A Meta-Analysis of Interventions to Reduce Loneliness

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Abstract

Social and demographic trends are placing an increasing number of adults at risk for loneliness, an established risk factor for physical and mental illness. The growing costs of loneliness have led to a number of loneliness reduction interventions. Qualitative reviews have identified four primary intervention strategies: (a) improving social skills, (b) enhancing social support, (c) increasing opportunities for social contact, and (d) addressing maladaptive social cognition. An integrative meta-analysis of loneliness reduction interventions was conducted to quantify the effects of each strategy and to examine the potential role of moderator variables. Results revealed that single-group pre-post and nonrandomized comparison studies yielded larger mean effect sizes relative to randomized comparison studies. Among studies that used the latter design, the most successful interventions addressed maladaptive social cognition. This is consistent with current theories regarding loneliness and its etiology. Theoretical and methodological issues associated with designing new loneliness reduction interventions are discussed.

The formation of meaningful social connections is an integral part of human nature (Baumeister & Leary, 1995; Cacioppo & Patrick, 2008). Some individuals have difficulty forming meaningful social connections, whereas others form such social connections but lose them through separation, widowhood, or other vagaries of life. Individuals without meaningful social connections are at risk for loneliness, an aversive experience that all humans experience at one time or another. Although the health consequences of persistent loneliness are on par with those of many psychiatric illnesses, our understanding of the origins and treatment of loneliness is still limited (O'Luanaigh & Lawlor, 2008). To properly treat loneliness, a better understanding of the nature and mechanisms underlying loneliness is needed. Therefore, the goals of this article are to review the definitions, prevalence, health effects, and current theories regarding loneliness, to describe the relationship between these theories and previous studies of loneliness reduction strategies, and to use meta-analytic techniques to quantify the loneliness-reducing effects of studies that meet our analysis criteria.

Definitions

Loneliness is typically defined as the discrepancy between a person's desired and actual social relationships (Russell, Peplau, & Cutrona, 1980). Although sometimes considered synonymous with social isolation, loneliness and social isolation are related but distinct concepts. The latter reflects an objective measure of social interactions and relationships, whereas loneliness reflects perceived social isolation or outcast. Accordingly, loneliness is more closely associated with the quality than the number of relationships (Peplau & Perlman, 1982; Wheeler, Reis, & Nezlek, 1983). The importance of relationship quality takes origin in the fundamentally social nature of the human species. Both phylogenetically and ontogenetically, humans require not simply the presence of others but also the presence of others who value them, whom they can trust, and with whom they can communicate, plan, and work together to survive, prosper, and care for their offspring sufficiently long that they too reproduce (Cacioppo & Patrick, 2008). As a result, an individual may be lonely in a crowd or socially contented while alone.

Loneliness was traditionally thought to be a gnawing sensation or chronic distress without redeeming features (Weiss, 1973), but more recently loneliness has been conceptualized as a biological construct, a state that has evolved as a signal to change behavior—very much like hunger, thirst, or

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physical pain—that serves to help one avoid damage and promote the transmission of genes to the gene pool (Cacioppo, Hawkley, et al., 2006). That is, loneliness has been posited to be an aversive signal that motivates us to become sensitive to potential social threats and to renew the connections needed to survive and prosper. Like hunger, thirst, and pain, loneliness is typically mild and transient because it contributes to the maintenance or repair of meaningful social connections—as occurs when a child is reunited with his or her parent following separation or when a spouse returns home following a trip. When meaningful social connections are perceived as severed or unavailable, however, loneliness can produce deleterious effects on cognition and behavior (Cacioppo & Hawkley, 2005) that, in turn, increase the likelihood that loneliness becomes chronic (Cacioppo & Hawkley, 2009; Young, 1982). Interventions to reduce loneliness have been developed because the chronic form of loneliness is highly aversive (Peplau & Perlman, 1982; Weiss, 1973), is a significant risk factor for mental and physical health problems (Danese et al., 2009; Hawkley & Cacioppo, 2007), and adversely affects others around them (Berscheid & Reis, 1998; Cacioppo, Fowler, & Christakis, 2009).

Weiss (1973) distinguished between emotional and social loneliness on theoretical grounds. Various factor analytic studies have provided some evidence that the experience of loneliness can be partitioned into separable dimensions (Hawkley, Browne, & Cacioppo, 2005; Knight, Chisholm, Nigel, & Godfrey, 1988; McWhirter, 1990a), but these factors have also been found to be highly correlated, and their antecedents and consequences have been found to be sufficiently overlapping that loneliness is generally conceptualized and measured as a unidimensional construct (Hawkley et al., 2005; Russell, 1996; Russell et al., 1980).

Prevalence

Research reveals a significant prevalence of loneliness among both children and adults. In a study of kindergarteners and first graders, 12% reported feeling lonely at school (Cassidy & Asher, 1992). Among third- through sixth-grade children, 8.4% scored in the lonely range using the Asher et al. Loneliness Scale (Asher, Hymel, & Renshaw, 1984; Asher & Wheeler, 1985). Among middle-aged and older adults, from 5% to 7% report feeling intense or persistent loneliness (Steffick, 2000; Victor, Scambler, Bowling, & Bondt, 2005) and up to 32% of adults older than age 55 report feeling lonely at any given time (De Jong Gierveld & van Tilburg, 1999). According to the 2002 Health and Retirement Survey, 19.3% of U.S. adults older than age 65 reported feeling lonely for much of the previous week (Theeke, 2009). Several factors suggest the prevalence of loneliness could increase in the coming decades. One is the aging of the U.S. population. In 1900, 4.1% of Americans were 65 years or older. By 2006, that percentage had increased to 12.4%, representing 37.3 million Americans (Administration on Aging, 2008). Older age is associated with disability-related obstacles to social interaction as well as with longer periods of time living as widows or widowers. Moreover, delayed marriage (Goldstein & Kenney, 2001), increased dual-career families (Schneider & Waite, 2005), increased single-residence households (U.S. Bureau of Labor Statistics, 2003), and reduced fertility rates (Taylor et al., 2010) may also contribute to an increased prevalence of loneliness and its associated health effects.

Health Effects

The associations between loneliness and physical and mental health indicate that loneliness influences virtually every aspect of life in our social species. For example, loneliness not only involves painful feelings of isolation, disconnectedness from others, and not belonging (Hawkley et al., 2005) but also is a risk factor for myriad health conditions, including increased vascular resistance in young adults (Cacioppo, Hawkley, Crawford, et al., 2002; Hawkley, Burleson, Berntson, & Cacioppo, 2003), elevated systolic blood pressure in older adults (Cacioppo, Hawkley, Crawford, et al., 2002; Hawkley, Masi, Berry, & Cacioppo, 2006; Hawkley, Thisted, Masi, & Cacioppo, 2010), less restorative sleep (Cacioppo, Hawkley, Berntson, et al., 2002; Hawkley, Preacher, & Cacioppo, 2010), increased hypothalamic pituitary adrenocortical activity (Adam, Hawkley, Kudielka, & Cacioppo, 2006), diminished immunity (Kiecolt-Glaser et al., 1984; Pressman et al., 2005), underexpression of genes bearing anti-inflammatory glucocorticoid response elements (Cole et al., 2007), and abnormal ratios of circulating white blood cells (e.g., neutrophils, lymphocytes, and monocytes; Cole, 2008). In addition, longitudinal analysis reveals that adults who were socially isolated as children are more likely to have risk factors for cardiovascular disease, including overweight, high blood pressure, high total cholesterol, low high-density lipoprotein cholesterol, high glycated hemoglobin, and low maximum oxygen consumption (Caspi, Harrington, Moffitt, Milne, & Poulton, 2006) as well as elevated high sensitivity C-reactive protein (Danese et al., 2009).

Compared to nonlonely individuals, lonely people are also more likely to suffer from cognitive decline (Tilvis et al., 2004) and progression of Alzheimer's disease (R. S. Wilson et al., 2007). Animal studies are beginning to shed light on the mechanism by which these effects may occur. Among mice, social isolation reduces central anti-inflammatory responses and increases infarct size following induction of stroke (Karelina et al., 2009). In addition, socially isolated animals demonstrate less dendritic arborization in the hippocampus and prefrontal cortex (Silva-Gomez, Rojas, Juarez, & Flores, 2003) as well as decreased production of brain-derived neurotropic factors (Barrientos et al., 2003). Although

it is unknown whether similar effects occur in humans, experimental manipulation that leads people to believe they face a future of social isolation has been shown to impair executive functioning. Compared to controls, the "future alone" group performed similarly on a rote memorization task but consumed more delicious but unhealthy foods (Baumeister, DeWall, Ciarocco, & Twenge, 2005) and were more aggressive toward others (Twenge, Baumeister, Tice, & Stucke, 2001). Therefore, perceived future isolation did not reduce routine mental ability but rather impaired higher order executive functioning related to food consumption and social interaction.

Loneliness impairs executive functioning in part because it triggers implicit hypervigilance for social threats (Cacioppo & Hawkley, 2009). Heightened sensitivity to social threats results in biases in attention and cognition toward negative aspects of the social context. These social cognitions subtly influence behaviors, social interactions, and affect in a confirmatory fashion that exacerbates feelings of sadness and loneliness. Maladaptive social cognitions have consequences for mental health and well-being. Loneliness has been shown to predict depressive symptoms (Cacioppo, Hawkley, & Thisted, in press; Cacioppo, Hughes, Waite, Hawkley, & Thisted, 2006) and suicidal ideation and behavior (Rudatsikira, Muula, Siziya, & Twa-Twa, 2007). The impact of loneliness on such diverse aspects of physical and mental health provides justification for interventions to mitigate this experience.

Theories of Loneliness

As described above, loneliness can be a fleeting, unpleasant mood for some individuals or a persistent, aversive experience for others. Most people are capable of feeling loneliness acutely, but some are unable to escape the grip of loneliness. Research indicates that loneliness is approximately 50% heritable and 50% environmental (Boomsma, Willemsen, Dolan, Hawkley, & Cacioppo, 2005; McGuire & Clifford, 2000). For a species to survive, not only must one generation procreate, but the offspring of that generation must procreate as well. Human offspring have the longest period of dependency of any species and rely on their parents to feed and protect them for many years. During hunter–gatherer times, survival of children to reproductive age would have depended on parents sharing food and resources with their children even if at cost to themselves. Parents who felt no "pangs" of loneliness when parted from their children would have been less likely to maintain nurturing and protective parental connections compared to parents who experienced distress when separated from the family and tribe. Thus, although loneliness is unpleasant for the individual, it may be essential for species survival (Cacioppo, Hawkley, et al., 2006). Because infant attachment is not predictive of adult attachment and adult attachment can change, childhood attachment appears not to be a major determinant of loneliness in most adults (Cacioppo & Patrick, 2008; Shaver, Furman, & Buhrmester, 1985).

Of course, having a gene or genes for loneliness does not mean an individual will be lonely. What appears to be inherited is the level of distress aroused by social disconnection. For individuals of all ages, loneliness may arise upon moving to a new city, losing a friend, or losing a loved one. Analysis of data from a population-based, racially diverse sample of men and women aged 50 through 68 revealed several factors were positively associated with loneliness. These included number of physical symptoms, chronic stress from employment, and chronic stress from social life and recreation. Factors negatively associated with loneliness included social network size, satisfaction with social network, and having a spousal confidant (Hawkley et al., 2008). These results suggest that the success of interventions to reduce loneliness may hinge on the degree to which one's social environment and social interactions are improved.

Research over the past several decades has shaped our understanding of the nature of loneliness. Early studies focused on individual differences between lonely and nonlonely people. This research demonstrated that compared to the nonlonely, lonely individuals approach social encounters with greater cynicism and interpersonal mistrust (Brennan & Auslander, 1979; Jones, Freemon, & Goswick, 1981; Moore & Sermat, 1974), rate others and themselves more negatively, and are more likely to expect others to reject them (Jones, 1982). In addition, lonely people have lower feelings of self-worth (Peplau, Miceli, & Morasch, 1982), tend to blame themselves for social failures (Anderson, Horowitz, & French, 1983), are more self-consciousness in social situations (Cheek & Busch, 1981), and adopt behaviors that increase, rather than decrease, their likelihood of rejection (Horowitz, 1983). This "individual differences" model of loneliness has influenced loneliness reduction interventions to date. Specifically, these interventions have attempted to correct deficits in social skills, social support, opportunities for social interaction, and/or maladaptive social cognition.

More recent research suggests that loneliness is not an immutable trait but rather can be exacerbated or ameliorated by social interactions. In an illustrative study, hypnosis was successfully used to induce participants to feel high and low levels of loneliness (Cacioppo, Hawkley, et al., 2006). Increasing feelings of loneliness also increased feelings of shyness, anxiety, and anger and decreased feelings of social skills, optimism, self-esteem, and social support, suggesting that loneliness is syndrome-like in carrying with it a range of attributions, expectations, and perceptions that reinforce feelings of loneliness (Cacioppo, Hawkley, et al., 2006). Conversely, these findings suggest that interventions that enhance a feeling of social connectedness can alter self- and other-perceptions along dimensions that have the potential to improve the quality of social interactions and relationships and keep loneliness at bay.

To examine the role of the social context in loneliness, investigators studied loneliness in the Framingham Heart Study (Cacioppo, Fowler, et al., 2009). Using social network analysis and self-reported data from more than 6,000 participants between 1983 and 2001, the authors identified several unique phenomena. Specifically, they found that lonely people tend to be linked to other people who are lonely, an effect that is stronger for geographically proximal friends but extends to three degrees of separation. In addition, nonlonely individuals who are around lonely individuals tend to grow lonelier over time. This suggests that loneliness can be induced and operates not unlike a biological contagion. Finally, analysis revealed that lonely individuals were consistently moved to the periphery of social networks, as if they had been metaphorically pushed there by others in the network. From an evolutionary perspective, such marginalization may protect the structural integrity of the network. These findings also go beyond the individual differences model of loneliness and demonstrate not only the power of social networks but also the ability of people who become lonely to have a negative effect on nonlonely people.

A mechanism for the contagion of loneliness may lie in the reciprocal effects of social interaction quality and affect. In an experience sampling study, 134 undergraduates were queried regarding their psychosocial and behavioral states at nine random times during the day on seven consecutive days (Hawkley, Preacher, & Cacioppo, 2007). Information regarding the positivity or negativity of their affect and their interactions (if they were interacting with someone at the time their programmable watched beeped) was collected via diary entries. Of primary interest was the ability of loneliness to predict variability in affect and interaction quality and their interrelationship. Using multilevel modeling, the authors found that loneliness was associated with decreased positivity and increased negativity in affect and interaction quality across all measurement occasions. In longitudinal analysis, positive and negative interaction quality predicted subsequent positive and negative affect, and in a reciprocal causal fashion, positive and negative affect predicted subsequent interaction quality. Moreover, the influence of interaction negativity on negative affect persisted over a longer duration than the influence of interaction positivity on positive affect. In addition, negative affect influenced subsequent interaction positivity and negativity, whereas positive affect influenced only subsequent interaction positivity. Finally, loneliness was characterized by greater negative affect and more negative interactions. Together, this pattern of results suggests that lonely individuals not only communicate negativity to others but also elicit it from others and transmit it through others. This perpetuates a cycle of negative interactions and affect in the lonely individual and also transmits negativity to others to affect their interactions as well. These results may explain the mechanism by which lonely individuals increase feelings of loneliness among those with whom they interact. The authors concluded that interventions that reduce perceptions of negativity in interactions or affect have the potential to break the cycle of negativity that people experience when lonely.

