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)	Publication trends in newspapers and scientific journals for SSRIs and suicidality: a systematic longitudinal study
AUTHORS	J.F. Hernandez, A.K. Mantel-Teeuwisse, G.J.M.W. van Thiel, S.V. Belitser, J.A.M. Raaijmakers, T. Pieters

VERSION 1 - REVIEW

REVIEWER	Amanda Wilson Research Academic University of Newcastle Australia I have no competing interesting to declare.
REVIEW RETURNED	21/08/2011

THE STUDY	Research Question The research question is presented in a nebulous fashion and while the general idea can be inferred it would be better for the authors to state exactly what their research aims were especially in relation to their expected outcomes. If the research question is to ascertain the difference (in terms of numbers and categories) between negative and positive news and journal articles on SSRIs and suicide, then the overall study design appears appropriate. Participants The participants in this research are the journal and newspaper articles. Although these are classified as either: case study; research; opinion; policy; interview; news report; science journalism, there is no definition of what each of these actually constitute or what differences exist between news reports, interviews and science journalism (which only appear in newspapers). I would like to know how these articles were differentiated and also how they were categorised. Also, it would be informative to know what the subject of these articles was. There is some description of law stories, what other kinds of stories were involved? Differences between article authors is also important. Were all the writers of the newspaper stories journalists, if so were they speciality journalists? Who wrote the opinion articles and what constituted a policy article? How were the articles categorised and who did this? What was the protocol behind this? Another aspect that is not addressed in this paper is the vast difference between journal and newspaper articles. The difference between the authors and the audiences is important and should be identified. Methods The methodology for article categorisation, above, needs to be included. Also, the methods for rating the articles needs to be described in more detail. What were the inclusion and exclusion criteria for the articles (apart from the keywords)? Was there a

	rating/data extraction instrument? If so, was it validated? More description of the rating process is needed.
	Outcome measures
	The main outcome measures, like the aims, are relatively unclear. They appear to be, how many negative and positive articles were published in journals and newspaper of a 10 year period. This is well covered. However, the results pertaining to the type of article are uninformative as we don't have a clear idea of what these categories really represent or how they are important.
	References
	References 5, 6, 26, 27 and 29 are getting a bit old and the paper would benefit from more current documents.
	Supplemental documents not applicable.
RESULTS & CONCLUSIONS	**Interpretation and Conclusions**
	Is there any good evidence that misleading topics are deliberately selected to increase readership? I haven't seen a content study that convincingly identified consistent examples of misleading reporting of a single health issue. However, studies have found responsible reporting of drug safety issues for example, news media coverage of the Women's Health Initiative results was found to be generally balanced, consistent and accurate (Canales, Breslau et al. 2008). The authors appear to imply that the negative reporting may have influenced restriction of SSRIs in under 18 years old while stating "a causal association between this sudden publication growth and the resulatory warnings was not assessed". If there is no evidence to support this, it really shouldn't be in the paper as it can easily become a zombie statement which takes on a life of its own through repetition although there is no underlying evidence.
	This paragraph contains a lot of speculative terms such as 'might substantiate the possible influence" "might also indicate". These speculations are interesting but not supported by the evidence offered in this paper and should perhaps be left for a more appropriate forum such as an opinion piece.
	While the interpretation of the data seems adequate, some of the conclusions appear to fall outside the scope of the analysis. The opening sentence of the conclusion says that publication trends "appear to be more affected by regulatory actions, opinions or lawsuits that... evidence-based medicine." This would appear to come from the fact that there were higher numbers of this type of article in newspapers than research related reporting however we are not told how the content of these articles related to a drug safety issue and I wasn't convinced that the data presented supports this conclusion.
	Is the message Clear?
	I'm not clear on what the final message of this paper was. The final box – what the study adds - provides 3 points that were not fully explored in the paper. The first, that 'bad' news was related to opinion not evidence seems to based on the fact that more opinion pieces rather than news stories were categorised as negative. However, the evidence (level of evidence, sources of information) underpinning the content of these articles was not presented (was it even identified?). On this basis, is it possible to make this statement?
	The second point is confusing – what wasn't uniform?
	The third point introduces stakeholders for the first time but who the stakeholders are is not defined.
GENERAL COMMENTS	I'd advise against the use of the term lay, as in lay public page 16 - it looks patronising and in this case is tautological.

