

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Are depressive symptoms more common among British South Asian compared to British White cancer patients?: a cross sectional survey.
AUTHORS	Symonds, Raymond; Lord, Karen; Ibrahim, Kausher; Kumar, Sawan; Mitchell, Alex; Rudd, Nicky

VERSION 1 - REVIEW

REVIEWER	Dr. JNW Lim Senior Lecturer (Primary and Public Health) Faculty of Health, Social Care and Education Anglia Ruskin University Cambridge I have no conflict of interest.
REVIEW RETURNED	13-Feb-2013

THE STUDY	Some references are missing. There are other studies on south asian cancer patients, for example KArbani etal 2011, Asia Pacific Journal of Cancer Prevention, and the work by the Primary care group in Nottingham University and UWE. UCL published a paper in BJCa in 2009 about knowledge and awareness of cancer symptoms amongst the different ethnic SA groups in the UK.
RESULTS & CONCLUSIONS	The results were presented are general and broad. With existing data collected, further and in-depth analysis can be done to help with the interpretation of the results. This will make the article meaningful and interesting. It would interesting to know if there is differences, feg. between south asian from Africa (33%) and those from India. The authors refered to their previous pilot study (2005) and existing evidence on the cultural practices in relation to health seeking and experience can be drawn on to help with intepretation and discussion.
REPORTING & ETHICS	There is no reporting guideline for longitudinal study.
GENERAL COMMENTS	There is the issue of generalising the BSA and in this case, the patients were Gujarati and Hindi. The largest BSA in the England is the Pakistani. The article needs to address this issue from the beginning so as not to mislead the readers that the findings apply to all South Asians in this country. There is much documented evidence about diversity by religion, culture and identity within the South Asian group on this country. This issue should also be stated as a limitation in terms of generalisation of findings.

REVIEWER	Lora MA Thompson, PhD Assistant Member, Clinical Faculty Moffitt Cancer Center USA I have no competing interests to report.
REVIEW RETURNED	04-Mar-2013

THE STUDY	<p>This study presents prevalence rates of depression and anxiety in British White (BW) and British South Asian (BSA) cancer patients as measured by questionnaires, most of which are validated for use with English speakers or in India. The study also aimed to examine differences in coping strategies between the 2 groups. There are a number of concerns that need to be addressed by the authors.</p> <ol style="list-style-type: none"> 1. The introduction gives background which suggests that coping strategies may be related to depression and anxiety. No background is given about other possible variables, which are assessed in the study, including illness perception, patient-physician trust, and cancer beliefs. Is there any theory, which suggests how these variables might impact the mental health of cancer patients? 2. Lines 24-39: Please clarify what the primary objectives are. The first line of this paragraph is misleading to this reviewer. After reading the manuscript, it seems that the first aim was to identify and compare the prevalence rates of depression and anxiety over time in the two groups. The comparison of the coping strategies appears to be a second aim. Were the examination of physical symptoms and cancer perceptions/beliefs considered secondary aims? 3. Please clarify exactly what ethnicities are considered BSA. Statements in the manuscript suggest that this group is primarily made up of individuals from western India who speak Gujarati or Hindi. However, line 48 and Table 1 indicate that 33% were born in Africa. Are the African born individuals of Indian or other South Asian descent? 4. "...difficulty recruiting ...and retention" is a stated limitation of the study. However, the study procedures section does not describe recruitment or attrition rates for the BW or BSA groups. It would be helpful to know how many patients were sent letters/approached in clinic and declined to participate as well as how many withdrew from the study. 5. Study procedures: What were the inclusion and exclusion criteria? How were eligible patients identified (for ex, were they selected from an appointment list)? Line 31-32 in the introduction describes the assessment time frame and may be better placed in the study procedures section. 6. Statistical analyses: Was any a prior power analysis conducted to determine what the sample size should be? This reviewer is not familiar with the convention of reporting cut-off scores after the names of the tools, such as HADS-D 7v8. Is this typical for BMJ manuscripts? 7. Abstract. This reviewer recommends rephrasing the last sentence of the Objectives to better describe the aim to examine associations between mood and coping strategies and other variables. The
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	<p>conclusion only describes the prevalence rates, which were already presented in the Results section. Perhaps a statement of how these results might useful in the future would be best here.</p>
<p>RESULTS & CONCLUSIONS</p>	<p>1. Result: Table 1. What does “Interview Language” refer to? Does this mean that 96% of participants chose to complete the questionnaires in English? The percentages need to be shifted down one line.</p> <p>2. Result: Table 1. This reviewer is not familiar with the term “radical” and would suggest revising or adding a definition in parentheses.</p> <p>3. Result: Table 1. It is noted that 29.7% of BSA (vs. 1.7% of BW) had no formal education. This raises concerns about the literacy level of the BSA group and whether they could have had difficulty understanding and completing the questionnaires in either language.</p> <p>4. Results. Did you consider examining other demographic variables that might be related to differences in depression and anxiety, such as younger age, initial vs. recurrent cancer, and stage of disease?</p> <p>5. Results. Line 14. Please change “HADS score” to “HADS-D score” so readers are certain you are referring to the depression subscale. Line 25: what does it mean when you say “inclusive of more patients”. Do you mean to say that the prevalence rate of anxiety was higher when using the Anxiety Thermometer?</p> <p>6. Use of the term “maladaptive.” While the coping strategies more often used by the BSA are considered maladaptive, perhaps it would be best to rephrase some of the statements to highlight that BSA and BW differed in the type of strategies used rather than stating that BSA used more maladaptive strategies (e.g., Abstract Line 53; Line 31,) Under the Questionnaires section, you could then include a statement which indicates that the Mini-Mac scale assesses adaptive (list subscales) and maladaptive (list subscales) coping styles so that the reader will know what is being measured.</p> <p>7. It would be helpful if you have more consistent language to describe the time 1 data. For example, this time frame is referred to as "early after diagnosis" (pg 11 Line 5), "at presentation" (pg 14, line 30), or "initially" (see pg 13 line 9) in different parts of the results section.</p> <p>8. Discussion Lines 34 to 48. The authors should be cautious about stating that BSA patients experienced more physical symptoms than BW patients. If this is based on Distress Thermometer Checklist data, which instructs patients to state whether a particular symptom is a problem, then it would be more accurate to state that BSA patients were more likely to report that physical symptoms were a problem than BW patients. As for the difference in pain at 9 months, have you considered that this could be related to difference in adequate pain control rather than somatization?</p> <p>9. Discussion; Also Summary:Limitations. The use of self-report questionnaires alone without a standardized clinical interview (the best method for documenting the presence of a depressive or anxious disorder) should be acknowledged as a study limitation.</p>
<p>REPORTING & ETHICS</p>	<p>1. “...difficulty recruiting ...and retention” is a stated limitation of the study. However, the study procedures section does not describe recruitment or attrition rates for the BW or BSA groups. It would be helpful to know how many patients were sent letters/approached in</p>

