The Effect of Telephone-Administered Psychotherapy on Symptoms of Depression and Attrition: A Meta-Analysis

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Increasingly, the telephone is being used to deliver psychotherapy for depression, in part as a means to reduce barriers to treatment. Twelve trials of telephone-administered psychotherapies, in which depressive symptoms were assessed, were included. There was a significant reduction in depressive symptoms for patients enrolled in telephone-administered psychotherapy as compared to control conditions (*d* = 0.26, 95% confidence interval [CI] = 0.14–0.39, *p* < .0001). There was also a significant reduction in depressive symptoms in analyses of pretreatment to posttreatment change (*d* = 0.81, 95% CI = 0.50–1.13, *p* < .0001). The mean attrition rate was 7.56% (95% CI = 4.23–10.90). These findings suggest that telephone-administered psychotherapy can produce significant reductions in depressive symptoms. Attrition rates were considerably lower than rates reported in face-to-face psychotherapy.

Key words: **depression, meta-analysis, psychotherapy, telemental health.** ***[Clin Psychol Sci Prac 15: 243–253, 2008]***

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The telephone was invented by Alexander Graham Bell in 1876. The first report of telemedicine in a major medical journal, which described the use of the telephone to diagnose a child's cough, occurred three years later in 1879 ("The Telephone as a Medium of Consultation and Medical Diagnosis," 1879). The telephone quickly became a widely used tool in the practice of primary-care medicine. In contrast, providers of psychotherapy were slow to adopt the telephone to deliver mental health–related services. To the best of our knowledge, the first report of the use of the telephone in the administration of psychotherapy was published in 1949, 70 years after the first telemedicine report (Berger & Glueck, 1949). In 1996, a report developed by an American Psychological Association task force found that empirical evidence concerning telephone-administered psychotherapy was scant to nonexistent (Haas, Benedict, & Kobos, 1996). In the last decade, this has changed considerably.

Most of the work in telephone-administered psychotherapy has focused on treating depressive symptoms. Depression is common and is a significant cause of disability (Murray & Lopez, 1997). Psychotherapy is an attractive treatment option for many patients, as evidenced by the finding that approximately two-thirds of depressed patients prefer psychotherapy over antidepressant medication (Bedi et al., 2000; Brody, Khaliq, & Thompson, 1997; Dwight-Johnson, Sherbourne, Liao, & Wells, 2000; Priest, Vize, Roberts, Roberts, & Tylee, 1996). However, only 20% of all patients referred for psychotherapy ever enter treatment (Brody et al., 1997; Weddington, 1983) and, of

those who enter, nearly one-half will drop out (Wierzbicki & Pekarik, 1993). One reason for this discrepancy between interest and failure to initiate or follow through with psychotherapy is that there are considerable barriers for many patients, including time constraints, transportation problems, caregiving responsibilities, stigma concerns, disability, or living in a rural area that lacks adequate mental health services (Alvidrez & Azocar, 1999; Hollon et al., 2002; Mohr et al., 2006; Yuen, Gerdes, & Gonzales, 1996). Indeed, a recent study of primary-care patients found that 74% of depressed patients identify one or more barriers that make it very difficult or impossible to attend regularly scheduled psychotherapy sessions (Mohr et al., 2006). Many of these barriers could potentially be mitigated through the use of the telephone in administering psychotherapy.

The telephone, found in 95.5% of all households in the United States (Federal Communications Commission, 2003), is the most widely available telecommunications medium. Recognizing the potential for outreach, many provider organizations, including insurance companies, health maintenance organizations, the United States Veterans Health Administration, and others, have begun implementing telemental health procedures, including telephone-administered psychotherapy (Maheu, Pulier, Wilhelm, McMenamin, & Brown-Connolly, 2005; VHA Telemental Health Field Work Group, 2003). Furthermore, more than two-thirds of psychologists use telephone-administered psychotherapy to some extent in their practice (VandenBos & Williams, 2000).

