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A Propensity Score Analysis of Brief Worksite Crisis Interventions after the World Trade Center Disaster:

Implications for Intervention and Research

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Abstract

Background—Postdisaster crisis interventions have been viewed by some as appropriate to enhance the mental health status of persons affected by large-scale traumatic events. However, studies and systematic reviews have challenged the effectiveness of these efforts.

Objectives—The focus of this study was to examine the impact of brief mental health interventions received by employees at the worksite after the World Trade Center disaster (WTCD) among workers in New York City (NYC).

Research Design—The data for the present study come from a prospective cohort study of 1121 employed adults interviewed by telephone in a household survey 1 year and 2 years after the WTCD. All study participants were living in NYC at the time of the attacks. For the current study, we used propensity scores to match intervention cases (n = 150) to nonintervention controls (n = 971) using a 1:5 matching ratio based on a bias-corrected nearest-neighbor algorithm.

Results—Approximately 7% of NYC adults (~425,000 persons) reported receiving employer-sponsored, worksite crisis interventions related to the WTCD provided by mental health professionals. In addition, analyses indicated that attending 1 to 3 brief worksite sessions was associated with positive outcomes up to 2 years after the WTCD across a spectrum of results, including reduced alcohol dependence, binge drinking, depression, PTSD severity, and reduced anxiety symptoms.

Conclusions—Although our study had limitations, it is one of the few to suggest that brief postdisaster crisis interventions may be effective for employees after mass exposure to psychologically traumatic events. The reasons for the effectiveness of these interventions are unclear at this time and warrant further investigation.

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Keywords

emergency services; crisis interventions; community disasters; mental health; alcohol abuse; occupational medicine; propensity scores

Although the psychologic sequelae after exposure to traumatic events often appear brief, studies suggest that community-wide disasters characterized by a large-scale loss of life, extensive property damage, economic disruptions, and those related to human intent result in increased rates of mental health problems.[1,2] All of these factors were present in the terrorist attacks on the World Trade Center in New York City (NYC) on September 11, 2001.[3–5] Research 6 months after the disaster suggested that although these symptoms resolved over time, many persons not directly affected by the attacks developed symptoms.[6,7] However, initial surveys after this event indicated that only small population-level increases in mental health service utilization actually occurred,[3,6] despite the availability of these services.[4]

The focus of the present study was to examine the impact of mental health crisis interventions received by New Yorkers at the worksite after the World Trade Center disaster (WTCD). This investigation was part of a larger study focusing on the immediate and long-term effects of the WTCD among NYC residents. As discussed herein, we defined worksite crisis intervention as any brief sessions related to coping with the WTCD shortly after this event directed by a mental health professional or counselor arranged by area employers for employees.

In our study, altogether 10% (95% confidence interval [95% CI] 8.9–11.8) of NYC adults reported receiving some type of crisis interventions conducted by mental health professionals within a year after the attacks. Approximately 7% of NYC adults (~425,000 persons) reported receiving employer-sponsored, worksite crisis interventions related to the WTCD. Crisis interventions after traumatic events have been used for many years.[8,9] However, the effectiveness and safety of these interventions have been challenged.[9–13] Nevertheless, crisis interventions have been commonly recommended after traumatic events.[14] In the past, evaluation of these interventions has been hampered by both mixed results[14,15] and by limited research designs.[10,13] The purpose of the current study was to conduct a more advanced and focused analysis of preliminary findings reported elsewhere.[16] This earlier study, which examined both mental health and alcohol abuse outcomes, suggested that those who attended 1 to 3 brief sessions had better outcomes 2 years after disaster, based on logistic regression modeling. On the basis of these findings we refined the outcomes examined, eliminated non-workers from the study, expanded the number of covariates to control for bias, and used propensity score matching to assess the average effect of the intervention.

