PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Association of work-related stress with mental health problems in a special police force unit
AUTHORS	Magnavita, Nicola; Garbarino, Sergio; Cuomo, Giovanni; Chiorri, Carlo

VERSION 1 - REVIEW

REVIEWER	Dr Peter C Winwood University of South Australia
REVIEW RETURNED	07-Mar-2013

THE STUDY	This is not the first time I have been asked to review this paper or at least another version of it in another journal. The current paper would appear to be a 'salami slice', since the other paper was a longitudinal study with 4 data measurements, before deployment, before training for G8 deployment, during deployment and after deployment. I was not impressed by that paper and am equally not impressed with this one.
RESULTS & CONCLUSIONS	The use of the data from the EFI, and DCR scales is not in line with their author's recommendations. I am suspicious that the nature of the results reflects more of the way the data has been manipulated than anything else. Neither scale is now regarded as cutting edge measurement of work related stress compared with the Demand/Resources model of Demorouti and Bakker. The BDI is too blunt for use in a First Responder group, when a PTSD scale or a Psych Injury scale such as the PIRI would have been more appropriate. The low level of depression cases doesn't really say anything about the effects of long term involvement in First Response policing. Other authors have shown consistently higher levels of stress related injury among police using more appropriate measures. None of the tables are in appropriate APA format and are impossible to read meaningfully.
REPORTING & ETHICS	I think this is a 'salami slicing' paper.

REVIEWER	Arthur R. Rademaker, PhD Research Centre Military Mental Healthcare - Dutch Ministry of Defence, and dept. Psychiatry, University Medical Centre Utrecht, the Netherlands
REVIEW RETURNED	19-Mar-2013

THE STUDY	A cross-sectional study does not suffice in this case. More refs of previous studies in the same cohort are required. Literature on the association between depressive symptoms and reward sensitivity was overlooked.
GENERAL COMMENTS	The aim of this study was to examine the relationship between job stress and symptoms of depression in an elite group of Italian policemen. The authors set out to empirically test two models that may explain the relationship between (perceived) job stress and depressive symptoms in what they describe as a carefully selected and highly trained cohort of police officers, who, by virtue of their profession, are frequently exposed to threatening events. For this purpose, the authors collected data from a cohort of specialized police officers, selected and trained to maintain order during the 2009 G8 meeting in Genoa. The authors address an interesting and highly relevant topic, and they were able to obtain an almost perfect response rate in their sample. There are however, a number of issues that need to be resolved. First, the cross-sectional design of the study is an important limitation to the originality and scientific relevance of the study at hand. The limitation associated with cross-sectional data are especially salient in view of their observed relationship between experience of reward and depressive symptoms. It is a well-established fact these are associated and the present study does little to further our knowledge on this subject. Second, I feel that the relevance of the present study could be improved if the data that are presented would be more firmly linked to previously reported findings from this cohort. Relatedly, I am wondering why data on BDI sores are presented only. Did the authors also examine their models in relation to anxiety, burn-out and other variables? Their previous studies make it clear that these variables were assessed in this cohort and I feel that including them into the present study would greatly enhance the relevance of the manuscript. Finally, I recommend that the authors have their manuscript carefully proof-read and corrected by a native English-speaker. Below I provide a detailed review-report. **Abstract/ summary** **1.** **P. 2, line 46-48.** ‘The aim of this work […] risk of mental disorders in policemen’. The study focussed on prevalence of depressive symptoms only. The authors did not examine the prevalence of depressive disorders (the BDI is a screening tool, not a diagnostic instrument), nor did they examine the prevalence of any other mental disorders. This should be corrected. **2.** Please add the depression questionnaire that was used (BDI) and the cut-off point as well as the observed prevalence rate, under the **methods**/ **results** headings**.** **3.** **P. 3, line 74.** Please rephrase ‘Even in special forces ….depression’ into something like ‘Although prevalence rates were low, a positive association between distress (or job stress) and depressive symptoms was found’. **4.** **P.3, line 79-80.** Awkward sentence, please rephrase **Introduction**