Accordingly, telephone-administered psychotherapy —while slow to be developed and implemented—is now increasingly part of the mental health care landscape. There have also been a growing number of empirical studies evaluating the utility of telephone-administered psychotherapy. In light of these developments, it is time to take a first "snapshot" of this research. Accordingly, we have undertaken a meta-analytic review with the following aims:

(1) To evaluate the efficacy of telephone-administered psychotherapies in reducing symptoms of depression. We planned to evaluate telephone-administered psychotherapy compared with treatment-as-usual (TAU) and, if significant, evaluate the magnitude of change in depressive symptoms from pretreatment to posttreatment in telephone-administered psychotherapy.
(2) To obtain an estimate of the attrition rate from telephone- administered psychotherapy.

When analyses revealed significant unexplained variability, we planned to evaluate the effects of four potential moderators: treatment orientation, treatment format, specialization of therapist, and therapist level of training.

METHODS

Identification of Studies

MEDLINE and PsycINFO were searched in March 2006, using key words "telephone" and "psychotherapy or counseling or therapy." References from reviewed articles were checked. Requests for unpublished or prepublished data were made through list serves, including the American Psychological Association's Division 12 and Division 38 list serves and the Society for Behavioral Medicine.

Inclusion Criteria

A trial was included if it met the following conditions:

(1) The study was a trial of a telephone-administered form of psychotherapy. Only randomized controlled trials (RCTs) were included in the analysis of controlled trials, while all trials, including single arm trials, were included in the evaluation of pretreatment versus posttreatment effects.
(2) All contact between therapists and patients was over the telephone; no face-to-face contact between therapist and patient occurred.
(3) Intervention had to include at least four sessions, thereby eliminating evaluations of telephone hotlines, crisis counseling services, etc.
(4) Treatments had to include a treatment manual or have a clearly identified treatment approach.
(5) Interventions had reduction in depressive symptoms as a treatment outcome.
(6) A validated measure of depressive symptoms was required. Measures of depressive symptoms were selected on the basis of consistency with other studies in the trial (e.g., if two measures were used, the measure that was used most often in other studies was used for this meta-analysis). Self-report measures were selected where possible to avoid any potential effects of detection bias.
(7) Participants were adults.

Data Extraction

Two investigators (DCM, LV) independently reviewed the identified studies for inclusion and pertinent data. All effect sizes, standard errors, and weights were calculated according to methods described by Lipsey and Wilson (2001). For analyses comparing telephone-administered psychotherapy to a control condition, the standardized mean difference of the posttreatment scores was computed as the effect size of interest. For all but one study, means and standard deviations were used in this calculation; the effect size from Lynch, Tamburrino, and Nagel (1997) was calculated using the means and t-value as Lynch did not provide standard deviations. The pre–post analyses of telephone-administered psychotherapy used the standardized mean gain computed from pretreatment and posttreatment depression scores as the effect sizes. Attrition analyses used the proportion of participants who dropped out of treatment from the time of randomization to the end of treatment (i.e., dropouts/total randomized).

When data were insufficient to calculate an effect size, study authors were contacted and data were requested. In two instances, requisite additional data were provided (Simon, Ludman, Tutty, Operskalski, & Von Korff, 2004; Tutty, Simon, & Ludman, 2000). Several studies included two control conditions, a TAU condition and a second control condition that included a minimal intervention (attention control or case management; Heckman et al., 2006; Sandgren & McCaul, 2003; Simon et al., 2004). In these cases, the TAU condition was used to improve consistency in control conditions across studies.

Statistical Analyses

Mean effect size across the studies for each of the three analyses was calculated using the mean effect size macro written for the statistical software program SPSS and found through Lipsey and Wilson (2001). Analyses of telephone-administered psychotherapy versus control condition effect sizes used the standardized mean difference effect size. Pretreatment versus posttreatment analysis effect sizes used the standardized mean gain. Attrition was calculated using the proportion of participants who dropped out of therapy with the number of subjects randomized to telephone treatment as the denominator. Subgroup analyses for a priori and post hoc hypotheses were performed using Lipsey and Wilson's ANOVA macro in SPSS. These analyses included treatment orientation, treatment format, therapist specialization, and therapist level of training. A random effects model was used in both mean effect size analyses and subgroup analyses if the homogeneity analysis (Q-test) was significant, indicating significant heterogeneity between studies.