Taken together, these studies suggest that when individuals feel lonely, they think and act differently than when they do not feel lonely. Accordingly, their perceptions of the social environment, their social cognitions, and their interpersonal actions have all been targeted in interventions to reduce loneliness.

Previous Reviews of Loneliness Interventions

Since 1984, six articles have reviewed the literature regarding strategies to reduce loneliness, social isolation, or both. Of these reviews, all are qualitative, rather than quantitative, and most explicitly or implicitly discuss four primary strategies of loneliness reduction interventions: (a) improving social skills, (b) enhancing social support, (c) increasing opportunities for social interaction, and (d) addressing maladaptive social cognition. Because the number of friends or social interactions is not as predictive of loneliness as the quality of their relationships, increasing opportunities for social interaction and enhancing social support may address social isolation more than loneliness. In contrast, improving social skills and addressing maladaptive social cognition focus on quality of social interaction and therefore address loneliness more directly. All of the reviews identified both successful and unsuccessful loneliness reduction strategies, and five of the six reviews concluded that loneliness can be mitigated with specific interventions. However, all of the reviews concluded that questions remain regarding the efficacy of interventions and that more rigorous research is needed in this area.

The earliest review cited more than 40 loneliness reduction interventions dating back to the 1930s (Rook, 1984). Most of these interventions fell into the four categories described above. Depending on the study, interventions to improve social skills emphasized one or several of the following: conversational skills, speaking on the telephone, giving and receiving compliments, handling periods of silence, enhancing physical attractiveness, nonverbal communication methods, and approaches to physical intimacy. In one study, a social skills intervention among lonely college students was associated with decreased loneliness, self-consciousness, and shyness compared to two control groups (Jones, Hobbs, & Hockenbury, 1982). Among interventions that enhanced social support, professionally initiated interventions for the bereaved (Vachon, Lyall, Rogers, Freedman-Letofsky, & Freeman, 1980), for the elderly whose personal networks had been disrupted by relocation (Kowalski, 1981), and for children whose parents had divorced (Wallerstein & Kelly, 1977) all demonstrated

loneliness reductions. Increasing opportunities for social interaction also reduced loneliness in some studies. An example is a blood pressure evaluation program conducted in the lobbies of single-room occupancy hotels that housed older individuals. Although the residents tended to stay in their rooms because of physical disability and fear of crime, the program increased social interaction in the lobbies and, over time, helped participants identify shared interests (Pilisuk & Minkler, 1980). Another example involved isolated seniors working together to collect and distribute food for the needy. As the study progressed, the seniors formed informal support networks (Pilisuk & Minkler, 1980). Finally, programs that focused on maladaptive social cognition through cognitive behavioral therapy (CBT) appeared somewhat successful in reducing loneliness (Young, 1982). The cornerstone of this intervention was to teach lonely individuals to identify automatic negative thoughts and regard them as hypotheses to be tested rather than facts. Rook (1984) acknowledged that many of the studies in her review were not successful or lacked experimental rigor but indicated that interventions that focused on social skills, social support, opportunities for social interaction, and social cognition held promise for reducing loneliness.

A 1990 review also identified social skills training, opportunities for social interaction, and CBT as potentially effective in reducing loneliness (McWhirter, 1990b). The author noted that although social skills training was initially developed to reduce anxiety and shyness, it has been successfully adapted to treat loneliness (Twentyman & Zimering, 1979). Other programs have achieved success by providing individuals with opportunities to find others with common goals and by arranging activities of interest for small groups of lonely individuals (Cutrona & Peplau, 1979). McWhirter (1990b) referred to several CBT-based studies that succeeded in reducing loneliness (Anderson & Arnoult, 1985; Anderson et al., 1983; Young, 1982). Some studies even showed that combining CBT with social skills training was more effective in treating lonely and socially anxious adults than either treatment alone (Glass, Gottman, & Shmurak, 1976; Rook & Peplau, 1982).

A third review examined 21 interventions designed to reduce loneliness among older individuals (Cattan & White, 1998). Although references to the specific interventions were not provided, the authors grouped them into four categories: (a) group activities, (b) one-to-one interventions, (c) service delivery, and (d) whole-community approaches. Taking design quality into consideration, the authors concluded that the most effective interventions included group activities, self-help, or bereavement support, targeted specific groups (e.g., women and widowers), used more than one intervention strategy, had an evaluation that coincided with the intervention, and gave participants some level of control. The lone study that evaluated a community approach was deemed inconclusive because of poor study design.

A subsequent review identified 17 loneliness reduction interventions published between 1982 and 2002 (Findlay, 2003). This report used a classification scheme similar to that of Cattan and White (1998; e.g., group interventions, one-to-one interventions, service provision, and Internet usage). Although this typology does not perfectly match that of Rook (1984) or McWhirter (1990b), most of the studies addressed social skills, social support, opportunities for social interaction, or social cognition. For example, the one-to-one interventions included telephone-based and gatekeeper programs designed to enhance social interaction and social support, respectively. Similarly, the group interventions included teleconferencing, support groups, and friendship enrichment training, which were also designed to improve social interaction and social skills. The service provision interventions focused on social support, whereas the Internet programs represented an approach to increasing opportunities for social interaction. Although some of the programs in this review showed benefit, Findlay (2003) noted that many were flawed by weak study design. For example, only 6 of the 17 studies were randomized controlled trials. As a result, this review concluded there was little evidence to support the notion that interventions can reduce loneliness among older people.

Cattan, White, Bond, and Learmouth (2005) conducted a qualitative review of studies published between 1970 and 2002 and found 30 articles that evaluated loneliness prevention interventions among older adults. In this review, the authors used their previous typology (e.g., group activities, one-to-one counseling, service provision, and community development). These categories were further refined to include group activities with an educational component; group interventions to provide social support; home visits to provide assessment, information, or social services; home visits or telephone contact to provide directed support or problem solving; and one-on-one interventions to provide social support. As in previous reviews, these interventions addressed social skills, social support, opportunities for social interaction, and social cognition. Because only 16 of the 30 studies were randomized controlled trials, Cattan et al. also highlighted the dearth of methodological rigor among loneliness reduction interventions. Nonetheless, of the 13 studies considered to be of high quality, 6 were considered effective, 1 was considered partially effective, 5 were considered ineffective, and 1 was considered inconclusive. Consistent with their previous review, Cattan et al. (2005) concluded that the most effective programs were group interventions that included an educational component or a targeted activity, targeted specific groups (e.g., women, caregivers, the widowed, the physically inactive, or people with serious mental health problems), tested a representative sample of the intended target group, and enabled some level of participant and/or facilitator control.

The final review examined 36 studies and focused on persons with severe mental illness, a population whose

prevalence of loneliness is approximately twice that of the general population (Perese & Wolf, 2005). Interventions to reduce loneliness in this group were similar to those developed for the general population, including social skills training, enhanced social support, increased opportunities for social interactions, and cognitive behavioral training. Support groups were noted to be the primary method for social skills training in this population. In one study, this approach was associated with a decline in unmet needs for friends (Perese, Getty, & Wooldridge, as cited in Perese & Wolf, 2005). In contrast, mutual help groups represented the primary strategy for enhancing social support among those with mental illness. Although few studies have evaluated this approach, one study found mutual help groups reduced psychiatric symptoms, hospitalizations, and social isolation among the mentally ill (Galanter, 1988).

According to Perese and Wolf (2005), one way to increase opportunities for social interaction is befriending, which "aims to develop a relationship between individuals that is distinct from professional/client relationships" (A. D. Cox, 1993, p. 9). Originally developed to reduce loneliness, its goals have grown to include improving quality of life, reducing social isolation, helping people meet emotional needs, and promoting and maintaining mental health (Andrews, Gavin, Begley, & Brodie, 2003). Although befriending appears to reduce *social isolation*, studies to date have not assessed the effect of befriending on *loneliness* among individuals with mental illness or the general population. Finally, deficits in social cognition were addressed through self-help groups, which attempted to change thinking from negative and fearful to positive and self-supportive (Murray, 1996). The self-help groups in this review focused on problems brought up by members and on coping techniques taught by professional group leaders. The review noted that little research has assessed the efficacy of this approach. However, one study found that family members who attended self-help groups reported improvements in their relationships with mentally ill family members (T. Heller, Roccoforte, Hsieh, Cook, & Pickett, 1997).

In summary, six previous qualitative reviews of loneliness reduction studies identified both successful and unsuccessful interventions. Five of the reviews concluded loneliness could be reduced with certain interventions, but one concluded there was little evidence that current techniques can reduce loneliness, especially among lonely elders (Findlay, 2003). In three of the reviews, interventions were explicitly classified as addressing social skills, social support, opportunities for social interaction, or impairments in social cognition (McWhirter, 1990b; Perese & Wolf, 2005; Rook, 1984). In the other three reviews, this classification was implicit, although not all reviews included studies that addressed impaired social cognition (Cattan et al., 2005; Cattan & White, 1998; Findlay, 2003). All of the reviews noted a dearth of randomized controlled trials, and all called for increased rigor in evaluating loneliness reduction interventions.

Purpose of the Meta-Analysis

The goal of this meta-analysis is to provide the rigor called for by previous reviews and quantify the efficacy of the primary intervention strategies. Although previous reviews suggested that certain interventions can reduce loneliness, the results were mixed and a significant number of interventions were not associated with loneliness reduction. It may be that the success of certain interventions was more because of study design than the quality of the intervention. For example, pre-post studies, nonrandomized group comparison studies, and randomized group comparison studies are nonequivalent designs in terms of comparing effect sizes (Lipsey & Wilson, 2001). Using meta-analysis, mean effect sizes can be compared across study designs and within groups of studies of the same design. Within study design, heterogeneity of effect sizes can be assessed and, when evident, examined to determine whether efficacy varies as a function of intervention format (group based vs. individual based), intervention mode (technology based vs. non–technology based), the type of loneliness measure used, the frequency and duration of the intervention, and the age and sex of the study participants. Each of these variables has the potential to influence intervention efficacy, and the studies we reviewed provided data regarding these characteristics. We did not evaluate marital status as a potential moderator because very few studies provided data on this variable.

Interventions to date have relied on an "individual differences" model, in which the lonely were considered to have deficits in social skills, social support, opportunities for social interaction, and/or social cognition. Given recent insights regarding the centrality of social cognition to loneliness (Cacioppo, Fowler, et al., 2009; Cacioppo, Hawkley, et al., 2006; Hawkley et al., 2007), we hypothesized that interventions that address maladaptive social cognition will have a greater impact than those that address social skills, social support, or opportunities for social interaction.

Method

Selection of Studies Included in the Meta-Analysis. Applying recently published guidelines for meta-analysis (APA Publications and Communications Board Working Group on Journal Article Reporting Standards, 2008), the literature review identified trials that specifically targeted loneliness among adults, adolescents, and/or children. PubMed and PsycINFO were searched for relevant studies using combinations of the following keywords: *loneliness*, *intervention*, *treatment*, *prospective*, *medication*, and *pharmacology*. Eligible studies had to be published from 1970 through September 2009, in English and in a peer-reviewed journal or doctoral dissertation, had to be designed as an intervention specifically to lower loneliness, and had to measure loneliness quantitatively.

The initial search produced a total of 818 references in Pubmed and 777 references in PsycINFO, with significant duplication in references between the sources. As shown in Figure 1, the abstracts of 928 unique references were reviewed, and 772 were excluded for lack of relevance based on the abstract. The remaining 156 studies were reviewed in detail. Of these, 12 studies were excluded because they were descriptive reviews that did not assess loneliness interventions either qualitatively or quantitatively. However, 2 additional studies were identified in these reviews. This resulted in 146 studies that were further evaluated. Of these, 78 did not meet our initial inclusion criteria. A request for relevant studies posted on the listserver for the Society for Personality and Social Psychology (spsp-announce-l@list.cornell.edu) failed to generate any additional eligible studies. Email requests to individual authors in North America and Europe known to conduct research on loneliness elicited only one positive response. T. Fokkema indicated that an article had been published in 2007, in the Dutch language, that reported the results of 18 loneliness interventions conducted among older adults in the Netherlands (C. M. Fokkema & van Tilburg, 2007). The authors forwarded an English version of this paper), and nine of the studies described met our initial inclusion criteria. Adding these studies to the others that met our initial criteria yielded 77 studies, which were then evaluated to determine whether they met established meta-analytic criteria.

Meta-Analytic Criteria. The *first* criterion for inclusion in the meta-analysis was that the intervention had to directly target loneliness. Seven studies were excluded because the interventions were directed at stress relief (Whitehouse et al., 1996), anxiety and/or depression (Mynatt, Wicks, & Bolden, 2008; Ransom et al., 2008), or health behaviors (de Craen, Gussekloo, Blauw, Willems, & Westendorp, 2006; Hedberg, Wikstrom-Frison, & Janlert, 1998; Hopman-Rock & Westhoff, 2002; Soholt Lupton, Fonnebo, Sogaard, & Fylkesnes, 2005). One study (Hu, 2009) examined the effect of an intervention on an induced state of loneliness and was excluded from the analysis because induced loneliness is not comparable to the loneliness targeted in other included studies. In addition, the Wish Fulfillment Study (C. M. Fokkema & van Tilburg, 2007) was excluded for a lack of adequate information regarding the nature of the intervention. The *second* criterion was that the intervention effect had to be measured and reported quantitatively to enable the calculation of effect size. Although 12 studies originally failed to meet this criterion (Andersson, 1985; Brown, Allen, Dwozan, Mercer, & Warren, 2004; Clarke, Clarke, & Jagger, 1992; Evans & Jaureguy, 1982; Evans, Smith, Werkhoven, Fox, & Pritzl, 1986; Jones et al., 1982; McLarnon & Kaloupek, 1988; Routasalo, Tilvis, Kautiainen, & Pitkala, 2009; Seepersad, 2005; Stewart, Reutter, Letourneau, & Makawarimba, 2009; van Kordelaar, Stevens, & Pleiter,
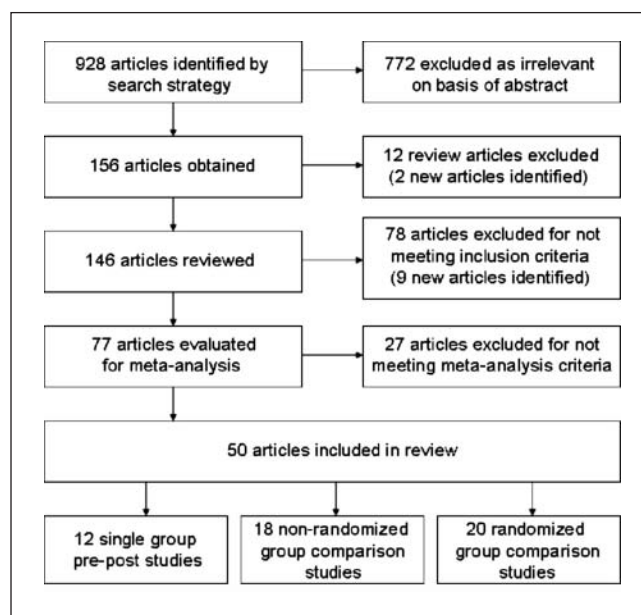
Figure 1. Identification of eligible studies for meta-analysis

2004; van Rossum et al., 1993), attempts to recover quantitative data from the authors were successful in two cases (Evans et al., 1986; Seepersad, 2005). The *third* criterion was that each study had to report original data not reported in another article to avoid inflating effect sizes. Two studies were excluded based on this criterion. One study (Stevens, Martina, & Westerhof, 2006) was excluded because it duplicated data and because more complete results were reported in Martina and Stevens (2006), which was already included as an eligible study. Similarly, the other study (Add LUSTRE to Your Life, in C. M. Fokkema & van Tilburg, 2007) was excluded because more detailed data of the same intervention were reported in Kremers, Steverink, Albersnagel, and Slaets (2006), which was already included. The *fourth* criterion was that the intervention had to involve a treatment group, not individual cases. On this basis, one study was excluded because the study focused on only two participants (Guevremont, MacMillan, Shawchuck, & Hansen, 1989). A total of 50 studies ultimately qualified for meta-analysis.

