatry, University of California San Francisco; Kendea Oliver, Psychiatry and Behavioral Health, Lahey Hospital and Medical Center; Jolanta Opacka-Juffry, Department of Life Sciences, University of Roehampton; Mikko Pänkäläinen, Department of Psychiatry, Päijät-Häme Central Hospital; Lauri Pasch, Department of Psychiatry, University of California San Francisco; Madelon Peters, Department of Clinical Psychological Science, Maastricht University; Natalia Petros, Department of Psychosis Studies, Institute of Psychiatry, Psychology and Neuroscience, King's College London; Ovidiu Popa-Velea, Department of Medical Psychology, Faculty of Medicine, "Carol Davila" University of Medicine and Pharmacy; Melanie Price, Centre for Medical Psychology and Evidence-Based Decision-Making (CeMPED), School of Psychology, University of Sydney; Katri Räikkönen, Department of Psychology and Logopedics, Faculty of Medicine, University of Helsinki; Batia Rappaport, The Foundation for the Benefit of Holocaust Victims in Israel; Nathaly Rius Ottenheim, Department of Psychiatry, Leiden University Medical Centre; Michael Rohrbaugh, Department of Psychology, University of Arizona; Bruce Rollman,

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Meaghan A. Barlow

comes loaded on one factor (an "optimism" factor reflecting the affirmation of optimism or not), whereas items assessing expectations for negative outcomes loaded on a second factor (a "pessimism" factor, reflecting the affirmation of pessimism or not). Consistent with the factor analytic results, correlations between the optimism and pessimism subscales are modest (Mens, Scheier, & Carver, 2016).

Conceptually, it makes sense that optimism and pessimism are somewhat distinct. Clearly, someone who is not pessimistic is not necessarily optimistic. It only means that there is an absence of pessimism. Similarly, someone who is not optimistic is not necessarily pessimistic. It just means that there is an absence of optimism. People can be neither optimistic nor pessimistic. This is one reason why the two factors are thought to reflect the presence or absence of the characteristic in question. Consistent with this construal, the same terminology is used throughout this article to refer to the two ends of the optimism and pessimism dimensions.

There are differences in opinion about what to make of the factor analytic studies. Monzani, Steca, and Greco (2014) believe that the two factors are observed because of response style and that optimism should still be conceptualized as a single dimension. Others have argued that optimism and pessimism are distinct properties that may have differential effects on various aspects of physical health (e.g., Kubzansky, Kubzansky, & Maselko, 2004). Several attempts have used item response theory to resolve the issue. This research suggests that a single dimension may fit the LOT-R best (e.g., Steca, Monzani, Greco, Chiesi, & Primi, 2015). However, the issue is far from resolved psychometrically.

Research from the field of behavior genetics offers further support for the idea that optimism and pessimism are distinguishable. A variety of studies now support the idea that there is a genetic basis for differences in dispositional

Division of General Internal Medicine, Center for Behavioral Health and Smart Technology, University of Pittsburgh School of Medicine; John Ruiz, Department of Psychology, University of Arizona; Elena Salmoirago-Blotcher, Brown University Medical School and School of Public Health; Brisa Sanchez, Department of Biostatistics, University of Michigan School of Public Health; Michael F. Scheier, Department of Psychology, Carnegie Mellon University; Anna Serlachius, Department of Psychological Medicine, University of Auckland, and Department of Psychology and Logopedics, The University of Helsinki; Aladdin Shadyab, Department of Family Medicine and Public Health, University of California San Diego School of Medicine; Laura Smart Richman, Duke University School of Medicine; Gabrielle Snyder, Magee Women's Research Institute, University of Pittsburgh; Marcia Stefanick, Stanford Wellness Living Laboratory, Stanford University School of Medicine; Jesse Stewart, Department of Psychology, Indiana University-Purdue University; Angelina Sutin, College of Medicine, Florida State University; Joshua D. Swanson, Department of Psychology, Carnegie Mellon University; Jean Tillie, Department of Psychiatry and Behavioral Sciences, and Stanford Immunology Program, Stanford University; Hilary A. Tindle, Department of Medicine, Vanderbilt University Medical Center, and the Geriatric Research Education and Clinical Centers (GRECC), Veterans Affairs Tennessee Valley Healthcare System; Kirsti Toien, Department of Research and Development, Division of Critical Care, Oslo University Hospital; Janice Tomakowsky, Department of Psychology, Wayne State University; Gideon Uretzky, Department of Cardiothoracic Surgery, Tel Aviv University; Jason Van Allen, Department of Psychological Sciences, Texas Tech University; Ondine van de Rest, Division of Human Nutrition and Health, Wageningen University & Research; Jean Wactawski-Wende, Department of Epidemiology and Environmental Health, University at Buffalo; Penny Webb, Gynaecological Cancers Group, QIMR Berghofer Medical Research Institute; Howard Wey, College of Nursing, South Dakota State University; Andrew Wilson, College of Nursing, University of Utah; Owen Wolkowitz, Department of Psychiatry, University of California San Francisco; Carsten Wrosch, Department of Psychology, Concordia University; Joyce Yi-Frazier, Center for Clinical and Translational Research, Seattle Children's Research Institute; Pekka Ylöstalo, Unit of Oral Health Sciences, Medical Research Center Oulu (MRC Oulu), Oulu University Hospital and University of Oulu.

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Correspondence concerning this article should be addressed to Michael F. Scheier, Department of Psychology, Carnegie Mellon University, 5000 Forbes Avenue, Pittsburgh, PA 15213. E-mail: scheier@cmu.edu

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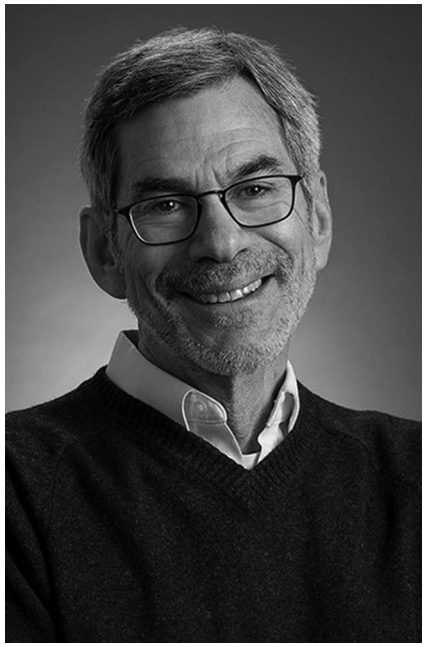

Joel B. Greenhouse

optimism (e.g., Caprara et al., 2009; Plomin et al., 1992). More important, there is also some evidence that the genetic origins of optimism and pessimism might be slightly different. For example, Plomin et al. (1992) have shown that shared environment is more important for optimism than pessimism. Using more complex modeling techniques, Bates (2015) has shown that optimism and pessimism contain genetic variation that separates them from both the Big Five personality factors and from each other.