	5. The flow and writing style of the first paragraphs could be improved. Also, this section contains several instances of uneven writing/ awkward formulations. **6.** As noted above, the authors should improve the integration of the present study with their previous work. I was surprised to see that they failed to mention a number of their studies the same cohort, including the Garbarino et al (2012) paper, and the Magnavita and Garbarino paper (in press, Am. J. Ind. Med). **7.** **P.4, line 84/85:** 'The impairment […] conditions'. Please provide a reference to support this claim. **8.** **P.4, line 92-96:** The authors state that police officers are 'particularly vulnerable to psychosocial stress'. I am afraid that I have to disagree on this one. There are several papers, including the authors' own previous studies, which suggest the contrary: that police officers are more resilient to stress than civilians. Moreover, in the following lines the authors contradict their previous statement when they state that the likelihood that police officers are exposed to events that are severe enough to cause PTSD, tends to be low. Please elaborate on this issue. **9.** **P.4, line 104-108.** 'pathopysiological reaction may be same […] limbic system makes no such distinction'. This section represents a gross simplification of the brain's stress response and it has no bearing on the study at hand. Therefore, I recommend that these sentences are removed. **10.** **P.5, line 108-110.** Please insert a reference to McEwen (2006). **11.** **P.6, line 243/144.** 'when distress […] disorder'. Please insert a citation. **12.** **P.8, line 183.** Since not all police officers are male, a more gender-neutral wording would be appropriate throughout the document. **Methods** **12.** Since the present study appears to report on the same cohort as some previous studies by this group (e.g., Garbarino ea, 2011, 2012, in press; Magnavita and Garbarino, in press;) the authors should refer to their earlier work to in the methods section. For instance, they provided a detailed description of the ERI and DCS models in the Garbarino et al (2012) paper, and could cite this paper in the Participants section. **13.** As noted before, I am wondering why the authors only included the BDI as an outcome variable in this study. They previously reported that anxiety (STAI) and burn-out (MBI) symptoms were also assessed in this cohort (Garbarino ea., 2012), therefore, I feel that the readers would be interested to know how the models perform in predicting these symptoms. **14.** When were the assessments performed? Apparently distress was measured at three time-points in 2009, and the authors have reported that stress levels varied across assessments (Garbarino et al., 2012). So which of these time points did they use for the present study? **15.** **P.9, line 223/224.** Correct font-size. **16.** **P.9, line 227/228.** Rephrase: 'participation that enhance <u>or counteract</u> the effects of stress'.

	17. **P.10, line 229/230**. Remove 'as this questionnaire …depression screening'. **18.** **P.10, line 236-240**. This section could be more streamlined. Stating that 'a cut-off of >10, which is commonly used for depression screening (ref), was used for the present study' would be sufficient. Additionally, the authors should describe that a score of 10 or higher is indicative of (at least) *mild* depressive symptoms. **Results** **19.** **P.12, line 300.** Replace 'rewards' with 'experience of reward' **Discussion** **20.** A large body of literature exist on the relationship between reward processing and depressive symptoms. See for instance Eshel & Roiser (2010) for a recent (neurobiological) review on this subject. Since reduced reward-sensitivity represents a cardinal trait of depressive disorders, the observed (exclusive) relationship between experience of reward and depressive symptoms in the present study is hardly surprising. The question that remain are whether the observed reduction in experience of reward is an epiphenomenon of the presence of depressive symptoms, whether the ERI and BDI actually tap on the same underlying construct and whether the two are causality related in this sample. The cross-sectional nature of the present study foregoes inferences on this matter and renders the scientific value of the study at hand largely reduced. Merely reporting that there is an association between these variables does little to improve our knowledge of aetiology of stress-related depressive symptoms in this cohort. I am not sure whether this issue can be resolved – one way to examine the relationship would be adopt a longitudinal design – but this issue should at least be thoroughly addressed in the Discussion section. **21.** **P.13, line 317-320 and p. 14, line 340/341.** The results show that when the DCS and ERI models are included in the regression analysis simultaneously, only (one of) the ERI variables – experience of reward- significantly predicts depressive symptoms. Therefore, the DSC model appears to be redundant in this sample. This should be stated more clearly. **22.** **P.13, line 316.** What do the authors mean with 'avant-garde' unit? **23.** **Line 320.** Replace 'mental ill health' with 'depressive symptoms'. **24.** **Line 321.** Rephrase 'cohort reported <u>mild</u> depressive symptoms'. **25.** **P.14, line 343.** Replace 'increased' with 'predicted'. **26.** **P15, line 361.** The amount of exposure to adverse events was not assessed therefore the claim that the cohort was homogeneous with respect to exposure needs to be attenuated.