	<p>clinic and declined to participate as well as how many withdrew from the study.</p> <p>2. Lines 17-21: Were the nurse specialist and radiographers also providing clinical care to the participants? Did these recruiters have any research training in how to provide informed consent and minimize coercion while recruiting patients? Were the consent forms and recruitment letters available in patients native language?</p> <p>3. Please add a statement indicating that this study was approved by a review board.</p>
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REVIEWER	Lisa Mackenzie PhD Candidate University of Newcastle, Australia
REVIEW RETURNED	14-Mar-2013

THE STUDY	<p>RESEARCH QUESTION: The research question, or specific study aims appear to differ between the abstract, article summary, background and discussion (examination of how physical symptoms affect mood is mentioned only in the abstract and discussion; longitudinal assessment component not mentioned in abstract objectives). The introduction could be revised to better clarify and justify the study objectives:</p> <ul style="list-style-type: none"> - I see some justification for assessment of depression, but not for anxiety. There is a need to separately justify why the authors have explored anxiety, depression and coping. Additionally, I would like to see more justification for the need for longitudinal studies in these population groups, as I understand this is a novel aspect of this study. - I would like to see a reference or better justification for introducing the unreferenced pilot study, particularly given the authors used HADS D\geq10 in their pilot study but have then used HADS D\geq8 in the main study. - Please reconsider the use of language, e.g. Para 1 line 12: Please rephrase “particularly striking” and “on both sides of the Atlantic”. - Please move hypotheses from the methods section to the end of the introduction. <p>PARTICIPANT DESCRIPTION: The description of participants is not adequate. It needs to be clarified whether all patients attending the cancer centre during the study period were potentially eligible, and how and by whom eligibility was assessed.</p> <p>REPRESENTATIVENESS OF SAMPLE: The authors need to provide total numbers and reasons for exclusion (e.g. not being aware of cancer diagnosis); the proportion of eligible patients (BSA and BW) from whom consent was sought; consent & attrition rates to provide some indication of the representativeness of this sample. i.e. attrition as reported in other published work using this sample (e.g. Lord, Mitchell et al., 2012; Lord, Ibrahim et al., 2012).</p> <p>DESCRIPTION OF METHODS:</p> <ul style="list-style-type: none"> - Please clarify: Were all patients attending the cancer centre during the study period potentially eligible? How eligibility was assessed (and by whom)? - Please include a description of all measures and references
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	<p>supporting their reliability and validity.</p> <ul style="list-style-type: none"> - For instance, please indicate that the DT was used to assess physical symptoms (if you wish to use these results and discuss them) and describe which coping strategies are considered maladaptive. - Please consider reporting other questionnaires completed by this sample, as reported in Lord, Mitchell et al (2012) and Lord, Ibrahim et al (2012). - Please outline qualitative data collection strategy if this data is to be included in the results section. <p>- Justification for the selection of cutpoints/threshold scores for each of the psychological distress measures should be included. This should at least be done for the English language scales and the validated Gujarati and Hindi scales. This would be an ideal place to provide some background to allow the concept of “severe depression” to be introduced in the results section.</p> <p>- Please clarify “Back-to-back” translation. Do the authors mean that they conducted an iterative back translation process? If they followed the process described in the Brislin (1970) manuscript, please clarify how translations measures against the 50-error standard. More recently, there has been a shift toward using the IQOLA approach to translation. Maybe some discussion of limitations of the translation process could be included.</p> <p>STATISTICAL METHODS: Statistical methods for testing between group differences are not clearly described in the statistical analysis section. In Table 1 it is unclear whether analyses were based on full variables or selected categories within variables.</p> <p>REFERENCES: I would like to see additional references relating to research looking at trajectories of psychological distress (and heterogeneity in trajectories) in cancer patients (e.g. Helgeson et al, 2004, Health Psychology; Lam et al, 2010, Psycho-oncology). I would also like to see some reference to any other work done with psychological distress and distress trajectories amongst BSA, and discussion about this (Williams et al, 2010, Ethnicity & Health).</p> <p>CLARITY OF MAIN OUTCOME MEASURE: Several tools were used to assess the primary outcome of psychological distress to overcome biases when using unvalidated scales in culturally and linguistically diverse populations. The inclusion of the additional questionnaires is not well justified in the introduction or methods section.</p> <p>ABSTRACT/SUMMARY/KEY MESSAGES: Key messages section should clarify that these findings are based on findings from multiple questionnaires.</p> <p>STROBE STATEMENT: Not provided</p>
RESULTS & CONCLUSIONS	<p>PRESENTATION OF RESULTS: The results section needs major revision to better answer the specified aims of this study.</p> <ul style="list-style-type: none"> - Please be more conservative when describing anxiety and depression early after diagnosis: be careful claiming double, as this was not the case for the DepT, nor for the anxiety measures. - Please reconsider the use of “at presentation” throughout the manuscript and tables to describe this time point. Suggestion: “baseline” or “early after diagnosis”. - Influence of deprivation and qualitative data: there are no aims or

hypotheses about either of these. The authors should include extra aims or remove these sections.