METHODS

Study Participants

The data for the present study come from a prospective cohort study of English- or Spanish-speaking NYC adults who were living in NYC on the day of the WTCD. Using random-digit dialing, we conducted a baseline telephone survey a year after the attacks among a random sample of NYC households. As part of the overall study, we over-sampled residents who reported receiving any mental health treatment in the year after the attack by use of a screener question at the start of the survey. The baseline population was also stratified by the 5 NYC boroughs and gender and sampled proportionately. Questionnaires were translated into Spanish and then back-translated by bilingual Americans to ensure linguistic and cultural appropriateness. The baseline survey occurred between October and December 2002, and a follow-up survey was conducted 1 year later, between October 2003 and February 2004. The data-collection procedures were the same for both survey waves. Trained interviewers using a computer-assisted telephone interviewing system conducted the interviews. All interviewers were supervised and monitored by the survey contractor in collaboration with the investigative staff. The mean duration of the survey was 45 minutes for the baseline and 35 minutes for follow-up interviews. A $10 survey incentive was offered for the baseline and a $20 incentive for the follow-up survey. The Institutional Review Board of The New York Academy of Medicine reviewed and approved the study's protocols. For the baseline, 2368 individuals completed the survey. We were able to reinterview 1681 of these respondents in the follow-up survey. Using industry standard survey definitions, the baseline cooperation rate was 63%,[4] and the reinterview rate for the follow-up study was 71%.[17]

Outcomes Assessed

For our study, we included outcome measures related to both alcohol use and mental health status. Consistent with previous epidemiologic studies,[18] we collected data in our follow-up survey on the respondent's consumption of alcohol in the past 12 months based on the CAGE criteria for dependence,[19] a widely used and validated scale for alcoholism screening.[20] For this, we defined respondents as meeting criteria for alcohol dependence if they had 2 or more positive responses on the CAGE scale (eg, drank first thing in the morning) in the 12 months before the follow-up survey. Our second alcohol measure related to binge drinking. Similar to other studies,[18] we asked respondents how many times during the past year they had 6 or more alcoholic drinks on a single occasion. We then coded this as less than "monthly/never" versus "monthly or more" to provide a measure of binge drinking.[18] We also included a measure of PTSD during the follow-up survey, which has been described elsewhere.[7] This measure was based on the *Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition* (DSM-IV),[21] and has been used in other population surveys.[22,23] To meet the criteria for this disorder in our study, the respondent had to have positive results for the PTSD criteria A–F.[4] The respondent was then defined as a PTSD case, if the criteria were met within the past 12 months. Our PTSD measure has been shown to be a valid and reliable measure[4,24] and has been used in previous WTCD studies.[4,6,7]

In the follow-up survey, we also assessed the occurrence of major depression in the past year. To classify respondents for this condition, we used a version of the depressive disorder scale from the SCID clinical interview for nonpatients.[25] This scale had been previously adopted for use in telephone-based population surveys and in recent WTCD studies.[4,6,22] After DSM-IV criteria,[21] respondents met criteria for major depression if they had 5 or more depression symptoms for at least two-weeks in the past 12 months. Data related to the validity and reliability of this scale were also previously reported and suggested that this scale can successfully diagnose depression in the general population.[4,6]

Our follow-up also included mental health symptom measures in the past 30 days, including PTSD symptom severity, depression symptoms, and anxiety symptoms. For PTSD symptom severity, we asked respondents who reported any PTSD symptoms in the past 12 months to indicate whether that symptom bothered them "not at all," "just a little," "somewhat," or "a lot" in the past 30 days. We summed the 17 PTSD symptom severity items (scored 0, 1, 2, and 3, respectively) into a total symptom severity score. This assessment method is similar to the one commonly used for PTSD Symptom Checklist (PCL).[26] For depression and anxiety, our 30-day symptom scales were based on the Brief Symptom Inventory-18 (BSI-18), a self-reported psychiatric instrument derived from the Hopkins Symptom Checklist.[27] The BSI-18 has been standardized based on a national community sample and has been shown to be a reliable and valid psychiatric symptom instrument.[27,28]

