PEER REVIEW HISTORY

This paper was submitted to the BMJ but declined for publication following peer review. The authors addressed the reviewers' comments and submitted the revised paper to BMJ Open. The paper was subsequently accepted for publication at BMJ Open.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Time to publication for NIHR HTA Programme-funded research: a cohort study
AUTHORS	Chinnery, Fay; Young, Amanda; Goodman, Jennie; Ashton-Key, Martin; Milne, Ruairidh

VERSION 1 - REVIEW

REVIEWER	Ross, Joseph Yale University School of Medicine, Internal Medicine
REVIEW RETURNED	16-Jul-2013

GENERAL COMMENTS	In this manuscript, Chinnery and colleagues utilize methods established by my research group as well as those of Tricco's in order to examine the time to publication of primary research and evidence syntheses funded by NIHR's HTA Programme. This analysis is well done and contributes to our understanding of time to publication for important evidence that can be used to inform patient and physician decisions. However, the authors present their findings in a way that minimizes their innovation and without further depth, suggesting that there are opportunities to improve the investigation. Given the data presented, these findings might be better suited to dissemination as a research letter than a full original research article. Originality and Importance While important, this study is not original. Rather than framing the entire Introduction around the prior work in this area, much of which informed this investigation (the papers by Turner, Tricco and my group), I would favor instead justifying why a comparison to NIH and Cochrane is needed. In truth, selective and delayed publication is a global issue, impacting patients and physicians worldwide. The better question is whether we should expect there to be differences in time to publication among research funded by NIHR as opposed to by the NIH, what policies might impact this expectation. Scientific Reliability This study appears to be very well done. However, the 1st sentence of the Results text in the Abstract should be rewritten. As stated, it appears that the investigators are reporting on only a third of their data. Rather, they should write, "Of 458 included projects, xxx (xx%) were primary research projects and xxx (xx%) were evidence syntheses."

	Overall Design of Study I thought the investigators could have been more innovative in their analyses – as written, they are simply reproducing previous approaches. Were there differences in time to publication for different scientific areas of focus? What about differences among different universities or research groups? What led to more rapid publication in an external journal as opposed to Health Technology Assessment – were these all published in BMJ or Lancet and so were fast-tracked? My one question is why examine time to publication in both Health Technology Assessment and other external journals? Since publication in Health Technology Assessment constitutes formal publication, in a MEDLINE-indexed journal available to the scientific community, does it matter? As an American, it is not clear to me why investigators are publishing in multiple journals. Wouldn't publication in one disqualify publication in another? Methods It would be helpful to know more about the sample being studied. What diseases are these studies and syntheses focused on? How many were co-funded by industry? How many of the clinical research studies were single site versus multisite? How many were examining therapeutic interventions versus diagnostic tests versus epidemiology studies? Was most of this work done by investigators at a few universities and colleges, in the UK versus Wales or Ireland? Specific comment: Information about the journal Health Technology Assessment does not belong in the Methods. Specific comment: As this is a stand-alone paper, I would provide further description of the sample construction used by Turner – it's not clear without going to the cited paper what projects were included in the larger sample and how generalizable the sample might be. Specific comment: Which (and how many) members of the project team abstracted all of the relevant information, including project dates. We have found this information can be complicated to abstract and requires validation. Results Specific comment: The presentation of the Results is a bit confusing. I would suggest modifying the language to state "the median time to any publication was xx months, xx months for publication as a monograph in Health Technology Assessment and xx months for publication in any other external journal". Specific comment: The median times to publication are provided, but I would also suggest including IQR or Ranges. Specific comment: The investigators describe medians, but frequently use the language 'on average'; this should be avoided. Interpretation and Conclusions

	I was impressed by the relatively rapid dissemination of findings among research funded by the NIHR – what can other funders learn from this success?

REVIEWER	Grant, Sean University of Oxford, Centre for Evidence-Based Intervention
REVIEW RETURNED	04-Sep-2013

