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Loneliness and Social Isolation as Risk Factors for Mortality: A Meta-Analytic Review

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Abstract
Actual and perceived social isolation are both associated with increased risk for early mortality. In this meta-analytic review, our objective is to establish the overall and relative magnitude of social isolation and loneliness and to examine possible moderators. We conducted a literature search of studies (January 1980 to February 2014) using MEDLINE, CINAHL, PsycINFO, Social Work Abstracts, and Google Scholar. The included studies provided quantitative data on mortality as affected by loneliness, social isolation, or living alone. Across studies in which several possible confounds were statistically controlled for, the weighted average effect sizes were as follows: social isolation odds ratio (OR) = 1.29, loneliness OR = 1.26, and living alone OR = 1.32, corresponding to an average of 29%, 26%, and 32% increased likelihood of mortality, respectively. We found no differences between measures of objective and subjective social isolation. Results remain consistent across gender, length of follow-up, and world region, but initial health status has an influence on the findings. Results also differ across participant age, with social deficits being more predictive of death in samples with an average age younger than 65 years. Overall, the influence of both objective and subjective social isolation on risk for mortality is comparable with well-established risk factors for mortality.

Keywords
social isolation, loneliness, mortality

Several lifestyle and environmental factors are risk factors for early mortality, including smoking, sedentary lifestyle, and air pollution. However, in the scientific literature, much less attention has been given to social factors demonstrated to have equivalent or greater influence on mortality risk (Holt-Lunstad, Smith, & Layton, 2010). Being socially connected is not only influential for psychological and emotional well-being but it also has a significant and positive influence on physical well-being (Uchino, 2006) and overall longevity (Holt-Lunstad et al., 2010; House, Landis, & Umberson, 1988; Shor, Roelfs, & Yogev, 2013). A lack of social connections has also been linked to detrimental health outcomes in previous research. Although the broader protective effect of social relationships is known, in this meta-analytic review, we aim to narrow researchers' understanding of the evidence in support of increased risk associated with social deficits. Specifically, researchers have assumed that the overall effect of social connections reported previously inversely equates with risk associated with social deficits, but it is presently unclear whether the deleterious effects of social deficits outweigh the salubrious effects of social connections. Currently, no meta-analyses focused on social isolation and loneliness exist in which mortality is the outcome. With efforts underway to identify groups at risk and to intervene to reduce that risk, it is important to understand the relative influence of social isolation and loneliness.

Living alone, having few social network ties, and having infrequent social contact are all markers of social isolation. The common thread across these is an objective quantitative approach to establish a dearth of social contact and network size. Whereas social isolation can be an

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objectively quantifiable variable, loneliness is a subjective emotional state. Loneliness is the perception of social isolation, or the subjective experience of being lonely, and thus involves necessarily subjective measurement. Loneliness has also been described as the dissatisfaction with the discrepancy between desired and actual social relationships (Peplau & Perlman, 1982).

Is there a need to distinguish between social isolation and loneliness in assessing mortality risk? People lacking human contact often feel lonely (Yildirim & Kocabiyik, 2010); however, social isolation and loneliness are often not significantly correlated (Coyle & Dugan, 2012; Perissinotto & Covinsky, 2014), suggesting that these may be independent constructs and that one may occur without the other. For instance, some may be socially isolated but content with minimal social contact or actually prefer to be alone; others may have frequent social contact but still feel lonely. Because of the conceptual distinction between social isolation and loneliness, understanding their relative influence on mortality may provide insights into possible independent pathways by which each influences risk and, in turn, guides intervention efforts.

There are several processes by which actual and perceived social isolation may influence mortality risk (also see other reviews in this special section). Social connections, or the lack thereof, can influence health and risk of mortality via direct and indirect pathways (see Uchino, 2006). Both loneliness and social isolation are associated with poorer health behaviors including smoking, physical inactivity, and poorer sleep (Cacioppo et al., 2002; Hawkley, Thisted, & Cacioppo, 2009; Theeke, 2010). Each is also associated with health-relevant biological processes, including higher blood pressure, C-reactive protein, lipid profiles, and poorer immune functioning (Grant, Hamer, & Steptoe, 2009; Hawkley & Cacioppo, 2010; Pressman et al., 2005). Researchers that have included both social isolation and loneliness have linked these factors independently to poorer health behaviors and biological risk factors (Pressman et al., 2005; Shankar, McMunn, Banks, & Steptoe, 2011). However, few researchers have examined these concurrently, and little is known about their relative or synergistic influence.

