

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Proximity to terror and post-traumatic stress: A follow-up survey of governmental employees after the 2011 Oslo bombing attack
AUTHORS	Hansen, Marianne; Nissen, Alexander; Heir, Trond

VERSION 1 - REVIEW

REVIEWER	Yuval Neria, Professor of Clinical Psychology, Departments of Psychiatry and Epidemiology, Columbia University, NY.
REVIEW RETURNED	22-Feb-2013

THE STUDY	<p>This paper is based on a cross sectional study conducted 10 months after exposure to mass trauma. The paper should be carefully edited throughout.</p> <p>Only one outcome measure was assessed (PTSD), with a low response rate.</p> <p>In general, the findings have only marginal contribution to what is already known after mass trauma events (dose response relations between severity of exposure and PTSD).</p> <p>I found only little synthesis of previous knowledge about risk factors of PTSD, in the design of the study or when interpreting the results. Overall, shallow and not innovative, study, and a poorly written paper.</p>
GENERAL COMMENTS	<p>This paper is based on a cross sectional study conducted 10 months after exposure to mass trauma. The paper should be carefully edited throughout. Weaknesses of the paper are multiple including: 1) Only one outcome measure was assessed (PTSD), 2) Low response rate, 3) The findings have a marginal contribution to what is already known after mass trauma events (dose response relations between severity of exposure and PTSD), 4) Little synthesis of previous knowledge about mass trauma and mental health in the design of the study or when interpreting the results.</p>

REVIEWER	<p>Tom Lundin, professor Dept of Neuroscience Uppsala University Sweden</p> <p>I have no conflicts of interest. I find this article very well suitable for publication. The background makes it important to be published.</p>
REVIEW RETURNED	03-Mar-2013

THE STUDY	The following references are missing in the referencelist: Weisaeth &
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	Eitinger 1993, Jordan 2004, Norris 2002, Shimazu 2008, Smith 2008, Sherman 2012
GENERAL COMMENTS	This is a very good paper; but look through and complete the reference list!

REVIEWER	Michael Duffy Senior Lecturer Cognitive Psychotherapist (specializing in Trauma Disorders) Queen's University Belfast Northern Ireland
REVIEW RETURNED	26-Mar-2013

GENERAL COMMENTS	<p>need to be more explicit and clarify the issue of protection of confidentiality in relation to questionnaires and non-identification of employees by research team and employer, plus how this matter was explained to staff - please comment on how the process may have affected response rate</p> <p>need to explain if staff were offered a follow up service - to made available to those who met criteria for PTSD and if so how this would be offered respecting anonymity</p> <p>This paper addresses an important issue - the mental health of Government employees</p> <p>exposed to terrorist attacks. The abstract conclusion suggests implications for planning and</p> <p>prioritizing health services after such attacks and the large sample size is useful. It would</p> <p>have been useful if the authors had selected the directly exposed group and then from the</p> <p>regression analysis considered how much of the variance in PTSD is explained by the other</p> <p>known predictor variables if the data set allows for such analysis (see Ehrling, Ehlers and</p> <p>Glucksman, 2006; - in this study, most of the established predictors together explained 40–46% of the variance of PTSD, phobia and depressive symptom severity).</p> <p>Exposure and other predictors of PTSD</p> <p>The study is limited by only considering the relationship between proximity to the</p> <p>bomb and PTSD. It is already well established in the literature that direct exposure is a</p> <p>predictor of PTSD so this is not a novel finding. Also you need to</p>
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	<p>explain what proximity and "present at work" mean in this study in relation to exposure- provide more details in relation to the direct exposure group- what were these respondents exposed to; were they exposed to gruesome scenes, did they see people dead or injured, did they think they were going to die, were any of these respondents injured, etc. ?</p> <p>Whilst exposure variables have been traditionally recognized as important predictors (length of exposure; type of exposure etc.) distinctions within the direct exposure group are important- what factors differentiate between those who are more or less likely to develop PTSD? Repeatedly factors such as gender, intelligence, previous traumas, previous mental health problems, prior traumas and social supports have been found to be risk factors for PTSD. Did you capture any other of these factors in your data set?</p> <p>There are important studies on predictors that you can compare this "employee" group responses against. In one meta analysis, seven variables were identified as the best-established predictors of PTSD following trauma: prior trauma, prior psychological adjustment, family history of psychopathology, perceived life threat during the trauma, post-trauma social support, peritraumatic emotional responses and peritraumatic dissociation (Ozer, Best, Lipsey, & Weiss, 2003). In another meta analyses, (Brewin, Andrews and Valentine, 2000), risk factors for PTSD included: gender (female), younger age, low socio-economic status, lack of education , low intelligence, race (minority status), psychiatric history, childhood abuse , previous trauma, other adverse childhood, family psychiatric history , perceived life threat, perceived support, peri-traumatic emotions and peri-traumatic dissociation. Shalev and colleagues found that the presence of depression in the period following the traumatic incident was a key predictor (Shalev, Freedman, Peri, Brandes, Sahar, Orr, & Pitman, 1998). Are you able to comment on these factors? For example, studies suggest that cognitive variables such as rumination and memory fragmentation may be strongly associated with severe PTSD (Murray, Ehlers, & Mayou, 2002). Are you able to comment from your data set on these issues? In addition to the PCL were any measures used in this study to capture other such predictors ?</p>
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Findings