Interventions Assessed

Because assessing the effects of brief crisis interventions at the work site was a component of our study design, we queried respondents about participation in this type of intervention during the baseline survey, 1 year after the disaster. Specifically, we asked, "Since the World Trade Center disaster, have you attended any brief sessions related to coping with the World Trade Center disaster conducted by a mental health professional or counselor that were arranged by your employer or an organization such as a community group or religious group?" The majority of persons who attended these sessions, 70%, indicated that they were at the worksite. For analytical purposes, those who attended brief sessions at other locations, such as religious places and community centers (n = 59), were excluded from analyses, as were the unemployed (n = 471). Also, we wanted to assess the effectiveness of short-term interventions and our pilot study suggested no improvement among those who attended 4+ sessions (n = 30), we excluded these persons from our analyses as well.[16] We did this because we wanted to avoid confounding our study with longer-term therapy seekers who likely would be more impaired.[29]

On the basis of these selection criteria, respondents were classified as either the brief worksite intervention group (n = 150) or the nonintervention group (n = 971). We also inquired about the content of the sessions attended (eg, worker was educated about stress symptoms, taught to relax), which was developed from previously published descriptions of recommended PTSD interventions (p. 14).[30] Finally, we asked the intervention group to rate the effectiveness of these sessions in helping with emotional problems since the WTCD. Although our study did not collect data on when the intervention was initiated (eg, we only asked about brief interventions in the past 12 months at baseline), numerous anecdotal reports suggest that these interventions generally occurred within a month or 2 after the event.[31,32] Thus, our study was essentially a 2-year outcome study, since the interventions generally occurred shortly after the attacks and the end-point was the follow-up survey 2 years after disaster.

Study Matching Variables for Propensity Analysis

Because our study was a population-based observational design, we statistically controlled for selection bias as the result of observable factors by matching intervention cases to controls using a propensity score method.[33,34] In this study, variables for the propensity score were those potentially related to receiving worksite interventions and included demographic factors, exposure to WTCD events, residential location, treatment history, etc. It is noted that because these worksite interventions were provided by area employers and *not* directly sought by employees, per se, we anticipated that self-selection bias would be limited, compared with studies where most persons actively sought treatment.[4,31] For the latter case, results would likely be biased by patient self-selection,[35] whereby those who actively sought treatment would more likely have poorer mental health,[29] as has been previously reported following the WTCD.[3,4,6]

To control for potential bias, study matching variables in our study included age, gender, marital status, level of education, household income, race/ethnicity, immigrant status, language spoken, borough of residence, exposure to WTCD events, history of mental health treatment, history of depression, and having experiences a peri-event panic (PEP) attack during the WTCD. For our propensity score estimation, age was coded as age in years. Education (coded as less than high school to graduate work) and household income (coded as <\$20,000 to > \$100,000) were both used as ordinal variables. Gender, marital status, immigrant status, language spoken (coded English versus Spanish), history of mental health treatment, history of depression, and PEP were coded as binary variables. History of mental health treatment was based on ever having seen a mental health professional but excluded the brief intervention being assessed. For lifetime depression, we again used the SCID's major depressive disorder scale, as noted previously.[25] Per the DSM-IV criteria,[21] respondents met the criteria for lifetime major depression if they had 5 or more depression symptoms for at least 2 weeks in their lifetimes. PEP was a version of the Diagnostic Interview Schedule (DIS) scale for panic designed to assess symptoms that occurred during or shortly after the WTCD, which was used in recent surveys.[4,6] This scale was adopted directly from the DIS/DSM-IV criteria[21,36] and was found to be a very good prognostic indicator of later postdisaster mental health status.[3,4,6,7,37]

Exposure to WTCD events was coded as a continuous variable, ranging from 0 to 8 or more event exposures. Our WTCD event scale was based on our baseline survey and had been used in previous WTCD studies.[6] This scale was the sum of 14 possible WTCD-related events potentially experienced during or after the attacks (eg, having a friend or relative killed, being forced to move). For our analysis, we summed these into a continuous exposure scale. This scale was developed from other disaster studies[37] and had been used in previous WTCD research.[4] Finally race/ethnicity (classified as white, black/African American, Hispanic, and other) and borough residence (classified as Manhattan, Bronx, Brooklyn, Queens, and Staten Island) were use as categorical variables. All of the demographic variables were from the baseline survey, unless the data were missing, in which case the follow-up data were substituted. All measures discussed for the current study were used and/or validated in previous WTCD studies.[4,6,17] We note that although our study did not include actual predisaster baseline measures, we believe that the inclusion of depression history, previous treatment, disaster exposure level, PEP attack, and other key variables in our analyses, provided an acceptable method to control for factors potentially affecting outcomes among the intervention versus nonintervention group in our study.