GENERAL COMMENTS	This manuscript describes a retrospective cohort study that assesses the time-to-publication of primary research and evidence syntheses funded by the NIHR HTA in the Health Technology Assessment journal and in external journals. This study adds to the published literature by building on a previous study (Turner et al. 2013—Ref 7) that assessed the publication rate of studies funded by the NIHR HTA programme. The current study assesses a sub-sample of the Turner study: those research projects registered in the NIHR research programmes database that planned to submit their draft final report to the HTA journal on/before 9 December 2011. In addition to providing the publication rate of this sub-sample (Turner provided overall rates), the authors of the current study also provide original data concerning the median time-to-publication and the publication rate at 30 months of this sub-sample. The authors helpfully provide this data for evidence syntheses and primary research/trials separately (as well as the whole sample), and for publication in the HTA journal and external journals separately (as well as the whole sample). This work addresses an important topic on which general readers could benefit from more data. The publication practices and biases that this study aims to investigate are prevalent in the research community, so I welcome the opportunity for “researcher-readers” to further understand via new empirical data the implications of the current health research publication process. These publication practices and biases are also of direct consequence to health policy and practice, and particularly so for this study sample, as it consists of government-funded, applied research that is intended to inform the UK NHS and other health services. Given the importance and potential use of new data in this area, there are some methodological items in line with the STROBE guidelines for observational research that should be addressed before revisiting the decision to publish this manuscript. I have listed these items in order as they appear in the manuscript. The introduction section could benefit from a few quick clarifications of the specific research questions or hypotheses of the study. The authors state that the objective of this study is to determine time-to-publication for this sub-sample of HTA-funded research. However, there is no mention of the other purposes of this study (as judged by the analyses): i.e., to investigate the overall publication rate and the publication rate at 30 months of this sub-sample. The authors also state that the aim of the study is to “compare time to publication with other public sector funders.” I am not sure whether this is appropriate to explicitly report as a study objective, as the authors

	do not analyse time-to-publication of other funders themselves, but rather compare the results of their analyses on HTA research to the results of other studies that also have different time frames and different methodologies. As the methods of this study are designed only to assess publication rates for this sub-sample of NIHR HTA-funded research, the reported study objectives should reflect only this, and comparisons of the results of this study to the results other studies should more appropriately be moved from the results section to the discussion section. Regarding the overall study design, I am not sure whether "retrospective cohort" is an appropriate label for this study given what appear to be the actual analyses of interest. In a retrospective cohort study, researchers investigate "participants" (which, for this study, are HTA-funded research projects) in which an exposure was either present or absent to see whether this presence or absence leads to differences in the outcome of interest. As such, I take the "exposure" for the current study to be the type of research: primary research vs. evidence syntheses. While these data are analysed and reported separately, the introduction and discussion do not explicitly discuss how time-to-publication may differ between the two and why this matters. Instead, the comparison of interest appears to be between the overall sub-sample of this study and the samples of studies about other public sector funders. The authors should either provide more explication of the importance of the comparison between primary research vs. syntheses (e.g., when citing the Turner and Tricco studies in the introduction), or perhaps label the study as a cross-sectional analysis with sub-group analyses (like the Ross 2012 study cited). A serious concern that I have about the methods is how the authors located publications of HTA funded research in external journals. The authors state that they searched the HTA journal website for the online publication date of the first report of all projects in an external journal. As they themselves note in the discussion, this could significantly underestimate the publication rate and median time-to-publication in other journals. Without running searches in other databases, searching trial registries, looking at researchers' CVs/bios, etc., the authors of this article are entirely relying on the authors of other studies to report external publication to HTA. Consequently, I am apprehensive as to whether the data provided about external publications is accurate or is rather an artifact of the study methods. For example, this search strategy could be is one of the reasons why a main comparison of the retrospective cohort method—median time-to-publication of evidence syntheses in HTA vs. external journals—was not possible: the authors note in the discussion that preliminary work has found PIs to under-report their external publications by 15.8%. If the current study methods yielded 99 evidence syntheses published in external journals, and such publication is under-reported by 15.8% according to previous estimates, than the 99 found is really 84.2% of the 118 external evidence synthesis publications "out there." As 118 = 52.9% of the total number of evidence syntheses in this sub-sample, the study methods could have prevented a key analysis in this paper (e.g., comparing median time-to-publication of syntheses in HTA vs. other journals) if the under-reporting estimate is accurate. This problem is doubly confounded by the authors' note in the discussion "strengths and limitations" section that they may also be underestimating how long HTA-funded studies take to publish overall due to exclusion of older projects. If the authors are underestimating both how long