Because the effect size obtained from a single-group pre-post study has a different meaning than the effect size calculated as the difference between two separate groups (Lipsey & Wilson, 2001), and because the effect size from a nonrandomized group comparison often provides a less satisfactory estimate of the true effect size than a randomized group comparison study, the studies were categorized based on research design and a meta-analysis was conducted within each research design type. Of the 50 interventions, 12 were single-group pre-post studies, 18 were nonrandomized group comparison studies, and 20 were randomized group comparison studies.

Coded Variables. Key characteristics of the included studies are provided, by design type, in Tables 1 to 3. These tables provide effect sizes and information employed in moderator analyses, including mean age of the sample (as reported,[1] or as inferred when means were not reported),[2] gender composition (percentage females, as reported or calculated),[3] intervention duration (in weeks, available for all but four studies),[4] intervention frequency (which was converted to total number of sessions for analysis purposes, and was calculable for all but 14 studies),[5] type of loneliness measure (e.g., UCLA Loneliness Scale, De Jong Gierveld Loneliness Scale, other),[6] intervention format and mode (e.g., individual or group based and non–technology based or technology based, respectively), and intervention type (social skills training, enhanced social support, increased opportunity for social interaction, or social cognitive training). Intervention format was categorized as individual based if the intervention was implemented on a one-on-one basis and as group based if more than one person participated in the intervention at the same time or if the intervention involved asynchronous interactions such as Internet-based chat room exchanges. Intervention mode was classified as technology based if a telephone or computer was used to facilitate the intervention. Intervention type was categorized (a) as social skills training if the intervention focused on improving participants' interpersonal communication skills, (b) as enhancing social support if the intervention offered regular contacts, care, or companionship, (c) as social access if the intervention increased opportunities for participants to engage in social interaction (e.g., online chat room or social activities), and (d) as social cognitive training if the intervention focused on changing participants' social cognition. Importantly, intervention type was not confounded with study design: Each intervention type was represented in each study design group (with the one exception that pre-post studies did not include a social skills intervention).

Effect Size Calculation. Established procedures were used to calculate the effect size for each of the qualified studies (Lipsey & Wilson, 2001). The standard error of each effect size was calculated to derive the inverse variance that served as our weighting unit for the mean effect size across studies. For a better depiction of the relative weight given to each study, the percentage of weight was calculated by dividing each individual weight by the sum of weights from each group of studies.

For single-group pre-post studies, effect sizes were calculated by taking the difference between pre- and posttreatment loneliness scores and dividing by the pooled standard deviation of the two scores. Correlations between pre- and posttreatment loneliness values were required to calculate standard errors of the pre-post effect sizes using the formula,

$$SE = \sqrt{\frac{2(1-r)}{n} + \frac{ES^2}{2n}},$$

where SE = the standard error of the effect size, r = the correlation between pretreatment and posttreatment loneliness values, n = the sample size, and ES = effect size. With two exceptions (Christian & D'Auria, 2006; E. O. Cox, Green, Hobart, Jang, & Seo, 2007), these correlations were not provided by study authors. These correlations were estimated to be .7, which approximates the test–retest reliability for loneliness over periods of a year or more and is consistent with test–retest correlations reported in the literature (Cacioppo, Hughes, et al., 2006; Russell, 1996).

For randomized and nonrandomized group comparison studies, effect sizes were calculated as the loneliness difference between the treatment and control groups divided by the pooled standard deviation of the two scores. Standard errors of the effect sizes were calculated by multiplying the pooled standard deviation with the square root of the sum of the inverse of each sample size.

If a study did not provide enough information regarding the means and standard deviations of the posttreatment loneliness scores but provided χ^2, F, or t test results on the difference between the treatment and control group after the intervention, an online effect size calculator was accessed to determine the effect sizes from those test results (D. B. Wilson, 2002).[7]

When the authors reported the effect sizes but not other statistics for their intervention (Banks & Banks, 2002; Savelkoul, de Witte, Candel, Van Der Tempel, & Van Den Borne, 2001), those effect sizes were used.[8] If the author reported subscale loneliness scores separately (McWhirter & Horan, 1996; Stewart, Craig, MacPherson, & Alexander, 2001), effect sizes were calculated for all subscales, and their mean was reported as the effect size for the given study.

Effect sizes based on posttreatment group differences and their pooled standard deviations are known as Cohen's g, which is said to be upwardly biased, especially for small samples (Hedges & Olkin, 1985). To adjust for this bias, g was multiplied by a correction term of $[1 - 3 / (4N - 9)]$, where N equals the sample size, to get an unbiased estimator known as Hedges' d (Hedges & Olkin, 1985), and this adjusted effect size was used for our analyses.

Studies were evaluated for baseline differences in loneliness between the treatment and control groups, especially studies with nonrandomized group comparison designs. Four of the studies reported baseline differences in loneliness between the treatment and control groups (Cohen et al., 2006; Hartke & King, 2003; Martina & Stevens, 2006; White et al., 1999). To avoid misleading effect sizes that would result from comparing only the posttreatment scores, the effect size was calculated as the difference between the changes of the treatment and the control groups. In addition, in one study (Kolko, Loar, & Sturnick, 1990) baseline differences in loneliness were not reported but were determined to be present because confidence intervals around treatment and control group loneliness means at baseline did not overlap.

Table 1. Single-Group Pre-Post Studies (*n* = 12)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Battles & Wiener, 2002	Seriously ill children visiting the National Institutes of Health (NIH) for treatment of chronic medical conditions. Participants recruited from NIH playrooms by a recreation therapist, principal investigator, or research assistant. *N* = 32.	Social access: Virtual environment designed to provide an interactive online community in which children played games, learned about their medical condition, or talked with other chronically ill children. Participants completed four 1-hr sessions over a period of 6 to 9 months.	−0.43	−0.72, −0.14	Individual	Tech	8–19 yrs; *M* = 14	47	8-item UCLA Loneliness Scale
Bauminger, 2007	Children who attended schools in middle-class, large urban areas and who had a prior clinical diagnosis of either high functioning autism or Asperger's syndrome. Schools and children were recruited through the Special Education Department in the Israeli Ministry of Education. *N* = 19.	Social cognitive training: Cognitive-behavioral-ecological program conducted in each child's school, implemented by the child's main teacher, and involved one typically developing older peer and the child's parents. Each child completed a cognitive-behavioral educational program and met with his or her assigned peer twice weekly, and the parents completed social tasks with their children over a 7-month period.	0.12	−0.23, 0.47	Individual	Nontech	7–11 yrs; *M* = 9	5	16-item Asher Loneliness Scale
Collins & Benedict, 2006	Using newsletters and promotional flyers, participants were recruited from senior centers and senior housing developments in Las Vegas and rural Clark County Virginia. *N* = 339.	Social support: Small-group class led by paraprofessionals, volunteer peer educators, and on-site staff. Topics included nutrition and food, personal safety, reducing accidents in the home, financial strategies to manage limited resources, general wellness, and productive aging. Class met weekly for 4 months.	−0.45	−0.54, −0.36	Group	Nontech	52–93 yrs; *M* = 73	80	4-item UCLA Loneliness Scale

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Table 1. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
deVries et al., 1997	Eligibility required a histologically confirmed diagnosis of malignant neoplasm, measurable disease with documented progression prior to protocol therapy, no options for further medical treatment, an acceptable clinical condition (Karnofsky at least 80), and no concomitant somatic disease that might influence length of survival and physical or psychosocial functioning. N = 35.	Social support: Patients were offered 12 sessions of individual psychosocial counseling once a week, each session lasting 1.5 to 2 hr. Patients also participated in fortnightly group meetings. These group sessions were guided by two psychotherapists and lasted 2.5 hr. Partners of the patients were also invited to participate in the individual and group sessions.	−0.14	−0.46, 0.18	Group	Nontech	27–73 yrs; M = 55	54	11-item De Jong Gierveld Loneliness Questionnaire
Kraut et al., 1998	1995 cohort comprised families with teenagers participating in journalism classes in four Pittsburgh high schools; 1996 cohort comprised families in which an adult was on the board of directors of one of four community development organizations. Households with active Internet connections were excluded, and children younger than 10 years of age were excluded. N = 169.	Social access: Families received a personal computer and software, a free telephone line, and free access to the Internet. Program lasted 2 years for the 1995 cohort and 1 year for the 1996 cohort.	−0.14	−0.26, −0.02	Individual	Tech	Not reported	56	3 items from the UCLA Loneliness Scale

Table 1. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Marshall, Bryce, Hudson, Ward, & Moth, 1996	Nonfamilial male child molesters incarcerated in a medium-security Canadian penitentiary participated in the study. They had volunteered for treatment in the Bath Institution Sex Offenders' Program, a comprehensive cognitive-behavioral program. N = 32.	Social cognitive training: Topics discussed in group meetings were benefits of being in a relationship, sexual relations, jealousy, development of relationship skills, and dealing with loneliness. This curriculum was part of an overall treatment package offered as a group therapy program. Duration of program not described.	−1.46	−1.91, −1.01	Group	Nontech	24–53 yrs; M = 37	0	20-item UCLA Loneliness Scale
McAuley et al., 2000	Sedentary, older, U.S. community-dwelling adults were recruited using fliers, advertisements in the local newspapers, and announcements on local radio shows and local television news programs. N = 174.	Social access: Aerobic intervention group classes were conducted by trained exercise specialists and employed brisk walking for up to 40 min 3 times per week for 6 months. Stretching and toning intervention group exercised under supervision for 40 min 3 times per week for 6 months.	−0.08	−0.25, 0.08	Group	Nontech	Range not reported; M = 67	72	20-item UCLA Loneliness Scale
Petryshen, Hawkins, & Fronchak, 2001	Community-dwelling adults who were socially isolated and living with a serious and persistent mental health problem and were eligible for a social recreation program within a Canadian community mental health center. N = 36.	Social access: Social recreation intervention, which included information sessions about opportunities and resources in the community, workshops on relationship development, workshops on healthy lifestyles, self-help groups, weekly community walks, and community forums on mental illness. Approximately 200 group activities	−0.59	−0.88, −0.30	Group	Nontech	18–65 yrs; M = 43	61	4-item UCLA Loneliness Scale

(continued)

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Table 1. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
		were offered per year. Participation varied considerably. The median number of activities completed by participants was 18.							
Sorenson, 2003	Women who reported significant psychological disruption after a traumatizing provider interaction during a birth experience unrelated to the baby's physical outcome. Sample was recruited in conjunction with an International Cesarean Awareness Network (ICAN) state affiliate, ICAN referral agencies, and midwifery practices in a U.S. metropolitan area. N = 9.	Social cognitive training: 5 monthly group cognitive behavioral therapy sessions, each lasting 4 hr. Group leader was a psychiatric mental health clinical nurse specialist who encouraged the development of positive interpersonal relationships and provided support to confront issues and develop new cognitive and relationship skills.	−4.81	−7.09, −2.53	Group	Nontech	26–45 yrs; M = 33	100	20-item UCLA Loneliness Scale
Stewart, Craig, MacPherson, & Alexander, 2001	Canadian widows older than 55 with no neurological deficits who spoke and wrote English and who were not currently attending a bereavement self-help or support group. N = 23.	Social support: Four support groups composed of 5–9 participants met for 1–1.5 hr weekly for a maximum of 20 weeks. Each group was led by a peer (widow) and a professional facilitator. Participants were invited to discuss their priority needs and relevant issues. Discussions were augmented by guest lecturers, case studies, audiovisual aids, and role-playing exercises.	−0.27	−0.60, 0.06	Group	Nontech	54–77 yrs; M = 66	100	15-item Emotional/ Social Loneliness Inventory

Table 1. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Stewart, Reutter, Letourneau, & Makawarimba, 2009	Homeless youths in Edmonton were referred from employment programs, drop-in centers, and a Community Advisory Committee. $N = 14$.	Social support: 20-week intervention program consisting of 4 support groups, which included one-on-one support, group recreational activities, and meals.	−0.34	−0.77, 0.08	Group	Nontech	16–24 yrs; $M = 19$	46	20-item UCLA Loneliness Scale
Yarnoz, Plazaola, & Etxeberria, 2008	Long-term separated or divorced adults with children. $N = 7$.	Social support: Attachment-based intervention, encouraging people to elaborate, through shared narratives with peers and the therapists, a representation of the events, the self, the other, and the relationships that contribute to a better adjustment to the situation of divorce. Participants met for a 2-hr session each week for 8 months. Each session was led by a psychoanalyst and an attachment-oriented professional on an alternating basis.	−0.09	−0.66, 0.49	Group	Nontech	50–60 yrs; $M = 54$	43	15-item Social and Emotional Loneliness Scale

Table 2. Nonrandomized Group Comparison Studies (*n* = 18)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Allen-Kosal, 2008	Children were recruited from six third-grade classrooms from four different elementary schools in Knox County, Tennessee. *N* = 72.	Social skills training: Group 1 received pretraining in cooperative behavior and completed 8 weeks of cooperative learning activities. Group 2 completed 8 weeks of cooperative learning activities but received no pretraining. Group 3 received pretraining in cooperative behavior but did not complete cooperative learning activities. Group 4 was composed of three classrooms and served as the control group. Intervention lasted for 8 weeks.	−0.49	−1.10, 0.13	Group	Nontech	Third grade; *M* = 8	Not reported	24-item Loneliness and Social Dissatisfaction Questionnaire
Cohen et al., 2006	Recruitment notices sent to senior centers or retirement communities requesting volunteers for a study to assess physical and mental health as well as involvement in activities. One notice sought singers for a chorale group, whereas the other notice did not mention that activity. *N* = 166.	Social access: Participation in a professionally conducted chorale in which there were weekly singing rehearsals as well as public performances several times during the intervention period. Duration of intervention was 30 weeks. Control group did not receive the intervention.	0.06	−0.27, 0.39	Group	Nontech	Range not reported; *M* = 79	78	20-item UCLA Loneliness Scale
Evans, Werkhoven, & Fox, 1982	Legally blind adults randomly selected from a register of more than 500 blind veterans in Washington state. *N* = 84.	Social support: Groups of 3 participants plus a facilitator participated in 8 weekly 1-hr group conference calls. Goals were provision of information about resources, problem	−1.42	−1.90, −0.95	Group	Tech	53–78 yrs; *M* = 62	7	20-item UCLA Loneliness Scale