Recent research in health psychology also contributes to the discussion of dimensionality by documenting that optimism and pessimism can be related to physical health differentially. For example, research suggests that it is pessimism that produces associations with inflammation, not optimism (Ikeda et al., 2011; O'Donovan et al., 2009; Roy et al., 2010). Pessimism was also found to be a stronger predictor than optimism of in vitro fertilization success (Bleil et al., 2012). In contrast, Kim et al. (2011) showed that optimism, but not pessimism, predicted incidence of stroke. Although only a handful of studies speak directly to this issue, the available evidence tends to suggest that the absence of pessimism might be a more important contributor to associations with physical health than the presence of optimism. Clearly, however, more research is needed on this issue.

In this regard, an organization called the Optimism/Pessimism Meta-Analytic Consortium (OPMAC) was formed to pool data from across studies to examine more systematically the effects of optimism and pessimism on physical health. Each member of the consortium has reanalyzed data from a previously published study in such a way that the effects of optimism and pessimism can be separated and compared. The purpose of this article is to present the results of the reanalyses that were conducted on the novel data that OPMAC members provided. Given the trend of the few available studies published before the present reanalyses were conducted, the absence of pessimism was expected to be a stronger predictor of positive physical health than was the presence of optimism.

Method

Literature Search Strategy and Inclusion/Exclusion Criteria

To identify relevant researchers to contact, literature searches were performed on the MedLINE and PsycINFO databases for relevant studies published in English-language peer-reviewed journals up until December 31, 2016 using combinations of the following keywords: optimism, pessimism, Life Orientation Test, LOT, Life Orientation Test–Revised, LOT-R, immune, inflammation, HIV or AIDS, arthritis, osteoarthritis, lupus, autoimmune, multiple sclerosis, pregnancy, infertility, cancer or neoplasms, cortisol, blood pressure, atherosclerosis, cardiovascular, coronary, heart, infarction, stroke, diabetes, glycemic, anemia, respiratory, tuberculosis, dementia, asthma, Huntington's, renal, kidney, influenza, pneumonia, cold, ulcer, sleep, survival, death, mortality, body mass index, wound, surgery, and metabolic. The search terms used to identify studies were largely derived from a meta-analysis of the same area conducted by Rasmussen et al. (2009), with extra terms added to capture biomarkers more fully. Review papers and references from relevant articles were used to identify additional studies of interest. After an initial prescreening for potentially relevant articles, based on the study title and abstract, a total of 549 full-length articles were downloaded for further evaluation based on our inclusion and exclusion criteria.

Articles were included for consideration if they met two inclusion criteria. First, the study utilized the LOT (Scheier & Carver, 1985), the LOT-R (Scheier et al., 1994), or a validated translation or adaptation of either the LOT or LOT-R (e.g., the Parent-rated Life Orientation Test of children; Lemola et al., 2010). This criterion was enacted because the objective of the present set of reanalyses was to evaluate the differential effects of optimism and pessimism on physical health. The LOT and LOT-R are the only scales available that allow for overall/combined optimism to be deconstructed into its underlying components. The LOT-R was created to remove two coping items that had been included in the original LOT. The LOT and the LOT-R correlate in the low .90s (Scheier et al., 1994). The psychometric properties of the LOT and LOT-R are well established (for a review, see Carver & Scheier, 2019), and they are used widely in the literature in health psychology. An

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Carsten Wrosch

example of a positively worded item is, "In uncertain times, I usually expect the best." An example of a negatively worded item is, "I hardly ever expect things to go my way." All items are answered along a 5-point Likert scale, ranging from "*strongly disagree*" at one end to "*strongly agree*" at the other.

Second, the study included an objective measure of physical health. Objective measures of physical health included biomarkers of various types (e.g., systolic and diastolic blood pressure, cortisol reactivity), disease incidence (e.g., stroke, acute myocardial infarction, diabetes, and cancer), hospital stay or rehospitalization, and survival or mortality. Review papers, unpublished data, dissertations, and conference abstracts were not included.

Two additional exclusion criteria were also used. First, studies were excluded if neither optimism/pessimism nor physical health were the primary focus of the study (the vast majority of these studies had simply included optimism or physical health as part of a wider set of covariates). The decision to exclude these studies was made largely on the basis of expected utility. That is, to provide useful data for the present reanalyses authors were required to reanalyze the data from their studies, breaking optimism and pessimism down into separate factors (as noted, the norm for published studies is to combine these components into an overall score). If the primary theoretical frameworks of authors were related to neither optimism/pessimism nor physical health, it seemed unlikely that they would put the needed effort into providing data for the reanalyses. For this reason, they were not pursued further.

Studies of primary interest in this analysis were those conducted in the field, often over prolonged periods of time. Consequently, studies were also excluded if they represented experimental laboratory studies that consisted of a single session, in which participants were randomly assigned to conditions. These studies were excluded because they were thought to be too dissimilar to the larger set of field studies of primary interest. Including them would have made interpretation of results difficult.

Upon evaluating the 549 downloaded articles, 189 relevant studies were identified that met our inclusion and exclusion criteria. Of these 189 studies, 16 were removed because they provided duplicate data across time. These papers tended to provide interim reports of ongoing longitudinal studies. The rule for serial publication of results was to take the longest follow-up time available. An additional 10 studies were removed because no e-mail was provided for the original authors. Four studies were removed because the measure of optimism was assessed after the measure of physical health. The corresponding authors of the remaining 159 articles were contacted and asked if they had an interest in joining the consortium.

Of the authors contacted, 44 did not respond to our request, and 50 reported that the data were no longer available. In addition, there were two cases (Ai, Seymour, Tice, Kronfol, & Bolling, 2009; Lai et al., 2005) for which incorrect analyses had been requested. Because the error was discovered late in the process of data analysis, these authors were not asked to provide corrected data. Finally, one study (Bennett et al., 2015) was excluded because the researchers only collected data on the optimism subscale, and data from both subscales were needed to conduct analyses.

Ultimately, the data from 62 papers were available for inclusion in the present reanalyses. Two pairs of these studies (Pänkäläinen, Kerola, & Hintikka, 2015 and Pänkäläinen, Kerola, Kampman, Kauppi, & Hintikka, 2016, and Ruiz, Matthews, Scheier, & Schulz, 2006 and Scheier et al., 1999) reported on the same sample, but included different outcomes from one paper to the next. These two pairs of studies were included in the analyses, but the data from the pairs of studies were considered to be dependent for purpose of analysis. That is, they were treated as providing multiple outcomes from the same sample. Another study (Konkoly-Thege et al., 2015) provided separate, independent samples in the same paper (one comprised of healthy controls and one comprised of patients). These samples were treated as independent in the analyses. Thus, a total of 61 independent samples was ultimately available for inclusion in the present reanalyses (see Figure 1 for a graphic display of the study selection process).

Data Collection

Initial contact. Potential consortium members were contacted by e-mail, informed of the purpose of the present

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Hilary A. Tindle

project, told what additional analyses needed to be performed, and invited to join the effort. If no response was received, a second e-mail was sent with the same information 2 to 4 weeks later. If no response was received to the second e-mail, a third e-mail was sent 2 to 4 weeks later. If no response was received to the third e-mail, recipients were identified as nonresponders. Recipients were also identified as nonresponders if, after a corresponding author expressed interest in contributing their data, at least 2 months had passed without receiving the requested data and no response was given to a follow-up e-mail regarding the status of their analyses.