RESULTS

Fifty-one studies were identified and were reviewed in detail. Of these, 12 studies satisfied the inclusion criteria. Of those satisfying inclusion criteria, nine were RCTs (Bailey, Mishel, Belyea, Stewart, & Mohler, 2004; Heckman et al., 2006; Heckman & Carlson, 2007; Lynch et al., 1997; Miller & Weissman, 2002; Mohr et al., 2000; Napolitano et al., 2002; Sandgren & McCaul, 2003; Simon et al., 2004), one was an RCT that employed two different types of telephone-administered treatments, both of which qualified as bona fide telephone-administered psychotherapies under the criteria for this meta-analysis (Mohr et al., 2005), one was a controlled study but the comparison group was drawn from another study and thus was not randomized (Tutty et al., 2000), and one was a single arm outcome study (Mohr, Hart, & Marmar, 2006). The characteristics of the included studies are provided in Table 1. Of the 39 that were rejected, most were rejected for multiple reasons. The primary reasons for the exclusion of each study were as follows: 12 studies were not clinical trials (e.g., they were surveys, case studies, or nondata-driven papers), 5 studies included face-to-face meetings with the therapist, 4 studies did not include four sessions (they only included one or two), 10 studies examined interventions that did not target depression (these studies targeted behaviors such as adherence to medical regimens or cancer screening, change in health behaviors such as smoking cessation, and anxiety), and 4 studies did not provide sufficient data to calculate effect sizes, and the authors were unable to provide the data.

Telephone-Administered Psychotherapy versus Control Conditions

There were 10 studies that had control conditions that could be included in this analysis. There was no statistically significant evidence of heterogeneity across these studies ($Q = 10.57$, $p = .31$). The mean effect size was $d = 0.26$, 95% confidence interval (CI) = 0.14–0.39, $p < .0001$. These results are displayed in Table 2.

Table 1. Characteristics of included studies

Author	Year	Telephone treatment orientation	Control condition & assignment	Tx format	Number of sessions	Mental health specialist	Therapist	Outcome measure	Baseline depression mean (SD)	Psych Dx and exclusions	Comorbid Med Dx	Mean patient age	Patient gender
Bailey	2004	Uncertainty intervention	Randomized—TAU	Individual	5 weekly sessions	No	Nurse	POMS-SF-D	Experimental Tx = 3.55(5.5); Control Tx = 1.53(2.6)	No information	Prostate cancer	75	100% male
Heckman	2006	CBT intervention	Randomized—wait-list control	Group	12 90-min sessions	Yes	MS/PhD psychologist	GDS	Experimental Tx = 17.3(8.2); Control Tx = 15.1(7.0)	MDD = 70%; partial remission of MDD = 21%; dysthymia = 6%; minor depressive disorder = 3%	HIV/AIDS	54	32% female
Heckman	2007	CBT intervention	Randomized—TAU or information support***	Group	8 sessions	Yes	MS/PhD psychologist	BDI	BDI = 22.1(10.5) for all treatment conditions	No information	HIV/AIDS	43	30% female
Lynch*	1997	CBT/problem solving	Randomized—TAU	Individual	6 weekly sessions	No	Students (psychology/MD/nurse)	HRSD	Experimental Tx = 14.4; Control Tx = 12.4	Minor depression	Family medicine clinic—no information on Dxs	49	87% female
Miller	2002	Interpersonal	Randomized—no treatment control	Individual	12 weekly sessions	Yes	PhD psychologist	HRSD	Experimental Tx = 8.34(5.39); Control Tx = 5.73(4.92);	Inclusion required Hx of depressive disorder. Excluded Dxs: bipolar, schizophreina or psychosis, severe depressive Sx	No information	32	100% female
Mohr	2000	CBT	Randomized—TAU	Individual	8 weekly sessions	Yes	Doctoral student/postdoctoral	POMS-D	Experimental Tx = 33.1(12.4); Control Tx = 27.9(12.1)	No information	Multiple sclerosis	42	72% female
Mohr	2005	CBT versus supportive emotion focused	Randomized comparative outcome study (2 telephone psychotreatment arms)	Individual	16 weekly sessions	Yes	PhD Psychologist	BDI-II	T-CBT: BDI = 27.0(7.8); T-supportive emotion-focused therapy = 28.3(7.9)	Current MDD = 71%. Excluded Dxs: severe psychopathology (psychosis, current substance abuse, current plan and intent to commit suicide, etc.)	Multiple sclerosis	48	77% female
Mohr**	2006	CBT	No control	Individual	8 weekly sessions	Yes	PhD psychologist	BDI-II	4/8 had current MDD and the other 4 were in partial remission; BDI = 34.3(10.3); HRSD = 23.8(5.1)	Current MDD = 50%. Excluded Dxs: severe psychopathology (psychosis, current substance abuse, current plan and intent to commit suicide, etc.)	Multiple chronic medical conditions	57	100% male
Napolitano	2002	CBT	Randomized—TAU	Individual	8 weekly sessions	Yes	Graduate student	GHQ-D	Experimental Tx = 14.8(2.9); Control Tx = 14.4(2.7)	No information	Lung transplant candidates	45	69% female