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Statistical Analyses

Our analytic strategy proceeded in several steps. First, we discuss descriptive statistics for the sample. We then describe the characteristics of the brief worksite intervention services. We next compare the descriptive characteristics for the crisis intervention group versus the nonintervention group. Finally, we estimate the average treatment effect for the intervention, whereby our 7 outcomes measures of interest are assessed by the intervention status, matched on the demographic, stress exposure, and risk factor variables discussed. These analyses assess the impact of brief worksite crisis counseling, controlling for selection biases and confounding variables related to receiving brief worksite interventions that could obscure these results.[33,38] If the brief crisis intervention sessions were beneficial, then we would expect better outcomes at follow-up. For our study, as discussed herein, matching was achieved by means of an error-corrected propensity score method based on nearest-neighbor estimators.[39,40]

The propensity score method has been shown to be superior to conventional risk adjustment methods.[33,38] Instead of inclusion of different variables in a model to control for bias,[41] a single score is developed as measure the “propensity” of individuals to be similar. This method compares the differences in the outcome variable between the intervention and matched-nonintervention cases at follow-up.[33] In our study, as noted, the propensity variables predicting “selection” to intervention included: female gender, age in years, level of educational attainment, annual household income level, married status, foreign born, language spoken, Borough of residence (including Bronx, Brooklyn, Queens, Staten Island, with Manhattan as the reference), race/ethnicity (including African American, Hispanic, and Other, with white as the reference), history of mental health treatment, history of depression, PEP, and WTCD exposure level. From the fitted model, each subject’s log odds (logit) of being in the intervention group was calculated based on the linear combination values for these predictors.[33,38] These logits were then converted to probabilities and used as an individual’s propensity score, a measure of multivariate similarity to subjects selected for the intervention.

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Nonintervention participants with high scores, thus, have multivariate profiles that are more like those of intervention participants, while those with low scores are less like intervention participants. In other words, by this method a collection of covariates is replaced by a single covariate, being a function of the original ones. For individual i (i = 1, …, n) with vector x_i of the observed covariates, the propensity score is the probability $e(x_i)$ of being in intervention ($Z_i = 1$) versus being in nonintervention ($Z_i = 0$):

$$e(x_i) = \Pr(Z_i = 1 \mid X_i = x_i)$$

where it is assumed that the Z_i are independent, given the X's.

On the basis of quintiles (or some other grouping method), individuals can be partitioned into propensity subclass and then, within each subclass, an intervention case is matched to one or more nonintervention cases.[33] After matching, a *t* test for 2 dependent samples, the paired McNemar test, or conditional logistic regression (or another method) is used in to assess the intervention.[33] To maximize use of our data, as noted later in this work, we did not use stratification matching but 1:5 matching with 1 intervention case matched to 5 nonintervention cases.[42] This approach used an optimal matching method, whereby the smallest possible distance between all possible pairs was used for matching.[42] Both multiple-matching and optimal-matching methods are generally considered better for propensity score analyses.[42]

For our analyses, we used Stata, version 9,[43] to generate frequency distributions, point estimates, propensity scores for matching, and for statistical tests. For our propensity matching, we first used the pscore program in Stata to assess the adequacy of propensity matching and confirmed that no statistical differences existed postmatch between the predictors and intervention status.[40] For our final matching algorithm, we used nnmatch in Stata and, as noted, a 1:5 match for intervention versus control.[38] The nnmatch procedure in Stata is considered superior to other methods because it uses a nearest-neighbor matching estimator that includes a bias-correction to adjust the p-values.[39] Compared with other matching estimators, this method generally functions to lowers the bias and increases the study variance.[39] The final results present the average (percent or mean) treatment effects for the intervention compared with the nonintervention group. All *P* values presented are based on 2-tail tests. Unlike previous reports,[4,6] other than our survey descriptive data (ie, Table 1), the data presented are unweighted for several reasons. First, the respondents for the analyses were selected out of the original survey population. Second, our intervention group was limited to 150 cases. Third, we matched cases to multiple controls. In these kinds of situations, use of extensive survey weights can be problematic.[44]