REVIEWER	John Coverdale M.D. M.Ed., FRANZCP, Professor of Psychiatry and of Medical Ethics, Baylor College of Medicine, Houston, Tx 77030, USA. Nikita Malani, Medical Student, Baylor College of Medicine, Houston, Texas 77030, USA. Neither reviewer has any competing interests or conflicts of interest.
REVIEW RETURNED	30/08/2011

THE STUDY	The main outcome measure of the positive or negative effect of a newspaper article is limited by having two components one of which is not diametrically opposite. Please see the comments below for a fuller explanation of this concern.
RESULTS & CONCLUSIONS	There is a lack of clarity to the main message that follows from the complexity of the research findings. This could be communicated in a more straight forward fashion for readers. Related to this matter perhaps not all of the detail of the findings merits reporting here.
GENERAL COMMENTS	The explicit goals of the study were to investigate the long term dissemination dynamics of the benefits and risks of SSRIs in light of a safety concern that SSRIs increase suicidal thoughts in the pediatric population. These goals arise because of a concern about the adequacy of information provided to the general public about this association. This paper does not attempt to address the relationship between SSRIs and suicidality nor the scientific merits of anti-depressants. Rather it simply aims to look at the relationship between the content of different types of articles on this topic that were published in scientific journals and newspapers in the 2000-2009 time-period. The main outcome measure was the "effect" of the message (depending on the type of article), which was classified as positive, neutral, or negative. Positive articles were those which reported on positive therapeutic outcomes of anti-depressants with no confirmation of "causality" between SSRIs and suicidal behavior. Negative articles were those that reported on an association between SSRIs and suicidality without mention of potential positive therapeutic outcome. Articles with a "balanced" outlook were classified as neutral. This study has several strengths, one being the systematic and well-defined process of identifying articles from scientific journals and newspapers. The search criteria used were clearly delineated and the authors clearly defined inclusion criteria. Furthermore, the duration of the study, in terms of using paper and articles from the 2000-2009 time-frame, allowed the authors to study changes in content over time. This is especially important in light of the fact that the years in which regulatory warnings were published were included in the study's time frame. Thus the authors could ascertain any general correlations between changes in content of scientific and newspaper articles and the regulatory warnings. The main methodological concerns were the following: 1. A content measure of analysis is limited by not capturing the richness of depictions and by not identifying potential competing themes. The evaluation did not provide elaboration on the construction of articles, weight of balance of positive and negative articles, accuracy, or the validity of the information presented.

Potentially negative or stereotyping elements that in turn might be influential in the general public's thinking were not identified. All in all, the context of the articles was not be ascertained.

2. It was unclear how the news reports related to other stories within the newspapers, in terms of location in the paper and surrounding articles and advertisements. Obtaining a sense of the contextuality of the articles would require a careful analysis of the construction of the newspaper article from its original source and an understanding of news-worthiness. The simple content analysis method identified in the study does not allow elucidation of these processes.

3. While the authors sought to analyze the "publication dynamics" and what was called the process of dissemination, little can be said about the methods by which the newspapers constructed their information or reports.

4. The authors noted that articles that reported on therapeutic outcomes with no mention of "causality" between SSRIs and suicidality were classified as positive. The negative category classification was not diametrically opposite to the positive category, as negative articles were classified as those that reported on an "association" between SSRIs and suicidality, not causality. Negative articles were also classified as making little or no mention of "positive therapeutic outcomes." Thus the major basis for classification as positive or negative was based on two factors which were not necessarily clearly different.

