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## Telephone screening, outreach, and care management for depressed workers and impact on clinical and work productivity outcomes: a randomized controlled trial

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### Abstract

**Context**—Although guideline-concordant depression treatment is clearly effective, treatment often falls short of evidence-based recommendations. Organized depression care programs significantly improve treatment quality, but employer-purchasers have been slow to demand these programs based on lack of evidence for cost-effectiveness from their perspective.

**Objective**—To evaluate the effects of a depression outreach-treatment program on workplace outcomes of concern to employers.

**Design**—Randomized controlled trial with allocation concealment and blinded assessment of depression severity and work performance at 6 and 12 months.

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The remaining authors report no conflicts of interest.

**Setting and Participants**—Two-stage screening of employees covered by a managed behavioral health plan identified 604 with clinically significant depression (excluding those with lifetime bipolar disorder, substance disorder, recent mental health specialty care, or suicidality).

**Intervention**—A telephonic outreach and care management program encouraged workers to enter outpatient treatment (psychotherapy and/or antidepressant medication), monitored treatment quality-continuity, and attempted to improve treatment by giving recommendations to providers. Participants reluctant to enter treatment were offered a structured telephone cognitive-behavioral psychotherapy.

**Main Outcome Measures**—Depression severity (Quick Inventory of Depressive Symptomatology, QIDS-SR) and work performance (WHO Health and Productivity Questionnaire, HPQ, a validated self-report instrument assessing job retention, time missed from work, work performance, and critical workplace incidents).

**Results**—Combining data across 6-month and 12-month assessments, the intervention group had significantly lower QIDS-SR scores (1.4 relative-odds of recovery), significantly higher job retention (1.7 relative-odds), and significantly more hours worked among the employed (equivalent to an annualized effect of approximately 2.5 weeks of work) than usual care subjects.

**Conclusions**—A systematic program to identify depression and promote effective treatment significantly improves not only clinical outcomes but also workplace outcomes. The financial value of the latter to employers in terms of recovered hiring-training and salary costs suggests that many employers would experience a positive return on investment from outreach and enhanced treatment of depressed workers.

## Keywords

depression; work performance; effectiveness trial; outreach; care management

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Depression imposes enormous societal burdens,<sup>1–3</sup> with annual US economic costs of tens of billions of dollars due largely to productivity losses.<sup>4, 5</sup> Indeed, comparative cost-of-illness studies show that depression is among the most costly of all health problems to employers.<sup>6–10</sup> Despite evidence that guideline-concordant treatment can be effective,<sup>11–21</sup> many depressed workers are untreated or inadequately treated.<sup>22–26</sup> Effectiveness trials have demonstrated that organized depression screening and enhanced care programs can significantly improve treatment and clinical outcomes.<sup>27–41</sup> Based on the magnitude of depression-related lost productivity, one might expect employer-purchasers to invest in enhanced depression screening-treatment programs. However, widespread uptake has not occurred<sup>42, 43</sup> due in part to employer-purchasers being unsure of the return-on-investment of such programs.<sup>44, 45</sup> Few controlled trials have evaluated effects of such programs on work outcomes and those few focused on primary care samples rather than on the workplace samples that would be the focus of employer-based screening, outreach, and disease management efforts.<sup>36, 41</sup> Without such information, employer-purchasers are likely to remain hesitant to invest in enhanced depression care. In this report, we present the results of a randomized controlled trial designed explicitly to address this issue. The trial examined the impact of depression screening, vigorous outreach, and care management of depressed workers employed by a number of large national firms. The primary outcomes included not only depression symptom relief but also job retention, decreased sickness absence, and increased work productivity.

## METHODS

### Sample

Participants were 604 depressed workers ages 18 and older enrolled in United Behavioral Health (UBH), a large managed behavioral health care company. Participating employers differed in whether UBH was the only behavioral health plan available, coverage criteria, the extent to which workers paid insurance premiums from salaries, and benefit design (co-payment size, visit caps, etc.).

### Recruitment

Recruitment occurred between 1/2004 and 2/2005 using a two-phase procedure. Phase one was a Health Risk Appraisal (HRA) survey conducted in 16 large companies from diverse sectors (airline, insurance, banking, public utility, state government, manufacturing) and containing broad distributions of occupations. An informed consent script was followed by a chronic-conditions checklist,<sup>46</sup> screen for psychological distress (K-6),<sup>47, 48</sup> questions about occupation and work performance, and socio-demographics.

Screen positives for possible depression (K-6  $\geq 9$ ) were invited by an introductory letter and telephone call from a survey interviewer, to participate in a second-phase telephone interview that assessed depression more specifically using the Quick Inventory of Depression Symptoms Self-Report version (QIDS-SR).<sup>49–51</sup> An initial informed consent script described the study and its voluntary and confidential nature; respondents were told they might be invited to participate in an innovative treatment program but there were no requirements to accept this or any specific treatments. Respondents with at least moderate depression severity (QIDS-SR  $\geq 8$ ) were eligible for randomization. Exclusion criteria included: positive responses to CIDI-SF<sup>52</sup> screening questions for a history of mania or substance dependence (subjects were informed and told to follow-up with a clinician), suicidal ideation or attempts in the prior week (subjects were immediately connected by telephone to a UBH Crisis Counselor), and treatment by a mental health specialist in the prior year.