Table 2. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
		solving within the group, development of camaraderie, and building confidence among group members. Control group did not receive the intervention.							
T. Fokkema & Knipscheer, 2007	Volunteer home visitors recruited Dutch seniors with chronic illness or a handicap who lived alone, had few opportunities to leave the home, and did not have a computer or Internet access. N = 30.	Social access: Participants received on loan, free of charge, a personal computer with Internet access, monitor, speakers, and a printer for 3 years. Participants were given five 2-hr lessons at home on how to email and how to use the Internet. Participants continued to be supported and coached by volunteers who made home visits. Control group did not receive any intervention.	−0.52	−1.30, 0.26	Individual	Tech	Range not reported; M = 66	92	11-item De Jong Gierveld Loneliness Questionnaire
C. M. Fokkema & van Tilburg, 2007	Inactive adults living on their own. Experimental group recruited through brochures and local newspapers. Control group recruited by approaching random older adults. N = 72.	Social skills training: Seen Through Other Eyes. 6 weekly group training courses (2.5 hr per week) aimed at improving social skills and promoting an active lifestyle, co-led by professionals.	0	−0.49, 0.49	Group	Nontech	Range not reported; M = 71	74	11-item De Jong Gierveld Loneliness Questionnaire
C. M. Fokkema & van Tilburg, 2007	Adults with physical or mental health problems and living on their own. Experimental group recruited through brochures and local newspapers and invited by helpers. Control group recruited by postal questionnaire among homosexual older adults. N = 52.	Social support: Buddy Care for Homosexuals. Social and/or emotional support visits by volunteer homosexuals aimed at enlarging social network and participation and improving self-management abilities. Home-based intervention of variable duration.	0	−0.55, 0.55	Individual	Nontech	Range not reported; M = 67	40	11-item De Jong Gierveld Loneliness Questionnaire

Table 2. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
van den Elzen & Fokkema, 2006, as reported in C. M. Fokkema & van Tilburg, 2007	Elderly adults living on their own and making use of extramural care. Sample was from register of care receivers. Experimental group contains those who were willing to participate whereas control group comprised of those who refused. N = 52.	Social support: Elderly Support Home Visits. Volunteer visits to assess social needs, followed by social and/or emotional support provision. Home-based intervention of variable duration.	0	−0.55, 0.55	Individual	Nontech	Range not reported; M = 79	71	11-item De Jong Gierveld Loneliness Questionnaire
C. M. Fokkema & van Tilburg, 2007	Adults in residential care for the elderly. Experimental group selected from list of interested residents and invited by caretakers. Control group recruited through self-selection (i.e., those who were not interested in the intervention). N = 71.	Social support: Group Activities in Residential Homes. Network building encouraged through group trips and eight group meetings by new residents co-led by professionals. No information on the length of the intervention.	0	−0.56, 0.56	Group	Nontech	Range not reported; M = 84	86	11-item De Jong Gierveld Loneliness Questionnaire
C. M. Fokkema & van Tilburg, 2007	Adults in residential homes for the elderly. Experimental group included all residents of two homes for the elderly. Control group included all resident of a different home for the elderly. N = 116.	Social support: Institutional Interventions in Residential Homes. Network building encouraged through group training course for caregivers focused on topics related to loneliness and systematic information provision. Co-led by professionals. Duration of intervention was 6 months.	0	−0.39, 0.39	Group	Nontech	Range not reported; M = 84	71	11-item De Jong Gierveld Loneliness Questionnaire
Hartke & King, 2003	Eligibility included 60 years or older; married or spousal equivalent and living with a stroke survivor as the primary caregiver for a minimum of 1 month, not currently in a caregiver support group,	Social cognitive training: Eight weekly 1-hr telephone conference calls with 2 clinician–facilitators. Each participant received a manual, which outlined 8 topics, including facts	−0.18	−0.60, 0.24	Group	Tech	Range not reported; M = 70	74	10-item UCLA Loneliness Scale

(continued)

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Table 2. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
	and had a telephone in the home. Recruitment occurred through review of spouse's admission records, media advertisements, and community outreach. N = 88.	about stroke and caregiving, communicating with one's spouse, dealing with spouse's problematic feelings and behaviors, stress as a caregiver, taking care of oneself as a caregiver, community resources for caregivers, and goal setting. Manual also included an audiotape of a relaxation procedure and a publication on stress management. Control group received the same manual but did not participate in conference calls. Duration of intervention was 8 weeks.							
Hopps, Pepin, & Boisvert, 2003	Community-living Canadian adults with physical disabilities were recruited via announcements posted on bulletin boards and placed in various mailings to associations for people with physical disabilities and neurological disease. N = 19.	Social cognitive training: Using home computers or computers in rehabilitation center, each participant completed twelve 2-hr group cognitive behavioral sessions via synchronous, text-based Inter Relay Chat. During each session, participants examined the nature of and factors involved in their loneliness, determined ways of reducing loneliness, assessed various loneliness-reducing actions, and shared experiences in learning from others. Control group did not receive the intervention initially.	−1.84	−2.92, −0.77	Group	Tech	Range not reported; M = 34	53	20-item UCLA Loneliness Scale

Table 2. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Kolko, Loar, & Sturnick, 1990	Children with conduct or developmental disorders admitted to a 24-bed child psychiatric unit in a U.S. hospital. Eligibility based on scores from a four-item social problems screen for interpersonal difficulties (e.g., rejection, social skill deficiencies). Children who received a score of at least 7, with at least one maximum rating, were eligible. $N = 56$.	Social skills training: Intervention consisted of fifteen 1-hr small-group sessions that emphasized skill development in gaze, physical space, voice volume and inflection, compliments, conversational openers, assertive requests, appropriate responses to provocation, and appropriate play and sharing with others. Control consisted of fifteen 1-hr small-group sessions that offered semistructured opportunities for socialization in the context of age- and development-appropriate group activities, games, and tasks.	−1.49	−2.10, −0.88	Group	Nontech	Range not reported; $M = 10$	32	24-item Asher Loneliness Scale
Martina & Stevens, 2006	Recruited by senior service agencies in four Dutch communities using local newspaper articles and publicly distributed printed material. $N = 60$.	Social skills training; 12 weekly small-group lessons with a mean of 10 participants per group. Investigator-led lessons focused on theory of friendship enrichment, practice in skills important in friendship, and role-playing. Control group was placed on an intervention waiting list and received no intervention during the initial study period.	−0.06	−0.42, 0.31	Group	Nontech	53–86 yrs; $M = 63$	100	11-item De Jong Gierveld Loneliness Questionnaire

Table 2. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Rosen & Rosen, 1982	Directors of six senior centers in Georgia identified community-dwelling individuals who seemed depressed, had experienced recent traumas, gave evidence of approaching senility, or gave other indications that they might benefit from mental health counseling. Comparison group included seniors at two other senior centers who were in need of mental health services but did not have access to those services. $N = 68$.	Social support: Meetings typically began with a discussion of happenings during the previous week. Intervention staff encouraged sharing of past experiences and current feelings as well as interactions among members between group sessions. In total, there were 40 to 49 weekly sessions of 2 hours over a period of 12 to 15 months.	−0.37	−0.89, 0.15	Group	Nontech	65+ yrs; $Mdn = 70$	80	Participants were asked if they were seldom or never lonely
Seepersad, 2005	Students recruited on a college campus via fliers, brochures, and emails and who reported frequent loneliness, no severe current psychopathology, and no unresolved trauma history and who had a capacity to function as a group member. $N = 16$.	Social skills training: Each week for 7 weeks, participants were given written educational materials and assignments regarding various theories and ideas associated with loneliness. Groups of 7–9 participants met weekly with a program facilitator for 2 hr. Meetings focused on developing and practicing listening and communication skills. Participants were also asked to keep a journal every week. Control group did not receive intervention.	−0.37	−1.05, 0.31	Group	Nontech	Range not reported; $M = 21$	74	20-item UCLA Loneliness Scale

(continued)

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Table 2. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Shapira, Barak, & Gal, 2007	Community- and nursing-home-dwelling Israeli seniors who had sufficient cognitive ability to participate in the offered activities. N = 22.	Social access: Computers were placed in common rooms in outpatient senior centers and nursing homes. Participants received in-person, small-group computer, Internet, and email training and were encouraged to use the computers and Internet between training sessions. Program lasted 15 weeks and included one or two 60-min lessons per week. Comparison group took part in other group activities, including painting, sewing, needlework, and ceramics.	−1.99	−2.81, −1.17	Group	Tech	70–93 yrs; M = 82	82	20-item UCLA Loneliness Scale
White et al., 1999	Recruited from a U.S. retirement community that included independent living, assisted living, and skilled nursing care facilities. Participants willing to commit 4–6 hr per week for 4 months were placed in the intervention group. Individuals who showed initial interest but declined to enroll because of scheduling conflicts were placed in the comparison group. N = 23.	Social access: Computer consultant provided 9 hr of instruction on use of the computer, email, and the Internet. Three computers were available to participants at all times in a common area. Help desk was available 3–4 hr/week for first 2 months then 1 hr/week for last 3 months. Duration of program was 5 months. Control group did not receive the intervention and did not use the computers during the study.	−0.51	−1.39, 0.36	Individual	Tech	Range not reported; M = 78	84	20-item UCLA Loneliness Scale

(continued)

Table 2. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Winningham & Pike, 2007	Institutionalized older adults were recruited from 6 U.S. assisted living facilities.	Social cognitive training: Intervention group participants attended 3 small-group meetings per week for 3 months. Meetings were designed to educate participants about the brain and memory, stimulate memory and cognitive activity and focus on making new memories. Activities were designed and conducted to facilitate social interactions and develop social support networks. Control group did not participate in small-group meetings.	−0.31	−0.82, 0.21	Group	Nontech	61–98 yrs; $M = 82$	Inferred as 80	20-item UCLA Loneliness Scale

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Table 3. Randomized Group Comparison Studies (n = 20)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Banks & Banks, 2002	Recruited from three nursing homes in a city in southern Mississippi. Inclusion criteria included no cognitive impairment, no history of psychiatric disorder, and a score of 30 or greater on the UCLA Loneliness Scale. N = 45.	Social support: Animal-assisted therapy (AAT) consisted of an attendant bringing a leashed dog to the participant's room for 30 min. Participants were allowed to hold, stroke, groom, talk to, play with, or walk the dog in the hallway. Interaction with attendant was minimized. AAT-1 group members had one session per week, whereas AAT-3 members had three sessions per week. Control group received no AAT sessions. Duration of study was 6 weeks.	−0.20	−0.92, 0.52	Individual	Nontech	Range not reported; mean inferred = 78	80	20-item UCLA Loneliness Scale
Banks, Willoughby, & Banks, 2008	Recruited from three nursing homes in St. Louis, Missouri. Inclusion criteria included no cognitive impairment, no history of psychiatric disorder, and a score of 30 or greater on the UCLA Loneliness Scale. N = 26.	Social support: AAT consisted of a weekly 30-min visit from either a living dog or a robot dog (AIBO) for 8 weeks. Sessions occurred in the resident's room and consisted of the resident sitting in his or her chair or upright in bed with the dog or AIBO next to the resident. Control group received no AAT sessions.	−0.88	−1.68, −0.07	Individual	Nontech	Range not reported; mean inferred = 75	Inferred as 80	20-item UCLA Loneliness Scale

(continued)

Table 3. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Chiang et al., 2009	Participants were recruited from a nursing home in the Taipei area. Inclusion criteria were (a) conscious and able to speak Mandarin or Taiwanese, (b) aged 65 years or older, and (c) scored greater than 20 on the Mini Mental State Examination. $N = 92$.	Social cognitive training; Intervention consisted of 8 weekly sessions of individual reminiscence therapy. Each session focused on a different topic, including sharing memories and greeting each other, increasing participant awareness of their feelings and helping them to express their feelings, and identifying positive relationships from their past and how to apply positive aspects of past relationships to present relationships.	−0.97	−1.40, −0.54	Individual	Nontech	Range not reported; $M = 77$	0	20-item UCLA Loneliness Scale
Christian & D'Auria, 2006	Recruited from four university-based cystic fibrosis (CF) centers in North Carolina. $N = 116$.	Social skills training; Educational problem-solving and social skills intervention designed to help children with CF deal with specific problems, including finding out about their CF diagnosis, explaining their CF-related differences, dealing with teasing about CF, and keeping up with peers during physical activity. Intervention included an individual home visit and a structured, small-group (4 children) session approximately 2 weeks later. Control group did not receive intervention.	−0.08	−0.44, 0.29	Group	Nontech	8–12 yrs; $M = 9$	49	16-item Asher Loneliness Scale

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Table 3. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Coleman et al., 2005	Women referred from hospitals in urban and rural communities in Arkansas and from the Arkansas Division of the American Cancer Society who were English speaking and diagnosed with TNM Stage 0, I, II, or III nonmetastatic breast cancer, had no major underlying medical problems or previous history of cancer (except for nonmelanoma skin cancer), and who entered the study 2–4 weeks postsurgery. N = 106.	Social support: In Phase I, oncology nurses provided weekly telephone social support from 2–3 weeks postsurgery through 3 months. In Phase II, weekly calls were continued through 5 months postsurgery and participants received a resource kit regarding adaptation to disease and treatment. In Phase III, calls were decreased to twice per month through 8 months postsurgery. In Phase IV, calls decreased to once per month until the 1 year anniversary of diagnosis. Control group received the same resource kit but no telephone social support.	−0.29	−0.67, 0.09	Individual	Tech	Range not reported: M = 57	100	20-item UCLA Loneliness Scale
Conoley & Garber, 1985	U.S. undergraduate students who scored 1 SD above the mean on UCLA Loneliness Scale and scored as moderately depressed on the Beck Depression Inventory. N = 38.	Social cognitive training: Two 30-min individual counseling sessions (1 week apart) which emphasized either reframing perception of loneliness or self-control (i.e., trying harder to overcome loneliness). Control group did not receive counseling sessions.	−0.32	−0.96, 0.32	Individual	Nontech	Range not reported; mean inferred = 20	100	20-item UCLA Loneliness Scale
E. O. Cox, Green, Hobart, Jang, & Seo, 2007	Community-dwelling older adults from the Denver area who were 55 years or older and required	Social support: 10 biweekly sessions of 1.5 to 2 hr related to specific themes and 2 review sessions. The	−0.19	−0.60, 0.22	Individual	Nontech	51–96 yrs; M = 79	77	Philadelphia Geriatric Morale Scale on Lonely Dissatisfaction

(continued)

Table 3. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
	a minimum of 6 hr of personal care per week because of stroke, heart disease, osteoporosis, mild dementia, cancer, or severe arthritis. $N = 92$.	goal was to increase the capacity of elderly care receivers to effectively manage their own care, including optimizing the relationship with their caregiver. Comparison group received needs assessment, referral and assistance, monthly follow-up, and ongoing telephone assistance at the request of the participant.							
Fukui, Koike, Ooba, & Uchitomi, 2003	Recruited from a group of outpatients with breast cancer who were surgically treated at a national cancer center hospital in Japan. All patients were younger than 65 years old, had surgery within the previous 4–18 months, and had no chemotherapy or had completed chemotherapy. $N = 46$.	Social support: 3 intervention groups of 6–10 patients met for 1.5 hr weekly for 6 weeks. Intervention consisted of health education, coping skills training, stress management, and psychological support. Goals of the intervention were to provide within-group support by professionals and peers, lessen the psychological distress associated with having cancer, and assist patients in learning effective coping methods for cancer-related concerns. Control group did not receive intervention.	0.15	−0.43, 0.73	Group	Nontech	Range not reported; $M = 54$	100	20-item UCLA Loneliness Scale
Heckman & Barcikowski, 2006	Eligibility criteria included 50 years of age or older; HIV infected or had	Social support: 12 weekly 90-min telephone conference calls emphasizing coping	0	−0.41, 0.41	Group	Tech	Range not reported; $M = 54$	32	10-item UCLA Loneliness Scale

