PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Dying to be famous: retrospective cohort study of rock and pop star mortality and its association with adverse childhood experiences
AUTHORS	Bellis, Mark; Hughes, Karen; Sharples, Olivia; Hennell, Tom; Hardcastle, Katherine

VERSION 1 - REVIEW

REVIEWER	Peter W de Leeuw, professor of medicine Dept of Medicine, University Hospital Maastricht Maastricht, The Netherlands No competing interest except a similar fascination with the topic of study.
REVIEW RETURNED	30-Sep-2012

THE STUDY	Paper is appropriate in this respect. The STROBE statement does not have to be included in the manuscript.
GENERAL COMMENTS	Comments (general) This is an interesting analysis of rock and pop star mortality. It builds further on earlier work by the same group. The study is a retrospective analysis with all of the problems and caveats associated with such an approach. In the discussion section the authors deal with most of the limitations. However, one aspect that is not highlighted enough, is how specific their results are. In other words, if one would do the same type of analysis with professors of medicine (or perhaps public health) who became famous through their publications, would one find the same thing? Intuitively, I think not but it cannot entirely be excluded. Specific questions In the methods section the authors state that they used Wikipedia, biographies etc. Some of the information, in particular that from Wikipedia may not always be reliable and also biographies may be biased. Is it possible to provide some more data, e.g. in supplemental files on the type of sources? How did the authors deal with people like Phil Collins who became famous with a band but then continued as solo artist or the other way around. In what category would such a person fit? What is the rationale to exclude some genres? My guess would be that for instance in the blues scene (with a lot of non-white stars) there are several artists that would be eligible for the present study and country also has its problems (take John Denver as an example). It would help the reader if the authors justify their choices.

	The weakest part of the study is that on the ACEs both in terms of data collection and analysis. Again, how specific are these data and could similar ACEs be found in survivors? It would be best if in the discussion section the authors weaken their conclusions a bit more. This study focuses on those who have reached fame. But what about those artists that had only one hit and very short periods of fame? Scott McKenzie, for instance, had only one major hit. Is he considered someone with fame or not?

REVIEWER	Dr Adam R Winstock Consultant Psychiatrist and Senior Lecturer SLAM NHS Trust and KCL
REVIEW RETURNED	03-Oct-2012

THE STUDY	limitations edction needs to be more robustly written and extended
GENERAL COMMENTS	Dr Adam R Winstock SLAM NHS Trust and KCL hank you for giving me the chance to review this novel paper that addresses topic rarely considered within academic circles. The paper is timely and its conclusions could usefully be promoted to alter the way we think about substance use in famous people. The methodology is sound though some clarification and consistency in terms is required and I think as shorter introduction would all improve the paper. The assessments of adverse childhood experiences are an excellent addition to the research literature. I think if these issues can be attended to them the paper may be suitable for publication. I have made some specific recommendations below. 1) Page 5 – strengths and limitations: Remove first point. Suggest add another saying there is considerable difficulty in determining the contribution of drugs and alcohol to some of the categories of death such as CVS, cancer, accidents and violence so defining substance related deaths accurately other than when due to an acute direct drug related cause (e.g. overdose, withdrawal) is not possible. 2) Page 6 Introduction: add ‘and’ in between exposure…arguably, suggest remove end of sentence after and behaviours. 3) Page 6 Introduction line 7 suggestion change to ‘hedonistic activities and their consequences once these prominent figures seek treatment when their substance use or …. 4) Introduction second paragraph – suggest reword ‘older age stars’ to ‘death in pop and rock stars in older age’ 5) Page 7 first paragraphs – suggest the advances in global communication is worth noting here. This whole paragraph is very clear and central to the thesis. 6) Methods. The first section here I think needs some introduction about what defines a rock and pop star for this study (which is of course outlined in detail but a global introduction seems useful – for example something about those above a certain threshold defined by their global album sales). 7) Clarification is needed that the study includes all band members so that the reader is clear as to what proportion of the n is represented by solo artists and what n and % is represented by those in a band. This can be done by changing Table 2 so that consistently n and % is given for each parameter. At present the table is confusing. In column one for example we have total n =