In this meta-analytic review, our primary aim was to focus on the relative effects of objective and subjective social isolation on mortality (the likelihood of death over a given time), to determine the magnitude and nature of the association with risk of mortality, and to identify potential moderating variables. We reviewed studies of mortality that included measures of loneliness, social isolation, or living alone. Because it is important to determine the effect of social isolation and loneliness independent of correlated lifestyle (e.g., smoking, physical activity) and psychological factors (e.g., depression, anxiety), we also examined inclusion of covariates.

Method

Identification of studies

We identified published and unpublished studies of the association between social relationships and mortality using two techniques. First, we searched for studies appearing from January 1980 to February 2014 using several electronic databases: MEDLINE, CINAHL, PsycINFO, Social Work Abstracts, and Google Scholar. To capture relevant articles, we used multiple search terms, including *mortality, death, decease(d), died, dead*, and *remain(ed) alive*, which were crossed with synonyms of the terms *social isolation, loneliness*, and *living alone*. To minimize inadvertent omissions, we searched each database twice, with searches ending on February 24, 2014. Second, we manually examined the reference sections of past reviews and of studies meeting the inclusion criteria to locate articles not identified in the database searches. A team of research assistants who were trained and supervised by the authors conducted the searches.

Inclusion criteria

We included in the meta-analysis studies written in English that provided quantitative data regarding individuals' mortality as a function of objective and subjective social isolation (operational definitions of social isolation, loneliness, and living alone are provided in Table 1). All studies needed to be prospective in design, meaning that the researchers measured one's objective or subjective social isolation at the study initiation and then followed participants over time (typically several years) to determine who remained alive and who was dead at the follow-up. Thus, risk for mortality is an estimate of the extent to which social isolation, living alone, and loneliness significantly predict the likelihood of being dead at follow-up.

We extracted data when authors used measures including the terms found in Table 1. In some cases, authors operationalized social isolation by contrasting the participants from the bottom quartile or quintile on a social network or integration measure (e.g., Social Network Index; Cohen, Doyle, Skoner, Rabin, & Gwaltney, 1997) but otherwise did not code data from measures of social networks/integration. Because we were interested in the impact of social deficits on disease, we excluded studies in which mortality was a result of suicide or accident. We also excluded studies in which the outcome could not be isolated to mortality (e.g., combined outcomes of morbidity and mortality). Although we excluded single-case designs and reports with exclusively aggregated data (e.g., census-level statistics), we included all other types of quantitative research designs that yielded a statistical estimate of the association between mortality and loneliness/isolation. Figure 1 shows the flow

Table 1. Descriptive Coding of the Measures Used to Assess Objective and Subjective Isolation

Type of measure	Description	Example of measure
Objective		
Social isolation	Pervasive lack of social contact or communication, participation in social activities, or having a confidant	Social Isolation Scale (Greenfield, Rehm, & Rogers, 2002) Social Network Index (bottom quartile; Berkman & Syme, 1979)
Living alone	Living alone versus living with others	Binary item: yes, no Number of people in household
Subjective		
Loneliness	Feelings of isolation, disconnectedness, and not belonging	Loneliness Scale (De Jong-Gierveld & Kamphuis, 1985) UCLA Loneliness Scale (Russell, Peplau, & Cutrona,1980)

Note: UCLA = University of California, Los Angeles.