You need to add commentary on age and educational level not being associated with PTSD in

this study which is contrary to many other studies.

.Additional points and suggested changes:

page 6, line 8 - staff questionnaire access via a personal code- was this confidential; did the research team or employer have access to identity of staff -need to explain- might this factor be related to non-responders?

page 6- line 53 - add gender to the list of demographic characteristics

page 7 - line 27 - change "employees" to "responders"

page 8 line 49- can you describe the sample as a low risk group simply because of the protective factors high education and cohesive working environment- how does the group score on the other predictors of PTSD identified in many studies- see two meta analyses below

p 10 - line 25 - you suggest a number of factors may explain the lower levels of PTSD in leaders. Are you able to comment on resilience. Various studies have shown that resilience correlates negatively with posttraumatic psychopathology (Ahmed, 2007; Alim et al., 2008; Bonanno, 2004, 2005; Bonnano, Galea, Bucciarelli, & Vlahov, 2006, 2007. Did the leaders receive any special training that may have contributed to resilience, sense of coherence etc.

p 10 - line 41- the authors comment "the sample is probably not confounded by pre-existing psychopathology and other risk factors" - are you able to justify these comments- did you ask about prior traumas, previous mental health and other risk factors identified in previous studies and if so then you need to include these responses to support the above statement or remove

p 10 - line 48- response rate - it appears from table 1 that 9.4 % of non-responders were present at the bomb scene- how did you identify this number from "non-responders" need to explain

p 11, line 32- the comment that many employees "had acute stress reactions" needs to be supported by a reference of other source.

p 11, line 38 - install the word "and" connect sentence ending in "attack" with next sentence beginning with "That"

p 11, line 47 - replace the word "on" with "a".

- I cannot see any reference to table 1 in the main text- (do you need this table- most of the details are explained in the paper)?

- The authors need to be careful with terminology, in the section on participants (page 5, line

31) it is stated "44 were excluded from the group"- yet in table 1 "excluded group" means

something else (n= 1550) (non-responders)

- the age range of the sample would be helpful in table 12 (already in text).

There are other useful studies on PTSD rates in health exposed to terrorist attacks . For

example, studies from Omagh, Northern Ireland consider health staff exposed to trauma -

these studies also include follow up data: see Luce, A., Firth-Cozens, J., Midgley, S., &

Burges, C. (2002). After the Omagh bombing: Post-traumatic stress disorder in health

service staff. *Journal of Traumatic Stress*, 15, 1: 27–30

Other important papers on prevalence -you can compare rates with this sample of employees:

Kessler et al (1995) estimated a lifetime prevalence of PTSD of 7.8% and found that the risk of developing PTSD after a traumatic event is 8.1% for men and 20.4% for women.