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Table 1: Needs some editorial work

- Language in Table – Urdu, in text – Hindu. Please be consistent
- Please place notes (i.e. IMDS) below table
- Educational attainment > add note to indicate that this cell did not add to total (only 272), if this is indeed the case
- Interview language > not correctly aligned
- Diagnosis row : please include total n as well as % for consistency

Figures:

- Please consider including y axis in all figures as readers may be interested in upper and lower bound 95% CIs
- 1a-1f: Please include 95% CI
- 1b: Consider including AnxT be on the left of the figure to correspond with Figure 1a?
- 2a-g: Please check 95% CIs
- Figure 2a: typo “CI 9%”

7 tables presenting longitudinal associations and 3 supplementary tables:

These tables need to be introduced in text if the authors wish to include. I also recommend they be moved to the supplementary section. Please include notes sections below each table to clarify abbreviations:

- Longitudinal associations between coping strategies and depression via PHQ9
- Longitudinal associations between coping strategies and depression via DepT
- Longitudinal associations between coping strategies and anxiety via HADS A
- Longitudinal associations between coping strategies and depression (HADS D)
- Longitudinal associations between coping strategies and depression via PHQ-9
- Longitudinal associations between treatment intent and depression
- Longitudinal associations between treatment intent and anxiety
- Supplementary Table 1: Influence of deprivation
- Supplementary Table 2: Ethnic differences in anxiety and depression
- Supplementary Table 3 Ethnic differences in reporting of physical symptoms

INTERPRETATIONS AND CONCLUSIONS:

There needs to be some discussion of study limitations (single site, potential for bias due to consent/attrition rates, differences in education and SES characteristics). The authors mention that approximately one third of BSA were born in Africa. This may warrant some discussion.

I find a number of points raised in the discussion when no results have been clearly presented (discussion of curative vs palliative, trust in doctors). Discussion of these points appears to be beyond the scope of this paper. Additionally, the suggestion for greater GP involvement was not clearly linked to the findings in this manuscript. Conclusions may need to be revised so that they are warranted by and sufficiently derived from/focused on the data.

	<p>DISCUSSION IN LIGHT OF PREVIOUS EVIDENCE: I would like to see some discussion how selected measures and threshold scores influence reporting of likely cases of anxiety/depression. Please consider which threshold scores may have been used for the general UK population norms. Here I would also like to see some reference to any other work done with psychological distress and distress trajectories amongst BSA, and discussion about this (Williams et al, 2010, Ethnicity & Health).</p> <p>CLARITY OF MESSAGE: It is recommended that the authors include a brief statement summarising and reflecting on hypotheses, or alternatively group the discussion into clear subheadings to reflect the separate study aims.</p>
REPORTING & ETHICS	<p>STROBE REPORTING: Reporting does not adhere to STROBE guidelines for reporting of observational studies.</p> <p>ETHICS APPROVAL AND INFORMED CONSENT: Statement about ethics approval is not provided.</p> <p>CONCERNS ABOUT PUBLICATION ETHICS: Some prior publications have arisen from this sample, but have answered different research questions:</p> <p>1.K. Lord, A.J. Mitchell, K. Ibrahim, S. Kumar, N. Rudd, P. Symonds, The Beliefs and Knowledge of Patients Newly Diagnosed With Cancer in a UK Ethnically Diverse Population, <i>Clinical Oncology</i>, Volume 24, Issue 1, February 2012, Pages 4-12, doi: 10.1016/j.clon.2011.05.008.</p> <p>2.K. Lord, K. Ibrahim, S. Kumar, N. Rudd, A.J. Mitchell, P. Symonds, Measuring Trust in Healthcare Professionals—A Study of Ethnically Diverse UK Cancer Patients, <i>Clinical Oncology</i>, Volume 24, Issue 1, February 2012, Pages 13-21, doi: 10.1016/j.clon.2011.05.010.</p>
GENERAL COMMENTS	<p>This manuscript provides valuable new information comparing the likely presence of, and longitudinal changes in, depression, anxiety and coping styles in British South Asian (BSA) and British White (BW) cancer patients. Despite the significance of this study, I have a number of concerns relating to reporting that would need to be addressed before publication.</p> <p>Overall, I would like to see clearer links between the study aims, results and discussion sections. For instance, the abstract objectives need to be reviewed to capture the longitudinal nature of this study to align with what is stated in the study title, article focus and manuscript.</p> <p>I would also like to see more information about the recruitment process (cancer centre clinics involved, numbers and reasons for exclusion, survey consent rates) in the methods section, in alignment with STROBE statement reporting requirements. I would also like the authors to revise the manuscript to follow BMJ Open manuscript formatting (heading levels, in-text referencing, citation of data supplement files within the text of the article). Additionally, a statement about ethics approval needs to be provided in the manuscript.</p> <p>As a general comment, I would suggest that the authors use more conservative language when discussing the “incidence of anxiety and depression” in their manuscript. The measures used in this study are screening tools, are not able to identify the prevalence of</p>

	<p>depression in the sample. Rather, they are screening for a likely presence or absence of depression or depressive symptoms. Please clarify and include some discussion of this, particularly in relation to the use of multiple measures and threshold scores.</p> <p>Finally, the discussion section needs to interpret the key findings of this study in light of past research, including research reporting on trajectories of psychological distress in cancer patients (e.g. Helgeson et al, 2004, Health Psychology; Lam et al, 2010, Psycho-oncology), and psychological distress and distress trajectories amongst BSA in the general population (Williams et al, 2010, Ethnicity & Health).</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: Dr. JNW Lim

Senior Lecturer (Primary and Public Health) Faculty of Health, Social Care and Education Anglia Ruskin University Cambridge

I have no conflict of interest.

Some references are missing. There are other studies on south asian cancer patients, for example KArbani et al 2011, Asia Pacific Journal of Cancer Prevention, and the work by the Primary care group in Nottingham University and UWE. UCL published a paper in BJCa in 2009 about knowledge and awareness of cancer symptoms amongst the different ethnic SA groups in the UK.

We are grateful to the referee for pointing these references out. However, we have followed reviewer advice and given the paper more focus. Consequently these papers are no longer relevant although of relevance to the wider study. The depth of discussion was extended

The results were presented are general and broad. With existing data collected, further and in-depth analysis can be done to help with the interpretation of the results. This will make the article meaningful and interesting. It would interesting to know if there is differences, feg. between south asian from Africa (33%) and those from India.

See comment to Managing Editor

We have added There was no significant difference in depressive symptoms between those BSA patients originating from Africa compared to the Indian Sub continent at baseline (MW: Africa 31/80 Md 4 (2,9) Indian Sub Continent Md 5.5 (2,11) Z=-1.184 p=0.23). Neither was there a significant difference in the experience of symptoms frequently associated with depression (pain (p=0.23); sleep disturbances (p=0.91); fatigue p=0.52) p 15(of manuscript)

The authors referred to their previous pilot study (2005) and existing evidence on the cultural practices in relation to health seeking and experience can be drawn on to help with interpretation and discussion.