The data collection process began on August 11, 2016. All data were received by May 31, 2017. Recipients who participated were given $200 as a token of appreciation for their effort and were entered into the Optimism/Pessimism Meta-Analytic Consortium (OPMAC).

Requested analyses. Each consortium member was asked to conduct three separate analyses, one using the overall/combined optimism score as the predictor variable, one using the pessimism subscale as the predictor variable, and one using the optimism subscale as the predictor variable. All analyses treated optimism and pessimism as continuous variables. Items were recoded so that a high score indicated high optimism (for the overall/combined scale and the optimism subscale) or low pessimism (for the pessimism subscale). Effect sizes were coded such that a positive effect size indicated better health. Thus, the overall/combined scale, the optimism subscale, and the pessimism subscale should all be related in a positive manner with the health outcomes assessed.

If a published study contained physical health outcomes in the primary outcomes reported, those same physical health measures were requested as outcomes in the reanalysis. If a published study contained physical health outcomes, but did not report them as primary outcomes, all relevant physical health measures included in the study were requested as outcomes in the reanalysis. Relevant physical health measures were defined as those that had been used as a primary outcome in at least one other study in the pool of studies in the analysis. This strategy was

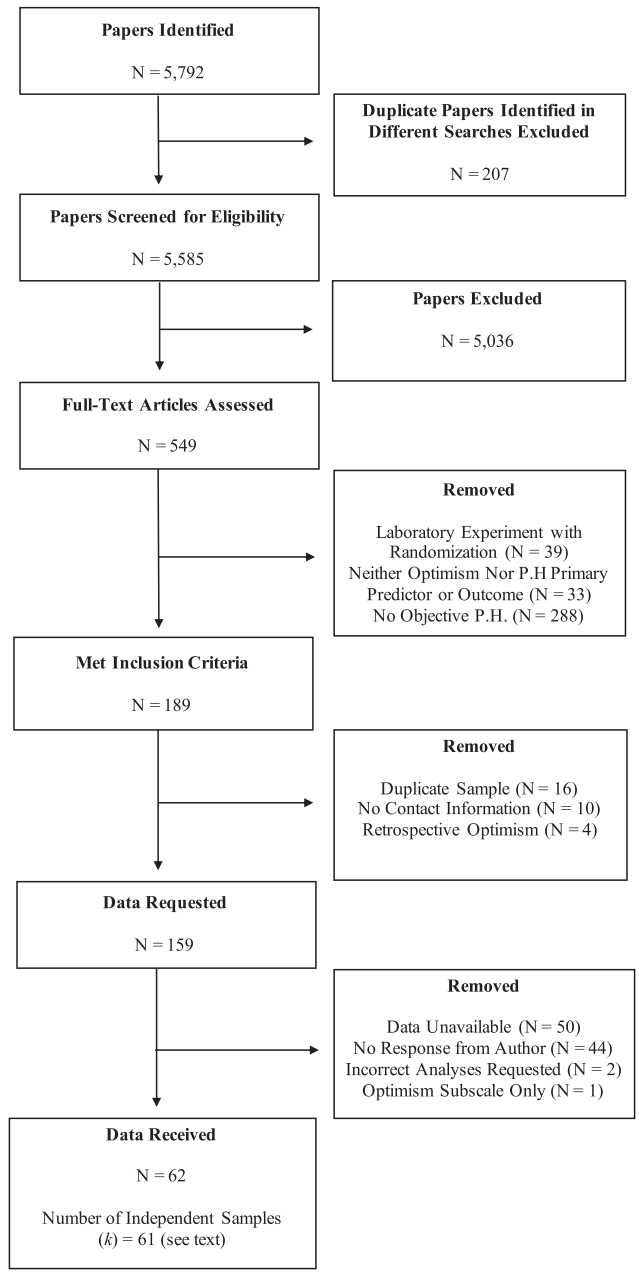

Figure 1. Flowchart showing inclusion/exclusion of studies identified from initial search.

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employed to avoid including an abundance of studies with idiosyncratic outcomes (i.e., outcomes that were not of established interest to the research literature on optimism and health more widely). Table 1 in the online supplemental materials lists the outcome measures obtained for each of the studies in the analyses.

When requesting covariates for the reanalyses, consortium participants were asked to use the same set of covariates that was used in the published paper. Some of the studies had an extraordinarily large number of covariates. Consequently, the number of covariates requested for inclusion in the reanalyses was capped at 20. Major classes of covariates included demographic variables (e.g., gender, education level), psychosocial variables (e.g., depressive symptoms, negative affectivity), or factors related to the study design (e.g., length of follow up from baseline to final assessment).

Several categories of covariates were explicitly excluded from the reanalyses. These included measures of coping styles and strategies, social support, situational expectations for the health context studied, biomarkers and preclinical indicators of disease (e.g., C-reactive protein and body mass index, respectively), and health behaviors (e.g., smoking, physical activity). These classes of covariates were excluded because existing data has shown that these variables are predicted by optimism (for a review, see Scheier & Carver, 2018). Because of this covariation, these variables could reflect underlying mechanisms whereby the impact of optimism on downstream health outcomes is mediated. Correcting for potential mediators could artificially reduce the effect size estimating the association between optimism and health by eliminating the contribution of indirect pathways (Gallo & Matthews, 2003). For this reason, potential mediators were excluded as covariates when reanalyses were conducted. The covariates included for each of the studies in the analyses can also be found in Table 1 in the online supplemental materials.

When possible, consortium members were asked to reanalyze their data in the same way they analyzed their data in the original study. If the original study did not conduct an analysis using optimism as a predictor and physical health as an outcome, consortium members were requested to conduct either a linear or logistic regression, depending on whether the physical health outcome was continuous or dichotomous. As previously noted, all of the predictors (the combined overall scale, the pessimism subscale, and the optimism subscale) were treated as continuous variables. In addition to the requested effect sizes, researchers were also asked to provide the internal consistency reliability for the overall/combined optimism scale, the pessimism subscale, and the optimism subscale, as well as the correlation between the optimism and pessimism subscales.

Abstracted data. In addition to effect size data, pertinent data from the original article and from the requested reanalysis were abstracted. Abstracted data included year study was published, scale used to assess optimism, information about the number of participants in the study, the mean age of participants, the percent of the sample that was female, the percent of the sample that was White, the type of sample studied (i.e., clinical or nonclinical), the country from which the sample was drawn, the number and type of covariates included (e.g., demographic, psychosocial), and the study design (e.g., prospective or cross-sectional). Finally, the aim of the original study was also coded to distinguish between original studies that were explicitly focused on both optimism and physical health (and the relationship between the two of them), and studies that were primarily interested either in optimism or in physical health (but not explicitly with the association between the two).

For the purpose of this analysis, studies were coded as being prospective in design if they met one of the following two conditions: (a) the requested reanalyses controlled for the physical health outcome at baseline; (b) the study examined either mortality or disease incidence and screened out all participants with prior or current illness such that the sample was assumed to be physically healthy at baseline. Longitudinal studies were those that assessed optimism/pessimism measures at baseline and then documented health outcomes at a later point in time. Unlike prospective studies, however, health outcomes were not controlled for in some fashion at baseline. Cross-sectional studies were those that assessed predictor and outcome at the same point in time. These distinctions are consistent with the description of study design differences presented by Cohen, Evans, Stokols, and Krantz (1986).