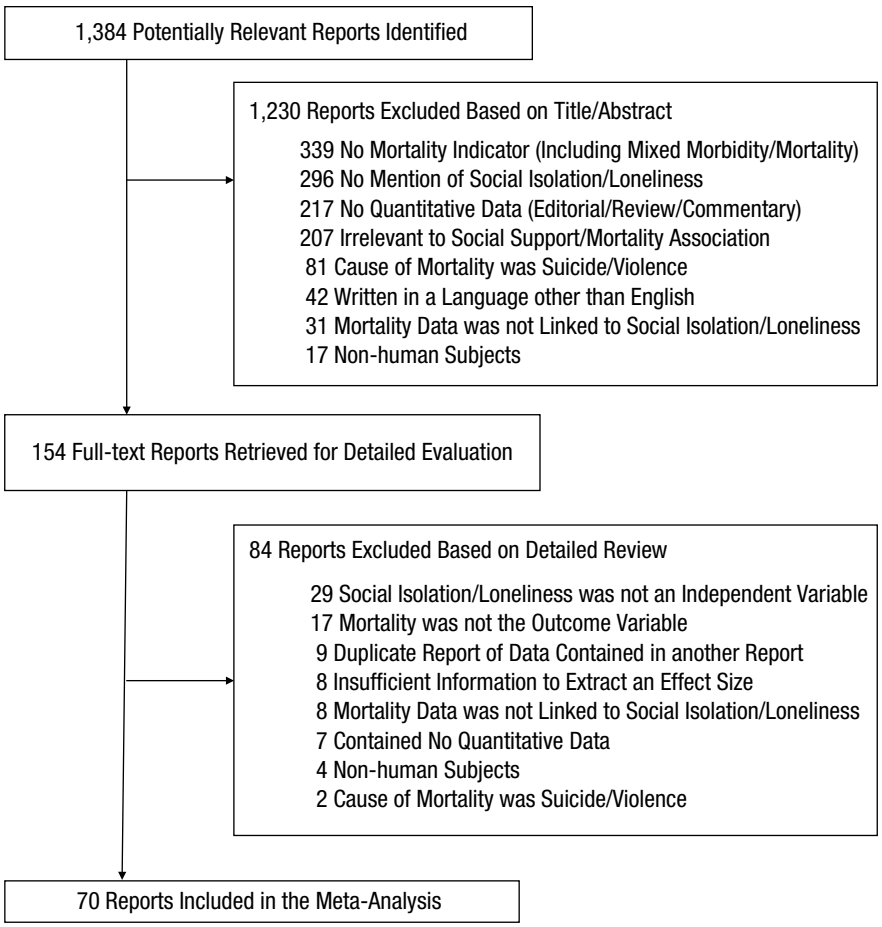

Fig. 1. Reports evaluated for inclusion in the meta-analysis.

diagram containing the details of study inclusion (included in the Supplemental Material available online).

Data abstraction

A team of research assistants and the authors performed the data searches and coding. To reduce the likelihood of human error in coding, a team of two raters coded each article twice. Two different raters performed the second coding of each article. Thus, two distinct coding teams (four raters) coded each article. Coders extracted several objectively verifiable characteristics of the studies: (a) the number of participants and their composition by age, gender, health status, and preexisting health

conditions (if any), as well as the cause of mortality; (b) length of follow-up; (c) research design; (d) type of social isolation (actual/perceived) evaluated; (e) number and class of covariates included in the statistical model; and (f) exclusion of participants who were severely ill or who died shortly after study initiation. The latter two variables helped to address possible confounds (e.g., depression, health status, physical mobility, age) and reverse causality, whereby individuals with impaired health would be more likely to report increased social isolation or loneliness because of an inability to engage in social contact.

For each study, we extracted the reported effect size, making sure that odds ratio (OR) values greater than one represented an increase in mortality as a function of social isolation, loneliness, or living alone—and a decrease in mortality when individuals were not isolated, lonely, or living alone. Effect sizes less than one indicated the opposite. To analyze the data, we temporarily transformed the reported effect sizes to the natural log of the OR and subsequently transformed them back to ORs for purposes of interpretation.

When researchers reported multiple effect sizes within a study at the same point in time, we averaged the several values (weighted by standard error) to avoid violating the assumption of independent samples. We therefore used the *shifting units of analysis* approach (Cooper, 1998), which minimizes the threat of nonindependence in the data while allowing for more detailed follow-up analyses. In a few cases in which researchers reported multiple effect sizes across different levels of social isolation (high vs. medium, medium vs. low), we extracted only the value with the greatest contrast (high vs. low). When a study contained multiple effect sizes across time, we extracted the data from the longest follow-up period. We extracted both unadjusted data and the data from the model involving the greatest number of statistical controls (although we also extracted the data from the model utilizing the fewest number of statistical controls for a subsequent comparison after recording the type and number of statistical controls used within both models).

Overall, the interrater agreement for data abstraction was adequately high for categorical variables (with Cohen's kappa averaging .73) and for continuous variables (with intraclass correlations for single measures averaging .95). We resolved discrepancies across coding teams through further scrutiny of the article until we obtained consensus.

Results

Description of the retrieved literature

We located 79 articles reporting pertinent data, 9 of which were excluded because they contained the same data as another article, resulting in 70 independent studies that met the full inclusion criteria. The complete list of references and a table summarizing the characteristics of those studies (Table S1) are found in the Supplemental Material available online. Studies typically involved older adults, with a mean age of 66.0 years at initial data collection and with a mean length of follow-up being 7.1 years. Most studies (63%) involved normal community samples, but 37% of studies involved patients with a medical condition, such as heart disease. See Table 2 for further descriptive data.

Three studies included data on both loneliness and one of the objective independent variables: two for loneliness and social isolation, and one for loneliness and living alone. Using a shifting units of analysis approach (Cooper, 1998), we included data from those distinct measures in the analyses specific to the type of measurement, but all other studies contributed a single data point to the analyses.