	Tables **27.** Content of Table 2 is unclear. Please provide labels for the rows in Table 2 to indicate that they reflect different models. **28.** Check font-size of text in Tables. **29.** Abbreviations should be explained in a note under the Table. **Additional References:** Bonde, J.P.E. (2008) Psychosocial factors at work and risk of depression: a systematic review of the epidemiological evidence. Occup Environ Med,65, 438-445. doi:10.1136/oem.2007.038430 Eshel, N. and J. P. Roiser (2010). "Reward and punishment processing in depression." Biological Psychiatry 68(2): 118-124. DOI: 10.1016/j.biopsych.2010.01.027 Garbarino, S., Magnavita, N., Chiorri, C., Brisinda, D, Cuomo, G., Venuti, A., & Fenici,R. (2012) Evaluation of Operational Stress in Riot and Crowd Control Police Units: A Global Challenge for Prevention and Management of Police Task-Related Stress. *J Police Crim Psych*, 27:111–122. DOI 10.1007/s11896-012-9104-8 Magnavita, N., & Garbarino, S. (in press). Is Absence Related to Work Stress? A Repeated Cross-Sectional Study on a Special Police Force. *America Journal of Industrial Medicine*. DOI 10.1002/ajim.22155 McEwen, B. S. (2006). Stress, adaptation, and disease: Allostasis and allostatic load. Annals of the New York Academy of Sciences, 840(1), 33-44.

REVIEWER	Prof. dr. PG. van der Velden INTERVICT, Tilburg University The Netherlands
REVIEW RETURNED	20-Mar-2013

GENERAL COMMENTS	I have read the manuscript, as well as their previous papers on the same study sample. Combining the information of the published papers and submitted manuscript to BMJopen, raised several important questions with regard to all variables measured at each of the 3 waves. Remarkably, the submitted manuscript was positioned as a cross sectional study while in fact they conducted a longitudinal study (see correspondence with Richard Sands). My main point was that I wanted to be sure that depression or any other (mental) health problem or symptoms was/were only measured once (and when?). Otherwise the submitted manuscript could be an example of publishing 'least significant publishable units'. Therefore I contacted your colleague Richard Sands in the past week, to gain information on all variables measured at each of the 3 waves. The author(s) answered my clear question with a relatively long letter. However, the requested information was not provided by the authors (only a small detail). I contacted Richard Sands for the

	second time because I was still inclined to review the paper, but was not satisfied with the answers of the author(s). The author(s) responded again with a similar letter describing their difficulties -that were in my view not abnormal- when conducting the research project. Much to my surprise, the author(s) for the second time still didn't answer my clear and simple question: what did they exactly measure at each of the three waves? (A simple table with 3 columns would solve this 'problem'). Thus, they don't explicitly declare that (mental) health problems or symptoms (including depression) were assessed in only one wave: they leave it open while implicitly and vaguely suggesting that they did measure it only once. I have reviewed many papers in the past years for High Impact and Lower Impact journals and was never confronted with a situation where the author(s), after two clear and simple requests, refused to provide requested and very simple information. I don't want to speculate on the motives of the author(s) to refuse to provide the information after two requests, but I consider this as a serious intentionally act against scientific transparency. Therefore, for the first time in my career, I have decided to withdraw from further reviewing this manuscript. In sum, they don't explicitly declare that (mental) health problems or symptoms (except depression) were assessed in only one wave. It shall be clear however, that when it turns out that the authors did have data on any (mental) health problem or symptom assessed at the other two waves we must consider this as a falsification of data, i.e. deceptive reporting of findings and omission of conflicting data, or willful suppression of data. I realize that this is very serious suggestion, but the authors were totally free to provide the information after two requests: it isn't that difficult to declare that no other (mental) health problems were assessed at the other waves. I leave it up to you to further examine if this is a case of scientific misconduct. It shall be clear however that, at least in my view, there are enough signs to investigate this possibility. I hope my letter is of help and in case you need more information, please let me know.