Abstracted data were double-entered. Discrepancies in coding were resolved through discussion of the coders. Coder reliability across entries averaged 84%, ranging from 52% to 99%, with the most disagreement occurring for the coding of study design.

Statistical Analyses

General considerations. Before the questions of primary interest could be answered, the data from the different samples needed to be harmonized, aggregated, and summarized. The analytic approach is based on methods used in meta-analysis for combining information from similar studies. Random effects models (that assume that samples are drawn from different populations and allow for both random variance and variance because of true population differences) were used for all analyses conducted. Given the different contexts represented across studies, random effects models were assumed to provide a more accurate estimate of confidence intervals (CIs) than fixed effects models (see, e.g., Schmidt, Oh, & Hayes, 2009).

Calculation of effect sizes. For outcomes that were treated as continuous variables, consortium members pro-

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vided standardized beta coefficients from their analyses. For dichotomized outcomes, consortium members provided odds ratios or hazard ratios, depending upon the specific analysis conducted. The data received were than transformed into Fisher Z (Z') scores, following the guidelines offered by Borenstein, Hedges, Higgins, and Rothstein (2009). These converted Z' scores were then used in the meta-analytic analyses that were conducted, as well as in tables and figures that are presented. Z' estimates were transformed into r estimates for purposes of data presentation in text.

Heterogeneity. Heterogeneity of the variances in the effect sizes from the primary analyses were evaluated using the I^2 index, which is an indicator of the proportion of variance explained by heterogeneity. An I^2 index above 50% suggest a heterogeneous effect size distribution, which warrants additional moderator analyses (Higgins & Thompson, 2002).

Analytic strategy. Multiple outcomes within a study were treated as dependent, as the outcomes assessed were likely to be correlated to a greater or lesser extent. Therefore, the robust variance estimation (RVE) method was used to account for dependency among samples with multiple effect sizes because it allows one to specify the within-study correlation among effects. We utilized the default within-study correlation value of .80 in our analyses. Further, the small sample adjustment was applied to correct for bias in p values (Tanner-Smith, Tipton, & Polanin, 2016).

More traditional meta-analytic techniques were used to compute estimates for which the empirically calculated degrees of freedom fell below four. For these estimates, an average effect size for that study was computed and used in the relevant analysis. This strategy was selected given that the estimated p values can be inaccurate when the empirically calculated degrees of freedom fall below four (Tanner-Smith et al., 2016).

For the RVE analyses, we tested the basic metaregression model to estimate the mean effect size (i.e., intercept only, no predictors). Subsequently, moderator analyses were conducted by adding the respective moderator variable as a predictor to the metaregression model. For continuous moderators, the coefficients can be interpreted as the estimated amount of change in strength of the association (i.e., mean effect size) given a one unit increase in the moderator. For categorical moderators, dummy codes were used and can be interpreted as the mean effect size difference between the relevant groups.

Before analyses, the following two sets of variables were identified as potential moderators, depending upon whether the variable was categorical or continuous in nature. Continuous moderators included year the study was published, average age of sample, percent of sample that was female, percent of sample that was White, number of psychosocial covariates used in the analyses, and total number of covariates included in the analyses. Categorical moderators included study objective (whether the focus of the study was on optimism, physical health, or both), study design (whether the study was cross-sectional, longitudinal, or prospective), participant status (healthy vs. patient), scale used to assess optimism (LOT vs. LOT-R), and the country of origin for the study (United States vs. elsewhere).

Finally, analyses were conducted to test for differences between optimism and pessimism. Preliminary analyses of the effect sizes for the pessimism and optimism subscales showed both distributions to be nonnormal. As a result, a Wilcoxon signed-ranks test for paired samples was used to test the significance of the difference between the two subscales. For these comparisons, an average effect size for each study within each subscale was computed and employed in the relevant analysis (inasmuch as the RVE software used did not provide ES estimates for individual studies).

Software. The RVE analyses were conducted in R (Version 3.5.1) using Robumeta package (Fisher & Tipton, 2014) to estimate mean effect sizes and metaregression models and the clubSandwich package (Pustejovsky, 2015) to estimate the multiparameter F tests. The standard meta-analysis estimates were obtained using Comprehensive Meta-Analysis Software Version 3 (BiostatTM, Englewood, NJ). Finally, all nonmeta-analytic analyses were conducted using IBM SPSS Statistics Version 25.

Results

Study Characteristics

The number of participants in the studies reported here totaled 221,133. The participants averaged 63.71 years of age, were 91.44% female, and were 92.07% White (based on the 35 studies that reported the race of the participants). The majority of the studies were conducted in the United States, 90.12%. The high percentage of White women can be attributed largely to two studies, Kim et al. (2017) and Tindle et al. (2009), which were all women and largely White and contributed 167,274 to the participant count.

Cronbach's α for the overall/combined scale, the optimism subscale, and the pessimism subscale were 0.75, 0.72, and 0.75, respectively. A one-way repeated measures analysis of variance (ANOVA) was conducted to evaluate the significance of the differences between the alphas. This overall analysis was not significant, Wilk's $\lambda = .904$, $F(2, 54) = 2.88$, $p > .06$ (not all of the researchers provided alphas, which accounts for the fewer than 59 *df*). Because the significance level from this overall analysis approached significance, it was followed by pairwise comparisons using Bonferroni adjust-

ments. None of the pairwise comparisons was statistically significant, all ps $> .08$. Thus, differences in the reliabilities of the three measures were unlikely to have caused any observed differences in effect size. The correlation between the pessimism subscale (with items reverse coded) and the optimism subscale was .33, $p < .02$. The standard deviation of the correlation between the scales was .20.

Primary Analyses

Primary analyses involved evaluating effect size estimates using all outcomes from all studies (see Row 1 of Table 1). The effect size for the overall/combined scale was significant ($k = 61$, $n = 201$, $r = .026$, 95% CI [.013, .040], $p < .001$), as were the effect sizes for the pessimism subscale ($k = 61$, $n = 201$, $r = .029$, 95% CI [.018, .041], $p < .001$) and the optimism subscale ($k = 61$, $n = 201$, $r = .011$, 95% CI [.002, .019], $p < .018$). Optimism, as assessed via the overall/combined scale or the optimism subscale, and the absence of pessimism, as assessed by pessimism subscale, were all associated with better physical health. It is also clear, however, that the effect size associated with the pessimism subscale was considerably larger than the effect size associated with optimism subscale, just under three times as large. This difference in the magnitude of the effect sizes was statistically significant ($Z = -2.403$, $p < .02$). Thus, the absence of pessimism was a significantly better predictor of physical health than was the presence of optimism. Forest plots containing individual study effect sizes categorized according the manner in which optimism and pessimism was assessed can be found in Figure 2 (overall/combined scale), Figure 3 (optimism subscale), and Figure 4 (pessimism subscale).