5. As above, negative articles were identified as making little or no mention of "positive therapeutic outcomes." However, the word anti-depressant in and of itself suggests therapeutic outcome. Thus the articles might inherently be suggesting a therapeutic outcome, and this suggests a need for a more rich or sophisticated classification method.

6. The authors noted that their collection of newspaper articles was potentially incomplete. It would have been helpful to check the penetrance of the searches performed against a hand-search of the newspapers used.

Nevertheless, the article has merit, as there are few papers of this type and the analysis present in this study is relevant to all fields of medicine. The fact that patients receive information (that may have a certain bias) about medications that are prescribed to them poses a challenge in the doctor-patient relationship. The study effectively used the SSRI and suicidality controversy to explore the biases present in both scientific journals and general publications such as newspapers. The positive bias inherent in journal articles could potentially leave physicians with an incomplete view of the medications that they are prescribing to patients. On the other hand, the negative bias present in newspapers (and other forms of media) has implications for patient compliance with medication, willingness to see physicians, and trust in the doctor-patient relationship.

Therefore, all in all the authors should attempt to clarify the classification methods of negative and positive articles and note in their limitations any limitations in these definitions and how the methodology is also limited by its content-analysis nature. The results are also complicated and difficult to understand in terms of their impact. The authors might therefore consider re-thinking how

	they portray the main findings and their potential impact for the reader.

VERSION 1 – AUTHOR RESPONSE

Reviewer #1: Amanda Wilson, Research Academic, University of Newcastle, Australia

Research Question
The research question is presented in a nebulous fashion and while the general idea can be inferred it would be better for the authors to state exactly what their research aims were especially in relation to their expected outcomes.
If the research question is to ascertain the difference (in terms of numbers and categories) between negative and positive news and journal articles on SSRIs and suicide, then the overall study design appears appropriate.

• The reviewer's point has been adopted in the manuscript. We reformulated the research question based on the suggestions given by the reviewer.

Participants
The participants in this research are the journal and newspaper articles. Although these are classified as either: case study; research; opinion; policy; interview; news report; science journalism, there is no definition of what each of these actually constitute or what differences exist between news reports, interviews and science journalism (which only appear in newspapers). I would like to know how these articles were differentiated and also how they were categorised. Also, it would be informative to know what the subject of these articles was.

• We would like to draw the attention of the referee to the method section, where we described that the categorization of all articles was based on a full-text content analysis. If an article comprehended one of the features required for categorization, such an article was indexed under that type of article. As mentioned in the article, categorization was reliable, since we observed less than 5% scoring discrepancies between both scorers. To further clarify, we expanded the definition of each type of article, in scientific journals and newspapers, in the revised version of the manuscript (page 8, second paragraph under the header "data classification").
Regarding the subject of the article, we only included articles that addressed SSRIs, depression, suicidal thoughts, or suicide as main topic, as reported in the "data classification" section. Articles discussing other themes and peripherally SSRIs, suicide, depression, etc. were classified as "out of context".

There is some description of law stories, what other kinds of stories were involved?

• The main subject of the stories that were analyzed was SSRIs, depression, suicidal thoughts, and suicide. The context in which those stories were reported was broad, varying from lawsuits to murders, suicides or attempts, mental health, the health care system, post-natal depressions, treatment, side effects, celebrities, and writers, among others. Unfortunately, we were limited in the amount of data to present in this study and therefore we left out a presentation of the full analysis of this important facet of the data. We now realize that presenting only parts of this analysis may lead to confusion and questions by readers, and therefore decided to remove these elements from the results section. We consider that this did not affect the main message of our paper.

Differences between article authors is also important. Were all the writers of the newspaper stories journalists, if so were they speciality journalists?

• Agreed. We acknowledge your study published in PLoS Medicine in 2010, where specialty journalists (such as science or medical journalist) scored much better than regular journalists, in terms of accuracy, when reporting the benefits and risks of medical interventions. Regarding our study, we retrieved the names of the journalists or writers, but we did not analyze this reporting facet since it was not our primary aim. As we pointed out in the discussion, we were mainly interested to know what type of information the public was confronted with. However, to address your question, we observed that the writer of the article or column had different professional backgrounds, such as psychiatrists, psychologists, practitioners, professors, science journalists, sports journalists, regular journalists, and book writers.