VERSION 1 – AUTHOR RESPONSE

Reviewer: Dr Peter C Winwood
University of South Australia

A.The current paper would appear to be a 'salami slice', since the other paper was a longitudinal study with 4 data measurements, before deployment, before training for G8 deployment, during deployment and after deployment.
RESPONSE: This study is part of a research started in 2009 in a special police team constantly employed in tasks of public policy. This department has always been at the forefront and some of its components already took part in the G8 meeting in Genoa in 2001. Studying stress in this group is like studying the tip of the iceberg. Both workers and leaders of the police are carefully focusing on our work. This explains why not all the results collected are immediately available for publication. However, we are continuing to collect data in a longitudinal way, and we will publish these observations as soon as possible.
Our previous studies have shown that it is wrong to think that a single highly risky event should invariably cause stress for police officers. On the contrary, the stress felt by workers may be greater

during the year, when they are confronted with unpredictable hazards, rather than at an event in which they know the hazard. To demonstrate this fact we have adopted a short longitudinal epidemiological type design in a previous work (see ref. 5).
In other studies, we investigated the association of personality with occupational stress, and that of stress and sickness absence, and there we used an average value of stress in the period under examination. These papers were written together with the present work, but they have just been published, so we have added the references in this review.
In this present study, we want to see if there is association between occupational stress and mental disorders. For this reason we have integrated into a single measure the three assessments of perceived stress carried out in three different moments the same semester.

B.The use of the data from the EFI, and DCR scales is not in line with their author's recommendations. I am suspicious that the nature of the results reflects more of the way the data has been manipulated than anything else. Neither scale is now regarded as cutting edge measurement of work related stress compared with the Demand/Resources model of Demorouti and Bakker.
We assume that with EFI and DCR the reviewer is referring to ERI and DCS, respectively. We agree about the validity of the Demorouti and Bakker's model, but it does not have related measures such as the ERI and the DCS. Demerouti, Bakker, Nachreiner and Schaufeli (1999, J Appl Psychol, 86, 499-512) reported that they developed job demand and job resources measures selecting items from other measures, Karasek's included. To our best knowledge at the time we carried out the study, there was no validated Italian version of such measures, and the use of non validated translated instruments is likely to introduce biases. Instead, convincing evidence of the validity and reliability of the Italian DCS and ERI was already available, hence we chose these measures and employed them following to the recommendations of the authors.

C.The BDI is too blunt for use in a First Responder group, when a PTSD scale or a Psych Injury scale such as the PIRI would have been more appropriate.
The PIRI is undoubtedly an effective indicator of acute psychological harm. Unfortunately it has been published in the second half of 2009 and it was unknown at the time of our study. It is still not available in Italian. This questionnaire has been recently translated into Italian by one of the authors of this article (NM) , with the collaboration of the author of the questionnaire (Peter Winwood) and it is currently under study in some Italian workplaces. The paper containing the validation study of the Italian version of the PIRI will not be available before 2014.
The purpose of this study was not to analyze the association between acute psychological trauma and damage, but to check whether a certain level of occupational stress corresponds to an alteration of mental health. For this purpose, the BDI is the one, if not the best tool now available in Italian and has been used until now in more than 4200 studies in the world, as this questionnaire performs better than other tests for depression screening (see our ref. 38).

D.The low level of depression cases doesn't really say anything about the effects of long term involvement in First Response policing.
We agree. To evaluate the association between prolonged stress and mental health we must use a longitudinal method, as we said in the Discussion and as we committed to do in the future. In this cross-sectional study we investigated the association of the self-perceived stress level and that of depression. The police officers had a different length of service, and for this reason we have used a multiple regression approach to partial out the effect of this and other background variables.

E. Other authors have shown consistently higher levels of stress related injury among police using more appropriate measures.

We agree that exposure to acute stressors can induce in a minority of workers a post-traumatic stress disorder (PTSD). In this population, however, none of the workers (who are undergoing medical examination by one of the authors, SG, a physician specialist in neuro-physiology with PhD in mental illnesses) had symptoms consistent with PTSD.
Recent cohort studies, such as the one of van der Velden et al. that we mentioned in the references (ref. 66), report that police officers are not a high-risk group for the development of mental health disturbances. However, we believe that the issue of mental health is central in the health surveillance of police and fight to ensure that these topics are not covered, but openly debated in the scientific literature.

F.None of the tables are in appropriate APA format and are impossible to read meaningfully.
Tables have been reformatted to meet APA format.