Stratification by Outcome

In addition to the overall analyses, several subsidiary analyses were conducted. These analyses grouped outcomes a priori into several different categories, including biomarkers, disease prevalence/incidence/progression, survival/mortality, hospital stay or reoccurrence, cardiac-related, metabolic, immune function, pulmonary, and pregnancy/fertility. These categories were not mutually exclusive (e.g., systolic blood pressure was coded as both a biomarker and as cardiac-related). Additional groupings were identified, but not analyzed because they contained less than six studies per group. The findings relevant to the outcomes examined can be found in the lower portion of Table 1.

These subgroup analyses generally paralleled the findings obtained for the primary analyses. The effect sizes associated with the pessimism subscale tended to be larger and were more likely to be significant than those associated with the optimism subscale. The effect sizes and significance levels of the overall/combined scale fell in between the two subscales. More specifically, except for outcomes dealing with disease prevalence/incidence/progression, survival/mortality, hospital stay/readmittance, and those that were cardiac-related, the effect sizes for the overall/combined scale were significantly different from zero. With respect to the optimism subscale, six effect sizes were not significantly different from zero: biomarkers, disease prevalence/incidence/progression, hospital stay/readmittance, cardiac-related, metabolic, and pulmonary. In contrast, only one of the nine effect sizes (hospital stay/readmittance) was not significantly different from zero for the pessimism subscale. For three sets of outcomes (biomarkers, immune function, and pregnancy) the difference in magnitude of the effect sizes for the optimism and pessimism subscales was statistically significant ($Z = -2.749$, $p < .007$, $Z = -2.293$, $p <$

Table 1
Effect Size Estimates for the Overall/Combined Scale, the Optimism Subscale, and the Pessimism Subscale

	Optimism/pessimism overall/combined					Optimism subscale only					Pessimism subscale only					Subscale difference[a]	
Outcomes	k	n[b]	Z'	95% CI	$p <$	k	n[b]	Z'	95% CI	$p <$	k	n[b]	Z'	95% CI	$p <$	Z	$p <$
All	61	201	.026	[.013, .040]	.001	61	201	.011	[.002, .019]	.018	61	201	.029	[.018, .041]	.001	−2.403	.016
Biomarkers	39	140	.030	[.015, .046]	.001	39	140	.006	[−.008, .020]	.352	39	140	.046	[.030, .062]	.001	−2.749	.007
Disease prevalence/ incidence/progression	15	30	.012	[−.009, .034]	.189	15	30	.011	[−.008, .031]	.191	15	15[c]	.008	[.003, .012]	.001	−0.625	.532
Survival/mortality	9	15	.024	[−.014, .061]	.162	9	9[c]	.007	[.002, .011]	.006	9	9[c]	.020	[.007, .033]	.004	−0.980	.327
Hospital stay/re-admit	7	11	.002	[−.040, .045]	.899	7	11	.018	[−.011, .047]	.161	7	11	−.002	[−.062, .057]	.921	−0.845	.398
Cardiac-related	20	71	.016	[−.006, .038]	.121	20	71	.014	[−.007, .034]	.158	20	20[c]	.012	[.007, .016]	.001	−0.672	.502
Metabolic	13	29	.028	[.000, .056]	.049	13	29	.006	[−.026, .038]	.672	13	13[c]	.049	[.035, .063]	.001	−1.572	.116
Immune function	10	10[c]	.011	[.004, .018]	.003	10	10[c]	.005	[−.015, .025]	.022	10	10[c]	.023	[.000, .046]	.050	−2.293	.022
Pulmonary	6	6[c]	.008	[.001, .015]	.032	6	6[c]	.008	[.001, .015]	.753	6	6[c]	.011	[.004, .018]	.004	0.314	.753
Pregnancy/fertility	7	7[c]	.042	[.013, .071]	.005	7	7[c]	.010	[−.031, .051]	.043	7	7[c]	.062	[.034, .091]	.001	−2.028	.043

Note. CI = confidence interval.
[a] As determined by the Wilcoxon Signed Ranks Test. [b] Number of effect sizes. [c] Estimated using one effect size per study given empirical degrees of freedom < 4.

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.022, and $Z = -2.028$, $p < .043$, respectively). For all of these subsets, the absence of pessimism was a stronger predictor of specific health outcomes than was the presence of optimism.[1]

Sensitivity

To determine if effect size estimates were driven by a single study, "leave-one-out" analyses were conducted to determine how the significance level of the aggregated effect sizes would change as each study in turn was removed from the analysis (Greenhouse & Iyengar, 2009). For the primary analyses, the reported effect sizes for the overall/combined scale and the pessimism subscale were not dependent upon any single study or studies. Each study in the analysis could be removed one by one and the effect size estimate still remained significant. The reported effect size for the optimism subscale, however, was rendered statistically insignificant when one of two separate studies were removed (Price et al., 2016; Sutin, 2013).

Leave-one-out analyses were also conducted for the subgroup analyses. The removal of one study did sometimes make the effect size become nonsignificant, and this happened more frequently for subgroup analyses involving the optimism subscale (rather than the overall/combined scale or the pessimism subscale). These differences among the predictors are not surprising, inasmuch as the effects for the optimism subscale were often weaker to start with. Predictably, subgroup analyses that contained fewer studies were also more vulnerable to leave-one-out analyses. More details on sensitivity are presented in Table 2 in the online supplemental materials.

Moderator Analyses

In the primary analyses, the amount of heterogeneity of variance associated with the effect sizes for the overall/combined scale and the pessimism subscale were quite large ($I^2 = 62.62\%$ and $I^2 = 60.20\%$, respectively). The heterogeneity of variance in the effect sizes for the optimism subscale was considerably smaller ($I^2 = 27.02\%$). Although the I^2 for the optimism subscale was below the suggested cutpoint identified by Higgins and Thompson (2002), moderator analyses were also conducted on the optimism subscale—both to be consistent across measures and because a set of potential moderator variables had been identified a priori.

The following moderators were evaluated: year published, study objective, study design, participant status, age, percent of sample that was female, percent of sample that was White, the country of origin for the study, scale used, the number of psychosocial covariates used in the analyses, and total number of covariates included in the analyses. No statistically significant moderator effects emerged for any of the three predictors used.

Publication Bias

Guidelines proposed by Rothstein, Sutton, and Borenstein (2005) were used to examine for the presence and magnitude of publication bias. First, as previously noted, sensitivity analyses were performed to see if obtained effects were dependent on one or two outlying studies. These sensitivity analyses for the primary analyses revealed two studies that, when removed, caused the effect size for the optimism scale to become nonsignificant. The effect sizes for the overall/combined scale and the pessimism subscale were not dependent upon any one study.[2] Next, funnel plots for the overall/combined scale, the pessimism subscale, and the optimism subscale from the primary analyses were inspected for bias (see Figures 1, 2, and 3, respectively in the online supplemental materials). For all the plots, studies with larger standard errors and larger effect sizes were clustered at the bottom of the plot, less so for the optimism subscale.