Sandgren	2003	Emotional expression	Randomized—TAU or education***	Individual	5 weekly sessions	No	Nurses	POMS-D	Experimental Tx = 8.5(8.9); Control Tx = 10.7(10.7)	Information unclear	Breast cancer	55	100% female
Simon	2004	CBT	Randomized—TAU by primary care (pharmacotherapy)	Individual	4 weekly sessions + 4 sessions as needed	Yes	Master's level	SCL-20	Experimental Tx = 1.5(0.58); Control Tx = 1.6(0.62)	Excluded Dxs: bipolar disorder, schizophrenia	Primary-care patients—no information on comorbidities	44	76% female
Tutty	2000	CBT	Nonrandomized—TAU by primary care (pharmacotherapy)	Individual	6 weekly sessions	Yes	Master's level	SCL-20	Experimental Tx = 2.0(0.5); Control Tx = 1.8(0.7)	Excluded Dxs: Bipolar disorder, schizophrenia, schizoaffective disorder, substance abuse	Primary-care patients—no information on comorbidities	48	65% female

* Not included in pre–post analysis due to lack of sufficient data.
** Not included in analysis of telephone-administered psychotherapy versus TAU due to absence of a control condition.
*** Two control arms.
Note: BDI, Beck Depression Inventory; CBT, cognitive–behavioral therapy; Dx, diagnosis; GDS, geriatric depression scale; GHQ-D, general health questionnaire–depression scale; HRSD, Hamilton rating scale for depression; Hx, history; POMS-D, profile of mood states–depression dejection scale; POMS-SF-D, profile of mood states–short form–depression dejection scale; SCL-20, symptom checklist–20 (depression scale); TAU, treatment-as-usual; Sx, symptoms; Tx, treatment.

Pretreatment to Posttreatment Effects of Telephone-Administered Psychotherapy

There were 12 studies containing pre–post data that could be used in this analysis. This analysis found a pre–post effect size of $d = 0.81$, 95% CI = 0.50–1.13, $p < .0001$. These results are displayed in Table 2. There was significant heterogeneity among these studies ($Q = 241.5$, $p < .0001$). Because there was significant heterogeneity, we examined the potential role of the following moderators: treatment orientation, treatment format, therapist specialization, and therapist level of training.

Treatment Orientation. Eight studies had orientations that were coded as having a fundamental cognitive-behavioral orientation, while four had other orientations, including interpersonal psychotherapy (Miller & Weissman, 2002), supportive emotion-focused therapy (Mohr et al., 2005), emotional expression therapy (Sandgren & McCaul, 2003), and an uncertainty intervention (Bailey et al., 2004). The random effects model, including treatment orientation, reached only a trend level of significance for the between-group homogeneity Q value ($Q_b = 3.08$, $p = .08$), suggesting but not confirming that cognitive-behavioral treatments were more effective ($d = 1.01$, $p < .0001$) than other treatment orientations ($d = 0.45$, $p = .08$).