Breslau and colleagues (1991) found an overall risk of developing PTSD after a traumatic incident of 23.6% with a gender difference of 13% for men and 30.2% for women

Other important references you may wish to check

	<p>Ahmed, A. S. (2007). Post-traumatic stress disorder, resilience and vulnerability. <i>Advances in Psychiatric Treatment</i>, 13, 369-375.</p> <p>Bonanno, G. A. (2005). Resilience in the face of potential trauma. <i>American Psychological Society</i>, 14, 135-138.</p> <p>Bonanno, G. A., Galea, S., Bucciarelli, A., & Vlahov, D. (2006). Psychological resilience after disaster. New York City in the aftermath of the September 11th terrorist attack. <i>Psychological Science</i>, 17, 181-186.</p> <p>Brewin, C.R., Andrews, B., & Valentine, J. D. (2000). Meta-analysis of risk factors for post-traumatic stress disorder in trauma-exposed adults. <i>Journal of Consulting and Clinical Psychology</i>, 68, 748–766.</p> <p>Ehlers, A., Mayou, R. A., & Bryant, B. (1998). Psychological predictors of chronic PTSD after motor vehicle accidents. <i>Journal of Abnormal Psychology</i>, 107, 508–519.</p> <p>Kessler, R. C., Sonnega, A., Bromet, E., Hughes, M., & Nelson, C. B. (1995). Posttraumatic stress disorder in the National Comorbidity Survey. <i>Archives of General Psychiatry</i>, 52, 1048–1060.</p> <p>Ozer, E. J., Best, S. R., Lipsey, T. L., & Weiss, D. S. (2003). Predictors of post-traumatic stress disorder and symptoms in adults: A meta-analysis. <i>Psychological Bulletin</i>, 129, 52–73.</p>
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REVIEWER	Clark, David King's College London
REVIEW RETURNED	31-Mar-2013

THE STUDY	Only 56% of the potential sample participated and there is evidence that those who participated are not fully representative (more women than expected). However, 56% is a good response rate for this type of study.
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GENERAL COMMENTS

The main aim of this report is to determine the prevalence of PTSD among Norwegian Ministry workers who were, or were not, at work in Oslo Ministries during the 22nd July 2011 bombing. All relevant individuals were invited to take part in an online survey and 56% did so. There was some evidence of a sample bias among participants as women were over-represented.

Initial symptoms of PTSD are common after exposure to highly traumatic events. For many people these symptoms decline in the ensuing months but for a subset of people they become chronic. By conducting the survey 10 months after the bombing the authors have chosen to focus on chronic PTSD. This makes sense as natural recovery after 10 months is likely to be modest and the authors were concerned to document the need for further therapeutic intervention.

PTSD symptoms were elicited by a well-known self-report measure. Participants indicated whether they were present in the Ministries during the bombing and, if not, whether they were elsewhere in Oslo, were in Norway or were abroad.

The key findings are clear. In line with the findings from the 7/11 events in the USA, people who were present in the ministries have a markedly increased incidence of PTSD. Those who were not in the Ministries do not. The results for those present are not a surprise. They are very much in line with previous work. The findings for those who were in Oslo but not at work are more novel but I think they have been over-interpreted. The authors infer that the sense of being a target for the bomber is the key variable that creates an increased rate of PTSD. However, those in Oslo but not in the ministries will also have been (on average) further away, irrespective of whether they thought they were being targeted.

It is important for the field to understand the variables that determine whether chronic PTSD is experienced among people exposed to trauma. The paper provides little insight into this question. Among the 207 people at work during the bombing, female gender was associated with an increased risk and leadership responsibility with a decreased risk. The authors don't discuss the extent to which these findings may be an artifactual effect of sampling biases. Is it possible that affected women were more willing to complete the survey and that affected people with leadership responsibilities were less likely to complete the survey?