RESULTS

As reported elsewhere,[17] our baseline and follow-up survey samples were comparable to U.S. Census data for NYC and indicated no significant variations in terms of age, gender, race, or city borough. Thus, our final sample did not appear to be demographically biased.[45] In addition, although it was previously reported that there was little increase in mental health treatment seeking after WTCD,[4] our survey indicated that 10.2% of participants (95% CI 8.9–11.8%; 540,000 to 720,000 NYC adults) reported receiving some type of brief postdisaster mental health interventions (Table 1). In addition, 6.5% of participants (95% CI 5.5–7.6; 335,000 to 610,000 NYC adults) reported receiving brief after-disaster mental health interventions at the worksite. Furthermore, of those who received these interventions at work, the great majority, nearly 90%, received only 1 to 3 brief sessions (Table 1). Additionally, of those who received these sessions, greater than 60% reported a range of clinical interventions, from psychoeducation to anxiety management.[32] Finally, more than 80% of those who received these services thought that they were helpful for coping with the disaster (Table 1).

Descriptive statistics for the intervention group (n = 150) and the nonintervention group (n = 971), from which matching cases were drawn, are presented in Table 2. The current study population, unlike a preliminary report,[16] included only employed adults, excluded those who received brief intervention elsewhere, and excluded those who received 4 or more brief sessions at the worksite. As expected, because our study population was employed, respondents tended to be younger, with nearly 80% between 30 and 64 years of age. They were also well educated, with more than 50% having a college degree (Table 2). In terms of WTCD exposure, 80% reported experiencing 2 or more WTCD related events, and 25% met the criteria for a lifetime major depression episode (Table 2). In terms of differences between the intervention and the nonintervention group, 5 variables were significant. These included education, gender, household income, WTCD exposure status, and history of treatment, whereby the intervention group had a greater proportion of women, higher educated, higher income, higher WTCD exposed persons, a greater proportion of those with previous mental heath treatment, and PEP. These differences make sense and suggest that, in part, white-collar companies nearer the disaster site were more likely to provide intervention services for employees. Also noteworthy was that borough of residence, age, race/ethnicity, immigrant status, and history of major depression were *not* related to receiving worksite interventions, suggesting that the individual selection bias for the intervention was likely limited.[31]

Table 3 presents the results for the 7 outcomes of interest. The third row of the table shows that, with the exception of PTSD, the unadjusted outcomes were generally better for the intervention cases at follow-up, with a lower proportion of adverse outcomes for depression, alcohol dependence, binge drinking in the past year, as well as lower PTSD symptom severity, depression symptoms, and anxiety symptoms in the past month. The last row of this table presents the results showing the average treatment effect after 1:5 matching was achieved using the algorithm described. As can be seen, after matching, workers with employers that offered brief interventions had less severe PTSD symptoms, but no difference was found in the prevalence of PTSD in the past year. However, overall, the 2-year outcomes suggested that the brief worksite interventions were helpful for workers. The intervention cases had better outcomes at follow-up for major depression, alcohol dependence, and binge drinking in the past year, as well as reduced PTSD, depression, and anxiety symptoms in the past month.