Treatment Format. Ten studies used telephone intervention with individual patients while two applied the telephone intervention in groups (Heckman et al., 2006). We note that having only two studies in one comparison group may make these findings unreliable; however, the analyses are presented because they were part of the review's a priori hypotheses. The model, including treatment format, had a nonsignificant Q_b ($Q_b = 2.62$, $p = .11$), indicating that treatment format did not explain the heterogeneity.

Therapist Specialization. Ten studies used mental health specialists, while two used nurses (Bailey et al., 2004; Sandgren & McCaul, 2003). The random effects model, including mental health specialization, had a significant Q_b ($Q_b = 4.06$, $p = .04$) and the within-group homogeneity Q value (Q_w) was not significant ($p = .25$), indicating that mental health specification is sufficient to account for the heterogeneity in the effect size distribution. The treatments administered by mental health

Table 2. Effect sizes change in depressive symptoms

			Telephone-administered psychotherapy versus treatment-as-usual		Pretreatment versus posttreatment effects		Attrition	
			Outcome: depression severity		Outcome: depression severity		Outcome: attrition	
Study	N randomized to telephone-administered psychotherapy*	N randomized to control treatment*	Standard mean difference	95% CI	Standard mean gain	95% CI	Proportions (%)	95% CI
Bailey 2004	21	20	0.01	(–0.61, 0.63)	0.15	(–0.16, 0.46)	5.0	(–4.68, 14.7)
Heckman 2006	44	46	–0.04	(–0.45, 0.37)	0.30	(0.13, 0.47)	18.0	(6.4, 29.6)
Heckman 2007	108	107	0.08	(–0.19, 0.35)	0.26	(0.11, 0.41)	10.0	(4.2, 15.8)
Lynch 1997	15	14	1.08	(0.03, 2.13)			27.0	(5.7, 48.3)
Miller 2002	18	15	0.45	(–0.27, 1.17)	0.37	(0.16, 0.58)	17.0	(–0.4, 34.4)
Mohr 2000	16	16	0.86	(0.01, 1.71)	1.60	(0.83, 2.37)	31.0	(7.8, 54.2)
Mohr 2005 (T-CBT)**	62	N/A			1.27	(1.02, 1.52)	5.0	(–0.8, 10.8)
Mohr 2005 (T-SEFT)**	65	N/A			1.07	(0.86, 1.28)	6.0	(0.2, 11.8)
Mohr 2006	8	N/A			0.97	(0.43, 1.51)	0.0****	(0.0, 0.0)
Napolitano 2002	38	39	0.36	(–0.10, 0.82)	0.58	(0.37, 0.79)	5.0	(–2.7, 12.7)
Sandgren 2003	90	55	0.17	(–0.16, 0.50)	0.18	(0.03, 0.33)	1.0	(–0.9, 2.9)
Simon 2004	198	195	0.38	(0.17, 0.59)	1.50	(1.33, 1.67)	7.0	(3.1, 10.9)
Tutty 2000	28	94	0.35	(–0.08, 0.78)	2.10	(1.50, 2.70)	7.0	(–2.7, 16.7)
	Overall effect size		**0.26**	**(0.14, 0.39)**	**0.81*** **	**(0.50, 1.13)**	**7.4*** **	**(4.2, 10.9)**
	Chi-squared value		**Q = 10.6 (df = 9, *p* = .31)**		**Q = 241.5 (df = 11, *p* < .0001)**		**Q = 32.4 (df = 11, *p* = .0006)**	
	Z-value for overall effect		**4.18 (*p* < .0001)**		**5.07 (*p* < .0001)**		**4.44 (*p* < .0001, different from 0)**	

* For individual analyses N size changed according to data provided in each study (N = number).
** Mohr (2005) compared two telephone-administered psychotherapies; data presented separately for each treatment.
*** Effect size from the random effects model.
**** Not included in overall mean.

professionals produced significantly greater reductions in depressive symptoms compared with other professionals. Therapists in the mental health professions produced effect sizes of $d = 0.94$ (95% CI = 0.63–1.26, $p < .0001$), while treatments provided by non-mental health professionals did not produce statistically significant changes ($d = 0.17$, 95% CI = −0.52–0.85, $p = .63$). Again, because there are only two studies using non-mental clinicians, these findings may be unreliable and appropriate caution should be taken when interpreting these results.