Reviewer: Arthur R. Rademaker, PhD
Research Centre Military Mental Healthcare - Dutch Ministry of Defence,
dept. Psychiatry, University Medical Centre Utrecht, the Netherlands

1.A cross-sectional study does not suffice in this case… First, the cross-sectional design of the study is an important limitation to the originality and scientific relevance of the study at hand… The limitation associated with cross-sectional data are especially salient in view of their observed relationship between experience of reward and depressive symptoms. It is a well-established fact these are associated and the present study does little to further our knowledge on this subject
We agree. As we stated above (response D) we will be pleased to publish on this journal the results of the longitudinal study we are conducting on the same population. After 2009 the workers were followed and their personal levels of occupational stress have been checked annually. Our plan is to control the level of depression, anxiety and burnout five years after the start of the observations. We cannot agree that our study adds little knowledge: in fact, it is the first study conducted on the police in Italy, and is one of the few in the world where it is possible to know the state of health of a highly selected group of first-responders police officers.

2.More refs of previous studies in the same cohort are required. …Second, I feel that the relevance of the present study could be improved if the data that are presented would be more firmly linked to previously reported findings from this cohort. Relatedly, I am wondering why data on BDI sores are presented only. Did the authors also examine their models in relation to anxiety, burn-out and other variables? Their previous studies make it clear that these variables were assessed in this cohort and I feel that including them into the present study would greatly enhance the relevance of the manuscript.
We agree. We have included the citation of our previous work on the same cohort. We have also included data about anxiety and burnout, which we had not mentioned in the previous version because they are less significant than those on depression and tangential with respect to the aim of the study, that focused on depression.

3.Literature on the association between depressive symptoms and reward sensitivity was overlooked.
4.Finally, I recommend that the authors have their manuscript carefully proof-read and corrected by a native English-speaker. Below I provide a detailed review-report.
We substantially revised the paper taking into account these suggestions.

VERSION 2 – REVIEW

REVIEWER	Rademaker, Arthur Dutch Ministry of Defence, Research Center Military Mental Healthcare
REVIEW RETURNED	31-May-2013

THE STUDY	The study is cross-sectional but requires a longitudinal approach. Regression anylses are described for depressive symtoms as oucome variable only, not for anxiety and burn-out. Also Tables referring to anxiety and burn-out are presented as supplementary files rather than in the main text. References have not been porperly updated before submission of this revision.
RESULTS & CONCLUSIONS	Interpretation and conclusion of the analyses pertaining to anxiety and burn-out should be elaborated and improved upon.
REPORTING & ETHICS	The authors have not adequately adressed the issues that were raised concerning 'salamy-slicing'of their data set. Although they have now included some additional outcome variables in their revision, they have provided no satisfactory explanation as to why they choose to aggregate stress levels from three seperate assments into 1 variable. Nor have they provided a clear answer as to why the refrained from reporting prospective analyses.
GENERAL COMMENTS	The authors thoroughly revised the manuscript and adequately addressed many of my previous comments. Nevertheless, there are still a number of issues that need to be resolved to improve the Manuscript. First, in addition to the response to the previous reviewing, the authors made several changes to the Introduction section that did little to improve the quality of the manuscript. Sections that should have been retained in the Introduction were moved to the Discussion. Second, the authors failed to provide a satisfactory explanation on why they chose to retain cross-sectional analyses only, while their response indicates that prospective examination of the association between job stress and mental health would also be possible. Relatedly, by retaining the original cross-sectional analyses, they have done little to improve the salience of the observed tautological association between depressive symptoms and experience of reward. Third, I feel that the authors have not adequately addressed my previous comment to place the present study within the framework of their previous work in this cohort. Although additional references were added, an explicit description of their previous results is lacking from the revised Introduction section. Below I provide a point-by-point comment on the revised document.

	General comments **1.** The flow and writing style of the manuscript has been improved but there are still some instances of uneven writing and run-on sentences: e.g., line 102…106: 'It is generally thought that this category of workers…can still induce maladaptive reactions' **2.** (New) references need to be properly integrated in the manuscript. **3.** Since the manuscript now mentions anxiety and burn-out symptoms, not just depressive symptoms, perhaps the authors should change the title of their manuscript by replacing 'depression' with 'mental health problems' **Introduction** **4.** As noted above, the authors should improve the integration of the present study with their previous work. **5.** The authors moved several highly relevant and informative sections from the Introduction to the Discussion section. The text on the importance of monitoring work-related disorders (original manuscript page 7, line 156-165) was moved, as well as reference to work by Summerfeld (original manuscript page 7, line 169-171), and the description of previous studies of the DCS and ERI (original manuscript page 7, line 171-179). Since these sections provide the backdrop for the present study, they should be placed back into the Introduction. Relatedly, the reference to Iversen et al. (2009) (line 168/169) is (now) out of context as it pertains to a military sample, not police officers. **6.** **P.3, line 114**: replace 'ironically' with 'paradoxically' **7.** **P.7, line 142:** replace 'supposed' by 'proposed' **8.** **P.7, line 143:** Please insert a reference to the isostrain hypothesis **9.** **P.8, line 165:** 'in which workers have weapons' This is rather suggestive and should be rephrased or removed. **10.** **P.8, line 168-172:** Run-on sentence. **11.** **P.8, line 174:** Remove 'apparently for the first time'; **line 176:** remove 'takes as a control,' **line 179:** insert 'They are carefully selected'; replace 'among' with 'from'.