Phase one individual-level HRA invitations were sent to 113,843 workers (mainly by e-mail) in 12 of the 16 companies. Of those invited, 35,169 completed at least one HRA question, 2358 (7.7% of respondents) screened positive for depression, and 1422 consented to baseline eligibility assessments and had UBH coverage (N.B. all company employees were screened, not just those with UBH coverage).

In the four remaining companies (total of approximately 150,000 workers) where individual-level contact information was not available, mass e-mail invitations were sent to the entire workforce and individuals provided contact information only in the initial HRA; 11,715 responded by completing at least one HRA question, 942 (10.9% of respondents) screened positive for depression, of whom 331 also had UBH coverage and consented to baseline eligibility assessments. A separate enrollment procedure used the UBH Member Wellness Survey of behavioral health symptoms and functional impairments mailed to members who received an authorization for outpatient treatment.<sup>53, 54</sup> Of 114,635 mailed surveys, 30,402 were completed and returned, of which 6225 individuals agreed to participate in eligibility assessments.

These recruitment and consent procedures, including use of tape-recorded oral informed consent, were approved by Harvard Medical School's Human Subjects Research Committee.

## Randomization

Dispositions of eligible-consenting phase-one participants for whom phase-two eligibility assessments were attempted are shown in Figure 1. Of 604 eligible, 304 were randomized without blocking or stratification to the intervention and 300 to usual care. Randomization was carried out by the survey research firm conducting eligibility assessments with a computerized procedure that classified respondents for eligibility and used a random number generator to assign participants to intervention or usual care. Usual care subjects were informed their responses indicated possible depression and advised to consult with a clinician; they could receive any normally available insurance benefit or service (e.g., psychotherapy or pharmacotherapy), just not the additional telephone care management components provided to intervention subjects.

## Telephone Outreach, Care Management, and Psychotherapy

The structured telephone intervention program (provided without charge to participants) systematically assessed needs for treatment, facilitated entry into in-person treatment (both psychotherapy and antidepressant medication), monitored and supported treatment adherence, and (for those declining in-person treatment) provided a structured psychotherapy intervention by telephone.<sup>55</sup> Specific treatments were provided according to both clinical need and participant's willingness to accept treatment.

Care managers were licensed MA-level mental health clinicians employed by UBH. Additional training for this study included 12 hours of didactic instruction, role-play, and observed care manager contacts. Care managers also received approximately 60 minutes of supervision each week (from GES, EJJ, PSW, FA) throughout the study period and had approximately 50–70 individuals in their caseloads when functioning at capacity.

Initial telephone contacts included a structured assessment of depressive symptoms (PHQ-9),<sup>56</sup> prior treatment, complicating factors (including substance abuse), and motivation for treatment. For all participants with significant depressive symptoms, care managers recommended in-person psychotherapy as well as medication evaluation and provided treatment authorization and referral information. For participants declining in-person treatment, care managers provided a brief motivational intervention<sup>57</sup> and asked permission for continued phone contact. Following initial contacts, all participants were mailed a psycho-educational workbook<sup>58</sup> emphasizing behavioral activation, identifying and challenging negative thoughts, and developing long-term self-care plans.

For participants entering in-person treatment, subsequent care manager contacts included structured assessments of depressive symptoms, treatment adherence, and barriers to continuing treatment. As needed, care managers provided feedback and algorithm-based recommendations to treating providers. A UBH psychiatrist was also available for consultation to clinicians if needed. For participants receiving one mode of treatment, adding a second mode was recommended if significant depressive symptoms persisted after two months. For participants declining in-person treatment, care managers maintained regular telephone contacts. Intervals between contacts were determined by clinical need, ranging from weekly (for severe depressive symptoms) to bi-monthly (for stable, mild or minimal symptoms).

Participants declining in-person treatment and experiencing significant depressive symptoms after two months were offered a structured 8-session cognitive-behavioral psychotherapy program.<sup>38, 55, 59</sup> Approximately weekly sessions lasted 30–40 minutes and followed the workbook described above. Sessions included: assessment of motivation for treatment and motivational enhancement exercises;<sup>57</sup> focus on increasing pleasant and rewarding activities;<sup>60</sup> identifying, challenging, and distancing from negative thoughts;<sup>61, 62</sup> and

creating a personal self-care plan covering medication use, self-monitoring, and self management skills.<sup>63</sup> “Booster” sessions were scheduled every 4–8 weeks to monitor and support progress.

All care management activities were organized and supported by an electronic decision support system. Care manager training and procedural materials are available upon request (from GES).