Therapist Level of Training. Of the eight studies that did not use only doctoral-level therapists, two used nurses (Bailey et al., 2004; Sandgren & McCaul, 2003), two used advanced graduate students in psychology (Mohr et al., 2000; Napolitano et al., 2002), two used master's-level therapists (Simon et al., 2004; Tutty et al., 2000), and two used a mixture of master's-level therapists and PhD-level therapists (Heckman et al., 2006; Heckman & Carlson, 2007). The random effects model, including level of training, had a nonsignificant Q_b ($Q_b = 0.18$, $p = .67$).

Number of Sessions. Analyses were conducted to examine a post hoc hypothesis that number of sessions might be related to outcomes. Number of sessions intended by the protocol was used; the mean number of sessions actually completed was not consistently available. The number of sessions by protocol was not related to depressive symptoms (p = .61).

Attrition

There were 12 studies containing attrition data on the telephone-administered psychotherapies that could be used in this analysis. For all but two studies, the attrition rate was the number of dropouts from therapy divided by the total number of subjects randomized to the therapy group. Napolitano et al. (2002) noted that 2 of the 40 participants randomized to telephone-administered psychotherapy were removed from the study after randomization but before initiating treatment because they received a medical procedure that excluded their participation. We, therefore, considered the number enrolled to be 38 and did not count these two patients as dropouts. Miller &

Weissman (2002) reported that 15 subjects were randomized and 3 dropped out. However, Miller replaced the three dropouts. We, therefore, considered the number enrolled to be 18.

There was significant heterogeneity in attrition across these studies ($Q = 32.43$, $p = .0006$). As such, the mean attrition rate above was calculated using random effects estimation. These studies had a mean attrition rate of 7.56% (95% CI = 4.23–10.90, $p < .0001$). These results are displayed in Table 2. We examined the ability of the four variables noted above to explain the heterogeneity.

Treatment Orientation. Treatment orientation accounted for a significant portion of the variance in attrition ($Q_b = 14.17$, $p = .0002$), but this variable did not sufficiently account for all the excess variability in the effect size distribution, because the within-group homogeneity Q value remained significant ($Q_w = 18.26$, $p = .05$). Cognitive- behavioral treatment orientation was associated with significantly greater rates of attrition (M = 7.9%, 95% CI = 5.6–10.2, $p < .0001$), compared with other orientations (M = 2.1%, 95% CI = 0.09–4.03, $p = .04$).

Treatment Format. Treatment format accounted for a significant portion of the variance in attrition ($Q_b = 4.06$, $p = .04$), and this variable did sufficiently account for all the excess variability in the effect size distribution, because the within-group homogeneity Q value was not significant ($Q_w = 14.61$, $p = .15$). The group format was associated with significantly greater rates of attrition (M = 12.2%, 95% CI = 6.1–18.4, $p = .0001$), compared with the individual format (M = 5.4%, 95% CI = 2.9–7.9, $p < .0001$). However, as in the pre–post analysis, only two studies used a group format, rendering these findings as less reliable.

Therapist Specialization. The random effects model showed that mental health specialization explained a significant portion of the variability in attrition ($Q_b = 15.85$, $p = .0001$). This variable also sufficiently accounted for all the excess variability in the effect size distribution, because the within-group homogeneity Q value was not significant ($Q_w = 16.58$, $p = .08$). Studies with therapists in the mental health professions had a mean attrition rate of 7.6% (95% CI = 5.4–9.8, $p < .0001$), while those treated by non-mental health professionals had a nonsignificant mean attrition rate of 1.5% (95% CI = −0.57–3.6, $p = .15$).

Therapist Level of Training. The random effects model showed that level of interventionist training did not explain a significant portion of the variance ($Q_b = 0.04$, $p = .84$).

Number of Sessions. The number of sessions indicated by protocol was not significantly related to attrition ($p = .14$).

Analysis for Publication Bias

The potential for publication bias was analyzed by correlating the study sample size with the effect size. A significant positive correlation would raise the possibility that larger studies with larger effect sizes were more likely to be published. The relationship between sample size and treatment–control condition comparisons was $r = -.28$, $p = .43$, while the relationship between sample size and pre–post treatment outcomes was $r = .07$, $p = .83$. Thus, there was no evidence that these findings were influenced by publication biases.