DISCUSSION

Consistent with a preliminary report and using more advanced analytical methods, it appears that brief worksite interventions provided by NYC employers after the events of September 11, 2001 were associated with better mental health status across a spectrum of outcomes. Although the differences found were not huge, they were consistent and multifaceted. As was seen, these outcomes included a significant reduction in major depression, alcohol dependence, binge drinking, PTSD symptom severity, and in depression and anxiety symptoms, compared with individuals who did not receive this intervention. The consistency of results across the outcomes examined was somewhat surprising, but we think reinforces our findings. Our study suggests that a significant number of NYC employers brought in mental health professionals to provide services to employees at the worksite. Previous research had suggested that the majority of adults in NYC did not seek mental health services in the community following the WTCD event, even though they may have benefited and these were provided at no or little costs.[3,4] The current study suggests, however, that those who did receive brief worksite crisis intervention counseling provided by employers appeared to have benefited as many as 2 years after the WTCD.

A limitation of our analysis, of course, was that our sample was not based on random assignment of cases.[35] Instead, some employers elected to provide crisis interventions for employees. In addition, the provision of these services was not completely random, as we noted

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above. Although we controlled for potential bias by propensity score matching in our statistical models,[38] it is possible that our results are still biased because of “unmeasured” variables affecting outcomes, such as differences in the types of employers that offered interventions or some other unmeasured factor.[33,34,38,40] For example, it is possible that the interventions were confounded, whereby these were offered by more compassionate employers and it was chiefly this reason for the results found. Another potential limitation was that we over-sampled treatment seekers as part of our original study design,[4] and this may have biased our study results. Some other possible limitations include we omitted individuals without a telephone and those who did not speak either English or Spanish, hence selection bias may have affected our study results. Given that the sample matched the 2000 Census for NYC,[17,45] the absence of these households did not appear to have introduced a clear demographic bias at least. Nevertheless, we are limited in generalizing to other ethnic/language groups in NYC. In addition, given our study’s completion rate, nonresponse bias could have affected our results as well.[35] Another limitation was that while our mental health measures were based on standardized and validated scales and our treatment exposure variable was based on tested and standardized survey questions, these variables were based on self-report and, therefore, may contain recall bias. Furthermore, we did not know how soon after the disaster the worksite interventions actually occurred, although anecdotal evidence suggested that these generally were shortly after the attacks in most cases.[32] Finally, our sample size, especially for the intervention group was limited and this may have adversely affected our results.

Despite these limitations, it appears that brief crisis interventions at the worksite after the WTCD were possibly effective for as long as 2 years after intervention. In the current study, approximate 7% of New Yorkers reported receiving brief crisis interventions at the worksite after the WTCD event. This number was not insignificant, considering that many New Yorkers did not seek after-disaster mental health services on their own.[4] In addition, it had been reported that traditional barriers to mental health care seeking tended to persist among the public,[46] even given the extensiveness of service availability.[3,4,6] Although the impact of the worksite interventions on mental health status was noteworthy, the effect of these services on substance abuse was unexpected. Exposure to psychologic trauma has long been suspected in the onset of substance abuse,[47] and studies also have found evidence for this among the general population post-WTCD.[17,45,47]

As indicated, after-disaster crisis interventions have been in used for some time. However, the effectiveness and safety of these crisis interventions have been debated.[10,11] Although we plan to continue our evaluation, our current research suggests that worksite emergency mental health services were associated with better outcomes for New Yorkers as long as 2 years after the disaster. However, it is important to stress that although the propensity method represents an advance in observational research,[42] this study does *not* suggest that brief, single-session interventions are effective.[10] Rather, it suggests that 1 to 3 brief, multimodality mental health interventions conduct by professionals at the worksite appear to be an effective intervention after a catastrophic event. Given the current environment, we think, this finding may have implications for the practice of occupational medicine worldwide.[48,49] On the basis of our current findings, this study suggests that professional crisis interventions might be considered a potential strategy at the worksite for employees affected by large-scale community disasters. However, we note that the reasons for this association are unclear at this time (p. 173),[50] but may be due to mostly indirect affects, such as the perceived de-stigmatization of later treatment-seeking or by facilitating professional referrals, or by some other indirect outcome of the sessions. Nevertheless we believe our findings are intriguing will likely require further investigation and elaboration.