### Blinded Outcome Assessments

Blinded outcome assessments were performed at baseline, six, and 12 months by trained survey interviewers at the research firm conducting telephone interviews. Participants were advised not to offer information regarding their intervention status to preserve blinding.

**Quick Inventory of Depressive Symptomatology (QIDS-SR)**—The QIDS-SR is a fully-structured assessment<sup>50, 51</sup> that correlates significantly ( $r = .72$ ) with the 17-item clinician-administered Hamilton Rating Scale (HRSD)<sup>64</sup> and has good sensitivity to change.<sup>49</sup> QIDS-SR partial item-missing data were imputed using mean imputations (i.e., by assigning the mean score obtained from respondents in the same treatment arm with valid scores). Item-missing data occurred in only a small proportion of cases and were unrelated to treatment status.

**WHO Health and Productivity Questionnaire (HPQ)**—The HPQ is a fully-structured instrument<sup>65, 66</sup> assessing four broad dimensions of work functioning: (i) Work hours (absenteeism); (ii) Job performance, with priming and decomposition questions followed by an anchored global 0-to-10 rating scale that is the measure used to define performance; (iii) Job turnover (fired, quitting, changing jobs, disability leave); and (iv) Critical workplace incidents, assessed in open-ended questions about job-related accidents-injuries, other major negative events, and major positive events (e.g. receiving a promotion). Validation studies have documented significant associations ( $r = .61-.87$ ) of HPQ work hours assessments with payroll records<sup>66</sup> and job performance assessments with supervisor ratings ( $r = .52$ )<sup>65</sup> and other administrative records ( $AUC = .58-.72$ ).<sup>66</sup> HPQ partial item-missing data occurred in only a small proportion of cases, were unrelated to treatment status, and were imputed using mean imputations.

**Care Process Measures**—The care manager record system recorded contacts for recruitment-monitoring and telephone psychotherapy. Telephone psychotherapy data were missing for the first 30% of subjects and were imputed using multiple imputation (MI).<sup>67</sup> Other mental health services were assessed by self-report in the telephone interviews.

### Data Analysis

All comparisons were made in intent-to-treat analyses (based on original intervention assignment, regardless of treatments received). The propensity score method<sup>68</sup> was used to adjust for imperfect randomization in baseline characteristics. A logistic regression equation distinguishing intervention vs. usual care groups based on baseline depression severity, absenteeism, job performance, and socio-demographics, was used to generate predicted probabilities of intervention assignment. These predicted probabilities were used to weight the data without case-level matching so intervention and usual care groups had comparable distributions of characteristics (results available on request.)

MI was used to adjust for some subjects not completing either 6-month (35 intervention and 22 usual care) or 12-month (44 intervention and 30 usual care) interviews. Non-respondents differed from respondents both on socio-demographic variables (non-respondents were

older, less well educated, and more likely to be male) and baseline scores on outcomes (non-respondents had somewhat higher baseline depression severity and lower work performance). A two-part process was used to adjust for these differences: (i) Estimates were generated for missing values using regression equations with all available data in the baseline and completed follow-up interviews as predictors; and (ii) significance tests were adjusted for imputed values being estimated rather than observed using MI simulation methods.

Intervention effects on depression severity were estimated using MI multiple linear regression with simulated standard errors. QIDS-SR scores at 6 and 12 months were regressed on a dichotomous predictor for randomization status. Dichotomous measures of symptom improvement ( $\geq 50\%$  reduction in QIDS-SR scores) and complete remission (QIDS-SR scores of  $\leq 5$ ) were also examined using MI multiple logistic regression.

Comparable MI regression analyses were used to estimate intervention effects on work outcomes. The primary outcome was a composite measure of number of effective hours worked in the prior 7 days, where subjects no longer working contributed no hours and numbers of hours worked by employed respondents were weighted by job performance (e.g., 30 hours worked with an 8/10 performance rating was assigned a score of 24 effective hours). Intervention effects on components of this composite (i.e., probability of no longer working, number of hours worked among the employed, job performance during hours worked among the employed, and critical workplace incidents) were also examined. The MI multiple regression analyses estimated intervention effects at 6 and 12 months. We also estimated intervention effects constrained to be equal across these two time intervals to increase statistical power. This approach was implemented by pooling data across the two wave-pairs and including a dummy variable to distinguish the baseline-to-6-month panel from 6-to-12-month panel.

All linear regression coefficients (b) and odds-ratios (OR) from logistic regression equations were adjusted for outcomes at the prior assessment, expected hours of work at the prior assessment, and respondent age, sex, and education. In post-hoc subgroup analyses, intervention effects were examined in sub-samples defined by initial depression severity (moderate/severe cases corresponding to QIDS-SR  $\geq 11$ ) and recruitment procedure (HRA vs. UBH Member Wellness Survey).