DISCUSSION

This meta-analysis found that psychotherapy administered over the telephone was associated with significant reductions in depressive symptoms. The comparison of telephone-administered psychotherapy with control conditions, while meeting statistical criteria for significance, found a mean effect size of $d = 0.26$, which is somewhat less than the $d = 0.42$ reported in a meta-analysis that compared face-to-face psychotherapy to no-treatment controls (Wampold et al., 1997). However, the pre–post finding of $d = 0.82$ for telephone-administered psychotherapy is in line with many findings of meta-analyses of pre–post outcomes for face-to-face therapies in the $d = 0.71–0.73$ range (Nietzel, Russell, Hemmings, & Gretter, 1987; Robinson, Berman, & Neimeyer, 1990). Part of this discrepancy may be due to the control conditions used. Many of the control conditions used in studies included in this meta-analysis provided patients with active treatment conditions. For example, patients in the TAU condition in Simon's and Tutty's studies (Simon et al., 2004; Tutty et al., 2000) were under the care of primary-care physicians who prescribed antidepressant medications. Most of the remaining studies were conducted

with patients who had some form of severe medical condition (e.g., multiple sclerosis, lung cancer, breast cancer, AIDS), which put them in frequent contact with medical care providers who may or may not have prescribed medications (Bailey et al., 2004; Heckman et al., 2006; Mohr et al., 2000; Napolitano et al., 2002; Sandgren & McCaul, 2003). In contrast, many psychotherapy studies using no-treatment conditions prohibit any psychological or pharmacological intervention outside the study and/or do not include patients with medical conditions that bring them into frequent contact with physicians who could potentially identify and treat the depression.

The mean attrition rate was 7.6% across all the studies. A meta-analysis of 125 studies reporting dropout from face-to-face psychotherapy found a mean attrition rate of 46.9% (Wierzbicki & Pekarik, 1993). This figure may be somewhat higher than is found in clinical trials, as it included a broad range of studies. A review of 14 major clinical trials, including psychotherapy for depression, over the past 20 years found attrition rates ranging from 13.9% to 64.4% (Blackburn & Moore, 1997; DeRubeis et al., 2005; Elkin et al., 1989; Gallagher-Thompson & Steffen, 1994; Hollon et al., 1992; Keller et al., 2000; Miranda et al., 2003; Scott & Freeman, 1992; Shapiro et al., 1994; Thompson, Gallagher, & Breckenridge, 1987; Ward et al., 2000; Watson, Gordon, Stermac, Kalogerakos, & Steckley, 2003; Williams et al., 2000). These attrition rates fall outside the 95% CI for attrition from telephone-administered psychotherapy reported in this review. Therefore, these findings support recent observations that telephone administration of psychotherapy may reduce attrition by overcoming barriers to care (Mohr et al., 2005; Simon et al., 2004). However, as with the efficacy findings, due to differences in samples used in telephone-administered and face-to-face administered psychotherapy studies, it is premature to draw any firm conclusions regarding the relative attrition rates between telephone-administered and face-to-face administered psychotherapies.

There was significant heterogeneity across the studies for both the pre–post treatment and attrition analyses. Secondary analyses suggested that some of this variability could be accounted for by therapist specialization and that attrition may be higher in group treatments. In addition, cognitive–behavioral treatments had higher attrition rates, although this may have been driven by the group treatments. The secondary analyses accounting for unexplained variance are based on small numbers of analyses and are at best suggestive.

There are several other potential explanations for the heterogeneity in outcomes, including variability in baseline severity of depressive symptoms and the wide variety of comorbid illnesses across studies. Unfortunately, these could not be controlled for. The variety of measures for depressive symptoms used made it difficult to reliably rank the baseline depressive symptom scores on severity, thereby eliminating the possibility of covarying the effect of severity. Likewise, the populations from which telephone-administered psychotherapy study samples are drawn include a wide variety of primarily medical populations with barriers to treatment. Unfortunately, due to the degree of heterogeneity in comorbidities (seven studies focused on five different specific severe illnesses, one study identified a variety of chronic illnesses, three studies focused on medical practices with unspecified medical comorbidities, and one study targeted chronic depression), it was not possible to account for these comorbidities statistically.