Rank correlation and regression procedures were also used to evaluate publication bias. Kendall's τ (corrected for continuity) was nonsignificant for the overall/combined scale and each of the two subscales (all ps $> .55$). Egger's regression was significant for the overall scale (Intercept = .47, $SE = .21$, $p < .04$) and for the pessimism subscale (Intercept = .53, $SE = .20$, $p < .02$), but not for the optimism subscale (Intercept = .11, $SE = .13$, $p > .40$). Taken together, these general set of findings suggested that some publication bias did exist.

Given the evidence for publication bias, Duval and Tweedie's (2000) trim-and-fill procedure was used to provide a bias-corrected effect size estimate. Use of this procedure left the primary analyses essentially unchanged. Effects that were significant before correction for bias remained significant after correction. The magnitude of the effects sizes was also similar. More detailed data on publication bias for the overall analyses (as well as the stratified analyses by outcome) can be found in Table 2 in the online supplemental materials.

[1] As Table 1 shows, RVE could only be used for some of the analyses conducted because of constraints on degrees of freedom. As a supplement to these RVE analyses, traditional meta-analytic techniques were also used to replicate the findings produced using RVE. These supplemental analyses yielded largely the same effects as did the RVE method. All primary analysis ES's that were significant using one technique were significant using the other, and the ES's themselves were also quite similar. The biggest difference between the estimated ES's was for the optimism subscale. The RVE method produced a slightly larger ES estimate than did the analysis using one average ES per study (.011 vs. .007, respectively). The subgroup analyses that were conducted were also similar, especially for the two subscales. Convergence of these two methodologies increases the confidence in the results that are reported.

[2] RVE was used to assess sensitivity whenever the empirically calculated degrees of freedom for the analysis was 4 or greater. Traditional meta-analytic methods, using an average outcome per study, were used to assess sensitivity when the degrees of freedom were less than 4. Traditional meta-analytic methods were also used for the remaining publication bias analyses that are reported.

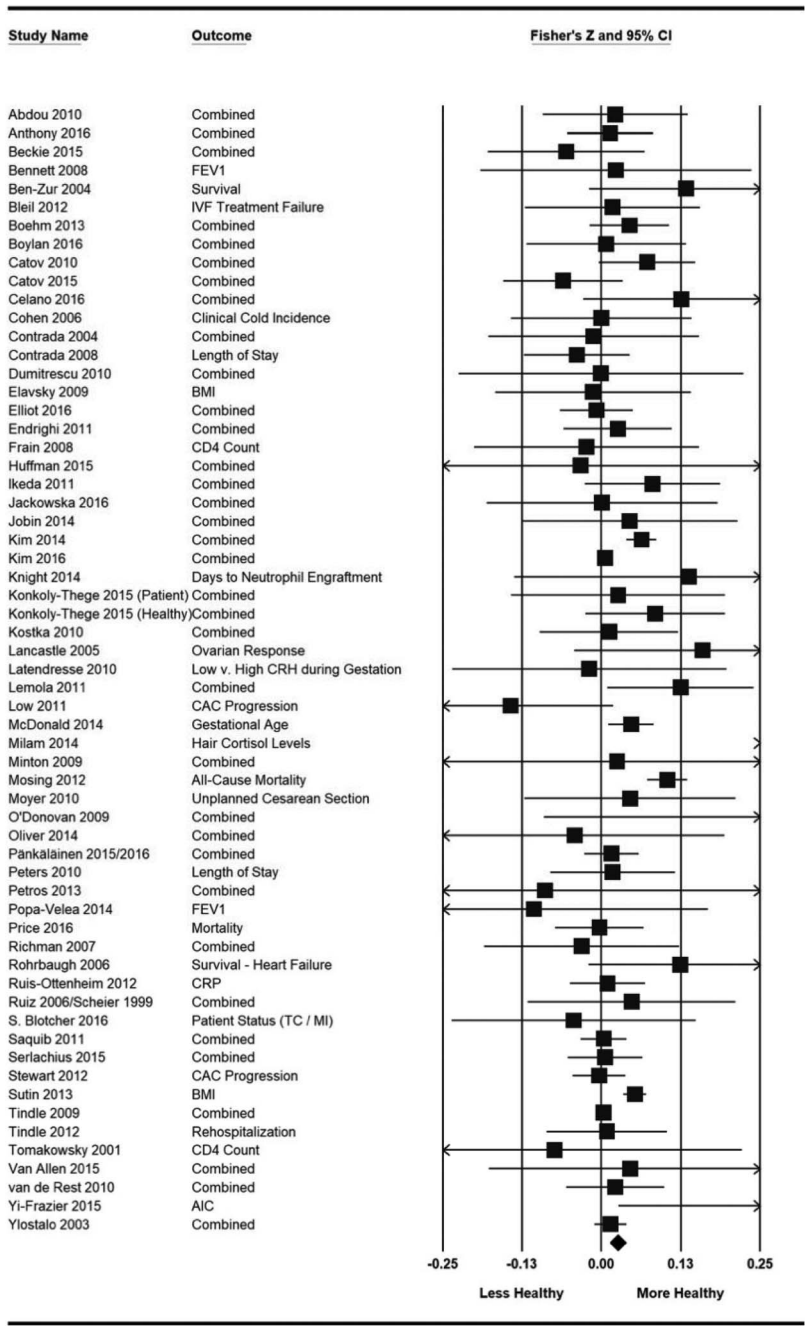

Figure 2. Forest plot of effect sizes associated with the overall/combined scale. Diamond symbol at bottom of forest plot reflects average effect size across studies.

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Discussion

The results of the present reanalyses confirm the findings from earlier quantitative and qualitative reviews. The presence of optimism combined with the absence of pessimism (as assessed by the overall/combined scale) is a reliable predictor of physical health. This was true for an analysis that pooled all of the outcomes together and also true for the majority of analyses that examined subgroups of outcomes separately. This replication of prior findings is noteworthy inasmuch as over 80% of the studies included in the present reanalyses were not included in the previous meta-analysis (Rasmussen et al., 2009).

The novel findings concern the relative strength of optimism and pessimism in contributing to associations with health. Although each was a significant predictor of physical health, the effect sizes associated with the absence of