(continued)

Table 3. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
	AIDS by self-report, diagnosed with major depressive disorder; partial remission of major depression, dysthymia, or minor depressive disorder, and score of 75 or higher on the Modified Mini-Mental State Examination. N = 90.	strategies to reduce psychological distress. Each group was composed of 6–8 participants and 2 leaders. Topics included sharing personal histories, identifying life stressors, sharing personal coping strategies, a discussion of adaptive problem-focused coping, adaptive emotion focused coping, and ways to increase social resources. Intervention duration was 3 months. Control group did not participate in conference calls.							
K. Heller, Thompson, Trueba, Hogg, & Vlachos-Weber, 1991	Telephone calls were made to a random sample of residences in low-income housing tracts in three Indiana communities to identify women living alone or with one other person in the household. Selection criteria were annual household income below median for Indiana senior citizens and either below median perceived support or above median loneliness. N = 102.	Social support: Initial randomization was to either 10 weeks of friendly staff telephone contact or control group. Staff called twice a week for 5 weeks then once a week for 5 weeks. Staff inquired about participant's health and well-being, events of the week, and stressful life events. After 10 weeks, those receiving the staff contact were randomly assigned to continue that contact or were paired in dyads to continue phone contact with one another. Control group received no intervention.	−0.12	−0.51, 0.27	Individual	Tech	Range not reported; Mdn = 74	100	7-item Paloutzian and Ellison Loneliness Scale

(continued)

Table 3. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Hill, Weinert, & Cudney, 2006	Community-living, chronically ill women living in rural western United States. Recruitment occurred through mass media, agency and service organization newsletters, and word of mouth. N = 100.	Social access: 22 weeks of participation in an online, asynchronous, peer-led support group, and a health teaching unit. Participants received in-home Internet access to email and an asynchronous chat room in which they exchanged feelings, expressed concerns, provided support, and shared life experiences. Intervention also included web-based health education modules. In addition, participants also engaged in expert-facilitated chat room discussions related to the health teaching unit activities. Control group did not receive intervention.	0.004	−0.39, 0.40	Group	Tech	35–65 yrs; mean inferred = 52	100	20-item UCLA Loneliness Scale
Jessen, Cardiello, & Baun, 1996	Men and women ages 65 or older admitted to two Midwestern skilled rehabilitation units. All newly admitted persons were approached but participation was voluntary. N = 40.	Social support: Caged bird (budgerigar) placed in participant's room for 10 days. Participants received verbal and written instructions regarding the bird but participants did not provide care to the bird. Control group did not receive the intervention.	0.40	−0.23, 1.02	Individual	Nontech	65–91 yrs; M = 76	68	20-item UCLA Loneliness Scale

Table 3. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
Kremers, Steverink, Albersnagel, & Slaets, 2006	Participants recruited through advertisement in local newspapers in two regions of the Netherlands. Single, community-dwelling women, 55 years and older, were asked by phone if they missed having people around them, wished to have more friends, participated in very few leisure activities, or had trouble initiating activities. $N = 119$.	Social skills training; Intervention consisted of six weekly group meetings that included 8–12 participants and two facilitators. Each meeting lasted 2.5 hr and focused on one or more techniques, including taking initiative in making friends, investing in friendships, having a positive frame of mind, finding and maintaining multifunctionality in friendship, and having more than one friend. Control group did not participate in group meetings.	0.12	−0.25, 0.49	Group	Nontech	Range not reported; $M = 64$	100	11-item De Jong Gierveld Loneliness Questionnaire
McWhirter & Horan, 1996	U.S. university students who responded to a publicized program at a university counseling center were accepted if they scored one standard deviation above the mean on the UCLA Loneliness Scale for college age populations, reported experiencing loneliness within the 4-week period prior to intake, and presented no clinical evidence of suicidal behavior or severe depression. $N = 22$.	Social cognitive training; A counselor led each structured, small-group (3–5 participants) experience, which met for 2 hr each week for 6 weeks. Intimate condition group used cognitive and behavioral techniques to focus on establishing and maintaining intimate relationships. Social condition group combined cognitive restructuring with modeling, role-play, and homework assignments for developing better	−0.89	−1.77, −0.01	Group	Nontech	18–38 yrs; $M = 25$	48	20-item UCLA Loneliness Scale, from which Intimate and Social Loneliness subscales were derived

(continued)

Table 3. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
		communication skills in social settings. Combined condition group included all elements of the intimate and social condition groups. Control condition met to express feelings and share experiences but counselor did not suggest ways to reduce loneliness.							
Morrow-Howell, Becker-Kemppainen, & Lee, 1998	Older U.S. adults in the St. Louis area who either called a suicide crisis hotline or were referred from family members, friends, or professionals because of depression, social isolation, or unmet needs in activities of daily living. N = 61.	Social support: Social worker made weekly telephone calls to administer three components of the program, multidimensional psychosocial evaluation, assistance with social services, and supportive therapy, through which the social worker encouraged the client to articulate problems, explore possible solutions, and take action. Median length of contact was 8 months. Control group did not receive intervention.	−0.09	−0.59, 0.41	Individual	Tech	61–92 yrs; M = 76	85	Older Americans Resources and Services Social Resource Rating Scale regarding loneliness frequency
Ollonqvist et al., 2008	Working in collaboration with seven independent rehabilitation centers and 41 municipalities throughout Finland, the goal was to recruit a representative sample of frail	Social support: Intervention consisted of a network-based group rehabilitation program which consisted of three separate inpatient periods at a rehabilitation center	−0.17	−0.33, −0.02	Group	Nontech	65–96 yrs; M = 78	86	"Do you feel yourself lonely?" 0 = never, 1 = seldom, 2 = sometimes, 3 = often, 4 = always

(continued)

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Table 3. (continued)

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Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
	persons older than 65 who were living at home but faced a risk of institutionalization within 2 years because of progressively decreasing functional capacity. N = 644.	within eight months. Participants had individual visits with the physician, physiotherapist, social worker, and occupational therapist. In addition, participants engaged in group activities which focused on various exercises as well as group discussions and lectures. Topics included promotion of self-care, psychological counseling, medical issues, social services, and recreational activities.	−0.17	−0.33, −0.02	Group	Nontech	65–96 yrs; M = 78	86	"Do you feel yourself lonely?" 0 = never, 1 = seldom, 2 = sometimes, 3 = often, 4 = always
Samarel, Tulman, & Fawcett, 2002	English-speaking U.S. women who had had surgery for nonmetastatic (TNM Stage 0, I, II, or III) breast cancer within 4 weeks prior to study participation, had no previous cancer diagnosis except for nonmelanoma skin cancer, and had no other major medical problems. N = 82.	Social support: Experimental group received weekly 2-hr group social support and education (topics included stress management, communication techniques, problem-solving skills, and understanding emotions and needs) as well as weekly individual telephone social support and education over 13 months. Control group 1 received weekly individual telephone social support and education over 13 months. Social support	−0.51	−0.97, −0.05	Group	Tech	30–83 yrs; M = 54	100	20-item UCLA Loneliness Scale

Table 3. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
		and education were provided by either oncology nurse–clinicians or social workers. Control group II received educational resource kit via a one-time mailing.							
Savelkoul, de Witte, Candel, Van Der Tempel, & Van Den Borne, 2001	Patients of rheumatology clinics in two regional hospitals in the Netherlands. Final sample was selected on the basis of chronic rheumatologic condition, duration of more than 1 year, age between 35 and 65 years, higher than median score on impact of rheumatic disease on functional health status, and a higher than median score on at least one of the following: loneliness, lack of social support, or impact of rheumatic disease on social behavior. N = 75.	Social support: Aim of coping intervention group was to increase action-oriented directed coping and coping by seeking social support. Emphasized four steps: describe the problem, think about possible solutions, choose one or more solutions, implement the solution and evaluate the results. Aim of mutual support group was to exchange information, experiences, feelings, and emotions. No coping skills were taught. There were 10 weekly 2-hr sessions in each intervention and each group was comprised of 10–12 participants. Control group did not receive the intervention.	−0.04	−0.49, 0.41	Group	Nontech	35–65 yrs; M = 51	68	11-item De Jong Gierveld Loneliness Questionnaire
White et al., 2002	Residents of four U.S. congregate-housing sites and two nursing facilities. Volunteers were solicited during information sessions open to all residents at each site. N = 93.	Social access: 9 hr of training over a 2-week period. Training covered basic computer operation, use of email, and introduction to accessing the Internet. Each participant also	−0.13	−0.54, 0.27	Individual	Tech	Range not reported; M = 71	76	20-item UCLA Loneliness Scale

Table 3. (continued)

Authors	Enrollment eligibility and sample size	Intervention type and duration	Effect size	95% CI	Intervention format	Intervention mode	Sample age	Sample % female	Loneliness measure
		received a training manual. Subsequently, the trainer was available at each site for about 2 hr per week for technical support. Trainer was also available by phone and email. Duration of program was 5 months. Control group did not receive the intervention.							
Williams et al., 2004	Recruits at basic training at the Naval Recruit Training Command at Great Lakes, Illinois. Those who scored 18 or higher on the Beck Depression Inventory of 30 or higher on the Perceived Stress Scale were classified as at-risk recruits. $N = 200$.	Social cognitive training; Cognitive behavioral intervention consisted of 10–15 at-risk recruits meeting for 45 min each week for 9 weeks. Groups were facilitated by a psychologist. Participants read a manual each week, then discussed and practiced strategies for coping, increasing one's sense of belonging, decreasing thought distortion, and stress management. Nonintervention and comparison groups participated in weekly meetings that focused on other topics, such as swimming skills and personal hygiene.	−0.36	−0.64, −0.08	Group	Nontech	Range not reported; $M = 20$	28	20-item UCLA Loneliness Scale

These groups were treated as statistically different at baseline, and the effect size was calculated accordingly.

Primary Effect Size. Effect sizes included in Tables 1 to 3 are "primary" effect sizes, which were calculated from the first available posttreatment measurement time point. In addition, in studies with more than one intervention group, the primary effect size was calculated for the intervention group that reflected the key feature of each intervention or that incorporated the fewest design flaws. In studies with more than one control group, the control group that was theoretically expected to exhibit the greatest difference from the treatment group was used to calculate the primary effect size.

Five studies had more than one intervention group. For three of these studies, the primary effect size was based on the intervention that best represented the key features of the intervention. In Allen-Kosal (2008), the three intervention groups received, respectively, a pretraining session, an 8-week class, or both a pretraining session and a class. The group with both the pretraining and the 8-week class was selected to calculate the primary effect size. In Banks, Willoughby, and Banks (2008), animal-assisted therapy was provided to one intervention group with a robotic dog and to a second group with a real dog. A sizable literature documents the benefits of owning "real" pets (Keil, 1988), so the real dog intervention was included as the primary intervention. In McWhirter and Horan (1996), the three intervention groups—intimate condition, social condition, and combined condition—focused on a different set of skills and techniques for improving intimate, social, or both types of relationships, respectively. The combined condition included both the intimate and social components of the intervention and was therefore treated as the primary effect.

In two additional studies with more than one intervention group, the intervention with the fewest implementation failures was selected to calculate the primary effect size. In E. O. Cox et al. (2007), a small-group-based version and an individual-based version of the "Care-Receiver Efficacy Intervention" were compared with a standard individual-based case management group. Randomization was not fully implemented because only participants who were able to access and participate in the group-based intervention were eligible for the small-group treatment, and all eligible participants were assigned to the small-group treatment. All individual-eligible participants were randomly assigned to individual-based treatment or the case management control group condition. The effect size from the individual intervention group was therefore treated as the primary intervention. In K. Heller, Thompson, Trueba, Hogg, and Vlachos-Weber (1991), the effect on loneliness and psychological well-being of telephone call support from staff was compared to that of telephone support from peers. Participants were first randomized into treatment or control groups. The treatment group received 10 weekly staff phone calls, whereas the control group received no intervention. After 10 weeks of regular staff phone calls, participants in the treatment group were randomly assigned to one of three intervention conditions. In one intervention, staff phone calls continued. In the second and third intervention types, participants were assigned to either receive or initiate regular phone calls with a peer in the study. The frequency of phone calls was held constant across all intervention types. However, because 27 out of the 125 participants (22%) in the second and third intervention groups declined to participate after the randomization and all of the participants in the staff contact group remained, the staff contact group was used to calculate the primary effect size to avoid the potential self-selection problem in the other two groups. The control group used for the calculation of the primary effect size was the group that received nothing throughout the study.

Three studies included more than one control group. In Samarel, Tulman, and Fawcett (2002), the treatment included telephone support and group social support along with a mailed education kit; one control group received telephone support with mailed materials, and the other control group received only the mailed materials. The primary effect size was calculated using the control group that received the mailed materials only (i.e., the group that was expected to exhibit the greatest difference relative to the treatment group). Conoley and Garber (1985) administered cognitive reframing as the main intervention. In addition to the control group that received no intervention, this study had another comparison group whose members were instructed "to try harder" to overcome loneliness. The primary effect size was calculated using the control group that received no intervention. Heckman and Barcikowski (2006) had two time-lagged intervention groups (immediate and delayed) serving as control groups for each other; effect sizes were calculated for both interventions, but the immediate condition was treated as the primary intervention because its control group did not receive any intervention and thus was more comparable to the control groups of other included studies.

Analyses. The meta-analytical procedure demonstrated in Borenstein, Hedges, Higgins, and Rothstein (2009) was used to calculate the mean effect size, identify the level of heterogeneity, and perform the subsequent moderator analyses. Because of the wide range of interventions included in this meta-analysis, a random effects model was selected. In contrast with the fixed effect model, which assumes that all studies have the same true effect size, the random effects model assumes that the true effect size varies across studies and follows a normal distribution around the mean. The summary effect size is thus an estimation of the mean of a distribution of effects, not the single true effect assumed and estimated by the fixed effect model. The random effects model takes two sources of variance into consideration: within-study error in the estimate of the effect size and between-study variation in the true effect size. The Q statistic and p value were calculated to test the assumption of

homogeneity in effect sizes. The T^2 statistic was calculated to estimate the magnitude of the between-study variance of the true effect sizes. The I^2 statistic was calculated to estimate the proportion of total observed variance attributable to between-study variation in effect size as opposed to random error. The more I^2 deviates from zero, the greater the justification for follow-up moderator analyses that explore reasons for the between-study variation. As benchmark values, Higgins, Thompson, Deeks, and Altman (2003) suggest that between-study variance of 25% is low, 50% is moderate, and 75% is high. Using procedures described by Borenstein et al. (2009), the influence of categorical moderator variables was assessed using subgroup analyses analogous to ANOVAs that partition the total effect size variance into variance within and between groups. Within-study variance was removed from the total variance, and the remaining between-group variance (Qb) was used to test whether effect sizes differed among categories of a given moderator. The influence of continuous moderator variables was assessed using weighted regression analyses.