	Methods **30.** The Methods section is rather messy. It should at least include a separate section for Materials with appropriate subheadings. **31.** References to the author's previous studies (e.g., Garbarino ea, 2011, 2012, in press; Magnavita and Garbarino, in press; as noted in my previous reviewing) are still missing. **32.** **P.9, line 203**: [lavori su assenze, personalità] ??? **33.** **As noted in my previous reviewing: P.10, line 229/230,** remove 'as this questionnaire …depression screening'. I presume that the authors did not use any other instruments to screen for depressive symptoms? If so, stating that the BDI is an effective screening tool (with references) would be sufficient. **34.** Please include recommended cut-off point with citations for the STAI and the MBI. **35.** **P.11, line 240**. 'highly selected' please elaborate or rephrase. **36.** Analyses pertaining to anxiety and burn-out should be described in the Statistical analyses section. **Results** No further comments **Discussion** **37.** The author should provide a more penetrating discussion of the results pertaining to anxiety and burn-out. **38.** **P.14, line 331**: remove 'the'. **39.** I feel that the Discussion in lines 365-377 should be placed back into the Introduction section. **40.** **Line 389-394**: 'However, this does not mean … safety of others'. Long sentence. **41.** **Line 409/410**: awkward sentence: 'Such population has a high exposure to homogenous occupational risks, while many studies include persons who perform very different tasks' **Tables** **42.** Table 1, statistic for the mental health variables are missing (presumably M/SD). **43.** Table 2: Please provide an English translation for

		‘agente’and ‘agente scelto‘
	44.	Tables 4a-d should be placed in the manuscript, not as supplementary files.
	45.	Analyses with STAI as DV can be separated from MBI results: e.g. anxiety - Table 4; MBI - Table 5a-c

VERSION 2 – AUTHOR RESPONSE

Reviewer: Arthur R. Rademaker, PhD
Research Centre Military Mental Healthcare - Dutch Ministry of Defence,
dept. Psychiatry, University Medical Centre Utrecht, the Netherlands

The authors thoroughly revised the manuscript and adequately addressed many of my previous comments. Nevertheless, there are still a number of issues that need to be resolved to improve the Manuscript. First, in addition to the response to the previous reviewing, the authors made several changes to the Introduction section that did little to improve the quality of the manuscript. Sections that should have been retained in the Introduction were moved to the Discussion.
Response: We agree. We have moved the sections the Reviewer is referring to back to the Introduction

Second, the authors failed to provide a satisfactory explanation on why they chose to retain cross-sectional analyses only, while their response indicates that prospective examination of the association between job stress and mental health would also be possible. Relatedly, by retaining the original cross-sectional analyses, they have done little to improve the salience of the observed tautological association between depressive symptoms and experience of reward.
R.: We added a new section in the Method (Procedure) that should make clear when the data were collected and we added as further predictors of mental health status personality scores, that are available in the dataset, as further control variables. As reported in the new section, personality scores were collected first, then there have been three waves of job stress measures, and finally measures of mental health were collected. As we now explain in the discussion, although job stress measures had been collected before mental health measures, this does not allow us to make claims of causality, since psychological dysfunctioning might have been present even before the first wave, and thus might have been the cause, rather than the effect, of job stress. However, this does not undermine the predictive usefulness of the models.

Third, I feel that the authors have not adequately addressed my previous comment to place the present study within the framework of their previous work in this cohort. Although additional references were added, an explicit description of their previous results is lacking from the revised Introduction section.
R.: We described briefly the previous work only for those aspects that are relevant for the present work

Below I provide a point-by-point comment on the revised document.

General comments
1. The flow and writing style of the manuscript has been improved but there are still some instances of

uneven writing and run-on sentences: e.g., line 102…106: ‘It is generally thought that this category of workers…can still induce maladaptive reactions’
R.: We tried to make this part clearer.