There is the issue of generalising the BSA and in this case, the patients were Gujarati and Hindi. The largest BSA in the England is the Pakistani. The article needs to address this issue from the beginning so as not to mislead the readers that the findings apply to all South Asians in this country. There is much documented evidence about diversity by religion, culture and identity within the South Asian group on this country.

The following was added about the pop. of Indians, Pakistanis and Bangladeshis in the England and Wales...

"n England and Wales those classified as Indian are in the majority accounting for 1412,958, Pakistan 1,124,511 and Bangladesh 447201" (ONS, 2011) (ONS,2011) p6

It is common practice in the UK to quote population statistics for England & Wales together. It would be a mistake to sum Pakistanis and Bangladeshis together.

This issue should also be stated as a limitation in terms of generalisation of findings.

Added to limitation section is....

"The BSA sample largely represents cancer patients within the Leicester Indian population and although of interest to other BSA cancer populations, these findings may not represent them."p24

Reviewer: Lora MA Thompson, PhD
Assistant Member, Clinical Faculty
Moffitt Cancer Center
USA

I have no competing interests to report.

This study presents prevalence rates of depression and anxiety in British White (BW) and British South Asian (BSA) cancer patients as measured by questionnaires, most of which are validated for use with English speakers or in India. The study also aimed to examine differences in coping strategies between the 2 groups. There are a number of concerns that need to be addressed by the authors.

1. The introduction gives background which suggests that coping strategies may be related to depression and anxiety. No background is given about other possible variables, which are assessed in the study, including illness perception, patient-physician trust, and cancer beliefs. Is there any theory, which suggests how these variables might impact the mental health of cancer patients?

We have refocused this article as previously outlined and increased the background section. We have signposted readers to associated articles dealing with cancer beliefs and patient/physician trust. An article about illness perception is anticipated.

2. Lines 24-39: Please clarify what the primary objectives are. The first line of this paragraph is misleading to this reviewer. After reading the manuscript, it seems that the first aim was to identify and compare the prevalence rates of depression and anxiety over time in the two groups. The comparison of the coping strategies appears to be a second aim. Were the examination of physical symptoms and cancer perceptions/beliefs considered secondary aims?

Objectives were clarified in abstract

"We investigated whether there were ethnic differences in depressive symptoms among British South Asian (BSA) cancer patients compared to British White (BW) patients across nine months following presentation at a UK Cancer Centre.. We examined associations between depressed mood coping strategies and the burden of symptoms..."p4

and in text.....

"We report the longitudinal incidence of depressive symptoms among a sample of BSA and BW patients. Coping styles and the burden of patient problems were examined to determine if they were implicated in depressive symptoms. We report these findings".p9

3. Please clarify exactly what ethnicities are considered BSA.

See previous comment

Statements in the manuscript suggest that this group is primarily made up of individuals from western India who speak Gujarati or Hindi. However, line 48 and Table 1 indicate that 33% were born in Africa. Are the African born individuals of Indian or other South Asian descent?

In discussion we have pointed out that Indians are the majority population in our sample
Also added is clarification that these findings may not represent other BSA populations.

“The BSA sample largely represents cancer patients within the Leicester Indian population and although of interest to other BSA cancer populations, these findings may not represent them”. (P16)
In the discussion section we have added .. “Indian Hindus comprised the majority of our BSA sample p13

4. “...difficulty recruiting ...and retention” is a stated limitation of the study. However, the study procedures section does not describe recruitment or attrition rates for the BW or BSA groups. It would be helpful to know how many patients were sent letters/approached in clinic and declined to participate as well as how many withdrew from the study.

Table 1 outlining recruitment and retention IS added. Readers are referred to another publication for a more thorough consideration of these issues.

5. Study procedures: What were the inclusion and exclusion criteria? How were eligible patients identified (for ex, were they selected from an appointment list)?

Eligibility criteria now clarified.

“Eligible patients were aged 18-85 with a confirmed diagnosis of cancer with evidence of being informed of the diagnosis. They were identified by the nurse specialist via the cancer registry with ethnicity confirmed by their surname and by hospital records”. –(p10)

Line 31-32 in the introduction describes the assessment time frame and may be better placed in the study procedures section.

Assessment time frame moved to study procedures. p11

“279 patients, who were aware they had cancer, were recruited at the Leicestershire Cancer Centre between September 2007 and January 2010 at their first or second appointment”.p10

6. Statistical analyses: Was any a prior power analysis conducted to determine what the sample size should be? This reviewer is not familiar with the convention of reporting cut-off scores after the names of the tools, such as HADS-D 7v8. Is this typical for BMJ manuscripts?

A power analysis statement has been added

“A prior power analysis based on our pilot studies determined 86 participants were required for each ethnic group”.(p12)

7. Abstract. This reviewer recommends rephrasing the last sentence of the Objectives to better describe the aim to examine associations between mood and coping strategies and other variables.

See previous response to reviewer.

The conclusion only describes the prevalence rates, which were already presented in the Results section. Perhaps a statement of how these results might be useful in the future would be best here.

conclusion has been reworked....

"Health professionals need to be aware of a greater probability of depressive symptomatology and how this may present clinically, including somatic symptoms, in the first nine months after diagnosis if this ethnic disparity in mental wellbeing is to be addressed".p25

1. Result: Table 1. What does "Interview Language" refer to? Does this mean that 96% of participants chose to complete the questionnaires in English? The percentages need to be shifted down one line.

1 table corrected

2. Result: Table 1. This reviewer is not familiar with the term "radical" and would suggest revising or adding a definition in parentheses.

'Radical' is treatment with curative intent

Results 2 radical defined in table and in text - give page reference (p14)

3. Result: Table 1. It is noted that 29.7% of BSA (vs. 1.7% of BW) had no formal education. This raises concerns about the literacy level of the BSA group and whether they could have had difficulty understanding and completing the questionnaires in either language.

Add in demographics paragraph

"Some patients had no formal education but spoke fluently up to five languages. Individual results in patients who reported little formal education were consistent across assessment tools suggesting good comprehension." (p14)

4. Results. Did you consider examining other demographic variables that might be related to differences in depression and anxiety, such as younger age, initial vs. recurrent cancer, and stage of disease?

We added "We have also analysed results by age, gender and tumour site."