	1489 with all subsequent rows being given as n not % but then % given for solo artists and no n in the same column (table 1 has adopted a this approach and is clearer for it) 8) Table 1 can we know how band have more than one album in the list – this might be measure of fame within the groups – i.e. is there dose response relationships between mortality and fame? 9) Page 10: I am sure the authors struggled with the classification of drug related deaths v other and are aware the ambiguity of the groups – accident, suicide and violence. 10) Calculating survival - I think there method is appropriate and well described 11) Table 2 may be better split in two with the likely cause of death presented separately. I think the total in each category may have been added up incorrectly 1-6 = 39.3 not 38.7%. Unless the bio review ascertained that 4-6 were drug / alcohol related then including these variables – suicide/violence/accident may be stretching things since there are many non drug and alcohol related causes for these. This should at least be raised in the limitations section. Equally CVS and cancer causes can easily be attributed to alcohol and tobacco use (as noted by the authors themselves in page 17 in the first paragraph of their discussion). Some data on how many smoked tobacco would be interesting – we forget that tobacco probably kills as many people who use drugs than drugs themselves (probably many many more). Reanalyses using the tighter drug related causes (just variables 1-3) may be of interest? 12) Need limitations section expanded. The current statement 'that our data collection had significant ones' (page 18 second paragraph) should be followed by the limitation section sits at present is 3 pages on. I think the limitations section as whole could be tighter and more robustly described. Things that come to mind are limited info of on presence or absence of other possible risk factors (that are often seen in those with high levels of ACE and drug related death) – e.g. family history, poly drug dependence, heroin/barbiturate use, injecting use or not, mental illness, significant chronic medical problems. Again these issues are discussed by the authors in page 18 under the discussion, but should this be highlighted as possible limitations of the study. 13) Discussion well written – relates to findings. The EU sample is really UK – an additional limitation is the absence of significant non English speaking stars. Some discussion that the potential for further analyses exist by reclassifying the categories of drug related death and type of drug/route could be mentioned. A possible case control study of dead v living members of the same band might be an interesting secondary study 14) The terms 'fame', 'pop and rock star', 'musician' among others are all used interchangeably – perhaps an agreed single term for the paper might be useful. 15) Some of the discussion e.g. page 19 middle paragraph almost might sit better in the introduction as to why this is an important study.

VERSION 1 – AUTHOR RESPONSE

Professor Mark A. Bellis

Reviewer 1: Peter W de Leeuw, professor of medicine, Dept of Medicine, University Hospital Maastricht, Maastricht, The Netherlands

General
1) This is an interesting analysis of rock and pop star mortality. It builds further on earlier work by the same group. The study is a retrospective analysis with all of the problems and caveats associated with such an approach. In the discussion section the authors deal with most of the limitations. However, one aspect that is not highlighted enough, is how specific their results are. In other words, if one would do the same type of analysis with professors of medicine (or perhaps public health) who became famous through their publications, would one find the same thing? Intuitively, I think not but it cannot entirely be excluded.

We have added the following sentence.

Finally, it is unknown whether the impacts of ACEs and fame in other groups (e.g. film stars, sports stars) would show similar relationships with mortality to those identified here.

Specific
2) In the methods section the authors state that they used Wikipedia, biographies etc. Some of the information, in particular that from Wikipedia may not always be reliable and also biographies may be biased. Is it possible to provide some more data, e.g. in supplemental files on the type of sources?

We have added in additional information about the websites used to collate the data and also highlighted that data were cross-referenced between sources.

Using and cross-referencing between key websites (e.g. Wikipedia, BBC Music, Last FM, All Music, official band websites), biographies, and published anthologies, each individual's date of birth and survival status on 20th February 2012 was identified.

3) How did the authors deal with people like Phil Collins who became famous with a band but then continued as solo artist or the other way around. In what category would such a person fit?
We have clarified this in the text.

However, they were classified as solo or band artists, with a performer considered a solo artist if they had a solo album in the study; regardless of whether this preceded or followed success as a band member (e.g. Phil Collins, Genesis; Sting, The Police).

4) What is the rationale to exclude some genres? My guess would be that for instance in the blues scene (with a lot of non-white stars) there are several artists that would be eligible for the present study and country also has its problems (take John Denver as an example). It would help the reader if the authors justify their choices.

We have added in an additional sentence to clarify this issue.

Individuals from genres typically regarded as not being mainstream in both North America and Europe (country, blues, jazz, vocal, celtic, folk, bluegrass and spoken word) were removed.

We have also added this as a limitation in the Discussion:

Our choice of music genres (Table 1) aimed to capture only mainstream genres across both continents but some stars of, for instance, folk, country and jazz that were not included have substantial popular followings (e.g. Damien Rice).