2. (New) references need to be properly integrated in the manuscript.
R.: We added some new references and integrated references to our previous works

3. Since the manuscript now mentions anxiety and burn-out symptoms, not just depressive symptoms, perhaps the authors should change the title of their manuscript by replacing ‘depression’ with ‘mental health problems’
R.: We agree, we have changed the title.

Introduction
4. As noted above, the authors should improve the integration of the present study with their previous work.
R.: We did it at the end of the introduction and we commented the consistency of the results of this paper with our previous works in the discussion.

5. The authors moved several highly relevant and informative sections from the Introduction to the Discussion section. The text on the importance of monitoring work-related disorders (original manuscript page 7, line 156-165) was moved, as well as reference to work by Summerfeld (original manuscript page 7, line 169-171), and the description of previous studies of the DCS and ERI (original manuscript page 7, line 171-179). Since these sections provide the backdrop for the present study, they should be placed back into the Introduction. Relatedly, the reference to Iversen et al. (2009) (line 168/169) is (now) out of context as it pertains to a military sample, not police officers.
R.: We moved back into the Introduction the sections the Reviewer is referring to and removed the reference to the Iversen et al. (2009)'s paper and other papers which might have been of tangential relevance.

6. P.3, line 114: replace ‘ironically’ with ‘paradoxically’
R.: We did it

7. P.7, line 142: replace ‘supposed’ by ‘proposed’
R.: This part has been removed.

8. P.7, line 143: Please insert a reference to the isostrain hypothesis
R.: we have included it

9. P.8, line 165: ‘in which workers have weapons’ This is rather suggestive and should be rephrased or removed.
R. we have removed it.

10. P.8, line 168-172: Run-on sentence.
R.: We did out best to improve the English throughout the manuscript.

11. P.8, line 174: Remove ‘apparently for the first time’; line 176: remove ‘takes as a control,’ line 179: insert ‘They are carefully selected’; replace ‘among’ with ‘from’.
R. we have removed it.

Methods
12. The Methods section is rather messy. It should at least include a separate section for Materials with appropriate subheadings.

R.: We agree, we have divided this section with new sub-headings

13. References to the author's previous studies (e.g., Garbarino ea, 2011, 2012, in press; Magnavita and Garbarino, in press; as noted in my previous reviewing) are still missing.
R.: We did it at the end of the introduction and we commented the consistency of the results of this paper with our previous works in the discussion.

14. P.9, line 203: [lavori su assenze, personalità] ???
R.: We are sorry for mis-referencing. We carefully revised the paper in order to amend this misprints.

15. As noted in my previous reviewing: P.10, line 229/230, remove 'as this questionnaire …depression screening'. I presume that the authors did not use any other instruments to screen for depressive symptoms? If so, stating that the BDI is an effective screening tool (with references) would be sufficient.
R.: we have rephrased this point.

16. Please include recommended cut-off point with citations for the STAI and the MBI.
R.: We included them referring to Italian norms.

17. P.11, line 240. 'highly selected' please elaborate or rephrase.
R.: We replaced "highly" with "thoroughly"

18. Analyses pertaining to anxiety and burn-out should be described in the Statistical analyses section.
R. We added this information.

Discussion
19. The author should provide a more penetrating discussion of the results pertaining to anxiety and burn-out.
R.: We did our best to address this issue.

20. P.14, line 331: remove 'the'.
R.: We removed it

21. I feel that the Discussion in lines 365-377 should be placed back into the Introduction section.
R. We agree, we have replaced it in the introduction

22. Line 389-394: 'However, this does not mean … safety of others'. Long sentence.
R.: We have rephrased the sentence.

23. Line 409/410: awkward sentence: 'Such population has a high exposure to homogenous occupational risks, while many studies include persons who perform very different tasks'
R.: we have rephrased this sentence

Tables
24. Table 1, statistic for the mental health variables are missing (presumably M/SD).
R.: We have included.(M+SD)

25. Table 2: Please provide an English translation for 'agente'and 'agente scelto'
R. we did it

26. Tables 4a-d should be placed in the manuscript, not as supplementary files.

R: we did it

27. Analyses with STAI as DV can be separated from MBI results: e.g. anxiety - Table 4; MBI - Table 5a-c
R.: We agree. We have combined into one table the results on burnout, since this journal allows a maximum of five tables or figures..