5. Results. Line 14. Please change "HADS score" to "HADS-D score" so readers are certain you are referring to the depression subscale. Line 25: what does it mean when you say "inclusive of more patients". Do you mean to say that the prevalence rate of anxiety was higher when using the Anxiety Thermometer?

HADS score corrected to HADS-D

Anxiety data deleted

6. Use of the term "maladaptive." While the coping strategies more often used by the BSA are considered maladaptive, perhaps it would be best to rephrase some of the statements to highlight that BSA and BW differed in the type of strategies used rather than stating that BSA used more maladaptive strategies (e.g., Abstract Line 53; Line 31,) Under the Questionnaires section, you could

then include a statement which indicates that the Mini-Mac scale assesses adaptive (list subscales) and maladaptive (list subscales) coping styles so that the reader will know what is being measured.

change to 'potentially maladaptive' in abstract and text

"BSA patients used coping strategies differently to BW patients particularly early after diagnosis when greater use of potentially maladaptive strategies were associated with higher rates of depressive symptoms"(p2)

Also added to methods.....

"Adaptive coping strategies (fighting spirit) and potentially maladaptive strategies (hopelessness/helplessness, fatalism, anxiety preoccupation, cognitive avoidance and denial) were assessed via the Mini-MAC scale [23]. (p16)

7. It would be helpful if you have more consistent language to describe the time 1 data. For example, this time frame is referred to as "early after diagnosis" (pg 11 Line 5), "at presentation" (pg 14, line 30), or "initially" (see pg 13 line 9) in different parts of the results section.

words changed to 'baseline' which was as close to diagnosis as possible

8. Discussion Lines 34 to 48. The authors should be cautious about stating that BSA patients experienced more physical symptoms than BW patients. If this is based on Distress Thermometer Checklist data, which instructs patients to state whether a particular symptom is a problem, then it would be more accurate to state that BSA patients were more likely to report that physical symptoms were a problem than BW patients.

Helpful comment. Phrasing clarified.

As for the difference in pain at 9 months, have you considered that this could be related to difference in adequate pain control rather than somatization?

Yes. Clarified.

The following was altered in results

"At baseline 13 out of 17 symptoms BSA patients were more likely to report physical symptoms as problems than BW patients" p26 (results)

The following was added to discussion.

"....Possible explanations include the somatisation of physical symptoms being undetected, inadequate symptom management, non compliance due to a lack of literacy and language skills or for a preference for traditional medicines. Our findings reflect the greater symptom burden found in other ethnic minority cancer patients" [Yoon, 2008, Fu 2009]. (p24)

9. Discussion; Also Summary:Limitations. The use of self-report questionnaires alone without a standardized clinical interview (the best method for documenting the presence of a depressive or anxious disorder) should be acknowledged as a study limitation.

Added to limitations (p24)

"Self reported questionnaires indicate the presence of depressive symptoms but given the absence of psychiatric interviews this is not diagnostic of a depressive disorder"

1. "...difficulty recruiting ...and retention" is a stated limitation of the study. However, the study procedures section does not describe recruitment or attrition rates for the BW or BSA groups. It would be helpful to know how many patients were sent letters/approached in clinic and declined to participate as well as how many withdrew from the study.

difficulty recruiting refer to new table p

2. Lines 17-21: Were the nurse specialist and radiographers also providing clinical care to the participants? Did these recruiters have any research training in how to provide informed consent and minimize coercion while recruiting patients? Were the consent forms and recruitment letters available in patients native language?

lines 17 to 21 – clarified..

“None were involved in the clinical care of the patients and all received training in ‘Good Clinical Practice’ and in the principles of informed consent”.

Consent forms and information sheets had been translated into Gujarati and Hindi (p10)

3. Please add a statement indicating that this study was approved by a review board.

The following was added

“The Leicestershire, Northamptonshire and Rutland Ethics Committee approved the study. (p10)

Reviewer: Lisa Mackenzie

PhD Candidate

University of Newcastle, Australia

RESEARCH QUESTION: The research question, or specific study aims appear to differ between the abstract, article summary, background and discussion (examination of how physical symptoms affect mood is mentioned only in the abstract and discussion; longitudinal assessment component not mentioned in abstract objectives). The introduction could be revised to better clarify and justify the study objectives:

The structure of the paper has been reviewed to provide more consistent continuity of ideas. The introduction has been clarified and adjusted.

- I see some justification for assessment of depression, but not for anxiety. There is a need to separately justify why the authors have explored anxiety, depression and coping. Additionally, I would like to see more justification for the need for longitudinal studies in these population groups, as I understand this is a novel aspect of this study.

Research question. We have removed reference to anxiety to make the paper shorter and more focussed. A section considering longitudinal studies has been added.

- I would like to see a reference or better justification for introducing the unreferenced pilot study, particularly given the authors used HADS D \geq 10 in their pilot study but have then used HADS D \geq 8 in the main study.

- There are no other studies in Leicester with which to compare our current study. We had added justified for the use of HADS-D \geq 8 (p11)

- Please reconsider the use of language, e.g. Para 1 line 12: Please rephrase “particularly striking” and “on both sides of the Atlantic”.

- Language reconsidered and rephrased: Both sides of the Atlantic changed to UK and USA (p6)

- Please move hypotheses from the methods section to the end of the introduction.

- hypothesis moved (p9)

PARTICIPANT DESCRIPTION: The description of participants is not adequate. It needs to be clarified whether all patients attending the cancer centre during the study period were potentially eligible, and how and by whom eligibility was assessed.

Participant description - see answers to referee 2

REPRESENTATIVENESS OF SAMPLE: The authors need to provide total numbers and reasons for exclusion (e.g. not being aware of cancer diagnosis); the proportion of eligible patients (BSA and BW) from whom consent was sought; consent & attrition rates to provide some indication of the representativeness of this sample. i.e. attrition as reported in other published work using this sample (e.g. Lord, Mitchell et al., 2012; Lord, Ibrahim et al., 2012).

See previous response to reviewer: Table for recruitment and retention table added and reader referred to Symonds 2012 article ()

DESCRIPTION OF METHODS:

- Please clarify: Were all patients attending the cancer centre during the study period potentially eligible? How eligibility was assessed (and by whom)?