It is a shame that the survey did not cover psychological variables (thought suppression, beliefs about intrusive memories, etc) that previous research has shown to be a good predictor of PTSD as these variables are amenable to therapy and so have greater practical significance than gender or management role.

The final paragraph of the article includes a new claim that is not supported by any evidence. The claim is that many employees who were NOT at work during the bombings experienced acute stress reactions. How do we know? No data is presented and no other empirical studies of the Oslo bombing are cited. The statement should be removed or substantiated.

VERSION 1 – AUTHOR RESPONSE

Reviewer: Yuval Neria, Professor of Clinical Psychology, Departments of Psychiatry and Epidemiology, Columbia University, NY.

This paper is based on a cross sectional study conducted 10 months after exposure to mass trauma. The paper should be carefully edited throughout.

Only one outcome measure was assessed (PTSD), with a low response rate.

In general, the findings have only marginal contribution to what is already known after mass trauma events (dose response relations between severity of exposure and PTSD).

I found only little synthesis of previous knowledge about risk factors of PTSD, in the design of the study or when interpreting the results. Overall, shallow and not innovative, study, and a poorly written paper.

We are sorry that our paper was found to be unoriginal and only contribute marginally to what is already known after mass trauma events. However, shortly after the tragedy, experts throughout the world put forth different opinions on how to define and reach individuals most in need of help. The goal of the paper was to investigate how decision makers in Norway approached this task, and to examine whether health care resources were allocated in an effective and scientifically justifiable manner in the aftermath of the bombing. The paper concludes in favor of how decision makers responded, giving priority to the individuals who were at work at the time of the explosion. We believe that this is an important message that can be helpful in the planning and future implementation of mental health care interventions after work place traumatic events.

We have done our best to edit the paper throughout.

Reviewer: I have no conflicts of interest. I find this article very well suitable for publication. The background makes it important to be published.

Tom Lundin, professor

Dept of Neuroscience

Uppsala University

Sweden

This is a very good paper; but look through and complete the reference list!

The following references are missing in the referencelist: Weisaeth & Eitinger 1993, Jordan 2004, Norris 2002, Shimazu 2008, Smith 2008, Sherman 2012

We are grateful for the positive comments. We apologize for not completing the reference list. This has been done in the revised paper.

Reviewer: Michael Duffy

Senior Lecturer

Cognitive Psychotherapist (specializing in Trauma Disorders) Queen's University Belfast Northern Ireland

This paper addresses an important issue - the mental health of Government employees exposed to terrorist attacks. The abstract conclusion suggests implications for planning and prioritizing health services after such attacks and the large sample size is useful. It would have been useful if the authors had selected the directly exposed group and then from the regression analysis considered how much of the variance in PTSD is explained by the other known predictor variables if the data set allows for such analysis (see Ehring, Ehlers and Glucksman, 2006; - in this study, most of the established predictors together explained 40–46% of the variance of PTSD, phobia and depressive symptom severity).

The aim of our study was to determine the risk of developing PTSD as a function of localization at the time of the explosion. The aim was developed as a consequence of the need to prioritize health care

resources in the aftermath of the terror attack. We wanted to investigate whether the choices made by the health authorities were adequate and scientifically justifiable. Our findings support the way resources were allocated, and we believe that this is an important message to communicate to the scientific community, health officials and policy makers.

In order to explain a considerable part of the variance in PTSD it is necessary to look at variables which, in the causal pathway, are close to PTSD symptoms. Such variables are for example perceived life threat, negative emotions and dissociation during the traumatic event, cognitive processing during and after the event, memory disorganization, negative appraisals of the trauma and its sequelae, and ongoing dissociation. We think, however, that this is outside the scope of this article, as these variables were unavailable when decisions on how to allocate resources had to be made.