Effect sizes in the 70 studies had been calculated by researchers using a variety of methods, with some researchers reporting unadjusted values and with other researchers using a variety of covariates. ORs ranged from 0.64 to 3.85, with exceptionally high heterogeneity across studies (I^2 = 97.8%, 95% CI [97.6%, 98.1%]; Q = 3,328.9, p < .0001), suggesting excessive variability in findings across all types of data. We therefore divided the analyses according to the number of covariates used. In the *unadjusted data* group, the researchers controlled for no other variables in the analyses. In the *partially adjusted data* group, the researchers typically controlled for one or two variables, usually age and gender. The *fully adjusted data* are the model within studies with the largest number of covariates. Effect sizes from each category were evaluated separately, such that a single study could contribute effect sizes to more than one category (see Table 3).

Overall, each of the measures (social isolation, loneliness, and living alone) for each type of data (unadjusted, partially adjusted, or fully adjusted) had an OR between 1.26 and 1.83. The three measures did not differ in their ORs for any of the three types of data, meaning that there was no overall difference among the two objective and one subjective factors. (Random-effects weighted analyses of variance across the measures yielded all ps > .20.)

However, the type of data did matter in the analysis. Unadjusted data yielded effect sizes of greater magnitude than fully adjusted data (see Table 3). The differences between unadjusted and fully adjusted data also reached statistical significance (p < .001) when comparing data within 27 studies in which researchers reported more than one statistical model (e.g., unadjusted compared with fully adjusted values) using multivariate meta-analytic methods after accounting for the .74 correlation of

Table 2. Characteristics of 70 Studies of the Association of Mortality With Subjective and Objective Measures of Social Isolation

Characteristic	*M*	Number of studies (*k*)	%
Year of initial data collection	1,993	46	
Years of participant follow-up	7	70	
% deceased by the end of data collection	24.7	66	
% female	52.6	67	
% smokers	31.2	28	
Sample size	48,673		
<200		6	9
200–499		7	10
500–999		10	14
1,000–2,999		20	29
3,000–9,999		16	23
>10,000		11	16
Age of participants[a]	66.0		
<50 years		8	11
50–59 years		12	17
60–69 years		11	16
70–79 years		21	30
>80 years		10	14
Location of data collection			
Inpatient medical treatment setting		15	21
Outpatient medical treatment setting		11	16
Community setting (normal populations)		44	63
World region of data collection			
Europe		38	54
North America		19	27
Asia		7	10
Australia		3	4
Multiple regions		3	4

Note: Not all variables sum to the total number of studies because of missing data.
[a]Average age category of participants at study initiation, although not all participants within the study would necessarily be in the category listed.

Table 3. Weighted Mean Effect Sizes (Odds Ratios) by Type of Measurement

Measure	*k*	OR_+	*SE*	95% CI
Unadjusted data				
Social isolation	3	1.83	0.185	[1.27, 2.63]
Living alone	20	1.51	0.072	[1.32, 1.74]
Loneliness	8	1.49	0.105	[1.22, 1.84]
Overall	31	1.53	0.035	[1.38, 1.70]
Partially adjusted data[a]				
Social isolation	6	1.46	0.162	[1.06, 2.00]
Living alone	8	1.55	0.132	[1.20, 2.00]
Loneliness	7	1.52	0.213	[0.99, 2.30]
Overall	21	1.51	0.117	[1.27, 1.79]
Fully adjusted data[b]				
Social isolation	14	1.29	0.100	[1.06, 1.56]
Living alone	25	1.32	0.075	[1.14, 1.53]
Loneliness	13	1.26	0.099	[1.04, 1.53]
Overall	52	1.30	0.116	[1.16, 1.46]

Note: k = number of studies; OR_+ = random-effects weighted odds ratio; CI = confidence interval.
[a]Typically one or two covariates, most often age and gender. [b]Data from the statistical model in studies that contained the most covariates; these adjusted data yielded effect sizes that were statistically significantly ($p < .05$) smaller than unadjusted data.

effect sizes within studies. Thus, unadjusted and fully adjusted data not only represented conceptually distinct classes of data but also yielded findings of different magnitude.

Moderator analyses

Given the substantial heterogeneity of the overall results ($I^2 > 80\%$), we analyzed the extent to which the variability in effect sizes could be attributable to study or participant characteristics. These analyses involved only the fully adjusted data because multiple factors predictive of mortality had been controlled (thus minimizing possible confounding explanations). Study and participant characteristics included both categorical and continuous data, so we report those analyses separately.