Power calculations indicated a sample of 300 per group would be needed to detect a difference of 0.2 standard deviations (SD) of work productivity (power = .8, 2-sided  $p = .05$ ) with 20% loss to follow-up. The minimum detectible effect size of 0.2 SD was based on previous evidence that low-to-moderate intensity depression interventions yield effects on clinical outcomes of approximately 0.33 SD<sup>35, 39, 40</sup> and that clinical improvements are correlated approximately 0.60 with improvements in work performance.<sup>13</sup>

## RESULTS

### Sample

Propensity score weights adjusted for baseline differences after randomization (Table 1), including the somewhat younger age, lower proportions of females and college graduates, and lower baseline depression severity in the intervention versus usual care group.

When compared to workers meeting study criteria in the nationally representative National Comorbidity Survey Replication (NCS-R),<sup>69</sup> trial participants were somewhat older, more likely to be female, more educated, somewhat less depressed, and worked somewhat more



hours, possibly reflecting the fact that the trial sample was confined to full-time workers in large national firms (results available upon request.).

### Effects on Depression Outcomes

QIDS-SR scores were significantly lower in the intervention than usual care group by 6 months, with an effect size ( $b = -1.0$ ) of approximately 1/3 SD (Table 2). This advantage was retained at 12 months ( $b = -1.1$ ). The proportion whose symptoms improved substantially (50% QIDS-SR improvement) was also significantly higher among intervention than usual care subjects, but not until the 12-month assessment (30.9% vs. 21.6%; OR = 1.7). The proportion of participants experiencing recovery (QIDS-SR  $\leq 5$ ) was also significantly higher in the intervention than usual care group, but not until 12 months (26.2% vs. 17.7%; OR = 1.7).

Post hoc subgroup analysis failed to find significant differences in intervention effects among participants with mild (QIDS-SR  $\leq 10$ ) versus moderate-severe (QIDS-SR  $\geq 11$ ) baseline depression and among participants recruited through HRAs versus the UBH Member Wellness Survey (results available upon request).

### Effects on Work Performance Outcomes

Scores on the summary effective hours worked measure were significantly higher in the intervention than usual care group at 6 ( $b = 3.0$ ) and 12 ( $b = 3.3$ ) months (Table 3). This overall effect was due to significant improvements in job retention (92.6% vs. 88.0% by 12-months; OR = 1.7) and hours worked among employed respondents ( $b = 2.0$ ). Job retention was defined from the employee perspective (i.e., the employee continuing to hold a job) rather than from the employer perspective (i.e., the employee continuing to work for the same employer). Subsequent analysis found that, among participants employed at follow-up, the proportion working for the same employer was unrelated to intervention. However, as overall job retention was significantly higher in the intervention arm, the unconditional proportion of baseline respondents working for the same employer was higher in the intervention than the usual care group.

Further analysis showed that extreme values did not explain the significant intervention effect on hours worked among employed respondents, as this effect remained significant when the outcome variable was transformed using a square root transformation ( $p = .049$ ). Decomposition showed, furthermore, that the overall effect on hours worked was due to an effect on number of hours worked controlling for expected work hours ( $b = 1.7$ ,  $p = .044$ ) rather than on number of expected work hours ( $b = 0.6$ ,  $p = .18$ ). The intervention effect on job performance, in comparison, was not significant ( $b = 0.2$ ,  $p = .11$ ), although it was consistently positive. No significant effects were found, finally, on critical workplace incidents or on taking a job with another employer (results available upon request). Subgroup analysis failed to find significant differences in intervention effects between mild vs. moderate-severe cases or those recruited through HRAs vs. the UBH Member Wellness Survey (results available on request).

### Effects on Use of Mental Health Care

Intervention group subjects were significantly more likely than those in usual care to receive any mental health specialty treatment (OR = 1.6), but somewhat less likely to obtain any depression treatment in primary care or non-medical settings (OR = 0.6–0.7). (Table 4) The mean number of treatment contacts across all settings (including care manager contacts) was nearly twice as large in the intervention vs. usual care group (12.7 vs. 6.5,  $t = 5.7$ ,  $p < .001$ ). Of those randomized to the intervention, 50% completed initial care management contact by 8 days, 75% by 22 days, 90% by 114 days, and 9% were never reached.

## COMMENT

The results suggest that enhanced depression care of workers has benefits not only on clinical outcomes, but also on workplace outcomes. Although direct comparison to earlier studies is difficult because our trial is the first conducted exclusively among employed people, it is noteworthy that our effect size on clinical improvement (approximately one-third of a SD on the QIDS-SR distribution) is similar to earlier primary care trials using low-to-moderate intensity interventions.<sup>35, 39, 40</sup> Our finding of effects among less-severe as more serious cases is consistent with earlier primary care trials<sup>41, 70</sup> and suggests the intervention has benefit to a wide spectrum of depressed workers.