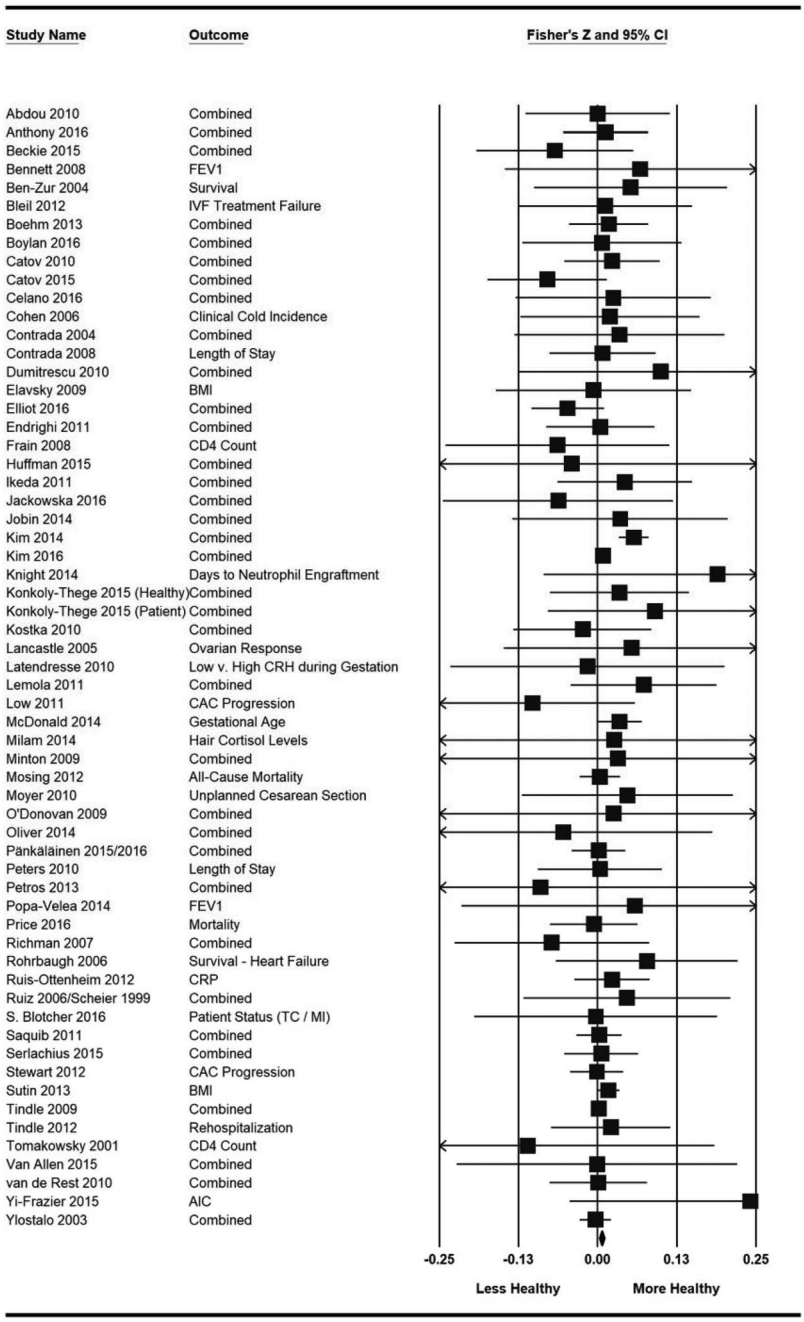

Figure 3. Forest plot of effect sizes associated with the optimism subscale. Diamond symbol at bottom of forest plot reflects average effect size across studies.

pessimism were generally greater in size than those associated with the presence of optimism. The magnitude of these differences was great enough to be significantly different for the analysis aggregating across outcomes, as well as for several of the analyses that investigated subgroups of outcomes separately. Adjustment of the findings for publication bias did little to alter the basic nature of the primary findings.

Moderator analyses were conducted on the effect sizes from the overall/combined scale, as well as the two subscales. These analyses failed to identify any significant moderator. It is of interest that there were no significant differences in effect sizes as a function of the type of study employed. Cross-sectional studies are open to a number of methodological criticisms, most notably the issue of reverse causality. Longitudinal studies examine associations across

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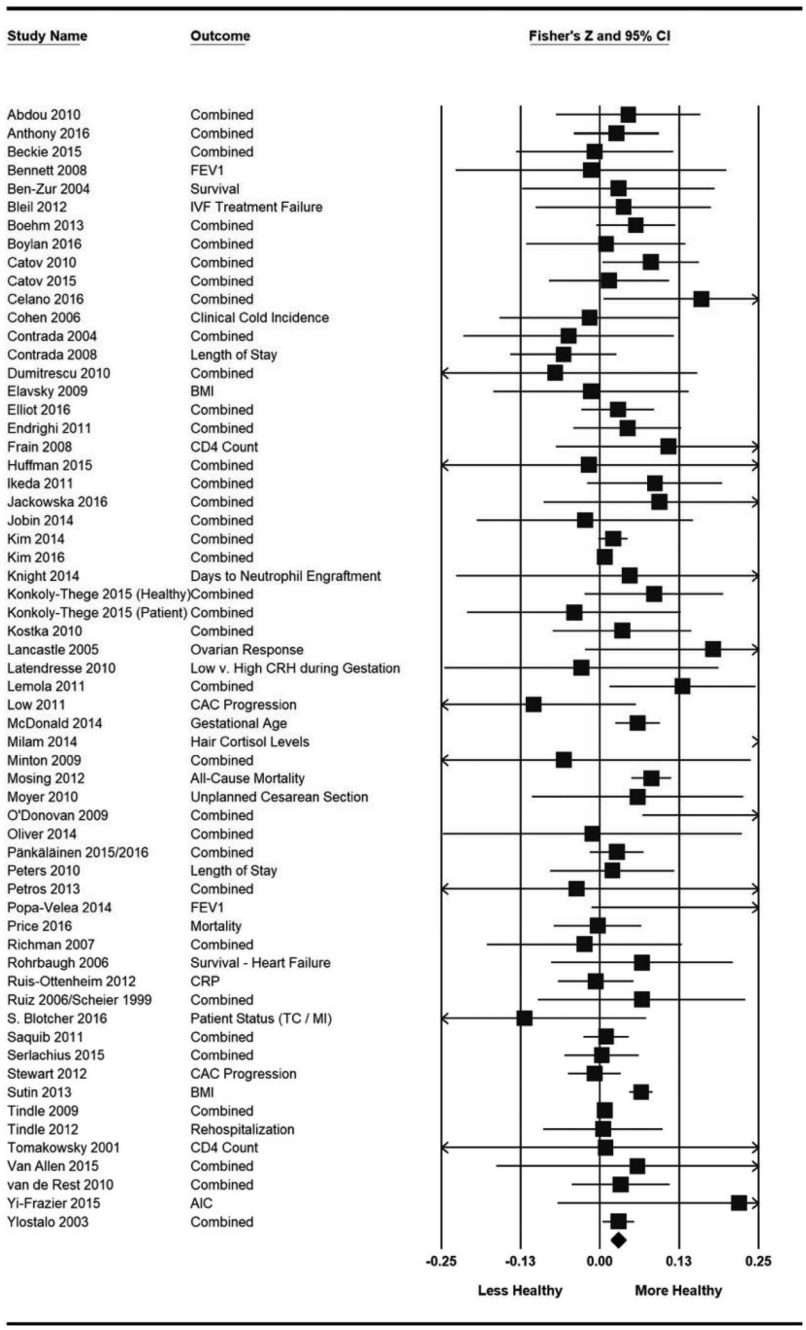

Figure 4. Forest plot of effect sizes associated with the pessimism subscale. Diamond symbol at bottom of forest plot reflects average effect size across studies.

time, but without provisions for equating the health of participants at baseline. As such, longitudinal studies are subject to many of the same criticisms as are cross-sectional studies. Prospective studies provide the gold standard, in that they offer an assessment of the change in the outcome variable over time (or otherwise start with participants who can be assumed to be equivalent in health at baseline). Given these considerations, it is especially striking that the moderator analyses revealed that study design did not significantly impact the magnitude of the effect sizes that were obtained.