Categorical variables. We examined categorical variables using random-effects weighted analyses of variance, beginning with the type of covariates used in the fully adjusted models. Eight studies included multiple covariates that were directly relevant to social support, such as marital status, social networks, and loneliness. These eight studies had lower averaged effect sizes (OR = 1.17) than those of 33 studies in which no covariates directly relevant to social support were included in the statistical model (OR = 1.27). Otherwise, the averaged effect sizes remained of similar magnitude irrespective of the particular covariates that were or were not included in the models ($p > .20$), including covariates relevant to depression, socioeconomic status, health status, physical activity, smoking, gender, and age. Different combinations of covariates across studies yielded similar results.

We found no substantive differences in effect sizes ($p > .15$) across the other categorical variables evaluated: world region, data collection setting, cause of mortality, research design, health status, and medical condition at intake. Finding no significant differences across participant health status when using the fully adjusted data was particularly notable because of a difference that we observed with the unadjusted data: Studies in which participants had a medical condition and were recruited from a medical setting had larger unadjusted average effect sizes (OR = 1.82) than studies with ostensibly healthy participants recruited from the general community (OR = 1.34, $p = .003$). Furthermore, with the unadjusted data, studies in which the researchers excluded participants with terminal conditions or participants who died shortly after baseline data collection (whose social isolation or social support may have been affected by their medical condition) had higher averaged effect sizes (OR = 1.95) than the studies in which the researchers did not report such exclusions (OR = 1.38, $p < .05$). Thus, accounting for participants' initial health condition in the research design resulted in systematically different findings across studies. In most (81%) of the multivariate statistical models, researchers had controlled for participant health status variables, such that we found no differences across those conditions in the fully adjusted data. Studies in which the researchers controlled for health status variables yielded substantially different findings from those studies in which this was not done.

Continuous variables. We examined study and participant characteristics involving continuous data in relation to the observed effect sizes using random-effects weighted regression coefficients (meta-regression). We observed no coefficients greater than the absolute value of .20 between effect sizes and the year of initial data collection, the length of follow-up, or the percentage of female participants in each study. However, the number of covariates included in multivariate models was moderately associated with effect size ($r = -.27$). Visual inspection of the corresponding scatter plot indicated that when studies included seven or more covariates, effect sizes tended to be more homogeneous, without extremely high values. To clarify, the inclusion of many covariates did not substantively reduce the magnitude of the general findings, which tended to remain in the range of OR = 1.20–1.40, but it did eliminate all OR values greater than 1.66.

Analyses also indicated that the association between the effect size and the average age of participants at intake was of a moderately strong magnitude ($r = -.34$ for adjusted data, and $r = -.46$ for unadjusted data). This association with participant age remained of the same magnitude when accounting for length of study follow-up (and participants' age at the end of the study) and when age was or was not used as a statistical covariate. Examination of the scatter plot and breaking down the data into three approximately equal categories of initial participant age helped to interpret the correlation: Studies involving participants of an average age less than 65 years had an average effect size of OR = 1.57 for adjusted data, and OR = 1.92 for unadjusted data; studies involving participants of an average age between 65 and 75 years had an average effect size of OR = 1.25 for adjusted data, and OR = 1.32 for unadjusted data; and studies involving participants of an average age greater than 75 years had an average effect size of OR = 1.14 for adjusted data, and OR = 1.28 for unadjusted data. Adults less than 65 years of age appeared to be at greater risk of mortality when they lived alone or were lonely compared with older individuals in those same conditions, even after controlling for the effect of age and other covariates on mortality.

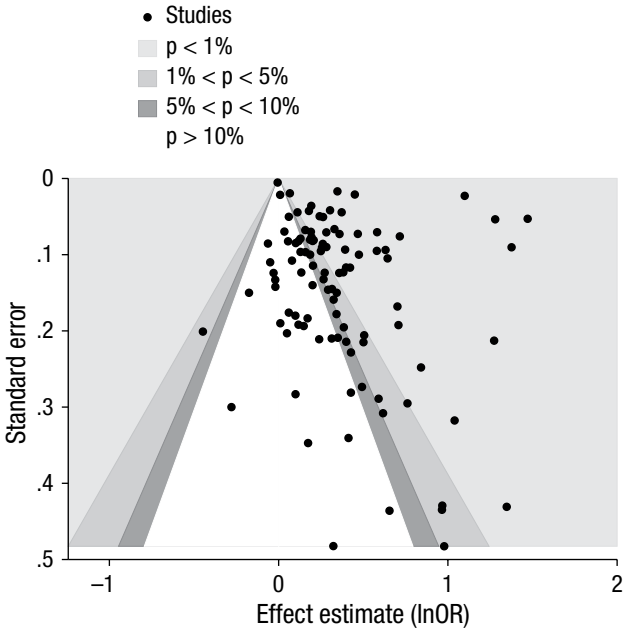

Fig. 2. Contour-enhanced funnel plot of effect sizes (natural log of the odd ratio [lnOR]) by standard error for 70 studies with measures of the association between mortality and social isolation, loneliness, or living alone.