	HTA-funded research takes to publish overall AND how many external pubs there are, then there is the further possibility that conclusion that NIHR HTA-funded research is promptly published, and the HTA journal a reason for this, are artifacts of study methods. In the methods “data analysis” section, the authors note that the cumulative percentage of HTA-funded studies published in the HTA journal series was compared to other peer-reviewed journals. However, no formal statistical analyses of such a nature were done, like they were with the median time-to-publication Mann-Whitney U tests. The authors should either change this claim or perhaps perform Chi2 analyses to indicate whether publication rates differ by type of project, journal outlet, and overall vs. 30-day benchmarks. In the results and figure 1, it would be helpful for the authors to differentiate between and specifically refer to the numbers of studies potentially eligible, examined for eligibility, confirmed eligible, included in the study, and analysed. For example, the beginning of the results text says that the authors “identified 458 projects for inclusion in our analyses”, which sounds like 458 studies were analysed, when 378 were analysed but 458 were eligible. In the results “primary research” section, please provide the inter-quartile range for all medians. It is particularly important to provide this variance data in this section, as the data disaggregated by monograph vs. external journal have higher medians than the median when they are combined. At the moment, I am also unclear how the data from Table 1 reported in this section are organised: perhaps this could be organised more clearly? For example, the authors could report the data for primary research overall, then trials, and then “other” research; or the authors could report median time-to-publication data, then the publication rate at 30 months, and then the publication rate overall. Lastly, if extracted, there are a few other pieces of data congruent with the study aims that would be interesting to know to contextualise results: i.e., (1) how many publications are published in both HTA/other journals vs. just HTA vs. just other journals at 30 months and overall, and (2) the data for “other primary research” as is reported for “trials”. I understand that these data may not have been collected, there are page limits to the manuscript, and/or the authors may rightfully reply that this data is not relevant, yet I thought I should flag my interest in this data while reading the paper just in case. In the discussion “comparison with other studies” section, the authors note a new study indicating more prompt publication of NIH-funded trials in recent years. In light of this, the authors need to be more cautious about how they compare their results to NIH-funded trials. Throughout, it would be more accurate to nuance claims by saying HTA-funded research is prompt “in comparison with other public sector funders” and that there is a possibility that newer trials/reviews overall (i.e., HTA funded or otherwise) may be improving publication practices in light of CONSORT, AllTrials, PROSPERO, etc. In the discussion, the authors should also comment on how NIHR HTA funding and Health Technology Assessment guidelines might lead to the observed results: e.g., the requirement to submit final reports within 14 days of project completion, need to retain copyright (which other journals might not like), average time from submission to publication = 40 weeks, etc., are important to know in order to

	contextualise results and provide possible mechanisms for the findings that other groups (e.g., NIH, Cochrane) can adopt. The researchers note in the “conclusion and recommendations” section that “researchers are contractually obliged to publish their findings in full”: are there any other HTA policies worth noting? E.g., do researchers have to publish in HTA before other journals? I am also not sure of the authors claim that the HTA journal series “provides a means of publication for those projects that would not otherwise reach the public domain”, as authors may not be publishing as much in external journals because they have to publish in HTA. A valid question for the authors to address here is: if HTA didn't exist, would the authors publish elsewhere because they want a publication for their research and didn’t already have to publish a full monograph in HTA? As the authors themselves note in the introduction, many authors see their duty to the public as ending with the final report. Due to the lengthiness of HTA reports, some authors might stop with the HTA journal when they otherwise would have published elsewhere. I whole-heartedly agree that the HTA monograph series is valuable, particularly in providing open access and more comprehensive reporting than the vast majority of external journals allow, but I am not sure the claim that “projects … would not otherwise reach the public domain” is accurate. In the discussion “implications” section, given the general audience of BMJ, the authors might consider the audience-dependent definitions of “laudable”/”prompt” given the lack of an objective or a priori standard for time-to-publication and publication rates. As a researcher, I agree that the data for NIHR HTA-funded research is laudable and prompt compared to the data about NIH-funded trials (from the Ross 2012 study) and Cochrane reviews, though policy-makers and practitioners may think that (in trials for example) waiting over two years from project completion to publication is a long time for these data to reach the public. Moreover, the Ross 2013 study indicates that NIH-funded trials may be published more quickly now. I think it is worth noting this context throughout the discussion section and perhaps commenting on whether the data from this study further indicate that health research in general needs to work on quickening the processes of making scientific data available to the public. Overall I think that this study could provide new, important empirical data for a general health research audience. The above points about study methods should be addressed before publication of this information is further considered. Stylistic comments: - In the section “What is already known in this topic” (and the rest of the article), please report the median time to publication in Cochrane reviews in months rather than years, as the NIH data is reported in months, and the data this study adds are also reported in months. There should be consistency in reporting of measurement units throughout the paper to facilitate comparisons. - In the abstract “results” section, please report the number of trials, as is done for primary research projects and evidence syntheses. Please report all “months” data consistently to one decimal point (this should also be done for the rest of the article: this inconsistency occurs in the introduction, results, and discussion). Also provide the exact percent for evidence syntheses published in an external journal rather than stating “fewer than half did”.