Ancillary analyses were used to determine whether meta-analytic results differed if the primary effect size was replaced with the alternative effect size calculated from delayed posttreatment measures. A total of 13 studies had delayed posttreatment measures. Of these, 3 used a single-group pre-post design (McAuley et al., 2000; Stewart et al., 2001; Stewart et al., 2009), 3 used a nonrandomized group comparison design (Allen-Kosal, 2008; Martina & Stevens, 2006), and 9 used a randomized group comparison design (Chiang et al., 2009; Christian & D'Auria, 2006; Coleman et al., 2005; Conoley & Garber, 1985; E. O. Cox et al., 2007; Fukui, Koike, Ooba, & Uchitomi, 2003; K. Heller et al., 1991; Kremers et al., 2006; McWhirter & Horan, 1996). Also examined was the effect of using the largest effect size in each study. This decision resulted in six new effect sizes:[9] One was a single-group pre-post design (Stewart et al., 2001), one was a nonrandomized group comparison design (Allen-Kosal, 2008), and four used a randomized group comparison design (Christian & D'Auria, 2006; Fukui et al., 2003; Heckman & Barcikowski, 2006; Kremers et al., 2006). Results of the ancillary analyses did not differ substantively from those reported in our primary analyses below.

Results

Studies With a Single-Group Pre-Post Design. A total of 12 studies met our criteria for single-group pre-post interventions to reduce loneliness. In terms of the target population, 2 of the studies focused on children, 7 had sample age ranges between 19 and 55 years old, and 3 focused on individuals aged 65 years or older. With the exception of 2 studies, the gender composition of the studies in this group consisted of more female than male participants. There was no social skills training intervention in this group, but the remaining three

types of interventions were equally presented. The majority of the interventions in this group were group based, with no utilization of technology. UCLA loneliness measures were used in 8 of the 12 studies. The details of these studies are summarized in Table 1.

The effect sizes in this group differed across studies, ranging from –4.81 to 0.12. As is shown in Table 4, the mean effect size for these 12 studies was –0.367 (95% CI = –0.55, –0.18; $p < .001$). The distribution of effect sizes is displayed in Figure 2. The degree of dispersion as indicated by the between-study variance statistic, T^2, was .18. A significant Q statistic (28.52, $p < .01$) indicated a heterogeneous distribution of effect sizes. The I^2 showed that 61% of the variance could be attributed to between-study variation. To examine whether heterogeneity was caused by the presence of an outlier, Sorenson (2003) was removed and the same analysis was conducted again. The mean effect size of the remaining 11 studies was –0.333 (95% CI = –0.51, –0.16; $p < .001$), with a Q score of 16.95 ($p = .075$), indicating that removal of Sorenson decreased the level of heterogeneity to nonsignificance. However, because the Q statistic is influenced by the number of studies and/or large within-study variance, a nonsignificant p value does not mean that the effect sizes are homogeneous across studies (Borenstein et al., 2009). The I^2 statistic showed that a large proportion of variance (41%) remained attributable to between-study variation. Sorenson (2003) was therefore included in the subsequent moderator analyses. Results of moderator analyses conducted without this study did not differ substantively from results of analyses that included this study.

The first moderator examined was intervention type. Mean effect sizes were significant for all three types of intervention.[10] The subgroup analyses indicated no difference in mean effect size ($Qb = 2.65$, $df = 2$, $p > .2$) among intervention types. Therefore, intervention type failed to explain the difference in effect size among the pre-post studies. Tests of moderation by intervention format and mode were not conducted because most of the single-group pre-post studies implemented a group-based format (9 out of 12 studies) and a nontechnological mode of delivery (10 out of 12 studies). A test of moderation by type of loneliness measure revealed a significant difference in mean effect size among loneliness measures ($Qb = 6.62$, $df = 1$, $p = .01$): Studies using the UCLA Loneliness Scale showed a mean effect size of –0.499 ($n = 8$; 95% CI = –0.74, –0.26; $p < .001$), whereas studies that used non-UCLA loneliness measures had a mean effect size of –0.103 ($n = 4$; 95% CI = –0.28, 0.08; $p > .2$). The gender and age composition of the sample, the number of intervention sessions, and the duration of the intervention did not moderate the effect size among the single-group pre-post studies.[11]

In sum, meta-analysis of the single-group pre-post studies revealed that the interventions appeared to be highly effective in reducing loneliness. Design features and sample characteristics did not moderate the effect size, but studies that measured loneliness with the UCLA Loneliness Scale

Table 4. Summary Statistics Regarding Loneliness Interventions

Study type	n	Mean effect size	SE	95% CI of effect size	z score	Q test for homogeneity	p value	I^2	95% CI	Characteristic
Single-group pre-post studies	12	−0.367	0.096	−0.55, −0.18	−3.781	28.52	<.01	61.43	27.7%, 79.4%	High between-study variance
Nonrandomized group comparison studies	18	−0.459	0.135	−0.72, −0.20	−3.400	20.89	.23	18.63	0%, 53.6%	Low between-study variance
Randomized group comparison studies	20	−0.198	0.062	−0.32, −0.08	−3.182	21.65	.30	12.25	0%, 47.53%	Approaching no between-study variance
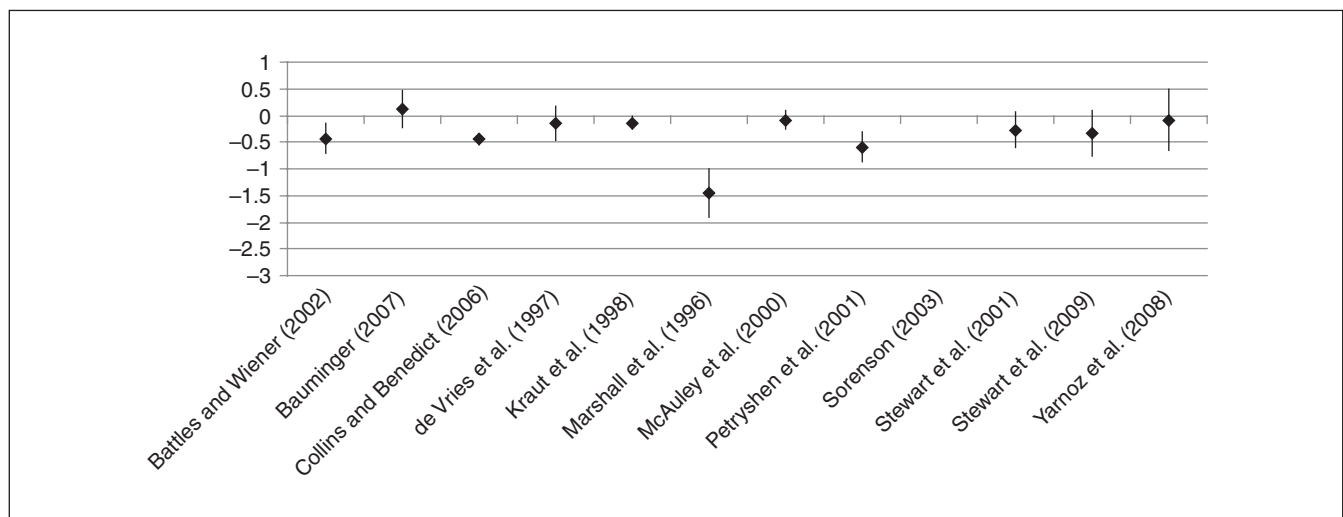
Figure 2. Effect size distribution: Single-group pre-post design (*n* = 12)
Note: To make the graphs comparable, the y-axis was set at (1.0 to −3.0). The result from one study with a larger effect size (−4.81) is therefore not fully demonstrated in this graph (Sorenson, 2003).

on average reported greater effect sizes than studies that used other loneliness measures.

Studies With a Nonrandomized Group Comparison Design. A total of 18 studies met our criteria for nonrandomized group comparisons design. In terms of the target population, 2 of the studies focused on children, 2 focused on young adults, and the remaining 14 focused on individuals aged 60 years or older. The majority of the samples in this group consisted of more female than male participants, as only one study focused mainly on a male population. All four types of interventions were present in this group. The majority of the interventions in this group had a group-based format, and about one third of the studies utilized technology in their interventions. The UCLA Loneliness Scale and the De Jong Gierveld questionnaire were administered by about the same number of studies, whereas 3 studies used other loneliness measures. The details of these studies are summarized in Table 2.

Effect sizes ranged from −1.88 to 0.11 for this group of studies, with 14 of the effect sizes having confidence intervals that included zero, whereas the remaining 4 appeared highly effective in reducing loneliness. As is shown in Table 4, the mean effect size for these 18 studies was −0.459 (95% CI = −0.72, −0.20; *p* < .01). The distribution of effect sizes is displayed in Figure 3. The between-study variance in effect size was estimated as $T^2 = .08$. The *Q* test did not reject the null hypothesis of homogeneity ($Q = 20.89$, $p = .23$), but the I^2 showed that 19% of the variation was attributable to between-study variance. Because the *Q* statistic has low power to detect heterogeneity when the sample size is small, moderator analyses were conducted to prevent premature conclusions.

Subgroup analyses showed no difference among the four intervention types ($Qb = 0.85$, $df = 3$, $p > .8$). In addition, the four aforementioned highly effective studies fell into four distinct intervention types and thus confirmed that, among
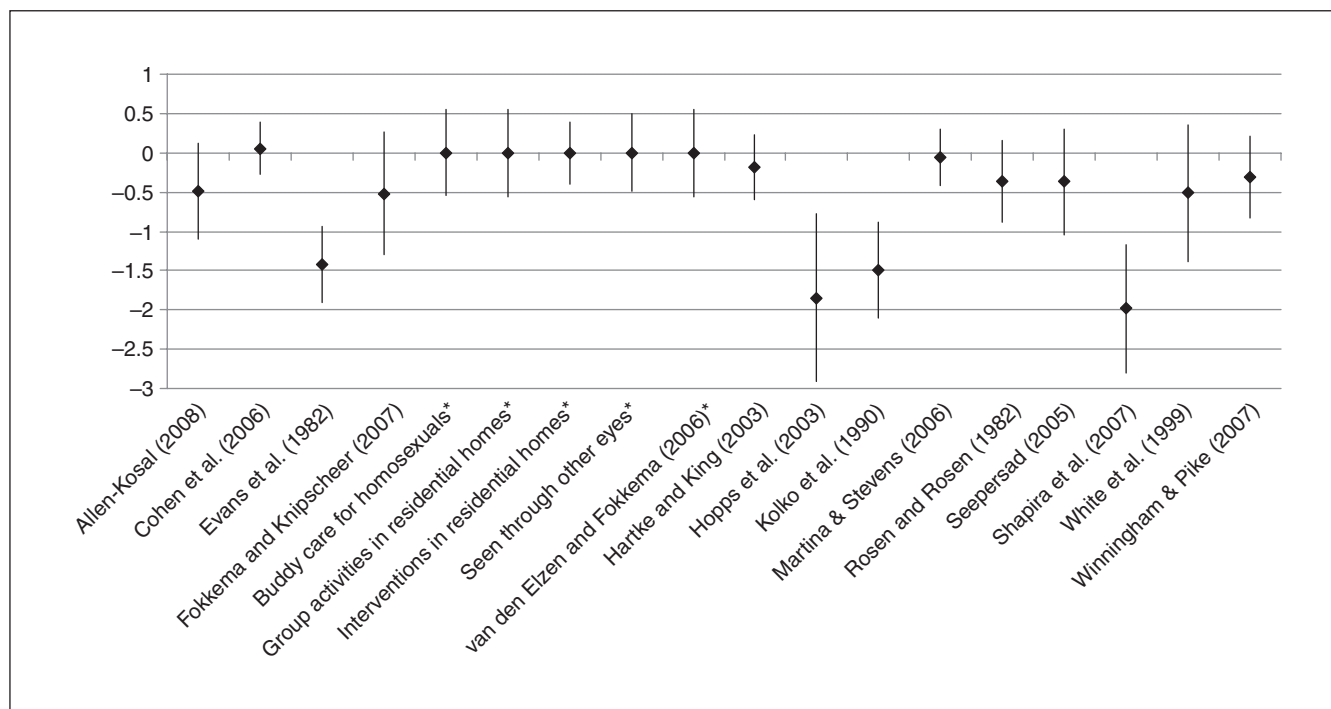
Figure 3. Effect size distribution: Nonrandomized group comparison design (*n* = 18)
Note: Studies marked with an asterisk were listed in the unpublished English translation of C. M. Fokkema and van Tilburg (2007).
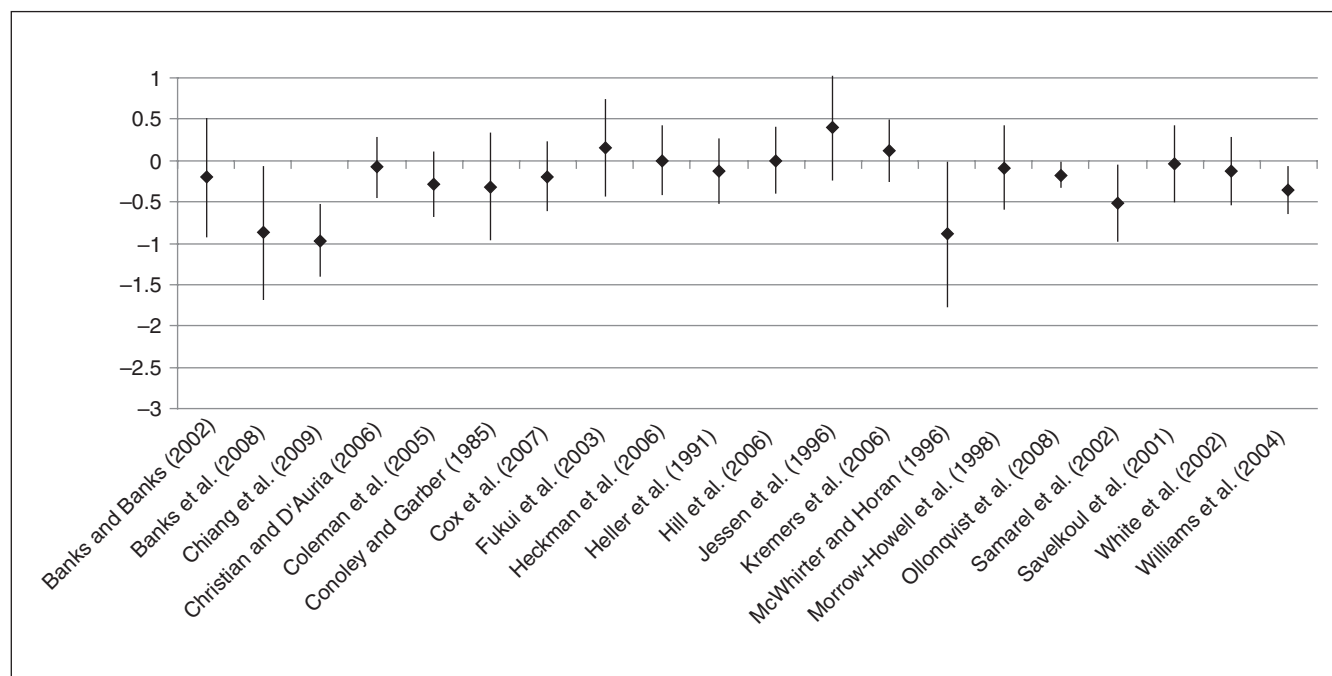
the nonrandomized group comparison studies, the intervention type was not the dominant factor contributing to the difference in effect sizes. For intervention format, group-based interventions on average had larger effect sizes than individual-based interventions,[12] but the difference was not statistically significant ($Qb = 2.51$, $df = 1$, $p > .1$). On the other hand, the utilization of technology showed a significant moderating effect ($Qb = 5.71$, $df = 1$, $p = .02$). The mean effect size of the interventions that used technology was –1.04 ($n = 6$; 95% CI = –1.68, –0.40; $p < .01$), as opposed to an effect size of –0.21 ($n = 12$; 95% CI = –0.43, 0.01; $p = .05$) in studies that did not use any kind of technology in the intervention. The instrument used to measure loneliness was significant in differentiating effect sizes ($Qb = 9.64$, $df = 2$, $p < .01$), with the De Jong Gierveld questionnaire producing the smallest mean effect size.[13] Follow-up analysis revealed that studies that used the De Jong Gierveld questionnaire (e.g., van den Elzen & Fokkema, 2006) reported significantly smaller effect sizes than studies with either the UCLA or other loneliness measures ($Qb = 9.65$, $df = 1$, $p < .01$). The gender and age compositions of the samples were also significant moderators of the effect size. The percentage of females in the sample was negatively correlated with the effect size ($\beta = 1.59$, $z = 3.15$, $p < .01$): The more females in the sample, the less loneliness reduction was observed. The mean age of the sample was negatively correlated with the effect size ($\beta = 0.01$, $z = 1.93$, $p = .05$), but the effect was small. Neither the intervention duration nor the number of sessions had a

moderating influence on the effect size.[14] Follow-up analysis with all the individually significant moderators (gender, age, technology, and loneliness measure) in one regression model showed that only the utilization of technology consistently showed a moderating effect ($\beta = –5.60$, $z = –2.28$, $p = .02$).