Need to be more explicit and clarify the issue of protection of confidentiality in relation to questionnaires and non-identification of employees by research team and employer, plus how this matter was explained to staff - please comment on how the process may have affected response rate need to explain if staff were offered a follow up service - to made available to those who met criteria for PTSD and if so how this would be offered respecting anonymity.

We have now included a more detailed explanation of how anonymity was secured, and how this was explained to the study participants (Methods section, paragraph 1). We have also added a section on follow-up service (Methods, last part of participant section)

Exposure and other predictors of PTSD

The study is limited by only considering the relationship between proximity to the bomb and PTSD. It is already well established in the literature that direct exposure is a predictor of PTSD so this is not a novel finding. Also you need to explain what proximity and "present at work" mean in this study in relation to exposure- provide more details in relation to the direct exposure group- what were these respondents exposed to; were they exposed to gruesome scenes, did they see people dead or injured, did they think they were going to die, were any of these respondents injured, etc. ?

We have included information on what employees who were present at work were exposed to (Results, last paragraph, and Methods/Measures, last sentence).

Whilst exposure variables have been traditionally recognized as important predictors (length of exposure; type of exposure etc.) distinctions within the direct exposure group are important- what factors differentiate between those who are more or less likely to develop PTSD? Repeatedly factors such as gender, intelligence, previous traumas, previous mental health problems, prior traumas and social supports have been found to be risk factors for PTSD. Did you capture any other of these factors in your data set?

There are important studies on predictors that you can compare this "employee" group responses against. In one meta analysis, seven variables were identified as the best-established predictors of PTSD following trauma: prior trauma, prior psychological adjustment, family history of psychopathology, perceived life threat during the trauma, post-trauma social support, peritraumatic emotional responses and peritraumatic dissociation (Ozer, Best, Lipsey, & Weiss, 2003). In another meta analyses, (Brewin, Andrews and Valentine, 2000), risk factors for PTSD included: gender (female), younger age, low socio-economic status, lack of education, low intelligence, race (minority status), psychiatric history, childhood abuse, previous trauma, other adverse childhood, family psychiatric history, perceived life threat, perceived support, peri-traumatic emotions and peri-traumatic dissociation. Shalev and colleagues found that the presence of depression in the period following the traumatic incident was a key predictor (Shalev, Freedman, Peri, Brandes, Sahar, Orr, & Pitman, 1998). Are you able to comment on these factors? For example, studies suggest that cognitive variables such as rumination and memory fragmentation may be strongly associated with severe PTSD (Murray, Ehlers, & Mayou, 2002). Are you able to comment from your data set on these

issues? In addition to the PCL were any measures used in this study to capture other such predictors ?

Factors that may help to shed light on the etiology of PTSD will be published later, particularly with respect to a longitudinal perspective. As stated above, we think that this is outside the scope of the present article.

Findings

You need to add commentary on age and educational level not being associated with PTSD in this study which is contrary to many other studies.

We have added commentaries on age and education in the discussion section. Thank you for this appeal. As a result, we discovered a mistake in Table 3 concerning education. This is now corrected both in the table and in the text.

Additional points and suggested changes:

page 6, line 8 - staff questionnaire access via a personal code- was this confidential; did the research team or employer have access to identity of staff -need to explain- might this factor be related to non-responders?

This has been addressed in the first paragraph of the Methods/Participants section, and the first paragraph of the Methods/Measures section.

page 6- line 53 - add gender to the list of demographic characteristics

This has been done.

page 7 - line 27 - change "employees" to "responders"

This has been done.

page 8 line 49- can you describe the sample as a low risk group simply because of the protective factors high education and cohesive working environment- how does the group score on the other predictors of PTSD identified in many studies- see two meta analyses below

We agree that we should be careful not to characterize the sample as a low risk group in this part of the discussion. Thus, we have deleted that sentence.

p 10 - line 25 - you suggest a number of factors may explain the lower levels of PTSD in leaders. Are you able to comment on resilience. Various studies have shown that resilience correlates negatively with posttraumatic psychopathology (Ahmed, 2007; Alim et al., 2008; Bonanno, 2004, 2005; Bonnano, Galea, Bucciarelli, & Vlahov, 2006, 2007. Did the leaders receive any special training that may have contributed to resilience , sense of coherence etc.