	- In the introduction, Reference 3 is from 1987, which is outdated considering the vast changes in publication practices over the last 15 years. Is there a more up-to-date reference to use instead? - In the methods “data source” section, please update the data about the HTA journal to reflect the 2012 JCR, which is now out and was available when the paper was submitted (study submitted 5 July 2013; 2012 JCR published 20 June 2013). - In the methods “cohort sample” section, could the authors please add a note explaining how one knows whether projects planned to submit their draft final report on/before 9 December 2011? Is there a section about this in the NIHR research programmes database? - In the methods “data analysis” section, there is a run on sentence: “compared to other peer reviewed journals, time to publication”. - In the results “primary research” section, please provide the full data for the Mann-Whitney U test rather than just the p-value, for example "U(df) = u value, Z = z value, p value". - In the results “evidence synthesis” section, I think it would help clarify why Table 2 has a cell missing by adding a clause like “fewer than 50% of evidence synthesis projects publish in other peer-reviewed journals, so it was not possible to test for statistical significance.” I was confused at first as to why Table 2 had that missing cell. - In the discussion “comparison with other studies section”, there are a few typos: "Sixty-eight per cent" should be "68%" to be consistent with rest of paragraph, and “Cochrane reviews have a median time to publication is 29 months” should change “is” to “of”. - In the discussion, should the statement “The median time to publication in the monograph and an external journal could only be compared for primary research (as over half of the evidence syntheses do not have a recorded external publication)” be moved from the “implications” to “strengths and limitations” section? As the purpose of the article (according to the title) is to investigate time-to-publication, this strikes me as a limitation that requires further discussion rather than an implication. - Throughout the manuscript, could the authors please italicise HTA when they are referring to the journal to avoid confusion? - In Figure 1, the authors should add two more boxes for clarification: "Total Primary Research in Cohort = 184" and "Total Evidence Syntheses in Cohort = 274". - In Table 1, a bit of re-formatting could make the table easier to scan. For example, the authors could remove the row that says "Number of studies (in the cohort)" and instead change the top row to "Primary research (n = 155)" and "Trials (n = 126)". Glancing over the first row as it stands, it is easy to confuse that all 155 studies have been published in the HTA and an external journal. The authors could also collapse the second and third rows to read "Number of studies published (%)", and columns to therefore read "144 (92.9%)", "137 (88.4%)", etc—same change for bottom two rows as well. I also ask for consistent use of decimal places (some

	months don't have a decimal place and others do). If 23.0, then say 23.0 not 23, or change the months with decimal places to be rounded up. If rounding is done in some cells and not others, it can make differences look bigger than they actually are, particularly for the “months” data. - In Table 2, a bit of re-formatting could also occur. For example, the top row can be removed and add to the end of the title “Publication characteristics of HTA Programme-funded evidence syntheses with a protocol online date (n = 223)”. Otherwise there is the same “confusion problem” noted for Table 1 Row 1 above. The authors could also collapse the two "published" rows, and collapse the two "published at 30 months" rows, as suggested for Table 1.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Recommendation:

Comments:
In this manuscript, Chinnery and colleagues utilize methods established by my research group as well as those of Tricco’s in order to examine the time to publication of primary research and evidence syntheses funded by NIHR’s HTA Programme. This analysis is well done and contributes to our understanding of time to publication for important evidence that can be used to inform patient and physician decisions. However, the authors present their findings in a way that minimizes their innovation and without further depth, suggesting that there are opportunities to improve the investigation. Given the data presented, these findings might be better suited to dissemination as a research letter than a full original research article.

Originality and Importance

While important, this study is not original. Rather than framing the entire Introduction around the prior work in this area, much of which informed this investigation (the papers by Turner, Tricco and my group), I would favor instead justifying why a comparison to NIH and Cochrane is needed. In truth, selective and delayed publication is a global issue, impacting patients and physicians worldwide. The better question is whether we should expect there to be differences in time to publication among research funded by NIHR as opposed to by the NIH, what policies might impact this expectation.

Scientific Reliability

This study appears to be very well done. However, the 1st sentence of the Results text in the Abstract should be rewritten. As stated, it appears that the investigators are reporting on only a third of their data. Rather, they should write, “Of 458 included projects, xxx (xx%) were primary research projects and xxx (xx%) were evidence syntheses.”