The significant 2.6 hour improvement per week in overall work functioning among intervention subjects is due to a combination of increased job retention and increased hours worked among the employed. Earlier analyses of working sub-samples in primary care trials have found generally comparable effects on retention<sup>41</sup> and absenteeism.<sup>71</sup> Although we did not find significant effects on work performance among the employed by one year, one primary care trial that followed patients for two years did.<sup>36</sup> The apparent effects on absenteeism and performance among the employed in our trial may also have been downwardly biased if the intervention led to retention of employees with more absences or worse performances. Unfortunately, direct comparisons with these earlier studies are difficult due to differences in intervention intensity, follow-up, and stratum definitions.

Formal evaluation of our intervention's return-on-investment to employers is not currently possible, as the latter requires information not yet available on duration of improvements, disability payments, overall healthcare expenditures, and hiring and training costs. However, the \$1800 annualized value of higher mean hours worked among intervention subjects retaining their jobs (assuming the median annual salary in the U.S. civilian labor force) by itself far exceeds the \$100–\$400 outreach and care management costs associated with low-to-moderate intensity interventions of the sort we implemented<sup>38, 39</sup>; these saving might also exceed or closely approximate the costs of approximately 10 additional mental health specialty visits made by intervention subjects over the course of a year.

These last observations suggests that outreach and enhanced care for depressed workers might be better conceptualized as an opportunity to invest in improving the productive capacity of workforces (referred to by employers as “human capital investments”) than as workplace costs.<sup>72, 73</sup> That the intervention also had positive impacts on job retention and the costs of hiring and training new workers are typically high<sup>1</sup> reinforce this interpretation.<sup>74</sup> However, it is important to recognize that these workplace benefits would not be realized by all employers, as hiring and training costs and extents to which employees are paid piecemeal, hourly, or by salary do vary.

The intervention had modest effects on self-reported use of treatments, consistent with earlier trials of low-intensity interventions.<sup>38, 39</sup> However, intervention subjects received twice as many contacts as usual care subjects when care manager contacts were included (12.7 vs. 6.5,  $t = 5.7$ ,  $p < .001$ ) and were 70% more likely to receive any mental health specialty treatment. Although it is difficult to identify active components from a trial with a single intervention arm, the finding that intervention subjects received more mental health specialty treatment is noteworthy in light of other data suggesting that mental health specialty care is more likely to meet evidence-based recommendations than treatments in other sectors.<sup>25</sup> A recent meta-regression of 28 depression collaborative care trials is also instructive in that it identified three “active ingredients”: systematic screening to identify patients (vs. other means such as clinician referral), use of mental health professionals as care managers, and regularly planned care manager supervision.<sup>27</sup> All three elements were



included in our intervention. The telephone CBT may also have been beneficial, as an earlier trial found a higher proportion of patients experiencing depression improvement in care management plus telephone CBT than in care management alone.<sup>38</sup>

Several potential technical limitations are noteworthy. First, the QIDS-SR might have misclassified cases, although clinical reappraisal studies show it has high concordance with blinded clinician assessments.<sup>49–51</sup> Second, the HPQ might have been systematically biased,<sup>75</sup> although significant associations between HPQ scores and independent administrative/archival records of absenteeism and work performance have been documented across a broad range of occupations.<sup>65, 66</sup> Third, workers who participated in our initial screening phase may have had a different prevalence, severity, or impairment associated with their depression than non-participants. While we had no way to evaluate this possibility, a prior study found initial participants in HRA screenings were comparable to initial non-respondents who participated only after more intensive recruitment efforts in terms of depression severity, work impairments, and associations between the two.<sup>76</sup> Fourth, the generalizability of our findings is unclear, as trial participants had less severe depressions and a different socio-demographic profile than a nationally representative sample of depressed workers. Although we found no differences in intervention effects across levels of depression severity or method of recruitment, such differences could exist across other subgroups (e.g. white vs. blue collar workers) and have relevance to employers whose workforces vary in these characteristics.

Potential conceptual limitations also need to be considered. Simple human capital metrics such as absenteeism and job performance might *overestimate* true costs to employers, as would happen if unperformed work during absences is made up by coworkers or the absent worker upon return.<sup>77</sup> However, it is also possible that the burdens of depression to employers are *underestimated* here because other costs, such as for hiring temporary workers, paying coworkers overtime, and adverse effects on coworkers' productivity were not considered.<sup>78</sup> Likewise, we did not assess intervention effects on outcomes such as suffering, marital stability, caregiver burden, and employee contributions outside the workplace that could have value from a societal perspective and might lead to long-term improvements in productive capacity.<sup>79</sup> An exclusive focus on work outcomes might devalue benefits of intervening among groups not in the workforce (e.g., the elderly) or in low-wage occupations, emphasizing that healthcare resource allocation decisions need to consider a societal as well as employer perspective. Within the constraints of these limitations, this study suggests that enhanced depression care for workers can have benefits for employers that go beyond improved health and diminished suffering in their workforces and extend to increased work productivity. Further study is needed to determine if intervention costs are offset by these workplace benefits and the variation in this offset across different employment settings.<sup>72, 73</sup> Toward this end, it is noteworthy that increased depression treatment among intervention subjects was largely telephone contacts with care managers and not more expensive in-person visits with traditional providers. We also did not consider if the intervention offsets any greater general medical utilization associated with depression, as has been observed in earlier primary care trials.<sup>32, 34, 35, 82</sup> Likewise, availability of web-based, e-mail, and interactive voice-recognition technologies should ensure the costs of screening and recruiting depressed workers into interventions are low.<sup>83</sup> These features may be critically important to potential purchasers, who are not just sensitive to interventions' returns-on-investment but also their absolute costs and impacts on per-member per month (PMPM) charges.<sup>44</sup> Attention to these issues in future research is needed to ensure that successful programs of outreach and enhanced depression treatment are widely disseminated.<sup>45</sup>