5) The weakest part of the study is that on the ACEs both in terms of data collection and analysis. Again, how specific are these data and could similar ACEs be found in survivors? It would be best if in the discussion section the authors weaken their conclusions a bit more.

We have altered the discussion to stress from the beginning the limitations of the study – especially relating to measurement of ACEs.

6) This study focuses on those who have reached fame. But what about those artists that had only one hit and very short periods of fame? Scott McKenzie, for instance, had only one major hit. Is he

considered someone with fame or not?

We have added in a sentence to clarify that we did not try to distinguish different levels of fame between artists as we could not identify any reasonable measure for doing this.

We did not attempt to distinguish different levels of fame among stars.

Reviewer: Dr Adam R Winstock; SLAM NHS Trust and KCL

General
7) Limitations section needs to be more robustly written and extended.

We have expanded the limitations section considerably and moved it to the beginning of the discussion so that readers are aware of the study limitations when considering the discussion.

8) Thank you for giving me the chance to review this novel paper that addresses topic rarely considered within academic circles. The paper is timely and its conclusions could usefully be promoted to alter the way we think about substance use in famous people. The methodology is sound though some clarification and consistency in terms is required and I think as shorter introduction would all improve the paper. The assessments of adverse childhood experiences are an excellent addition to the research literature. I think if these issues can be attended to them the paper may be suitable for publication. I have made some specific recommendations below.

We have shortened the introduction which is now under 500 words.

Specific
9) Page 5 – strengths and limitations: Remove first point. Suggest add another saying there is considerable difficulty in determining the contribution of drugs and alcohol to some of the categories of death such as CVS, cancer, accidents and violence so defining substance related deaths accurately other than when due to an acute direct drug related cause (e.g. overdose, withdrawal) is not possible.

We have altered the limitations as suggested and added in an additional sentence stating:

However, exact cause of death was more difficult to identify. In particular, for some stars deaths from accidents and longer-term conditions may have been due to alcohol and drug use but would not be coded as such unless this was specifically reported in biographical resources.

We have also expanded the definition of ‘substance use or risk-related deaths’ so that it is clearer that some deaths from suicide and violence may or may not have involved substance use.

Causes of death were dichotomised into ‘substance use or risk-related deaths’ (drug or alcohol-related chronic disorder, overdose or accident and other risk-related causes that may or may not have been related to substance use, i.e. suicide and violence)

10) Page 6 Introduction: add ‘and’ in between exposure…arguably, suggest remove end of sentence after and behaviours.

We have changed this as advised by the reviewer.

11) Page 6 Introduction line 7 suggestion change to ‘hedonistic activities and their consequences once these prominent figures seek treatment when their substance use or ….

We have revised this sentence.

International media coverage ensures that fans and the wider public are constantly informed of stars’ hedonistic displays and equally captures their consequences when behaviours become problematic and such individuals seek treatment.[2]

12) Introduction second paragraph – suggest reword ‘older age stars’ to ‘death in pop and rock stars in older age’

We have revised this wording.

13) Page 7 first paragraphs – suggest the advances in global communication is worth noting here. This whole paragraph is very clear and central to the thesis.

We have included in the text reference to changes in global communications.

14) Methods. The first section here I think needs some introduction about what defines a rock and pop star for this study (which is of course outlined in detail but a global introduction seems useful – for example something about those above a certain threshold defined by their global album sales).

We have included a sentence at the beginning of the methods to clarify this issue.

With no internationally agreed definition of what constitutes a rock or pop star we used large, established music polls to identify which individuals to include.

15) Clarification is needed that the study includes all band members so that the reader is clear as to what proportion of the n is represented by solo artists and what n and % is represented by those in a band. This can be done by changing Table 2 so that consistently n and % is given for each parameter. At present the table is confusing. In column one for example we have total n = 1489 with all subsequent rows being given as n not % but then % given for solo artists and no n in the same column (table 1 has adopted a this approach and is clearer for it)

We have changed Table 2 as advised by the reviewer. This has meant moving statistics on median year of birth and year of fame into the text. We have done this in the first line of the results section.

Between continents samples did not differ significantly in gender; although NA artists were younger (median year of birth; NA, 1965; EU, 1961; $Z=2.650$, $P<0.01$) reached fame more recently (median year of fame; NA, 1992; EU, 1985; $Z=4.288$, $P<0.001$) and were less likely to be white (Table 2).

16) Table 1 can we know how band have more than one album in the list – this might be measure of fame within the groups – i.e. is there dose response relationships between mortality and fame?