Likelihood of publication bias adversely influencing the results

Publication bias occurs when the data obtained in a meta-analysis fail to represent the entire population of studies because of the increased probability of nonsignificant results remaining unpublished (and therefore less accessible for meta-analytic reviews). As can be seen in Figure 2, the data in this meta-analysis were highly variable, and the distribution of effect sizes appeared somewhat imbalanced toward the right side of the graph. The distribution of the data was relatively sparse toward the bottom of the white-shaded center of the graph, the area of nonsignificance. This kind of distribution can suggest that some nonsignificant studies were missing from the meta-analysis. However, neither Egger's regression test nor an alternative to that test recommended for OR data (Peters, Sutton, Jones, Abrams, & Rushton, 2006) reached statistical significance ($p > .05$), which diminished the likelihood of possible publication bias. We found the fail-safe *N*—the number of hypothetically missing studies needed to reduce the present results to zero—to be 1,268, a number higher than the plausible number of studies conducted. Furthermore, using the trim and fill method (Duval & Tweedie, 2000), we did not estimate any "missing" studies; the distribution was overall fairly symmetric relative to the average effect size. It thus seemed unlikely that publication bias substantively affected the results of this meta-analysis.

Discussion

Social isolation results in higher likelihood of mortality, whether measured objectively or subjectively. Cumulative data from 70 independent prospective studies, with 3,407,134 participants followed for an average of 7 years, revealed a significant effect of social isolation, loneliness, and living alone on odds of mortality. After accounting for multiple covariates, the increased likelihood of death was 26% for reported loneliness, 29% for social isolation, and 32% for living alone. These data indicated essentially no difference between objective and subjective measures of social isolation when predicting mortality.

The prospective designs of these studies and the statistical models that controlled for initial health status (and several other potential confounds) provide evidence for the directionality of the effect. Although we cannot confirm causality, the data show that individuals who were socially isolated, lonely, or living alone at study initiation were more likely to be deceased at the follow-up, regardless of participants' age or socioeconomic status, length of the follow-up, and type of covariates accounted for in the adjusted models.

We caution scholars perusing the expanding research literature on the association of social isolation and loneliness with physical health against reliance on unadjusted data because those data fail to account for participant health status, a factor contributing to reverse causality (when individuals with impaired health report increased loneliness or social isolation because their health condition limits their social contacts). Averaged results with unadjusted data were of greater magnitude than the results from fully adjusted models (see Table 3), particularly when participants had a preexisting health condition and when physically ill participants were not excluded from the unadjusted analyses. In fully adjusted models accounting for health status and in studies with physically ill individuals removed from analyses (thus accounting for reverse causality), social isolation and loneliness remained predictive of mortality. Future researchers will need to confirm the hypothesis that when individuals are ill (and ostensibly needing support) their risk for mortality increases substantially when lacking social support.

Overall, the findings from this meta-analysis are consistent with prior evidence that has demonstrated higher survival rates for those who are more socially connected (Holt-Lunstad et al., 2010) and extend those findings by focusing specifically on measurement approaches that assess the relative absence of social connections. Notably, the present meta-analysis included more than double the number of studies and 10 times the number of participants compared with the previous meta-analysis. Thus, the field now has much stronger evidence that lacking social connections is detrimental to physical health.

The average effect sizes identified in this meta-analysis were lower than those reported previously for measures of social networks (OR = 1.45, 95% CI [1.32, 1.59]) and social integration (OR = 1.52, 95% CI [1.36, 1.69]) and were much lower than complex measures of social integration (OR = 1.91, 95% CI [1.63, 2.23]; see Table 4 of Holt-Lunstad et al., 2010). This difference may suggest that the salubrious effects of being socially connected may be stronger than the adverse effects of lacking connections. However, it is also likely that research methods that account for the multidimensionality of social relationships better predict mortality than measurement focused on any single aspect of sociality, such as social isolation. Nonetheless, identification of the relative effects of each component may be useful in targeting those that may be modifiable.