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**Table 1**

Characteristics of participants assigned to intervention and usual care

	Unweighted				Weighted*			
	Intervention Mean (se)	Usual Care Mean (se)	t-value	(p)	Intervention Mean (se)	Usual Care Mean (se)	t-value	(p)
<b>I. Socio-demographics</b>								
Age (Mean)	40.7 (0.6)	42.4 (0.6)	2.0	(.047)	41.3 (0.6)	41.3 (0.6)	0.0	(.99)
Female (%)	70.7 (2.6)	77.0 (2.4)	1.8	(.08)	74.4 (2.5)	74.4 (2.6)	0.0	(.99)
College graduates (%)	38.0 (2.8)	43.8 (2.8)	1.4	(.15)	40.7 (2.9)	40.7 (2.9)	0.0	(.99)
<b>II. Baseline values of outcomes</b>								
Depression symptom severity (Mean) <sup>†</sup>	13.3 (0.2)	13.8 (0.2)	2.0	(0.0)	13.5 (0.2)	13.5 (0.2)	0.0	(.99)
Effective weekly hours worked (Mean) <sup>‡</sup>	29.8 (0.8)	31.8 (0.7)	1.7	(.1)	30.4 (0.8)	31.2 (0.8)	0.7	(.48)
Actual weekly hours worked (Mean)	41.6 (0.8)	43.5 (0.8)	1.6	(.10)	42.5 (0.8)	42.5 (0.8)	0.0	(.99)
On-the-job work performance (Mean)	0.7 (0.0)	0.7 (0.0)	0.9	(0.4)	0.7 (0.0)	0.7 (0.0)	1.3	(.20)
(n)	(300)	(304)			(300)	(304)		

\* Based on a propensity score weight designed to reduce aggregate baseline differences between the two sub-samples

<sup>†</sup> QIDS-SR score

<sup>‡</sup> The product of relative hours and performance at work

**Table 2**  
The impact of the intervention on depression outcomes at 6 and 12 months after randomization

	Intervention		Usual care		t-value	(p)
	Mean <sup>†</sup> (se)	Mean <sup>†</sup> (se)	Est <sup>‡</sup> (se)	95% CI		
I. Symptom severity (Mean) <sup>§</sup>						
By 6 months	10.2 (0.3)	11.2 (0.3)	-1.0*	(-1.8, 0.2)	2.6	(.010)
By 12 months	8.9 (0.3)	10.0 (0.3)	-1.1*	(-1.8, 0.3)	2.8	(.005)
Pooled—			-1.0*	(-1.7, 0.4)	3.2	(.001)
II. Substantial improvement (%) <sup>¶</sup>						
By 6 months	21.7 (2.5)	17.4 (2.3)	1.2	(0.8, 2.0)	1.3	(.204)
By 12 months	30.9 (2.8)	21.6 (2.4)	1.7*	(1.1, 2.5)	2.6	(.010)
Pooled—			1.4*	(1.1, 2.0)	2.5	(.011)
III. Recovery (%) <sup>#</sup>						
By 6 months	18.2 (2.3)	12.6 (2.0)	1.7	(1.0, 2.5)	1.9	(.054)
By 12 months	26.2 (2.6)	17.7 (2.3)	1.7*	(1.1, 2.4)	2.6	(.011)
Pooled—			1.4*	(1.1, 2.0)	2.6	(.009)
	(n)	(300)		(304)		

\* Significant at the .05 level, two-sided test

<sup>†</sup>The means are reported without adjustment for control variables. The estimates of regression coefficients, in comparison, are based on analyses that include the control variables. As a result, the mean differences generally do not equal the linear regression coefficients and the odds-ratios that can be calculated from the means generally do not equal the odds-ratios obtained from the logistic regression equations.

<sup>‡</sup> Linear regression coefficient for (I) reduction in symptom severity; odds-ratio for (II) substantial improvement and (III) recovery.

<sup>§</sup> Continuous QIDS-SR scores

<sup>¶</sup> The test for the pooled data used both 6 and 12 months outcomes and constrained the two coefficients (odds-ratios or linear regression coefficient) to be equal.