Who wrote the opinion articles and what constituted a policy article?

• Opinion articles were written by mostly medical specialists, such as psychiatrists, practitioners, and, in other cases by writers. However, in most of the cases, the personal opinion of the writer was advocated.
We defined the nature of policy articles as reports discussing regulatory-related topics, such as reimbursement, or change of indication.

How were the articles categorised and who did this? What was the protocol behind this?

• We would like to draw the referee's attention to the methods section, under the header "data classification". In the revised version, we extended the definitions to avoid misunderstandings regarding the scoring process. Based on those definitions, FH and TP performed the categorization. There were two files: (1) the extracted articles file, and (2) the scoring table file. In the scoring table file, the most important information or features from the articles were documented, such as title, date, author, newspaper, year, classification "effect", length (words), resume (if available), type of article, pediatric or adult, section, etc.

Another aspect that is not addressed in this paper is the vast difference between journal and newspaper articles. The difference between the authors and the audiences is important and should be identified.

• We agree with the reviewer's comment and this is an interesting point. However, nowadays, journalists have more access to scientific evidence and scientific findings are reported more often than before in news media. In addition to that, the exponential growth in the use of other informative sources, such as internet or social media, has familiarized the public with scientific news. The main problem is that the translation of scientific information to the public is not always accurate, as various studies pointed that out (Moynihan, R. et al. 2000, Mebane, F.E. 2005, Dentzer, S. 2009 or Barlett, C. et al. 2002). We have not addressed the issue regarding the authors and audiences differences because we were more interested in the quantity and type of information both audiences were confronted with, and how publication trends are influenced during a drug safety controversy.

Methods
The methodology for article categorisation, above, needs to be included. Also, the methods for rating the articles needs to be described in more detail. What were the inclusion and exclusion criteria for the articles (apart from the keywords)? Was there a rating/data extraction instrument? If so, was it validated? More description of the rating process is needed.

• The methodological point raised by the reviewer was implemented and complemented in the revised version of the manuscript. We would like to draw the reviewer's attention to the methods section under the heading "data classification", where the inclusion/exclusion criteria of articles was adapted.

Regarding the extraction instrument, we elaborated a table based on the most salient characteristics of the articles (e.g. section, title, length (words), author, and newspaper name, date, page, summary (if available), type of article, and age group discussed). After reading completion of each article, the scorer proceeded with assigning a score in the table. The scoring procedure was validated when both scorers (FH and TP) independently assigned scores to the articles, based on the information conveyed in the article. The differences found between scorers amounted less than 5% of all the 1736 analyzed articles.

The main outcome measures, like the aims, are relatively unclear. They appear to be, how many negative and positive articles were published in journals and newspaper of a 10 year period. This is well covered. However, the results pertaining to the type of article are uninformative as we don't have a clear idea of what these categories really represent or how they are important.

• We agree with the reviewer. We tried to clarify the formulation of the general idea (please refer to page 5) and added to the definition of the categories used (please refer to page 8 under the header "data classification" of the revised version of the manuscript). We hope this will improve the understandability and the representation of the results satisfactorily.

References 5, 6, 26, 27 and 29 are getting a bit old and the paper would benefit from more current documents.

• We agree with the comments raised by the reviewer, and we gladly improved the use of references in the revised version of the manuscript.

Is there any good evidence that misleading topics are deliberately selected to increase readership? I haven't seen a content study that convincingly identified consistent examples of misleading reporting of a single health issue. However, studies have found responsible reporting of drug safety issues for example, news media coverage of the Women's Health Initiative results was found to be generally balanced, consistent and accurate (Canales, Breslau et al. 2008).