In sum, meta-analysis of the nonrandomized group comparison studies suggested a significant intervention effect on loneliness. Utilization of technology had a moderating effect on effect size independent of effect size differences associated with gender, age, and type of loneliness measure used.

Studies With a Randomized Group Comparison Design. A total of 20 studies met our criteria for randomized comparison design. In terms of the target population, 1 study focused on children, 3 focused on young adults, 6 studies focused on middle-aged adults, and the remaining 10 studies focused on individuals aged 60 years or older. In this group of 20 studies, 6 studies included only female participants and 1 study included only male participants. Of the remaining 13 studies, 8 had more female than male participants. All four types of interventions were present in this group. An equal number of studies used group-based and individual-based formats, and about one third of the studies utilized technology in their interventions. The UCLA Loneliness Scale was used in 13 of the 20 studies, whereas 2 studies administered the De Jong Gierveld questionnaire and 5 used other loneliness measures. The details of these studies are summarized in Table 3.

Figure 4. Effect size distribution: Randomized group comparison design (*n* = 20)

The effect sizes in this group ranged from –0.79 to 0.40, with 6 studies reporting efficacy in reducing loneliness (Banks et al., 2008; Chiang et al., 2009; McWhirter & Horan, 1996; Ollonqvist et al., 2008; Samarel et al., 2002; Williams et al., 2004). The remaining 14 studies showed no change in loneliness as indicated by 95% confidence intervals that included zero. However, as is shown in Table 4, the mean effect size for these 20 studies was –0.198 (95% CI = –0.32, –0.08; *p* < .01). The distribution of effect sizes is displayed in Figure 4. A forest plot that includes the mean effect size with the addition of each successively smaller study (Figure 5) demonstrates that the smaller studies exerted little bias and shifted the effect size only somewhat to the left (i.e., a greater reduction in loneliness). Orwin's (1983) fail-safe *N* indicated that 374 null studies would be required to reduce the effect size to –0.01 (an effect that is substantively equivalent to 0).

The between-study variance in effect size in the group of randomized group comparison studies was estimated as $T^2 = .01$. The *Q* test did not reject the null hypothesis of homogeneity (*Q* = 21.65, *p* = .30), and the I^2 showed that only 12.25% of the observed variance was attributable to between-study variance. However, because the upper confidence interval for I^2 approached 48%, and for comparability with prior analyses, moderator analyses were conducted.

The analog to the ANOVA test revealed that the difference among intervention types was significant (*Qb* = 7.73, *df* = 3, *p* = .05), and the 4 social cognitive training interventions (Chiang et al., 2009; Conoley & Garber, 1985; McWhirter & Horan, 1996; Williams et al., 2004) yielded greater
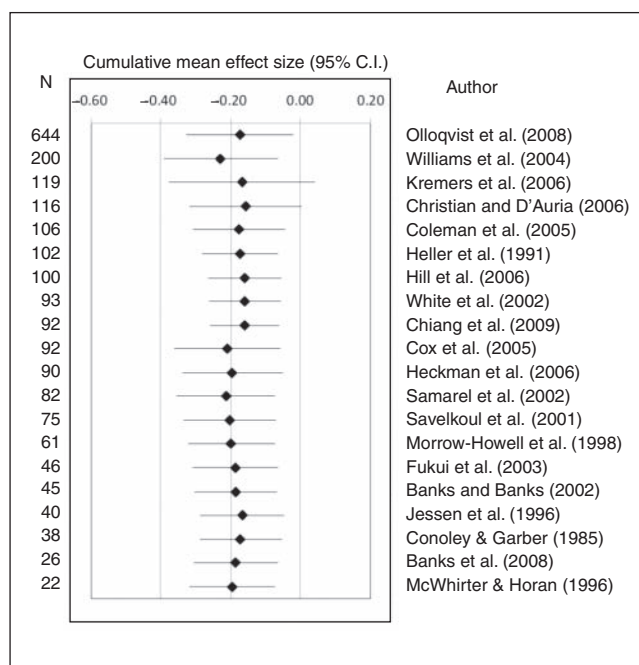
Figure 5. Forest plot showing results of cumulative meta-analysis of randomized group studies
Note: The mean effect size (and 95% CI) is recalculated with the addition of each successively smaller study.

loneliness reduction (mean effect size = –0.598, *p* = .001) compared to the 12 interventions to enhance social support (mean effect size = –0.162, *p* = .003), the 2 interventions to

improve social skills (mean effect size = 0.017, p = .90), and the 2 interventions to increase opportunities for social interaction (mean effect size = –0.062, p = .67). In addition, the mean effect size of the social support interventions did not differ significantly from the mean effect sizes of the social skills or social access interventions.

Neither group-based format nor the use of technology showed any moderating effects on the effect size.[15] In addition, the instrument used to measure loneliness did not moderate the effect size (Qb = 3.60, df = 2, p > .1).[16] The weighted regressions with each continuous moderator as the independent variable revealed that only gender composition had a moderating influence on the effect size.[17] Studies with more females in the sample showed a smaller reduction in loneliness. In summary, meta-analysis of the randomized group comparison studies revealed a small but significant effect of the interventions on loneliness. Of note, interventions that addressed maladaptive social cognition had a sizable mean effect compared to the other intervention types.

Discussion

Qualitative reviews of loneliness reduction interventions have identified diverse study designs (e.g., single-group pre-post studies, nonrandomized group comparisons, and randomized group comparisons) and intervention strategies (e.g., improving social skills, enhancing social support, increasing opportunities for social interaction, and addressing abnormal social cognition). Five of the six prior reviews, all of which were qualitative, concluded that certain interventions could reduce loneliness, although each review concluded that increased rigor was needed in evaluation of loneliness interventions.

The current study used meta-analytic techniques to determine quantitatively whether the outcomes of loneliness interventions varied based on study design, intervention type, or other study characteristic. Compared to single-group pre-post and nonrandomized group comparison studies, randomized group comparison studies had a small but significant mean effect size (–0.198, p < .05). Within this group, the mean effect size for interventions that addressed maladaptive social cognition was larger than that for interventions that attempted to improve social skills, enhance social support, or increase opportunities for social interaction. A primary criterion for empirically supported therapies is that they demonstrate efficacy in randomized controlled trials (Chambless & Hollon, 1998). By this criterion, our meta-analysis suggests certain interventions, particularly those that use CBT, can reduce loneliness.

Although the single-group pre-post studies and nonrandomized group comparisons exhibited larger mean effect sizes compared to the mean effect of randomized group comparisons, our confidence in the former studies is tempered. One reason is that single-group pre-post studies are vulnerable to

regression toward the mean, in which individuals who have high scores on a loneliness measure on one occasion are likely to score less extremely on a second occasion even if no intervention had occurred (Weeks, 2007). A second reason why results of pre-post studies should be viewed with caution is that loneliness may serve its adaptive purpose and motivate reconnection with others such that the group, on average, improves over time without intervention. Our meta-analysis of these studies indicated there was indeed a lowering of loneliness as measured before and after the interventions, but whether this result was because of the interventions, regression toward the mean, or the adaptive function of loneliness cannot be determined.

Nonrandomized group comparison studies also have important design flaws, including regression toward the mean and selection bias. Selection bias occurs when assignment of individuals to the experimental or control group is not random but is based on convenience, participant preference, or some other factor. When this occurs, individuals in the treatment arms may differ from individuals in the control arms in ways that affect the outcome of the studies. For example, people who volunteer to be in the treatment arm of a loneliness reduction study may be more gregarious by nature and may be more likely to become less lonely over time *regardless* of their exposure to the intervention. As a result, although our results suggest that nonrandomized group comparison interventions might be effective, it cannot be determined whether this finding is because of the interventions or a combination of regression toward the mean and selection bias.

In contrast, randomized group comparison studies eliminate selection bias and minimize the effect of regression toward the mean. The plurality of the intervention studies in our meta-analysis were randomized group comparison studies, and the mean effect size in this group (–0.198) was significantly different from zero. To interpret this effect size in familiar units, the 6 randomized studies that used the 20-item UCLA Loneliness Scale and reported loneliness means and standard deviations were further evaluated (Chiang et al., 2009; Coleman et al., 2005; Conoley & Garber, 1985; Hill, Weinert, & Cudney, 2006; Jessen, Cardiello, & Baun, 1996; Samarel et al., 2002). Using formulas provided by Lipsey and Wilson (2001), the pooled mean (41.17) and standard deviation (8.05) for the control groups were calculated. With an intervention effect size of –0.198, the average treatment group scored 0.198 standard deviations lower in loneliness, which is equivalent to 8.05×0.198 = 1.59 units on the UCLA Scale. Thus, with the control group mean at 41.17, the reduction in loneliness in the average treatment group was equivalent to a decrease from 41.17 to 39.58 on the UCLA Loneliness Scale. By comparison, a previous survey of 301 healthy, community-living individuals older than age 65 yielded a mean UCLA Loneliness Score of 31.5 with a standard deviation of 6.9. Because clinical significance is defined

as "returning to normal functioning" (Jacobson, Roberts, Berns, & McGlinchey, 1999), a 1.59-point decrease in the UCLA Loneliness score clearly did not return study participants to the level of healthy, community-living individuals. Moreover, a meta-analysis of 302 social and behavioral intervention meta-analyses (reviewed in Lipsey & Wilson, 2001) showed that, on average, interventions in this field have generated a mean effect size of 0.50. A mean effect size of –0.198 falls in the bottom 15% of this distribution, suggesting that loneliness interventions to date have not attained the degree of efficacy achieved by interventions targeting other social and behavioral outcomes.

On the other hand, despite not returning to the level of healthy, community-living adults, the small reduction in loneliness score is consistent with the notion of "improved but not recovered" (Jacobson et al., 1999). In addition, the mean effect size of the four randomized group comparisons that addressed abnormal social cognition was –0.598, which is comparable to the mean effect size found by Lipsey and Wilson (2001) for more than 300 social and behavioral meta-analyses. We did not convert the mean effect size of social cognition interventions to a reduction in the UCLA Loneliness Scale because there were only four studies of this type. Although well-designed loneliness reduction interventions achieved only modest success on average, interventions that address abnormal social cognition show promise in reducing loneliness. This result is consistent with the important role that social cognition plays in the development and persistence of loneliness (Cacioppo, Fowler, et al., 2009; Cacioppo & Hawkley, 2009; Hawkley et al., 2007). The surprisingly small effects of interventions to increase opportunities for social interaction or enhance social support suggest that reducing social isolation does not necessarily reduce loneliness. Nevertheless, the causes of loneliness are likely unique in each person, and matching specific therapies with specific interventions is worth further investigation and may prove valuable in future studies.

The reliable change index (RCI) was used to determine the reliability of a 1.6-point change in the UCLA Loneliness Scale (Jacobson & Truax, 1991). This index ensures that the degree of change is of sufficient magnitude to exceed the margin of measurement error. As such, the RCI is calculated as the posttest score minus the pretest score, divided by the standard error of the difference between these two scores. Using this formula as well as 8.1 as the standard deviation for the experimental group posttest and .7 as the test–retest reliability of the measure, the RCI of a 1.6-point reduction in the UCLA Loneliness Scale is 0.26. Values exceeding 1.96 are considered to be in the "recovered" zone, so with an RCI of 0.26, the most we can say is that these interventions achieve, at best, only modest improvement but not recovery. Thus, there is a need for improvements in interventions to reduce loneliness if clinically significant improvements are to be achieved.

Are there particular intervention types, formats, modes, or population characteristics that make some interventions more likely to succeed than others? Authors have suggested that interventions that enhance opportunities for social interaction via group activities or group-based interventions tend to be more successful (Cattan et al., 2005; Cattan & White, 1998). However, simply bringing lonely people together may not result in new friendships because the thoughts and behaviors of lonely individuals make them less attractive to one another as relationship partners (Jerrome, 1983; Stevens, 2001). To determine whether group-based interventions or other interventions characteristics moderated study efficacy, effect sizes in each study design group were first subjected to a test of homogeneity. This analysis revealed that the percentage of variance that could be attributed to between-study variation declined going from single-group to nonrandomized comparison to randomized comparison studies (61.43% to 18.63% to 12.25%). A significant Q statistic indicated heterogeneity of effect sizes among the single-group pre-post studies. However, the Q statistic was not significant for the nonrandomized and randomized group comparison studies. Because this statistic has low power to detect heterogeneity in small sample sizes, moderator analyses within each design type were conducted.

Intervention type as a moderator in single-group pre-post studies was examined first. Although effect sizes varied widely in these studies, intervention type did not explain this difference. In other words, increasing opportunities for social interaction were not more effective than enhancing social support or addressing abnormal social cognition. Because none of the single-group pre-post studies utilized social skills training, the hypothesis that this intervention can increase intervention success could not be tested. The effect sizes varied much less in the nonrandomized and randomized comparison studies. Moderator analyses revealed intervention type did not explain effect size variation in the nonrandomized comparison studies. In contrast, moderator analysis indicated that intervention type explained some of the variation in effect size among randomized comparison studies. Namely, the 4 social cognitive training interventions yielded greater loneliness reduction compared to the interventions designed to enhance social support, social skills, or opportunities for social intervention.

Most of the single-group pre-post studies utilized a group-based format and did not include a technology-based component. Therefore, moderator analyses were not performed using these variables in single-group studies. In contrast, both the nonrandomized and randomized group comparison studies utilized a greater variety of intervention formats and modes and were therefore subjected to moderator analyses using these variables. Among both the nonrandomized and randomized group comparison studies, group-based interventions were no more effective than individual-based interventions. In contrast, the use of technology-based

interventions was associated with greater efficacy among the nonrandomized studies. The reason for this is not clear but may be because of selection bias. Specifically, when randomization is not present, those who receive the intervention may be more predisposed to loneliness reduction compared to those who do not. Results from the randomized studies support this hypothesis as the presence of a technology component did not enhance their effect size. Stated another way, random assignment effectively removed the apparent advantage of the technology component. This finding is somewhat disappointing as technology-based interventions have been helpful in managing other chronic diseases (Celler, Lovell, & Basilakis, 2003; Gaikwad & Warren, 2009). Our results indicate that loneliness reduction interventions have yet to harness the power of technology.