Unfortunately, we do not have any information on how leaders scored in terms of resilience, nor do we know if they had undergone special training that would make them more resilient.

p 10 - line 41- the authors comment "the sample is probably not confounded by pre-existing psychopathology and other risk factors" - are you able to justify these comments- did you ask about prior traumas, previous mental health and other risk factors identified in previous studies and if so then you need to include these responses to support the above statement or remove

We have tried to strengthen our argument that our sample probably is more resilient than the average trauma studied population. We know from research that trauma populations often are less resilient than the general population due to psychosocial selection of traumatic experiences.

p 10 - line 48- response rate - it appears from table 1 that 9.4 % of non-responders were present at the bomb scene- how did you identify this number from "non-responders" need to explain

We have added this explanation in table 1.

p 11, line 32- the comment that many employees "had acute stress reactions" needs to be supported by a reference of other source.

We have reformulated the paragraph. We believe it is important to illustrate the dilemma between anecdotal reports and scientific evidence that policy makers may be faced with.

p 11, line 38 - install the word "and" connect sentence ending in "attack" with next sentence beginning with "That"

Both these sentences have been removed on the advice of another reviewer.

p 11, line 47 - replace the word "on" with "a".

The sentence has been removed.

- I cannot see any reference to table 1 in the main text- (do you need this table- most of the details are explained in the paper)?

We have added reference to Table 1 in the text and deleted information from the paper that is also explained in the table.

- The authors need to be careful with terminology, in the section on participants (page 5, line 31) it is stated "44 were excluded from the group"- yet in table 1 "excluded group" means something else (n= 1550) (non-responders)

We have changed the columns in Table 1 to "responders" and "non-responders".

- the age range of the sample would be helpful in table 12 (already in text).

We have included age range in table 1 and deleted this information from the text.

There are other useful studies on PTSD rates in health exposed to terrorist attacks . For example, studies from Omagh, Northern Ireland consider health staff exposed to trauma - these studies also include follow up data: see Luce, A., Firth-Cozens, J., Midgley, S., & Burges, C. (2002). After the Omagh bombing: Post-traumatic stress disorder in health service staff. *Journal of Traumatic Stress*, 15, 1: 27–30

Other important papers on prevalence -you can compare rates with this sample of employees:

Thank you for suggesting more literature. We have now included some of these references in our publication.

Kessler et al (1995) estimated a lifetime prevalence of PTSD of 7.8% and found that the risk of developing PTSD after a traumatic event is 8.1% for men and 20.4% for women.

Breslau and colleagues (1991) found an overall risk of developing PTSD after a traumatic incident of 23.6% with a gender difference of 13% for men and 30.2% for women

Other important references you may wish to check

Ahmed, A. S. (2007). Post-traumatic stress disorder, resilience and vulnerability. *Advances in Psychiatric Treatment*, 13, 369-375.

Bonanno, G. A. (2005). Resilience in the face of potential trauma. *American Psychological Society*, 14, 135-138.

Bonanno, G. A., Galea, S., Bucchiarelli, A., & Vlahov, D. (2006). Psychological resilience after disaster. New York City in the aftermath of the September 11th terrorist attack. *Psychological Science*, 17, 181-186.

Brewin, C.R., Andrews, B., & Valentine, J. D. (2000). Meta-analysis of risk factors for post-traumatic stress disorder in trauma-exposed adults. *Journal of Consulting and Clinical Psychology*, 68, 748–766.

Ehlers, A., Mayou, R. A., & Bryant, B. (1998). Psychological predictors of chronic PTSD after motor vehicle accidents. *Journal of Abnormal Psychology*, 107, 508–519.