There is also presently no research evidence to suggest a threshold effect. The aggregate results suggest more of a continuum than a threshold at which risk becomes pronounced. Although it is possible that individuals who are extremely lonely or socially isolated may account for much of the elevated risk, presently too few researchers target extremely isolated individuals in studies. Given the complexity (including objective and subjective aspects) of social relationships, identifying such a threshold seems unlikely.

Objective versus subjective isolation

Using the meta-analytic data, had we found that either social isolation or loneliness was more predictive of mortality, interventions to reduce risk could have become more targeted. However, we presently have no evidence to suggest that one involves more risk than the other for mortality. Unfortunately, in the vast majority of studies, researchers examined only one measurement approach (social isolation, loneliness, or living alone), precluding direct comparisons. Among the few studies in which researchers contrasted social isolation and loneliness, the evidence was mixed, with researchers finding that loneliness was more influential in one study (Holwerda et al., 2012), and with other researchers finding that social isolation had stronger effects than loneliness in a later study (Steptoe, Shankar, Demakakos, & Wardle, 2013). This inconsistency may be due to differences in methodological approaches to handling correlated psychological states, such as depression (Booth, 2000). Our analyses indicated that the elevated risk of mortality persisted even when controlling for correlated components of social networks and multiple other factors, including depression, with the use of covariates negating large effect sizes. In any case, the multiple, overlapping components of sociality make reliance on statistical adjustment less desirable than direct comparisons between components, such as loneliness and social isolation.

The equivalent effects of social isolation and loneliness reported here do not indicate interchangeability of these risk assessments. Rather, the available data suggest that efforts to mitigate risk should consider both social isolation and loneliness without the exclusion of the other. Because social isolation and loneliness are often weakly correlated (Coyle & Dugan, 2012), simply increasing social contact may not mitigate loneliness. Likewise, exclusively altering one's subjective perceptions among those who remain objectively socially isolated may not mitigate risk. The evolutionary perspective of loneliness proposed by Cacioppo and colleagues (Cacioppo et al., 2006; Cacioppo, Cacioppo, & Boomsma, 2014) presents loneliness as an adaptive signal, similar to hunger and thirst, that motivates one to alter behavior in a way that will increase survival. Accordingly, loneliness is a powerful motivator to reconnect socially, which, in turn, increases survival and opportunity to pass on genes. Consistent with this perspective, intervention attempts to alter the signal (e.g., hunger, loneliness) without regard to the actual behavior (e.g., eating, social connection) and vice versa would likely be ineffective. Extending this possibility, some data have shown that those who are both high in loneliness and social isolation had the poorest immune response (Pressman et al., 2005). Therefore, both objective and subjective measures of social isolation should be considered in risk assessment.

It is only through direct comparisons of social isolation and loneliness in the same sample that researchers can establish independent, relative, and synergistic effects. Consequently, it is possible that different combinations of social isolation and loneliness may represent different levels of risk. For instance, those low in both isolation and loneliness would presumably be at lowest risk, those high in both at highest risk, and those who are isolated but not lonely or lonely but not isolated to be at intermediate risk. Nonetheless, there is currently insufficient empirical evidence to test this hypothesis, highlighting an important weakness of the current literature that needs to be addressed in future research.

Isolation and aging

The data in this meta-analysis should make researchers call into question the assumption that social isolation among older adults places them at greater risk compared with social isolation among younger adults. Using the aggregate data, we found the opposite to be the case. Middle-age adults were at greater risk of mortality when lonely or living alone than when older adults experienced those same circumstances.

The moderating effect of age may seem counterintuitive in light of data indicating that individuals more than 65 years of age are more likely to report loneliness (Dykstra, van Tilburg, & de Jong Gierveld, 2005), but there are at least four plausible explanations for why middle-age adults may differ from older adults in terms of the relevance of social networks to physical health. First, it is possible that individuals who do not die early may be a particularly resilient group, with different social or health characteristics than those who die at earlier ages. Thus, the observed difference across age could be confounded with preexisting health status, although this interpretation is qualified by the fact that the researchers using multivariate statistical models accounted for participant age and health status. A second explanation involves changes in social networks as individuals transition from full-time employment to retirement, with decreases in socialization in occupational and public forums that are seen as culturally normative. This possible explanation is supported by one study in which researchers examined loneliness after retirement and found an effect for mental health (anxiety and depression) but not for physical health (functional status and number of chronic conditions; Bekhet & Zauszniewski, 2012). Third, it is plausible that individuals who are alone or lonely before retirement age may be more likely to engage in risky health behaviors or less likely to seek medical treatment early, whereas after retirement, people may attend more assiduously to their physical health. Finally, it is possible that the different results across participant age are confounded with marital status: Older adults are much more likely to be widows/widowers than middle-age adults. Our meta-analysis cannot shed light on these four possible explanations because the first three explanations involve variables inadequately evaluated in the present research literature, and the variable associated with the fourth explanation, marital status, was not coded in our analyses. Although many studies indicate that loneliness differs across marital status (Cacioppo & Patrick, 2008; Hughes et al., 2004; Victor & Bowling, 2012) and that marital status is significantly associated with mortality (Roelfs, Shor, Kalish, & Yogev, 2011), we did not include marital status as an indicator of social isolation because being unmarried does not necessarily mean that one is socially isolated, living alone, or lonely. Moreover, there would be multiple qualitative differences in the social networks of an older individual who had never been married compared with one who had been married and raised children but whose spouse had recently died, even though both are living alone. Rather than include all possibly correlated variables (e.g., marital status, depression, substance abuse), we evaluated only direct measures of social isolation, living alone, or loneliness. Given the limitations of the present meta-analysis, future researchers should confirm the apparent differences across participant age and should evaluate the relative merits of the several plausible explanations for that finding.