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TABLE 1

Study Crisis Intervention Descriptive Statistics for Population Baseline Survey (n = 2368)

Intervention Characteristics	% (Weighted)	95% CI	n (Unweighted)
No. brief crisis sessions			
None	89.9	88.4–91.3	2012
1	4.4	3.4–5.6	123
2–3	3.6	2.9–4.5	134
4+	2.1	1.7–2.8	89
Percent any brief crisis sessions	10.2	8.9–11.8	356
No. brief crisis sessions at worksite			
None	93.5	92.4–94.5	2124
1	3.4	2.6–4.4	103
2–3	2.3	1.8–2.8	99
4+	0.9	0.6–1.3	42
Percent any brief crisis sessions at worksite	6.5	5.5–7.6	244
Content of brief sessions, among those having *any* sessions (n = 356)			
Educated about stress symptoms	63.7	56.2–70.7	244
Talked about experiences	62.9	55.0–70.1	264
Taught to cope with things	65.1	57.8–71.7	246
Taught to think positively	64.1	56.8–70.8	238
Taught to evaluate thoughts	57.7	50.4–64.6	206
Taught to deal with emotions	69.1	61.8–75.5	255
Taught to relax	65.9	58.6–72.4	245
Reported helpfulness of crisis intervention sessions, among those having *any* sessions (n = 356)			
Not at all helpful	17.7	12.0–25.4	47
Helped a little	24.5	18.9–31.1	84
Helped some	25.4	20.2–31.6	101
Helped a lot	32.4	26.0–39.5	124

TABLE 2

Descriptive Statistics for Intervention Versus Nonintervention Employees at Follow-up, Unweighted Data (n = 1121)

Independent Variables	All Respondents % (n)	All Respondents 95% CI	Intervention n = 150, % (n)	Nonintervention n = 971, % (n)	$\chi^2(P)$
Borough of residence					
Manhattan	23.5 (263)	21.1–26.0	26.7 (40)	23.0 (223)	2.23 (0.694)
Bronx	14.6 (164)	12.7–16.8	16.7 (25)	14.3 (139)	
Brooklyn	28.8 (323)	26.2–31.5	26.7 (40)	29.2 (283)	
Queens	27.3 (306)	24.7–30.0	24.0 (36)	27.8 (270)	
Staten Island	5.8 (65)	4.6–7.3	6.0 (9)	5.77 (56)	
Age					
18–29	19.3 (216)	17.1–21.7	20.0 (30)	19.2 (186)	1.08 (0.783)
30–44	41.9 (470)	39.1–44.8	44.7 (67)	41.5 (403)	
45–64	35.2 (395)	32.5–38.1	32.7 (49)	35.6 (346)	
65+	3.6 (40)	2.7–4.8	2.7 (4)	3.7 (36)	
Gender					
Male	45.3 (508)	42.4–48.3	34.0 (51)	47.1 (457)	8.95 (0.003)
Female	54.7 (613)	51.7–57.6	66.0 (99)	52.9 (514)	
Education					
Noncollege graduate	47.6 (533)	44.6–50.5	28.7 (43)	50.5 (490)	24.75 (<0.001)
College graduate	52.5 (588)	49.5–55.4	71.3 (107)	49.5 (481)	
Income					
Less than $30,000	26.2 (294)	23.7–28.9	8.0 (12)	29.0 (282)	31.48 (<0.001)
$30,000–$99,999	52.8 (592)	49.9–55.7	68.7 (103)	50.4 (489)	
$100,000+	18.2 (204)	16.0–20.6	21.3 (22)	17.7 (172)	
Income not reported	2.8 (31)	2.0–3.9	2.0 (3)	2.9 (28)	
Race					
White	46.3 (519)	43.4–49.2	50.0 (75)	45.7 (444)	1.09 (0.781)
Black	25.3 (283)	22.8–27.9	22.7 (34)	25.6 (249)	
Latino	21.1 (237)	18.9–23.6	20.7 (31)	21.2 (206)	
Other	7.3 (82)	5.9–9.0	6.7 (10)	7.4 (72)	
Marital status					
Not married	55.5 (622)	52.6–58.4	55.3 (83)	55.5 (539)	0.06 (0.831)
Married	44.5 (499)	41.6–47.5	44.7 (67)	44.5 (432)	
Immigrant status					
Born in United States	67.5 (757)	64.7–70.5	73.3 (110)	66.6 (647)	2.67 (0.103)
Born outside United States	32.5 (364)	29.8–35.3	26.7 (40)	33.4 (324)	
Survey language					
English	95.3 (1068)	93.9–96.4	98.0 (147)	94.9 (921)	2.86 (0.091)
Spanish	4.7 (53)	3.6–6.4	2.0 (3)	5.1 (50)	
Exposure to WTCD					
Low (0–1 events)	19.5 (218)	17.2–21.9	6.0 (9)	21.5 (209)	51.27 (<0.001)
Moderate (2–3 events)	43.7 (490)	40.8–46.6	26.0 (54)	44.9 (436)	
High (4–5 events)	25.8 (289)	23.3–28.4	33.3 (50)	24.6 (239)	
Very high (6+ events)	11.1 (124)	9.4–13.0	24.7 (37)	9.0 (87)	
Lifetime depression					
No	74.6 (836)	71.9–77.0	70.0 (105)	75.3 (731)	1.91 (0.167)
Yes	25.4 (285)	23.0–28.1	30.0 (45)	24.7 (240)	
Ever had any mental health treatment					
No	48.3 (541)	45.3–51.2	40.0 (60)	49.5 (481)	4.73 (0.03)
Yes	51.7 (580)	48.8–54.7	60.0 (90)	50.5 (490)	
Peri-event panic attack					
No	88.2 (988)	86.1–89.9	81.3 (122)	89.2 (866)	7.66 (0.006)
Yes	11.9 (133)	10.1–13.9	18.7 (28)	10.8 (105)	