Also see previous response to reviewer: "Eligible patients were aged 18-85 with a confirmed diagnosis of cancer with evidence of being informed of the diagnosis. They were identified by the nurse specialist via the cancer registry with ethnicity confirmed by their surname and by hospital records". – paste and copy into referees' comments with page reference(p10)

- Please include a description of all measures and references supporting their reliability and validity.
- For instance, please indicate that the DT was used to assess physical symptoms (if you wish to use these results and discuss them) and describe which coping strategies are considered maladaptive.

Baseline physical symptoms the DT checklist was used to assess physical symptoms quote Roth reference.(p10)

The coping strategies which were considered maladaptive were clarified
"Adaptive coping strategies (fighting spirit) and potentially maladaptive strategies (hopelessness/helplessness, fatalism, anxiety preoccupation, cognitive avoidance and denial) were assessed via the Mini-MAC scale". (p11).

- Please consider reporting other questionnaires completed by this sample, as reported in Lord, Mitchell et al (2012) and Lord, Ibrahim et al (2012).

Not relevant as the paper has been reduced to the prevalence of depressive symptoms only

- Please outline qualitative data collection strategy if this data is to be included in the results section.

Clarified: "Personal statements illustrating how patients coped were generated by two qualitative questions, "how would you describe your current illness", and "what does having cancer mean to you?"

- Justification for the selection of cutpoints/threshold scores for each of the psychological distress measures should be included. This should at least be done for the English language scales and the validated Gujarati and Hindi scales. This would be an ideal place to provide some background to

allow the concept of “severe depression” to be introduced in the results section.

the justification for cut off scores listed (p11-12)

This study considers severe depressive symptoms. There is insufficient word space to consider suicidal ideation which is an element of the wider study

- Please clarify “Back-to-back” translation. Do the authors mean that they conducted an iterative back translation process? If they followed the process described in the Brislin (1970) manuscript, please clarify how translations measures against the 50-error standard. More recently, there has been a shift toward using the IQOLA approach to translation. Maybe some discussion of limitations of the translation process could be included.

The back-translation was carried out by a commercial company, Pearl Linguistics. p11

“A commercial company undertook an iterative back-translation process as described by Brislin,1970”

STATISTICAL METHODS: Statistical methods for testing between group differences are not clearly described in the statistical analysis section. In Table 1 it is unclear whether analyses were based on full variables or selected categories within variables.

“We report analysis by age, gender, deprivation, tumour site, place of birth and ethnicity” (p13).

REFERENCES: I would like to see additional references relating to research looking at trajectories of psychological distress (and heterogeneity in trajectories) in cancer patients (e.g. Helgeson et al, 2004, Health Psychology; Lam et al, 2010, Psycho-oncology). I would also like to see some reference to any other work done with psychological distress and distress trajectories amongst BSA, and discussion about this (Williams et al, 2010, Ethnicity & Health).

Reference to Lim (2010) plus Kroenke (2013), Breen (2009) Reyes Gibby (2012) Fu (2009) and Yoon (2008) Shi, (2011) given expansion of section concerning ‘Symptom burden’ and focus on longitudinal distress. We could not trace the Williams reference and having reviewed the Helgeson reference it was concerned with trajectories of change in adjustment over a longer period than this study (p9/10)

CLARITY OF MAIN OUTCOME MEASURE: Several tools were used to assess the primary outcome of psychological distress to overcome biases when using unvalidated scales in culturally and linguistically diverse populations. The inclusion of the additional questionnaires is not well justified in the introduction or methods section.

Added to abstract was...

“Three screening tools for depression were used to counter concerns about ethnic bias and validity in linguistic translation”.

And to methods.....

“Patients completed the Hospital Anxiety and Depression Scale (HADS) [18] and The Emotion Thermometers [19] which incorporates the Distress Thermometer [20] and depression thermometer (Dep T). The DT problem checklist identified the symptom burden experienced by patients.(Roth) All are validated but were not initially available in Gujarati or Hindi. Therefore a commercial company undertook an iterative back translation process as described by Brislin,1970. [21] A version of the Patient Health Questionnaire (PHQ-9), which was already validated into Gujarati and Hindi having been adapted for use in India, was the third questionnaire used.[22] Several tools were used to address the concern that some were ethnically biased”. (p10)

ABSTRACT/SUMMARY/KEY MESSAGES: Key messages section should clarify that these findings

are based on findings from multiple questionnaires.

STROBE STATEMENT: Not provided.

“Reference was made to the Strobe statement for reports of observational studies”

PRESENTATION OF RESULTS: The results section needs major revision to better answer the specified aims of this study.

Reviewed and altered as previously outlined.

- Please be more conservative when describing anxiety and depression early after diagnosis: be careful claiming double, as this was not the case for the DepT, nor for the anxiety measures.

Corrected

- Please reconsider the use of “at presentation” throughout the manuscript and tables to describe this time point. Suggestion: “baseline” or “early after diagnosis”.

Anxiety has been deleted. The word ‘baseline’ has been used throughout.

- Influence of deprivation and qualitative data: there are no aims or hypotheses about either of these. The authors should include extra aims or remove these sections.

The reason for referring to deprivation as a variable is

“Since socio-economic deprivation is closely associated with being a member of an ethnic minority we considered the extent to which deprivation influenced the strength of the relationship between ethnicity and depressive symptoms” (p13).

Table 1: Needs some editorial work

- Language in Table – Urdu, in text – Hindu.

Corrected in table “Urdu (verbal translation)”

Please be consistent

- Please place notes (i.e. IMDS) below table

Changed accordingly

- Educational attainment > add note to indicate that this cell did not add to total (only 272), if this is indeed the case

Clarified. Only 272 patients volunteered their degree of educational attainment

- Interview language > not correctly aligned

Corrected

- Diagnosis row : please include total n as well as % for consistency

Done

Figures:

- Please consider including y axis in all figures as readers may be interested in upper and lower bound 95% CIs

Y axis was added to figures 2a-f

- 1a-1f: Please include 95% CI

95% confidence limit inappropriate for a definite percentage.

95% confidence limit of a median score was 0 in some cases. Note written beneath graphs to that effect

- 1b: Consider including AnxT be on the left of the figure to correspond with Figure 1a?

Anxiety graphs deleted

- 2a-g: Please check 95% CIs
Checked

- Figure 2a: typo "CI 9%"
Corrected

7 tables presenting longitudinal associations and 3 supplementary tables:

These tables need to be introduced in text if the authors wish to include. I also recommend they be moved to the supplementary section.