Kessler, R. C., Sonnega, A., Bromet, E., Hughes, M., & Nelson, C. B. (1995). Posttraumatic stress disorder in the National Comorbidity Survey. *Archives of General Psychiatry*, 52, 1048–1060.

Ozer, E. J., Best, S. R., Lipsey, T. L., & Weiss, D. S. (2003). Predictors of post-traumatic stress disorder and symptoms in adults: A meta-analysis. *Psychological Bulletin*, 129, 52–73.

Reviewer: David Clark

King's College London

Only 56% of the potential sample participated and there is evidence that those who participated are not fully representative (more women than expected). However, 56% is a good response rate for this type of study.

Thank you for this comment. We think the response rate was quite acceptable.

The main aim of this report is to determine the prevalence of PTSD among Norwegian Ministry workers who were, or were not, at work in Oslo Ministries during the 22nd July 2011 bombing. All relevant individuals were invited to take part in an online survey and 56% did so. There was some evidence of a sample bias among participants as women were over-represented.

Yes, women were slightly overrepresented; however, this should not undermine the key points conveyed in this paper.

Initial symptoms of PTSD are common after exposure to highly traumatic events. For many people these symptoms decline in the ensuing months but for a subset of people they become chronic. By conducting the survey 10 months after the bombing the authors have chosen to focus on chronic PTSD. This makes sense as natural recovery after 10 months is likely to be modest and the authors were concerned to document the need for further therapeutic intervention.

We appreciate the insightful remarks concerning chronic PTSD, recovery and the need for further therapeutic intervention.

PTSD symptoms were elicited by a well-known self-report measure. Participants indicated whether they were present in the Ministries during the bombing and, if not, whether they were elsewhere in Oslo, were in Norway or were abroad.

The key findings are clear. In line with the findings from the 7/11 events in the USA, people who were present in the ministries have a markedly increased incidence of PTSD. Those who were not in the Ministries do not. The results for those present are not a surprise. They are very much in line with previous work. The findings for those who were in Oslo but not at work are more novel but I think they

have been over-interpreted. The authors infer that the sense of being a target for the bomber is the key variable that creates an increased rate of PTSD. However, those in Oslo but not in the ministries will also have been (on average) further away, irrespective of whether they thought they were being targeted.

We agree that we have been too eager in launching hypothesis. We have deleted our speculations about the importance of the sense of being a target for the bomber (in paragraph 4 of the discussion section as well as in the last paragraph of the discussion).

It is important for the field to understand the variables that determine whether chronic PTSD is experienced among people exposed to trauma. The paper provides little insight into this question. Among the 207 people at work during the bombing, female gender was associated with an increased risk and leadership responsibility with a decreased risk. The authors don't discuss the extent to which these findings may be an artifactual effect of sampling biases. Is it possible that affected women were more willing to complete the survey and that affected people with leadership responsibilities were less likely to complete the survey?

We have now included a short discussion in the limitations section on how sampling bias might have affected the results of the study.

It is a shame that the survey did not cover psychological variables (thought suppression, beliefs about intrusive memories, etc) that previous research has shown to be a good predictor of PTSD as these variables are amenable to therapy and so have greater practical significance than gender or management role.

We agree that psychological variables are important in the development of PTSD and that psychological variables also to a certain extent are amenable to therapy. In this study, though, we have chosen to focus on factors that were easily available to decision makers responsible for the allocation of health resources in the aftermath of the terror attack, especially employees' location at the time of the explosion.

The final paragraph of the article includes a new claim that is not supported by any evidence. The claim is that many employees who were NOT at work during the bombings experienced acute stress reactions. How do we know? No data is presented and no other empirical studies of the Oslo bombing are cited. The statement should be removed or substantiated.

VERSION 2 – REVIEW

REVIEWER	Michael Duffy Senior Lecturer Course Director & Cognitive Psychotherapist specializing in Trauma Queens University Belfast, Northern Ireland, UK No competing interests
REVIEW RETURNED	07-Jun-2013

- The reviewer completed the checklist but made no further comments.