Independent Variables	All Respondents % (n)	All Respondents 95% CI	Intervention n = 150,% (n)	Nonintervention n = 971,% (n)	$\chi^2(P)$

TABLE 3

Results For Crisis Interventions at Follow-Up Showing Unmatched and Propensity Score Matched Average Treatment Effects 2 Years After WTCD for Intervention (n = 150) Versus Nonintervention Employees (n = 971; Unweighted)

Results	PTSD in Past Year, Percent (95% CI)	Depression in Past Year, Percent (95% CI)	Alcohol Dependence in Past Year, Percent (95% CI)	Binge Drinking in Past Year, Percent (95% CI)	PTSD Symptom Severity in Past Month, Mean (95% CI)	Symptoms in Past Month, Mean (95% CI)	Month, Mean (95% CI)
Intervention Cases (n = 150)	6.7 (3.6–12.0)	10.7 (6.6–16.7)	1.3 (0.3–5.2)	9.3 (5.6–15.2)	1.5 (1.1–2.0)	49.6 (48.3–51.2)	49.9 (48.5–51.3)
Unmatched nonintervention cases (n = 971)	4.5 (3.4–6.0)	14.3 (12.8–17.3)	4.4 (3.3–5.9)	16.1 (13.9–18.5)	2.3 (3.3–5.9)	51.0 (50.4–51.6)	50.1 (49.5–50.7)
Difference for intervention vs. nonintervention¶	2.2 (—)	−3.6 (—)	−3.1 (—)	−6.8 (—)	−0.8 (—)	−1.4 (—)	−0.2 (—)
Propensity Matched Results	Percent Intervention Effect (SE)	Percent Intervention Effect (SE)	Percent Intervention Effect (SE)	Percent Intervention Effect (SE)	Mean Intervention Effect (SE)	Mean Intervention Effect (SE)	Mean Intervention Effect (SE)
Propensity score results based on 1:5 matching§	1.9 (0.020)	−7.2 (0.030)*	−4.8 (0.017)‡	−5.5 (0.032)†	−0.8 (0.327)*	−1.7 (0.778)*	−1.4 (0.813)†

†*P* < 0.10;

**P* < 0.05;

‡*P* < 0.01.

§To be consistent with unmatched crude results shown in table above, proportions were converted to percents, standard errors were left unchanged.

¶Since these are unmatched cases, CIs/SEs cannot be calculated.