• We agree with the reviewer that studies have found responsible reporting of drug safety issues. The Women's Health Initiative was exemplary in this regard. While Canales et al. pleaded for a consistent, balanced and accurate reporting in news media, the editor-in-chief of Health Affairs reported in the NEJM (Dentzer S, 2009) that: "too frequently, what is conveyed about health by many other journalists is wrong or misleading". Therefore, Mrs. Dentzer also used the Women's Health Initiative as an example to demonstrate that. In addition, several studies we cited in our manuscript (e.g., Moynihan R, et al. 2000, Barlett C, et al. 2002, Danovaro-Holliday MC, et al. 2002, Mebane FE, et al. 2005) reported "oversimplified view of risks", "coverage was overwhelmingly positive and focused mainly on the benefits", etc., all in all, giving a distorted and unbalanced picture regarding health-related issues to the public. If misleading topics are deliberately selected to increase readership, we cannot honestly say anything about that, since that is not our area of expertise, nor was the aim of the present study. Therefore, we decided to remove the term to avoid misunderstandings.

The authors appear to imply that the negative reporting may have influenced restriction of SSRIs in under 18 years old while stating "a causal association between this sudden publication growth and the resulatory warnings was not assessed". If there is no evidence to support this, it really shouldn't be in the paper as it can easily become a zombie statement which takes on a life of its own through repetition although there is no underlying evidence.

• The regulatory warnings were the result of safety concerns regarding suicidal ideation in pediatrics. These concerns were raised by studies that analyzed the withdrawal effect in SSRIs, and the analysis of 9 more trials that GSK submitted to the FDA (pursuing a 6-month market exclusivity extension).

During that time, media activity was measured, in particular in UK with the broadcast of the documentary "the secrets of seroxat", which evoked a rollercoaster of (negative) reactions. Finding an association between the sudden publication growth and the warnings is irrelevant since it could be only one of the several factors that might have influenced the restriction of SSRIs under the 18-years old. Therefore, we complied with the suggestions made by the reviewer and we removed that statement from the revised version of the manuscript.

This paragraph contains a lot of speculative terms such as 'might substantiate the possible influence" "might also indicate". These speculations are interesting but not supported by the evidence offered in this paper and should perhaps be left for a more appropriate forum such as an opinion piece.

• The inferences we made in this part of the article are supported by the data provided in Figures 2 and 3. As observed in the grey boxes in both figures, which represent the warnings period, the increment on the number of articles was highly connected to the first period 2003-2004 and in less intensity to the second period 2007-2008. We attempted to improve the wording in that paragraph in the revised version to diminish the speculations.

While the interpretation of the data seems adequate, some of the conclusions appear to fall outside the scope of the analysis. The opening sentence of the conclusion says that publication trends "appear to be more affected by regulatory actions, opinions or lawsuits that... evidence-based medicine." This would appear to come from the fact that there were higher numbers of this type of article in newspapers than research related reporting however we are not told how the content of these articles related to a drug safety issue and I wasn't convinced that the data presented supports this conclusion.

• We came to this conclusion based on the fact that while the scientific literature reported mainly research studies (observational studies, RCTs, meta-analyses, etc.) with a positive outcome, in terms of depression relieve and/or decreasing suicidality risk, the growth measured in the number of articles in both scientific journals and newspapers was characterized as being negative regarding treatment outcome. These negative outcome articles in scientific journals and newspapers were identified as opinion articles and articles on pediatrics. Based on the suggestions given by the reviewer, we reformulated the conclusion to improve the clarity of the manuscript.
The content of the articles was related to the drug safety issue according to the set of keywords we predefined and used to retrieve the articles, and to the full-text content analysis that was independently performed by two researchers.

Is the message Clear?
I'm not clear on what the final message of this paper was. The final box – what the study adds - provides 3 points that were not fully explored in the paper. The first, that 'bad' news was related to opinion not evidence seems to based on the fact that more opinion pieces rather than news stories were categorised as negative. However, the evidence (level of evidence, sources of information) underpinning the content of these articles was not presented (was it even identified?). On this basis, is it possible to make this statement?
The second point is confusing – what wasn't uniform?
The third point introduces stakeholders for the first time but who the stakeholders are is not defined.