To better evaluate differences across age, future researchers should involve participants from a broad range of age groups. Most of the data in this meta-analysis came from older adults. Only 24% of studies involved people with an average age of 59 years or younger, and only 9% of studies involved people younger than 50 years of age at intake. If future data collection with younger adult samples confirms the age differences we observed in this meta-analysis, then widespread beliefs about the health risks of social isolation being greatest among older adults are inaccurate. In any case, the meta-analytic data, taken together with evidence for detrimental influences across the life span (Qualter et al., 2015, this issue), suggest that future research (and possibly interventions) should expand beyond older adults.

Conclusion

Substantial evidence now indicates that individuals lacking social connections (both objective and subjective social isolation) are at risk for premature mortality. The risk associated with social isolation and loneliness is comparable with well-established risk factors for mortality, including those identified by the U.S. Department of Health and Human Services (physical activity, obesity, substance abuse, responsible sexual behavior, mental health, injury and violence, environmental quality, immunization, and access to health care; see www.hhs.gov/safety/index). A substantial body of research has also elucidated the psychological, behavioral, and biological pathways by which social isolation and loneliness lead to poorer health and decreased longevity (for reviews, see Cacioppo, Cacioppo, Capitanio, & Cole, 2015, this issue; Shankar et al., 2011; Thoits, 2011; see also Cacioppo et al., 2015; Hawkley & Cacioppo, 2003, 2010). In light of mounting evidence that social isolation and loneliness are increasing in society (McPherson & Smith-Lovin, 2006; Perissinotto, Stijacic Cenzer, & Covinsky, 2012; Victor & Yang, 2012; Wilson & Moulton, 2010), it seems prudent to add social isolation and loneliness to lists of public health concerns. The professional literature and public health initiatives can accord social isolation and loneliness greater recognition.

To draw a parallel, several decades ago scientists who observed widespread dietary and behavior changes (increasing consumption of processed and calorie-rich foods and increasingly sedentary lifestyles) raised warnings about obesity and related health problems (e.g., Brewster & Jacobson, 1978; Dietz & Gortmaker, 1985). The present obesity epidemic (Wang & Beydoun, 2007) had been predicted. Obesity now receives constant coverage in the media and in public health policy and

initiatives. The current status of research on the risks of loneliness and social isolation is similar to that of research on obesity 3 decades ago—although further research on causal pathways is needed, researchers now know both the level of risk and the social trends suggestive of even greater risk in the future. Current evidence indicates that heightened risk for mortality from a lack of social relationships is greater than that from obesity (Flegal, Kit, Orpana, & Graubard, 2013; Holt-Lunstad et al., 2010), with the risk from social isolation and loneliness (controlling for multiple other factors) being equivalent to the risk associated with Grades 2 and 3 obesity. Affluent nations have the highest rates of individuals living alone since census data collection began and also likely have the highest rates in human history, with those rates projected to increase (e.g., Euromonitor International, 2014). In a recent report, researchers have predicted that loneliness will reach epidemic proportions by 2030 unless action is taken (Linehan et al., 2014). Although living alone can offer conveniences and advantages for an individual (Klinenberg, 2012), this meta-analysis indicates that physical health is not among them, particularly for adults younger than 65 years of age. Further research is needed to address the complexities of social interactions, interdependence, and isolation (Parigi & Henson, 2014; Perissinotto & Covinsky, 2014), but current evidence certainly justifies raising a warning.

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Supplemental Material

Additional supporting information may be found at http://pps.sagepub.com/content/by/supplemental-data

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