Overall Design of Study

I thought the investigators could have been more innovative in their analyses – as written, they are simply reproducing previous approaches. Were there differences in time to publication for different scientific areas of focus? What about differences among different universities or research groups? What led to more rapid publication in an external journal as opposed to Health Technology Assessment – were these all published in BMJ or Lancet and so were fast-tracked?

My one question is why examine time to publication in both Health Technology Assessment and other external journals? Since publication in Health Technology Assessment constitutes formal publication, in a MEDLINE-indexed journal available to the scientific community, does it matter? As an American, it is not clear to me why investigators are publishing in multiple journals. Wouldn't publication in one disqualify publication in another?

Methods

It would be helpful to know more about the sample being studied. What diseases are these studies and syntheses focused on? How many were co-funded by industry? How many of the clinical research studies were single site versus multisite? How many were examining therapeutic interventions versus diagnostic tests versus epidemiology studies? Was most of this work done by investigators at a few universities and colleges, in the UK versus Wales or Ireland?

Specific comment: Information about the journal Health Technology Assessment does not belong in the Methods.

Specific comment: As this is a stand-alone paper, I would provide further description of the sample construction used by Turner – it's not clear without going to the cited paper what projects were included in the larger sample and how generalizable the sample might be.

Specific comment: Which (and how many) members of the project team abstracted all of the relevant information, including project dates. We have found this information can be complicated to abstract and requires validation.

Results

Specific comment: The presentation of the Results is a bit confusing. I would suggest modifying the language to state "the median time to any publication was xx months, xx months for publication as a monograph in Health Technology Assessment and xx months for publication in any other external journal".

Specific comment: The median times to publication are provided, but I would also suggest including IQR or Ranges.

Specific comment: The investigators describe medians, but frequently use the language 'on average'; this should be avoided.

Interpretation and Conclusions

I was impressed by the relatively rapid dissemination of findings among research funded by the NIHR – what can other funders learn from this success?

Abstract

See comments above.

Additional Questions:
Please enter your name: Joseph Ross

Job Title: Assistant Professor

Institution: Yale University

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

Fees for consulting?: No

Have you in the past five years been employed by an organisation that may in any way gain or lose financially from the publication of this paper?: No

Do you hold any stocks or shares in an organisation that may in any way gain or lose financially from the publication of this paper?: No

If you have any competing interests <A HREF='http://bit.ly/VW8GVB'target='_new'>(please see BMJ Group policy) please declare them here:

Reviewer: 2

Recommendation:

Comments:
This manuscript describes a retrospective cohort study that assesses the time-to-publication of primary research and evidence syntheses funded by the NIHR HTA in the Health Technology Assessment journal and in external journals.

This study adds to the published literature by building on a previous study (Turner et al. 2013—Ref 7) that assessed the publication rate of studies funded by the NIHR HTA programme. The current study assesses a sub-sample of the Turner study: those research projects registered in the NIHR research programmes database that planned to submit their draft final report to the HTA journal on/before 9 December 2011. In addition to providing the publication rate of this sub-sample (Turner provided overall rates), the authors of the current study also provide original data concerning the median time-to-publication and the publication rate at 30 months of this sub-sample. The authors helpfully provide this data for evidence syntheses and primary research/trials separately (as well as the whole sample), and for publication in the HTA journal and external journals separately (as well as the whole sample).

This work addresses an important topic on which general readers could benefit from more data. The publication practices and biases that this study aims to investigate are prevalent in the research community, so I welcome the opportunity for “researcher-readers” to further understand via new empirical data the implications of the current health research publication process. These publication practices and biases are also of direct consequence to health policy and practice, and particularly so for this study sample, as it consists of government-funded, applied research that is intended to inform the UK NHS and other health services.

Given the importance and potential use of new data in this area, there are some methodological items in line with the STROBE guidelines for observational research that should be addressed before revisiting the decision to publish this manuscript. I have listed these items in order as they appear in the manuscript.

The introduction section could benefit from a few quick clarifications of the specific research questions or hypotheses of the study. The authors state that the objective of this study is to determine time-to-publication for this sub-sample of HTA-funded research. However, there is no mention of the other purposes of this study (as judged by the analyses): i.e., to investigate the overall publication rate and the publication rate at 30 months of this sub-sample. The authors also state that the aim of the study is to "compare time to publication with other public sector funders." I am not sure whether this is appropriate to explicitly report as a study objective, as the authors do not analyse time-to-publication of other funders themselves, but rather compare the results of their analyses on HTA research to the results of other studies that also have different time frames and different methodologies. As the methods of this study are designed only to assess publication rates for this sub-sample of NIHR HTA-funded research, the reported study objectives should reflect only this, and comparisons of the results of this study to the results other studies should more appropriately be moved from the results section to the discussion section.