• The point raised by the reviewer (which was also raised by the BMJ Open) was adopted and the "final box" was removed from the revised version of the manuscript.

I'd advise against the use of the term lay, as in lay public page 16 - it looks patronising and in this case is tautological.

• This is a valid comment. We removed the term lay from the revised version of the paper.

Reviewer #2: John Coverdale M.D. M.Ed., FRANZCP, Professor of Psychiatry and of Medical Ethics, Baylor College of Medicine, Houston, Tx 77030, USA.
Nikita Malani, Medical Student, Baylor College of Medicine, Houston, Texas 77030, USA.

The main methodological concerns were the following:

1. A content measure of analysis is limited by not capturing the richness of depictions and by not identifying potential competing themes. The evaluation did not provide elaboration on the construction of articles, weight of balance of positive and negative articles, accuracy, or the validity of the information presented. Potentially negative or stereotyping elements that in turn might be influential in the general public's thinking were not identified. All in all, the context of the articles was not be ascertained.

• The reviewers raise an interesting point. The construction of articles was fully elaborated by the use of the predefined keyword sets, which were adapted to language, and background (jargon in newspapers differs from scientific journals). In addition, two reviewers independently scored the articles, which resulted in more than 95% rate of agreements.
Regarding the identification of potential competing themes, we did not analyze that facet of the articles, although it might be of significance.
During the early phase of the study, we had a weight balance of the effect category that was positive, positive/neutral, neutral, neutral/negative and negative. After the content analysis on full-text, we saw little relevance by using the extra scores (positive/neutral and neutral/negative). Therefore, we removed them and kept the three effect categories we presented in the paper.
Regarding the identification of potentially negative or stereotyping elements, we limited our study to the score of general terms (positive, neutral and negative). Based on our study design, we cannot say anything about these elements.
We agree with the reviewers' comment that states that the context of the articles was not ascertained. We did extract the context of the articles, wherein different contexts were identified (e.g., depression therapy, mental health, murder, suicide and suicide attempt, post natal depression, sports, etc.). Unfortunately, we were limited in the amount of data to present in this study and therefore we left out this important facet of the data. We consider that this did not affect the main message of our paper.

2. It was unclear how the news reports related to other stories within the newspapers, in terms of location in the paper and surrounding articles and advertisements. Obtaining a sense of the contextuality of the articles would require a careful analysis of the construction of the newspaper article from its original source and an understanding of news-worthiness. The simple content analysis method identified in the study does not allow elucidation of these processes.

• We agree with the reviewers' commentaries. However, analyzing the contextuality of the articles was not the aim of our study. We did characterize the context of the stories, such as location in the newspaper (section). The characterization revealed a broad spectrum of sections that reported about SSRIs and suicidality and varied in intensity from front page to sports, life style or show business, health, or science. We did not present these results due to the lack of space. We consider that leaving out this information did not influence the main message we aimed to convey. At least, not when related to our research question.

3. While the authors sought to analyze the "publication dynamics" and what was called the process of dissemination, little can be said about the methods by which the newspapers constructed their information or reports.

• We have to disagree with the reviewers' comments. The aim of our study was not defined to elucidate the methods on how newspapers construct news. Defining the rationale behind that might imply the development of questionnaires and perform interviews with journalists or editors.

4. The authors noted that articles that reported on therapeutic outcomes with no mention of "causality" between SSRIs and suicidality were classified as positive. The negative category classification was not diametrically opposite to the positive category, as negative articles were classified as those that reported on an "association" between SSRIs and suicidality, not causality. Negative articles were also classified as making little or no mention of "positive therapeutic outcomes." Thus the major basis for classification as positive or negative was based on two factors which were not necessarily clearly different.