We have added a sentence to clarify that we did not identify levels of fame. Although we considered this we could not identify a reasonable measure or proxy for level of fame.

We did not distinguish different levels of fame among stars. However, they were classified as solo or band artists, with a performer considered a solo artist if they had a solo album in the study; regardless of whether this preceded or followed success as a band member (e.g. Phil Collins, Genesis; Sting, The Police).

17) Page 10: I am sure the authors struggled with the classification of drug related deaths v other and are aware the ambiguity of the groups – accident, suicide and violence.

We have added text to clarify this (see point 9).
18) Calculating survival - I think there method is appropriate and well described

No response required.

19) Table 2 may be better split in two with the likely cause of death presented separately. I think the total in each category may have been added up incorrectly 1-6 = 39.3 not 38.7%. Unless the bio review ascertained that 4-6 were drug / alcohol related then including these variables – suicide/violence/accident may be stretching things since there are many non drug and alcohol related causes for these. This should at least be raised in the limitations section. Equally CVS and cancer causes can easily be attributed to alcohol and tobacco use (as noted by the authors themselves in page 17 in the first paragraph of their discussion). Some data on how many smoked tobacco would be interesting – we forget that tobacco probably kills as many people who use drugs than drugs

themselves (probably many many more). Reanalyses using the tighter drug related causes (just variables 1-3) may be of interest?

We have not split Table 2 but have reformatted it as suggested in point 15 and believe the new structure makes it apparent how the total percentage of 38.7% is reached. We have also made it clear that suicide and violence may not be substance use related but fit with the category of 'substance use or risk-related deaths'. We have changed the text to clarify this point (see point 9). We have not collected data on tobacco use but have now raised this in the limitations (see below). We have not reanalyzed using tighter drug related causes as the paper is examining relationships with risk behaviours including, but not limited to, substance use. This approach is consistent with the literature on the impacts of ACEs (adverse childhood experiences).

However, the standard ACE tool does not capture all possible adverse childhood experiences nor were all possible impacts of ACEs on mortality (e.g. smoking related deaths) recorded.

20) Need limitations section expanded. The current statement 'that our data collection had significant ones' (page 18 second paragraph) should be followed by the limitation section sits at present is 3 pages on. I think the limitations section as whole could be tighter and more robustly described. Things that come to mind are limited info of on presence or absence of other possible risk factors (that are often seen in those with high levels of ACE and drug related death) – e.g. family history, poly drug dependence, heroin/barbiturate use, injecting use or not, mental illness, significant chronic medical problems. Again these issues are discussed by the authors in page 18 under the discussion, but should this be highlighted as possible limitations of the study.

We have substantively increased the size and prominence of the limitations section (see point 5). We have also clarified that the standardised ACE tool does not capture all possible adverse aspects of childhood.

However, the standard ACE tool does not capture all possible adverse childhood experiences nor were all possible impacts of ACEs on mortality (e.g. smoking related deaths) recorded.

21) Discussion well written – relates to findings. The EU sample is really UK – an additional limitation is the absence of significant non English speaking stars. Some discussion that the potential for further analyses exist by reclassifying the categories of drug related death and type of drug/route could be mentioned. A possible case control study of dead v living members of the same band might be an interesting secondary study

We are pleased that the reviewer recognises that this study opens up a range of opportunities for further analyses and have included reference to some of these but feel that an extensive list or one that also identifies specific methodologies is not required.

Moreover, the extent to which ACEs occur in living pop stars and consequently their relationship with overall risk of mortality is an important research questions for further work. Finally, it is unknown whether the impacts of ACEs and fame in other groups (e.g. film stars, sports stars) would show similar relationships with mortality to those identified here. Consequently, this work on ACEs should be regarded as representing only an initial attempt to examine the impact of early life experiences in a unique group of individuals.

22) The terms 'fame', 'pop and rock star', 'musician' among others are all used interchangeably – perhaps an agreed single term for the paper might be useful.

We have edited the paper for consistency of terms throughout the paper and believe that terms now only differ where they are intended to convey different meanings (e.g. fame vs rock and pop stars).

23) Some of the discussion e.g. page 19 middle paragraph almost might sit better in the introduction as to why this is an important study.

As requested in point 8 we have tried to reduce the size of the introduction and so have not added this paragraph to it.

VERSION 2 – REVIEW

REVIEWER	Dr Adam R Winstock SLAM NHS Trust No competing interests
REVIEW RETURNED	23-Oct-2012

GENERAL COMMENTS	Happy to say all my issues have been addressed in the revised manuscript