Of note, studies that used the UCLA Loneliness Scale showed greater reductions in loneliness compared to studies that used other loneliness measures. This was true for the single-group pre-post studies and the nonrandomized group comparison studies but not for the randomized group comparison studies. The reason for this may be uninteresting. Of the 50 studies analyzed, 6 were from the C. M. Fokkema and van Tilburg (2007) article. All of these studies used the De Jong Gierveld Loneliness Questionnaire, and all were solicited from diverse public and private organizations as pilot studies, in contrast to the more focused professionally led studies that used the UCLA Loneliness Scale. Many of the latter found large effect sizes, especially among the single-group pre-post studies. Other explanations are also possible, including a longer duration of the U.S. interventions (which primarily used the UCLA Loneliness Scale) compared to pilot studies in the Netherlands (which used the De Jong Gierveld Loneliness Questionnaire) as well as cultural differences in perceptions of loneliness treatment in the two countries. These explanations may be moot, however, as no differences in effect size were found as a function of loneliness measure in the randomized group comparison design.

In the nonrandomized group comparison studies, participant age and proportion of female participants were inversely related to effect size, whereas the intervention duration and number of sessions did not have a moderating effect. These relationships were generally not present in the single-group pre-post test or the randomized group comparison studies. This inconsistency is difficult to explain but may be because of selection bias in which, for example, especially lonely older individuals volunteered to be in the treatment arm of the studies among elders, thereby blunting the effect of the treatment. The lack of association between effect sizes and age or intervention duration among the 20 randomized group comparison studies supports the notion of selection bias as an explanation among nonrandomized studies. As shown in Table 3, there was significant variation in duration of intervention, ranging from 10 days to 8 months.

On the other hand, the gender composition of the sample moderated the effect size in both the nonrandomized and the randomized group comparison studies. The greater the proportion of males in the study, the greater the effect of the intervention. Said differently, males were more responsive to the interventions than females. In the case of the nonrandomized studies, one could argue that women with more resistant forms of loneliness may have been drawn to studies with higher proportions of women. The fact that this gender difference was also observed in the randomized studies suggests a different interpretation. Females tend to be more self-reliant than males in finding and maintaining meaningful social relations, and interventions may therefore be more impactful in assisting males to forge a sense of connectedness and belonging. Conversely, the majority of participants in the randomized studies were older. Of the 20 randomized studies, 10 included adults aged 60 years and older, 6 included middle-aged adults, 3 focused on young adults, and only 1 included children. Given the disproportionate rates of widowhood among older women compared to older men, it is likely that many of the female study participants were widowed. Therefore, loneliness among widowed females may be more intransigent if they have failed to meet their social needs despite their stereotypical advantage in forming meaningful social relationships. This issue requires further examination to determine whether marital status- or gender-specific therapies are indicated.

An important finding of the randomized group comparison studies is that the four interventions that addressed maladaptive social cognition yielded greater reductions in mean loneliness scores compared to the other intervention types. Although none of studies that addressed social cognition utilized precisely the same intervention, all included a form of CBT or psychological reframing. Therefore, these studies begin to fulfill the criterion that the intervention be replicated by independent research groups to be considered empirically supported (Chambless & Hollon, 1998). The 12 studies that enhanced social support were associated with a much smaller effect size, and this effect did not differ from those of interventions that focused on social skills development ($n = 2$) or increased opportunities for social interaction ($n = 2$).

Limitations

The current study is at risk for the same limitations as other systematic reviews. Namely, despite a concerted effort, it is possible that our literature search failed to identify one or more interventions that met our study criteria. As mentioned above, this would be important only if such interventions were randomized group comparisons and showed nonsignificant treatment effects. Compared to studies with positive results, those with negative results are less likely to be

published. Exclusion of studies because of the "file-drawer" effect would weaken the conclusion that loneliness interventions have met with some success. However, our analyses indicated that as many as 374 null results would be needed to abolish the significant effect found here. A second potential limitation is our use of studies either published in English or described by an English translation of a Dutch review (C. M. Fokkema & van Tilburg, 2007). It is possible that randomized group interventions published in non-English journals demonstrated greater efficacy in reducing loneliness and that the intervention effect was therefore underestimated. Also, although our literature search did not exclude any age groups, only 5 studies evaluated interventions in children, and only 18 studies evaluated interventions among adults younger than age 60 years. Therefore, the extant literature on loneliness speaks most clearly to interventions among older adults.

In addition, studies in this meta-analysis did not distinguish between social and emotional loneliness. Although various studies have provided evidence that the experience of loneliness can be partitioned into separable dimensions, including social and emotional loneliness (Weiss, 1973), these features have also been found to be highly correlated, and their antecedents and consequences have been found to be sufficiently overlapping that loneliness is generally conceptualized and measured as a unidimensional construct (Hawkley et al., 2005; Russell, 1996; Russell et al., 1980). Because measures of social and emotional loneliness were typically not provided by the studies in this meta-analysis, the effect of various interventions on these dimensions of loneliness was not evaluated. Measurement of these dimensions in future interventions may permit investigators to determine whether certain interventions are more successful in reducing social versus emotional loneliness.

Conclusion

This report is the first to analyze loneliness reduction strategies in a quantitative manner. Previous reviews noted the dearth of well-designed intervention studies but found evidence that specific interventions showed promise in reducing loneliness. These included programs to improve social skills, enhance social support, increase opportunities for social interaction, and address deficits in social cognition. Importantly, intervention type did not differ across study design; each of these strategies was implemented in each of various study design types, including single-group pre-post evaluations, nonrandomized group comparisons, and randomized group comparisons. A consensus existed in the literature that these interventions were successful across the array of study designs, and our meta-analysis revealed that success was achieved in all three study design types. Given their design superiority, our analysis focused primarily on randomized group comparison studies and found a small but statistically significant effect of loneliness reduction interventions in this group.

Moderator analysis demonstrated that, among the randomized studies, interventions that addressed maladaptive social cognition had a larger mean effect size compared to interventions that addressed social support, social skills, and opportunities for social intervention. This result is consistent with our model of loneliness as regulatory loop (Cacioppo & Hawkley, 2009), in which lonely individuals have increased sensitivity to and surveillance for social threats, preferentially attend to negative social information (Cacioppo, Norris, Decety, Monteleone, & Nusbaum, 2009), remember more of the negative aspects of social events (Duck, Pond, & Leatham, 1994), hold more negative social expectations (Cacioppo & Hawkley, 2005), and are more likely to behave in ways that confirm their negative expectations. This loop has short-term self-protective features but over the long term heightens cognitive load, diminishes executive functioning, and adversely influences physical and mental health and well-being. Among the four intervention types, addressing maladaptive social cognition most directly addresses this regulatory loop. Therefore, our results shed light on the nature and mechanisms underlying loneliness and are consistent with the latest theories regarding this condition.

As for future directions, the recommendation of previous review authors to improve study design should be heeded. However, although randomized group comparisons provide the most internally valid results, nonrandomized studies can provide valuable insights. Investigators will have to consider whether randomized studies, which place lonely individuals into a usual-care or wait-list group, are ethical, especially given the potential negative health effects of untreated loneliness. Future interventions should also incorporate current understanding regarding the nature of loneliness. Of primary importance is an acknowledgment that loneliness is not equivalent to social isolation. Loneliness is the social equivalent of physical pain and, like physical pain, is functional in motivating individuals to alleviate the social pain by seeking out the connections they need to feel safe, secure, and content with life. For individuals who have a rich and forgiving social environment, loneliness has a high probability of accomplishing its purpose of motivating interactions and enhancing a sense of connectedness and belonging. For other individuals, however, loneliness becomes inescapable, and it is for these individuals that interventions are perhaps most necessary. Results from this meta-analysis suggest that correcting maladaptive social cognition offers the best chance for reducing loneliness. Given that temporal trends are placing an increasing number of individuals at risk for this condition, it is critical that results of this study be considered when designing interventions to address the potentially rising tide of loneliness.

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Declaration of Conflicting Interests

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Notes

1. For studies that reported sample age only as a threshold (e.g., 75 years or older), the threshold age was used as the mean age of the sample (Banks & Banks, 2002).

2. Allen-Kosal (2008): The sample was third grade children; the mean age was inferred to be 8 years old. Banks, Willoughby, and Banks (2008): The sample was institutionalized elderly people; the mean age was inferred to be 75 years old. Bauminger (2007): The sample age ranged from 7 years and 7 months to 11 years and 6 months; the mean age was inferred to be 9 years old. Conoley and Garber (1985): The sample was college students; the mean age was inferred to be 20 years old. Hill, Weinert, and Cudney (2006): The sample age ranged from 35 to 65 years old, with 92% older than 40 years old; the mean age was inferred to be 52 years old. Kraut et al. (1998): The sample was 93 families with both teens and adults; the mean age was not calculated because of the heterogeneous nature of the sample.

3. Banks et al. (2008) and Winningham and Pike (2007) did not report the gender composition of their samples. However, because their samples were both institutionalized older adults, we inferred the gender composition to be 80% female, the same as reported for an institutionalized sample in Banks and Banks (2002).

4. Marshall, Bryce, Hudson, Ward, and Moth (1996) and three studies listed in Fokkema and van Tilburg (2007): (a) buddy care for homosexuals, (b) elderly support home visits, and (c) group activities in residential homes.

5. Four studies did not provide information on intervention frequency: Marshall et al. (1996) and three studies listed in Fokkema (unpublished): (a) buddy care for homosexuals, (b) elderly support home visits, and (c) good company in a big home. Five had interventions that provided computer or Internet access; thus, no exact number of intervention sessions available: T. Fokkema and Knipscheer (2007), Hill et al. (2006), Kraut et al.

(1998), White et al. (1999), White et al. (2002). Three studies had intervention frequencies that varied among participants: Stewart, Reutter, Letourneau, and Makawarimba (2009), because of the unpredictable nature of homeless youth; Petryshen, Hawkins, and Fronchak (2001), because participants were offered a choice from about 200 group activities; and Morrow-Howell, Becker-Kemppainen, and Lee (1998), because of the different level of needs and suicide risks of their sample. Two studies had interventions that were in effect continuously for a period of time and thus could not be quantified into sessions: Jessen, Cardiello, and Baun (1996), who placed a caged bird in participants' rooms for 10 days; and Ollonqvist et al. (2008), who implemented an intervention that included three separate inpatient periods over 8 months.

6. Other loneliness measures included the following: (a) 15-item Emotional/Social Loneliness Inventory (Vinconzi & Grabosky, 1987) used in Stewart, Craig, MacPherson, and Alexander (2001); (b) 15-item short version of the Social and Emotional Loneliness Scale for Adults (DiTommaso, Brannen, & Best, 2004) used in Yarnoz, Plazaola, and Etxeberria (2008); (c) 16-item Loneliness Scale (Asher, Hymel, & Renshaw, 1984) used in Bauminger (2007) and Christian and D'Auria (2006); (d) 24-item Loneliness Scale (Asher & Wheeler, 1985) used in Kolko, Loar, and Sturnick (1990) and Allen-Kosal (2008); (e) 7-item loneliness scale (Paloutzian & Ellison, 1982) used in K. Heller, Thompson, Trueba, Hogg, and Vlachos-Weber (1991); (f) Philadelphia Geriatric Center Morale Scale (6 items on loneliness) used in E. O. Cox, Green, Hobart, Jang, and Seo (2007); (g) Frequency of loneliness (Older Americans Resources and Services Social Resource Rating Scale) used in Morrow-Howell et al. (1998); and (h) a single question asking the participants if they feel lonely used in Rosen and Rosen (1982) and Ollonqvist et al. (2008).

7. Hopps, Pepin, and Boisvert (2003) and Shapira, Barak, and Gal (2007) reported one-way ANOVA F statistics; Morrow-Howell et al. (1998), White et al. (2002), and Williams et al. (2004) reported t test statistics; and Rosen and Rosen (1982) reported χ^2 statistics.

8. In Banks and Banks (2002), the effect size was obtained from a one-way ANCOVA with the pretest loneliness score as a covariate, and in Savelkoul, de Witte, Candel, Van Der Tempel, and Van Den Borne (2001) the effect size was calculated by the authors from a multivariate regression model with pretest loneliness and self-reported functional health as covariates.

9. Five of these six largest effect sizes were from the delayed posttreatment measures of the primary interventions reported in our analyses. In Allen-Kosal (2008), the largest effect size was from an alternative intervention that contains only the pretraining component of the full intervention.

10. There is no social skills training intervention in this group of studies. The mean effect size of the social cognitive training interventions is -1.58 ($n = 3$; 95% CI $= -3.18, 0.02$; $p = .053$) as opposed to -0.340 ($n = 5$; 95% CI $= -0.49, -0.19$; $p < .001$) for social support interventions and -0.273 ($n = 4$; 95% CI $= -0.48, -0.07$; $p < .01$) for social activity and access interventions.

11. Gender composition of the sample ($\beta = -0.16$, $z = -0.43$, $p > .6$), mean age of the sample ($\beta = -0.002$, $z = -0.29$, $p > .7$), intervention duration ($\beta = -0.001$, $z = -0.26$, $p > .7$), and number of intervention sessions ($\beta = -0.007$, $z = -1.30$, $p = .20$).

12. The mean effect size for group-based interventions was -0.53 ($n = 14$; 95% CI $= -0.85$, -0.21; $p < .01$); for individual-based interventions it was -0.16 ($n = 4$; 95% CI $= -0.49$, 0.16; $p > .3$).

13. The mean effect size for studies that used the UCLA scale was -0.75 ($n = 8$; 95% CI $= -1.27$, -0.24; $p < .01$), for the De Jong Gierveld group it was -0.04 ($n = 7$; 95% CI $= -0.23$, 0.14; $p > .6$), and for other measures it was -0.77 ($n = 3$; 95% CI $= -1.46$, 0.08; $p = .03$).

14. T. Fokkema and Knipscheer (2007) was removed as an outlier in this analysis because their intervention lasted for 3 years as opposed to the average of 16.4 weeks for the rest of the group.

15. For group- versus individual-based comparison, $Qb = 0.87$, $df = 1$, $p > .3$. The mean effect size was -0.15 ($n = 10$; 95% CI $= -0.28$, -0.02; $p < .05$) for the group-based intervention and -0.27 ($n = 10$; 95% CI $= -0.50$, -0.05; $p < .05$) for the individual-based intervention. For technology-based versus nontechnological interventions, $Qb = 0.31$, $df = 1$, $p > .5$. The mean effect size was -0.16 ($n = 7$; 95% CI $= -0.31$, 0; $p = .5$) for the technology-based interventions and -0.23 ($n = 13$; 95% CI $= -0.41$, -0.04; $p = .01$) for studies using no technology.

16. The mean effect size was -0.28 ($n = 13$; 95% CI $= -0.48$, -0.08; $p < .05$) for the UCLA group, 0.05 ($n = 2$; 95% CI $= -0.23$, 0.34; $p > .7$) for the De Jong Gierveld group, and -0.16 ($n = 5$; 95% CI $= -0.28$, -0.03; $p = .01$) for other loneliness measures.

17. Gender composition of the sample ($\beta = 0.42$, $z = 2.16$, $p < .05$), mean age of the sample ($\beta = 0.001$, $z = 0.16$, $p > .8$), intervention duration ($\beta = -0.004$, $z = -0.81$, $p > .4$), and number of intervention sessions ($\beta = -0.003$, $z = -0.39$, $p > .6$).

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