Please include notes sections below each table to clarify abbreviations:

- Longitudinal associations between coping strategies and depression via PHQ9
- Longitudinal associations between coping strategies and depression via DepT
- Longitudinal associations between coping strategies and anxiety via HADS A
- Longitudinal associations between coping strategies and depression (HADS D)
- Longitudinal associations between coping strategies and depression via PHQ-9
- Longitudinal associations between treatment intent and depression
- Longitudinal associations between treatment intent and anxiety
- Supplementary Table 1: Influence of deprivation
- Supplementary Table 2: Ethnic differences in anxiety and depression
- Supplementary Table 3 Ethnic differences in reporting of physical symptoms

Tables relating to anxiety removed

Notes clarifying abbreviations added

Reference to tables added to main article text

Many subsequent comments are repetitious

INTERPRETATIONS AND CONCLUSIONS:

There needs to be some discussion of study limitations (single site, potential for bias due to consent/attrition rates, differences in education and SES characteristics). The authors mention that approximately one third of BSA were born in Africa. This may warrant some discussion.

I find a number of points raised in the discussion when no results have been clearly presented (discussion of curative vs palliative, trust in doctors). Discussion of these points appears to be beyond the scope of this paper. Additionally, the suggestion for greater GP involvement was not clearly linked to the findings in this manuscript. Conclusions may need to be revised so that they are warranted by and sufficiently derived from/focused on the data.

DISCUSSION IN LIGHT OF PREVIOUS EVIDENCE: I would like to see some discussion how selected measures and threshold scores influence reporting of likely cases of anxiety/depression. Please consider which threshold scores may have been used for the general UK population norms.

Here I would also like to see some reference to any other work done with psychological distress and distress trajectories amongst BSA, and discussion about this (Williams et al, 2010, Ethnicity & Health).

CLARITY OF MESSAGE: It is recommended that the authors include a brief statement summarising and reflecting on hypotheses, or alternatively group the discussion into clear subheadings to reflect the separate study aims.

“This study supports the original hypothesis that more BSA cancer patients would self report depressive symptoms than BW patient over time. Our hypothesis that a greater use of potentially maladaptive coping strategies would reflect higher rates of depression among BSA patients was supported but only until the three month point. A heavier symptom burden among BSA patients does appear to contribute to depression rates among this ethnic minority compared to the host population”.p24

STROBE REPORTING: Reporting does not adhere to STROBE guidelines for reporting of observational studies.

Addressed

ETHICS APPROVAL AND INFORMED CONSENT: Statement about ethics approval is not provided.

See response to previous comment

CONCERNS ABOUT PUBLICATION ETHICS: Some prior publications have arisen from this sample, but have answered different research questions:
Not now relevant to revised paper

1.K. Lord, A.J. Mitchell, K. Ibrahim, S. Kumar, N. Rudd, P. Symonds, The Beliefs and Knowledge of Patients Newly Diagnosed With Cancer in a UK Ethnically Diverse Population, *Clinical Oncology*, Volume 24, Issue 1, February 2012, Pages 4-12, doi: 10.1016/j.clon.2011.05.008.

2.K. Lord, K. Ibrahim, S. Kumar, N. Rudd, A.J. Mitchell, P. Symonds, Measuring Trust in Healthcare Professionals—A Study of Ethnically Diverse UK Cancer Patients, *Clinical Oncology*, Volume 24, Issue 1, February 2012, Pages 13-21, doi: 10.1016/j.clon.2011.05.010.
Above articles omitted

This manuscript provides valuable new information comparing the likely presence of, and longitudinal changes in, depression, anxiety and coping styles in British South Asian (BSA) and British White (BW) cancer patients. Despite the significance of this study, I have a number of concerns relating to reporting that would need to be addressed before publication.

Overall, I would like to see clearer links between the study aims, results and discussion sections. For instance, the abstract objectives need to be reviewed to capture the longitudinal nature of this study to align with what is stated in the study title, article focus and manuscript.

I would also like to see more information about the recruitment process (cancer centre clinics involved, numbers and reasons for exclusion, survey consent rates) in the methods section, in alignment with STROBE statement reporting requirements. I would also like the authors to revise the manuscript to follow BMJ Open manuscript formatting (heading levels, in-text referencing, citation of data supplement files within the text of the article). Additionally, a statement about ethics approval needs to be provided in the manuscript.

Addressed above

As a general comment, I would suggest that the authors use more conservative language when discussing the "incidence of anxiety and depression" in their manuscript. The measures used in this study are screening tools, are not able to identify the prevalence of depression in the sample. Rather, they are screening for a likely presence or absence of depression or depressive symptoms. Please clarify and include some discussion of this, particularly in relation to the use of multiple measures and threshold scores.

Finally, the discussion section needs to interpret the key findings of this study in light of past research, including research reporting on trajectories of psychological distress in cancer patients (e.g. Helgeson et al, 2004, Health Psychology; Lam et al, 2010, Psycho-oncology), and psychological distress and distress trajectories amongst BSA in the general population (Williams et al, 2010, Ethnicity & Health).

VERSION 2 – REVIEW

REVIEWER	Lora MA Thompson, PhD Psychologist Moffitt Cancer Center USA No conflicts of interest.
REVIEW RETURNED	25-Apr-2013

THE STUDY	The manuscript should be reviewed for grammatical errors. There are errors/omissions related to the use of commas, spaces, and periods. Mostly notably, the spelling of "HADS D" should be checked (see pg 15, line27; page 21, line 10 and 14).
GENERAL COMMENTS	The manuscript is much improved, and the authors have addressed the reviewers' comments. Acceptance is recommended after correction of grammatical errors.