<sup>#</sup> ≥50% reduction in QIDS-SR score compared to baseline

<sup>†</sup> QIDS-SR score ≤5

**Table 3**  
The impact of the intervention on work performance at 6 and 12 months after randomization

	Intervention		Usual care		t-value	p
	Mean <sup>§</sup> (se)	Mean <sup>†</sup> (se)	Est <sup>‡</sup> (se)	95% CI		
<b>I. Effective weekly hours worked<sup>§</sup> (Mean)</b>						
At 6 months	30.1 (0.9)	27.1 (0.9)	3.0*	(0.4, 5.6)	2.2	(.030)
At 12 months	29.5 (0.9)	26.0 (0.9)	3.3*	(0.9, 5.8)	2.7	(.008)
Pooled <sup>¶</sup>			2.6*	(1.0, 4.3)	3.2	(.002)
<b>II. Job retention (%)</b>						
At 6 months	96.1 (1.2)	90.1 (1.7)	2.5*	(1.2, 5.0)	2.7	(.007)
At 12 months	92.6 (1.7)	88.0 (1.9)	1.7	(1.0, 3.3)	1.8	(.07)
Pooled <sup>¶</sup>			1.7*	(1.1, 3.3)	2.3	(.022)
<b>III. Actual weekly hours worked among the employed (Mean)</b>						
At 6 months	42.0 (1.0)	40.1 (1.0)	1.8	(-0.8, 4.4)	1.3	(.18)
At 12 months	42.3 (0.8)	39.5 (0.9)	2.1	(-0.4, 4.5)	1.7	(.09)
Pooled <sup>¶</sup>			2.0*	(0.3, 3.7)	2.3	(.020)
<b>IV. On-the-job performance among the employed (Mean)<sup>‡</sup></b>						
At 6 months	0.8 (0.0)	0.8 (0.0)	0.2	(-0.2, 0.5)	0.9	(.35)
At 12 months	0.8 (0.0)	0.7 (0.0)	0.2	(-0.2, 0.6)	0.8	(.40)
Pooled <sup>¶</sup>			0.2	(-0.0, 0.1)	1.6	(.11)
<b>V. Critical workplace incidents (Mean)<sup>#</sup></b>						
At 6 months	-0.2 (0.0)	-0.2 (0.0)	0.00	(-0.1, 0.1)	0.1	(.93)
At 12 months	-0.2 (0.0)	-0.2 (0.0)	0.05	(-0.0, 0.2)	1.1	(.29)
Pooled <sup>¶</sup>			0.02	(-0.0, 0.1)	0.7	(.51)
	(300)	(304)				
		(n)				

<sup>§</sup>Odds-ratio for (II) job retention; linear regression coefficient for other outcomes.

<sup>¶</sup>Relative effective hours work is a product of job retention, relative hours among the employed, and on-the-job performance among the employed. The value is set to 0 for respondents who no longer work.

\* Significant difference between intervention and usual care at the .05 level, two-sided test

<sup>f</sup> The means are reported without adjustment for control variables. The estimates of regression coefficients, in comparison, are based on analyses that include the control variables. As a result, the mean differences generally do not equal the linear regression coefficients and the odds-ratios that can be calculated from the means generally do not equal the odds-ratios obtained from the logistic regression equations.

<sup>g</sup> Linear regression coefficient for (I) reduction in symptom severity; odds-ratio for (II) substantial improvement and (III) recovery.

<sup>h</sup> The test for the pooled data used both 6 and 12 months outcomes and constrained the two coefficients (odds-ratios or linear regression coefficient) to be equal.

<sup>#</sup> Critical workplace incidents include accidents/injuries, other important work failures, and important work successes (reverse coded)

Table 4

Service use of participants assigned to intervention and usual care at 6 and 12 months after randomization

	Intervention		Usual care		t-value	(p)
	Mean <sup>†</sup> (se)	Mean <sup>†</sup> (se)	Est <sup>‡</sup> (se)	Est <sup>‡</sup> (95% CI)		
<b>I. Any contacts (%)</b>						
A. Case manager calls						
At 6 months	90.5 (1.7)	0.0 --	--	--	--	--
At 12 months	71.0 (2.7)	0.0 --	--	--	--	--
Pooled	91.4 (1.6)	0.0 --	--	--	--	--
B. Case manager telephonic psychotherapy						
At 6 months	33.6 (2.8)	0.0 --	--	--	--	--
At 12 months	30.7 (2.7)	0.0 --	--	--	--	--
Pooled	33.6 (2.8)	0.0 --	--	--	--	--
C. Visits to GP for MD						
At 6 months	17.6 (2.3)	24.1 (2.5)	0.7	(0.4,1.0)	1.9	(.05)
At 12 months	14.9 (2.4)	21.0 (2.5)	0.7	(0.4,1.1)	1.6	(.10)
Pooled	25.1 (2.8)	32.8 (2.9)	0.7	(0.5,1.0)	1.9	(.05)
D. Visits to MH specialist for MD						
At 6 months	34.8 (2.9)	27.3 (2.6)	1.4*	(1.0,2.0)	2.0	(.047)
At 12 months	25.0 (2.8)	19.0 (2.4)	1.5	(1.0,2.3)	1.9	(.06)
Pooled	43.6 (3.1)	32.9 (2.8)	1.6*	(1.1,2.3)	2.5	(.013)
E. Visits to any other provider for MD						
At 6 months	6.8 (1.6)	11.8 (1.9)	0.5*	(0.3,1.0)	2.0	(.044)
At 12 months	5.7 (1.4)	9.1 (1.7)	0.6	(0.3,1.2)	1.4	(.18)
Pooled	11.1 (1.9)	16.0 (2.2)	0.6	(0.4,1.1)	1.7	(.08)
F. Any treatment contacts						
At 6 months	93.9 (1.4)	50.5 (3.0)	16.4*	(9.5,28.3)	10.0	(<.001)
At 12 months	82.2 (2.4)	41.7 (3.0)	7.0*	(4.6,10.6)	9.3	(<.001)
Pooled	96.0 (1.2)	59.9 (2.9)	17.2*	(9.0,32.8)	8.7	(<.001)
<b>II. Mean number of contacts</b>						