• This is a legitimate concern raised by the reviewers. The wording differences were improved in the revised version of the manuscript (please refer to page 8 under the header "data classification").

5. As above, negative articles were identified as making little or no mention of "positive therapeutic outcomes." However, the word anti-depressant in and of itself suggests therapeutic outcome. Thus the articles might inherently be suggesting a therapeutic outcome, and this suggests a need for a more rich or sophisticated classification method.

• We have to disagree with the reviewers' comments. That the word anti-depressant might suggest a therapeutic outcome does not imply that an antidepressant will always be effective, nor in all types of patients. This also depends on the type antidepressant used. The scientific literature illustrates the various conditions, known so far, in which antidepressants might be effective or not. So, in this regard, none of the articles we scored was inherently suggesting a therapeutic outcome. We considered antidepressants, as word and as medicament, as neutral.

6. The authors noted that their collection of newspaper articles was potentially incomplete. It would have been helpful to check the penetrance of the searches performed against a hand-search of the newspapers used.

• This would potentially be useful. But, in almost all systematic searches that use keywords, the researcher is limited to the validity and the reliability of the search machine or the completeness of the database. This also implies the way in which articles are indexed should be always constant. Our collection of newspaper articles, and also of scientific articles, represents a random sample of all articles published in those sources about SSRIs and suicidality between 2000 and 2009. We consider that our sample was significant enough to provide an overview of what has been published.

Nevertheless, the article has merit, as there are few papers of this type and the analysis present in this study is relevant to all fields of medicine. The fact that patients receive information (that may have a certain bias) about medications that are prescribed to them poses a challenge in the doctor-patient relationship. The study effectively used the SSRI and suicidality controversy to explore the biases present in both scientific journals and general publications such as newspapers. The positive bias inherent in journal articles could potentially leave physicians with an incomplete view of the medications that they are prescribing to patients. On the other hand, the negative bias present in newspapers (and other forms of media) has implications for patient compliance with medication, willingness to see physicians, and trust in the doctor-patient relationship.
Therefore, all in all the authors should attempt to clarify the classification methods of negative and positive articles and note in their limitations any limitations in these definitions and how the methodology is also limited by its content-analysis nature. The results are also complicated and difficult to understand in terms of their impact. The authors might therefore consider re-thinking how

they portray the main findings and their potential impact for the reader.

• We sincerely thank the reviewers for the acknowledgments given to our article. The points made by the referees about the definitions, and the methodology of the study, were improved in the discussion section within the limitations paragraph.

VERSION 2 – REVIEW

REVIEWER	John Coverdale, Professor of Psychiatry and of Medical Ethics, Baylor College of Medicine, Houston, Texas 77030, USA Nikita Malani, Medical student, Baylor College of Medicine, Houston, Texas 77030, USA. We have no competing interests.
REVIEW RETURNED	11/09/2011

GENERAL COMMENTS	The authors have adequately addressed our concerns from the previous review which included a request to add to the limitations of the manuscript especially by noting the relative absence of context of the results (as previously discussed), and by clarifying the definitions of the main outcome categories. The aim of the paper was more clearly defined and the implications of this research were better described. The paper is substantially improved in response to the comments by the other reviewer also. This is a paper on a topic of public health importance that should be of interest to readers.

REVIEWER	Amanda Wilson Research Academic Medicine and Public Health University of Newcastle NSW AUSTRALIA
REVIEW RETURNED	23/09/2011

GENERAL COMMENTS	The authors have addressed all the points I raised in the initial review to my satisfaction. I believe this research adds substantially to the literature in this area and recommend this paper is accepted. I have only a couple of further suggestions, one relating to the Interpretation on page 4 (lines 8-19). This is a bit unclear and could be reworded. The original wording starting with "Scientific journals and newspapers coincided..." was clearer and more informative - I hope my original comments didn't cause this deletion. The second comment refers to the addition on p 14 (lines 2-6) - are there any references to support this? Please note these are suggestions only.