The foregoing discussion speaks to the statistical reliability of the effects that emerged. A few words also need to be said about the magnitude of the effects that emerged. The effects sizes reported here appear small. Several considerations should be borne in mind, however, when evaluating

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the effect sizes obtained. First, as just noted, the effect sizes reported are adjusted for a host of factors, including those related to demographics, study design, and other confounding psychosocial factors. Thus, the effect sizes reported are unique to optimism and pessimism. It is not surprising that the effect sizes are somewhat small, especially so inasmuch as shared variance with related psychosocial factors has been removed.

The second point to make is that statistical effects, even small ones, can be quite meaningful when applied to large numbers of people. Take for example, the effect size characterizing the association between the pessimism subscale and mortality. The corresponding adjusted odds ratio for this effect in the present reanalysis is 1.074 (95% CI [1.024, 1.126]). In terms of the number of people who lived and died in the United States in 2016 (the year the most recent study in these reanalyses was published), this odds ratio implies that a one-point change in the pessimism direction of the pessimism subscale corresponds to an increase in 97,914 deaths from all causes (95% CI [32,540, 162,641]).

Finally, it is worth mentioning that the size of the effects obtained using the present meta-analytic techniques are quite comparable to effects reported in other meta-analyses of psychosocial factors and physical health when the studies are put on this same metric (see, e.g., Richardson et al., 2012 for a meta-analysis of perceived stress and incident coronary heart disease and Kivimäki et al., 2012 for a meta-analysis of job strain and coronary heart disease). Taken together, these considerations suggest that from a public health standpoint the magnitude of the effects obtained in the present analysis are nontrivial and quite comparable to other findings in the literature.

The present set of reanalyses has several potential limitations that should be highlighted. First, search terms for the present analysis relied heavily on the framework used by Rasmussen et al. (2009). The scheme used here is only one of many that could be adopted. Different search terms could yield a different corpus of studies, and the findings obtained using those different studies could be somewhat different.

Second, the yield rate for relevant studies was 32%. It is difficult to evaluate this yield rate compared with other meta-analytic studies. This is the case because the data required for the present study could not be extracted from published studies. Rather, the analysis was contingent on authors of those published studies reanalyzing their data and forwarding on the results of those reanalyses. It is likely that this extra requirement lowered the yield rate to some extent.

The third limitation concerns the homogeneous nature of the gender and racial composition of the participants. Although these factors differed somewhat from study to study, over 90% of the overall sample were White and women. Additionally, over 90% of the studies were conducted in the United States. More studies are clearly needed to determine if the effects reported here are replicable in more diverse populations.

Fourth, the conduct of the present research was a group effort. The analyses could not have been done if consortium members had not conducted the needed analyses and forwarded their findings to the primary authors for further meta-analytic processing. On the positive side, the project represents one of the best examples of collaborative science in the truest sense of the term. On the negative side, the more people involved, the more potential there is for error. This concern is mitigated by the fact that the researchers involved had already published peer reviewed papers with these same data, and as such had already demonstrated significant capability with these analyses.

Finally, the outcomes examined in the present study all involved physical health. It is unclear if similar findings would obtain if mental health outcomes were examined. Perhaps optimism and pessimism would be equally robust as predictors of psychological well-being. Perhaps optimism would be stronger. It is important not to extrapolate the findings obtained with the present set of outcomes to possible findings involving other outcomes. Future research on psychological well-being should report results for the optimism and pessimism subscales separately, to evaluate the relative strength of the two dimensions in predicting outcomes in that domain.

There is a more nuanced point to be made here than simply to acknowledge that the differential impact of optimism and pessimism on psychological well-being needs to be explored. That is, stress has been identified as one potentially important factor that might mediate the impact of optimism (and pessimism) on physical health (Scheier & Carver, 2018). How? The idea is that stress (and stress-related emotions) might modulate downstream biological systems that underlie health and disease.

Optimists cope with and psychologically react to adversity in a different way than do pessimists (Segerstrom et al., 2017). It would be interesting to see within this context if the presence or absence of optimism and the presence or absence of pessimism relate differentially to the various emotions that arise in reaction to stressful circumstances. It would further be interesting to see if these potentially different emotions (that characterize the reactions of optimists and pessimists to stress) might themselves be more or less strongly related to physical health outcomes. Answering questions such as these could further in a significant way our understanding of why it might be that the absence of pessimism is more strongly related to physical health outcomes than is the presence of optimism.

Limitations aside, the present findings have at least three implications. First, future research should, as a matter of course, provide effect size information for the overall/combined scale and the two subscales separately—a suggestion that has been made previously (Scheier et al., 1994). Such a

practice is even more important now that quantitative data exist documenting the differential associations of the two subscales with physical health. With the complete complement of effect sizes reported, future research could continue to evaluate the importance of the separate contributions of optimism versus pessimism without the need to establish consortiums.

The present findings also hold important implications for positive psychology (Peterson & Park, 2003; Seligman & Csikszentmihalyi, 2000). Positive psychology emphasizes those characteristics that enable people to experience full, industrious, and resilient lives. As such, it stands in contrast to traditional views that tend to focus on negative attributes, such as depression, anxiety, and other characteristics which undermine successful living. Dispositional optimism is often described as a good example of a variable falling within the positive psychology domain (e.g., Dunn, 2018). As the present data make clear, however, the presence of optimism does not provide the whole story. Optimism is important, but it does not appear to be as important as the absence of pessimism in predicting physical health.

In the future, researchers in positive psychology might benefit from taking these findings into account when planning and conducting research. Researchers should examine more closely the predictor variables they are using to see if negative and positive characteristics might be intermingled in the measures used. If so, an effort should be made to tease apart the positive and negative components of the measures to determine what is in fact responsible for doing the predicting. Ultimately, it may turn out that it is the positive aspects of the measures that are important, but it is also possible that the negative features are the ones driving the observed associations. Only by explicitly evaluating these possibilities will we know for sure.

The final implication concerns interventions. Future efforts to design and adapt interventions to promote better health should keep in mind the differential links between optimism, pessimism, and physical health. In this regard, it is interesting that some cognitive behavior therapies seem to put a greater emphasis on lessening pessimism than they do on promoting optimism. One example of such an intervention concerns cognitive restructuring (Leahy & Rego, 2012), in which participants are trained to challenge the automatic thoughts, beliefs, and expectancies underlying negative feelings. Participants confront their automatic, negative thinking by systematically, and explicitly monitoring their moods and assessing in a more objective fashion the information in the ongoing context that either supports or challenges their negative thoughts. Perhaps existing interventions that focus more on lessening pessimism such as those involving cognitive restructuring will be more successful in promoting better health than will those that place a greater weight on promoting optimism, or even those that place an equal weight on both components. Note that it is not a matter of causing harm, but more a matter of targeting the component that offers the most gain.

It is also possible, however, that things are more complicated. Perhaps what works best will depend on the nature of the outcome of interest (e.g., health behaviors vs. biological pathways). Intervention efforts with respect to optimism, pessimism, and physical health are still in their infancy. As research in the intervention domain continues to evolve, it would seem prudent to keep the distinction between optimism and pessimism in mind. Doing so may prove profitable both practically and theoretically.

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