	Intervention		Usual care		Est <sup>†</sup>	(95% CI)	t-value	(p)
	Mean <sup>‡</sup>	(se)	Mean <sup>‡</sup>	(se)				
A. Case manager calls								
At 6 months	4.0	(0.2)	0.0	--	--	--	--	--
At 12 months	2.2	(0.1)	0.0	--	--	--	--	--
Pooled	6.1	(0.2)	0.0	--	--	--	--	--
B. Case manager telephonic psychotherapy								
At 6 months	1.2	(0.1)	0.0	--	--	--	--	--
At 12 months	0.8	(0.1)	0.0	--	--	--	--	--
Pooled	2.0	(0.2)	0.0	--	--	--	--	--
C. Visits to GP for MD								
At 6 months	0.4	(0.1)	0.6	(0.1)	-0.2	(-0.4,0.0)	1.8	(.08)
At 12 months	0.3	(0.0)	0.5	(0.1)	-0.2	(-0.4,-0.0)	2.2	(.025)
Pooled	0.6	(0.1)	1.0	(0.1)	-0.4	(-0.8,-0.1)	2.6	(.01)
D. Visits to MH specialist for MD <sup>§</sup>								
At 6 months	2.8	(0.4)	2.1	(0.3)	0.7	(-0.2,1.6)	1.5	(.14)
At 12 months	1.9	(0.3)	1.7	(0.3)	0.3	(-0.6,1.1)	0.6	(.53)
Pooled	4.7	(0.5)	3.9	(0.5)	0.9	(-0.6,2.4)	1.2	(.25)
E. Visits to any other provider for MD-								
At 6 months	0.8	(0.3)	0.8	(0.3)	-0.0	(-0.9,0.9)	0.0	(.97)
At 12 months	0.4	(0.2)	0.7	(0.2)	-0.2	(-0.8,0.3)	0.9	(.37)
Pooled	1.2	(0.4)	1.5	(0.5)	-0.3	(-1.5,1.0)	0.5	(.66)
F. Any treatment contacts <sup>¶</sup>								
At 6 months	8.0	(0.5)	3.5	(0.5)	4.5*	(3.0,5.9)	6.1	(<.001)
At 12 months	4.7	(0.4)	2.9	(0.4)	2.0*	(0.9,3.0)	3.7	(<.001)
Pooled	12.7	(0.8)	6.5	(0.8)	6.3*	(4.1,8.4)	5.7	(<.001)
<b>III. Taking anti-depressant medication (%)</b>								
At 6 months	30.4	(2.8)	35.1	(2.8)	1.0	(0.7,1.4)	0.0	(.98)
At 12 months	30.5	(2.7)	34.1	(2.9)	0.8	(0.6,1.2)	1.1	(.30)
Pooled	41.1	(3.1)	41.5	(3.1)	1.0	(0.7,1.5)	0.0	(.99)
	(300)		(304)					



\* Significant difference between intervention and usual care at the .05 level, two-sided test

† The means are reported without adjustment for control variables. The estimates of regression coefficients, in comparison, are based on analyses that include the control variables. As a result, the mean differences generally do not equal the linear regression coefficients and the odds-ratios that can be calculated from the means generally do not equal the odds-ratios obtained from the logistic regression equations.

‡ Linear regression coefficient for (I) reduction in symptom severity; odds-ratio for (II) substantial improvement and (III) recovery. Odds-ratio for the outcomes in part I and part III; linear regression coefficient for the outcomes in part II.

§ Mental health specialists include psychiatrists, psychologists, therapists, and mental health counselors

|| Other providers include spiritual advisors, other human services professionals, and complementary-alternative medical providers

¶ Total contacts include contacts with care managers and visits to mental health specialists, doctors and other providers.