A potential limitation in the study is that the variability in medical comorbidities may have contributed to the heterogeneity in outcomes in at least two ways. First, the various medical illnesses may have had variable effects on depressive symptoms and/or may have had a moderating effect on the efficacy of psychotherapy. Prevalence of depression in some of these illnesses exceeds prevalence in the general population, and it is possible that some depressive symptoms may result from the pathology or pathogenic processes of the medical disorders (Feinstein et al., 2004; Mohr & Cox, 2001; Then Bergh, Kumpfel, Trenkwalder, Rupprecht, & Holsboer, 1999). While a growing number of studies indicate that for many medical illnesses, such as cancer, heart disease, HIV, multiple sclerosis, and others, face-to-face administered psychotherapies for depression are highly effective (Elliott & Roy-Byrne, 1998; Lett, Davidson, & Blumenthal, 2005; Meyer & Mark, 1995), it remains unclear if these medical illnesses moderate the effects of psychotherapy.

A related potential problem lies in the measurement of depressive symptoms. Many of these illnesses produce symptoms that are confounded with symptoms of depression,

such as fatigue and diminished cognitive capacity. However, the comorbid illnesses targeted by the studies included in this meta-analysis are all chronic or have symptoms that continue longer than the treatment periods. Thus, while it is possible that medical illness may elevate depressive symptom scores at any single assessment time point, any decrease in depressive symptoms over time is most likely due to changes in depressive symptoms and not to the more chronic medical symptoms.

We also want to emphasize that it is premature to generalize the results of this meta-analysis broadly. Individual studies suggest specific uses under specific circumstances; for example, telephone therapies may provide added benefit compared to care for depressive symptoms by a primary-care physician or to no care at all. However, because the depression symptom outcomes used in this meta-analysis were self-report instruments, the generalizability of these findings to clinically diagnosable depressive disorders is limited (Kendall & Flannery-Schroeder, 1995). Furthermore, the measures of depression used in this study had a wide range of specificity and sensitivity (Minami, Wampold, Serlin, Kircher, & Brown, 2007). Thus, the aggregated effect size estimates for depressive symptom severity should not be used as any sort of benchmark. In addition, the level of heterogeneity across studies suggests that we do not yet understand the characteristics of patients for whom such telephone interventions may be effective, and those for whom telephone intervention may not be appropriate. The heterogeneity in the severity of depressive symptoms and in medical comorbidities in the samples also limits generalizability.

Another important limitation of the study is that attrition was not defined with any specificity beyond having dropped out at any point during the study. This was due to the fact that trial reports typically do not distinguish attrition early in treatment from attrition later in treatment. This may mask important differences in the effect that attrition at different stages of treatment may have on outcomes. For example, failure to initiate treatment after randomization, or dropout in the initial three to four weeks of treatment (failure to engage), likely has very different ramifications compared with patients who remain in treatment for many weeks, but fail to complete the total number of sessions as specified in the protocol. It would be useful if reports of clinical trials differentiated among these different forms of attrition.

Thus, the most appropriate conclusion of this meta-analysis is that delivery of psychotherapy for depressive symptoms is promising but requires more research. A few questions critical to our ability to make broader clinical recommendations remain unanswered. Most centrally, it is not clear whether telephone-administered psychotherapy is equivalent to face-to-face administered psychotherapy in reducing depression and whether telephone-administered psychotherapy can produce lower attrition rates, compared with equivalent face-to-face treatments. These conclusions can only be drawn from randomized trials directly comparing face-to-face and telephone-administered treatments. Telephone administration of psychotherapies may also have deleterious effects. It will be important to begin identifying specific populations for whom such treatments are useful, and perhaps more importantly, populations for whom telephone-administered psychotherapy is contraindicated. Trials addressing these questions are urgently needed, as organizations that provide mental health care have already begun implementing telemental health programs (Maheu et al., 2005).

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