REVIEWER	Lisa Mackenzie PhD Candidate The University of Newcastle, Australia Competing interests: Nil
REVIEW RETURNED	30-Apr-2013

THE STUDY	<p>RESEARCH QUESTION: The introduction section would benefit from being more succinct, and more integration of the literature reporting on adaptive and maladaptive coping styles, including denial.</p> <p>PARTICIPANT DESCRIPTION: Further clarification of whether all eligible patients attending the cancer centre during the study period (or all eligible patients listed on the cancer registry?) were contacted by mail and invited to join the study is required.</p> <p>REPRESENTATIVENESS OF SAMPLE: If possible, the number of excluded patients, total number of patients contacted by mail, and consent rates should be reported in the results section, along with the attrition rates reported in other published work using this sample (e.g. Lord, Mitchell et al., 2012; Lord, Ibrahim et al., 2012). This will help to justify the statement in paragraph one of the results section</p>
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	<p>that “The BSA sample largely represents cancer patients within the Leicester Indian population....”</p> <p>ABSTRACT Line 54: Please reconsider use of “presentation” (keep consistent throughout article e.g. “baseline”)</p> <p>ARTICLE SUMMARY Line 34: Please reconsider use of “presentation” (keep consistent throughout article e.g. “baseline”) Line 48: Please change “statistically very significant differences” to “highly statistically significant differences” Line 57: Please rephrase “Changes in mood between the three data collection points are not represented” Strengths and limitations section – please change to point form</p> <p>STATISTICAL METHODS: Which statistical tests was the study powered on?</p> <p>REFERENCES: I still like to see some comment on other work done with psychological distress and distress trajectories amongst BSA in the general population, and how the use of multiple measures may have overcome this.</p> <p>STROBE STATEMENT: No in-text statement is required in the statistical analysis section, but the authors could consider attaching the completed STROBE checklist as supplemental material with manuscript submission.</p>
RESULTS & CONCLUSIONS	<p>PRESENTATION OF RESULTS:</p> <ul style="list-style-type: none"> •The results section needs some revision, as it currently reports data and analysis which seem to be beyond the aims of this manuscript. •Tables 1-4 are missing from the submission, so not possible to comment on these •Please ensure consistency in choice to use “at presentation” “baseline” “early after diagnosis” “soon after diagnosis” throughout the manuscript, tables and figures to describe this time point. •I would like to see some justification in the introduction for why associations between participant demographics and depressive symptoms were assessed. <p>Table 1: Not included with submission, so not possible to report on whether it was appropriate to refer to this in the methods section.</p> <p>Table 2: Not included with submission, so not possible to comment.</p> <p>Table 3: Referred to at the end of demographics and depressive symptoms section of the results, but not included with the submission, but not possible to comment.</p> <p>Table 4: Referred to at the end of the results section, but not included with the submission, but not possible to comment.</p> <p>Figures:</p> <ul style="list-style-type: none"> -Please reconsider use of “presentation” (keep consistent throughout article e.g. “baseline”) -Please consider including y axis in all figures as readers may be interested in upper and lower bound 95% CIs -1a-1d, Longitudinal comparison of depressive symptoms is BSA and BW patients: Please include 95% CI -2a-g, Longitudinal ethnic differences in coping strategies:

	<p>oPlease check the narrow 95% CIs for the BW group at baseline/presentation in figs 2a and 2c</p> <p>oThere are currently two figures labelled 2e</p>
REPORTING & ETHICS	<p>CONCERNS ABOUT PUBLICATION ETHICS: As reported in the initial review, some prior publications from this sample have answered different research questions:</p> <ol style="list-style-type: none"> 1. K. Lord, A.J. Mitchell, K. Ibrahim, S. Kumar, N. Rudd, P. Symonds, The Beliefs and Knowledge of Patients Newly Diagnosed With Cancer in a UK Ethnically Diverse Population, <i>Clinical Oncology</i>, Volume 24, Issue 1, February 2012, Pages 4-12, doi: 10.1016/j.clon.2011.05.008. 2. K. Lord, K. Ibrahim, S. Kumar, N. Rudd, A.J. Mitchell, P. Symonds, Measuring Trust in Healthcare Professionals—A Study of Ethnically Diverse UK Cancer Patients, <i>Clinical Oncology</i>, Volume 24, Issue 1, February 2012, Pages 13-21, doi: 10.1016/j.clon.2011.05.010.
GENERAL COMMENTS	<p>This manuscript provides valuable new information comparing the likely presence of depression, symptom burden and adaptive/maladaptive coping styles amongst British South Asian (BSA) and British White (BW) cancer patients. The authors also report on longitudinal changes in these outcomes. The authors have done a thorough job of integrating my comments into this revision. However, there are still some minor limitations which need to be addressed. The manuscript should be reduced in length to ensure a clear message is communicated to readers (e.g. more integration of the depression and coping literature in the introduction and discussion sections). The presentation of results should include response rates, if possible. Also, Tables 1-4 were not attached to this submission, and should be resubmitted for consideration.</p>

VERSION 2 – AUTHOR RESPONSE

Reviewer: Dr Lora MA Thompson

The paper has been reviewed for grammatical errors which have been corrected.

HADS-D denotes the depression scale from the Hospital Anxiety and Depression Scale throughout the paper.

Reviewer: Ms Lisa Mackenzie

Research Question

The following data relating to ethnic differences in the use of maladaptive coping styles from a pilot study has been added to the introduction.

Depressive symptoms were associated with potentially maladaptive coping strategies among both ethnic groups but were employed statistically significantly more frequently by BSA patients. For example, fatalism $p=0.0001$; denial $p=0.019$; hopelessness and helplessness, $p=0.007$.

Participant Description

The fact that this was a 'convenient' sample was added to the text.

'Representativeness' of sample

Additional data has been added to Table 1 which provides information on the recruitment and retention of participants.

Abstract

The term 'baseline' is now used throughout the paper.

Article summary

All suggestions carried out re lines 34, 48 and 57

Statistical methods

Prior power test included.

A prior power calculation determined a sample size of 86 participants was required for each ethnic group.

References

There is a dearth of evidence about this subject and the reason for this study.

Using multiple measures strengthens the significance of findings if they are similar.

A Strobe checklist is included.

Presentation of results

It is accepted practice to report demographics within a paper. To report how selected demographics influenced depressive symptoms was a logical addition to this paper. This has been clarified in the introduction.

Selected demographics, coping styles and the burden of patient problems were examined to determine if they were associated with depressive symptoms.

Tables were omitted by mistake. They have been cross checked with references in the text.

Figures

Re use of the term 'presentation' etc. Please see previous response under 'Abstract'.

A previous response to a reviewer comment indicated the reason for apparent absence of CI. A note now appears beneath the figures to clarify this.

Where appropriate the y-axis has been added, for example Fig 2a-f).

Fig 1-d illustrate percentage values. Confidence intervals are inappropriate for absolute values.

The two figures labelled 2e have been changed to 2e and 2f

Concern about publication ethics

These articles have already been removed after original reviewer comment.

Final paragraph from reviewer were a summary. Responses are as above.