Regarding the overall study design, I am not sure whether "retrospective cohort" is an appropriate label for this study given what appear to be the actual analyses of interest. In a retrospective cohort study, researchers investigate "participants" (which, for this study, are HTA-funded research projects) in which an exposure was either present or absent to see whether this presence or absence leads to differences in the outcome of interest. As such, I take the "exposure" for the current study to be the type of research: primary research vs. evidence syntheses. While these data are analysed and reported separately, the introduction and discussion do not explicitly discuss how time-to-publication may differ between the two and why this matters. Instead, the comparison of interest appears to be between the overall sub-sample of this study and the samples of studies about other public sector funders. The authors should either provide more explication of the importance of the comparison between primary research vs. syntheses (e.g., when citing the Turner and Tricco studies in the introduction), or perhaps label the study as a cross-sectional analysis with sub-group analyses (like the Ross 2012 study cited).

A serious concern that I have about the methods is how the authors located publications of HTA funded research in external journals. The authors state that they searched the HTA journal website for the online publication date of the first report of all projects in an external journal. As they themselves note in the discussion, this could significantly underestimate the publication rate and median time-to-publication in other journals. Without running searches in other databases, searching trial registries, looking at researchers' CVs/bios, etc., the authors of this article are entirely relying on the authors of other studies to report external publication to HTA. Consequently, I am apprehensive as to whether the data provided about external publications is accurate or is rather an artifact of the study methods. For example, this search strategy could be is one of the reasons why a main comparison of the retrospective cohort method—median time-to-publication of evidence syntheses in HTA vs. external journals—was not possible: the authors note in the discussion that preliminary work has found PIs to under-report their external publications by 15.8%. If the current study methods yielded 99 evidence syntheses published in external journals, and such publication is under-reported by 15.8% according to previous estimates, than the 99 found is really 84.2% of the 118 external evidence synthesis publications "out there." As 118 = 52.9% of the total number of evidence syntheses in this sub-sample, the study methods could have prevented a key analysis in this paper (e.g., comparing median time-to-publication of syntheses in HTA vs. other journals) if the under-reporting estimate is accurate. This problem is doubly confounded by the authors' note in the discussion "strengths and limitations" section that they may also be underestimating how long HTA-funded studies take to

publish overall due to exclusion of older projects. If the authors are underestimating both how long HTA-funded research takes to publish overall AND how many external pubs there are, then there is the further possibility that conclusion that NIHR HTA-funded research is promptly published, and the HTA journal a reason for this, are artifacts of study methods.

In the methods “data analysis” section, the authors note that the cumulative percentage of HTA-funded studies published in the HTA journal series was compared to other peer-reviewed journals. However, no formal statistical analyses of such a nature were done, like they were with the median time-to-publication Mann-Whitney U tests. The authors should either change this claim or perhaps perform Chi2 analyses to indicate whether publication rates differ by type of project, journal outlet, and overall vs. 30-day benchmarks.

In the results and figure 1, it would be helpful for the authors to differentiate between and specifically refer to the numbers of studies potentially eligible, examined for eligibility, confirmed eligible, included in the study, and analysed. For example, the beginning of the results text says that the authors “identified 458 projects for inclusion in our analyses”, which sounds like 458 studies were analysed, when 378 were analysed but 458 were eligible.

In the results “primary research” section, please provide the inter-quartile range for all medians. It is particularly important to provide this variance data in this section, as the data disaggregated by monograph vs. external journal have higher medians than the median when they are combined. At the moment, I am also unclear how the data from Table 1 reported in this section are organised: perhaps this could be organised more clearly? For example, the authors could report the data for primary research overall, then trials, and then “other” research; or the authors could report median time-to-publication data, then the publication rate at 30 months, and then the publication rate overall. Lastly, if extracted, there are a few other pieces of data congruent with the study aims that would be interesting to know to contextualise results: i.e., (1) how many publications are published in both HTA/other journals vs. just HTA vs. just other journals at 30 months and overall, and (2) the data for “other primary research” as is reported for “trials”. I understand that these data may not have been collected, there are page limits to the manuscript, and/or the authors may rightfully reply that this data is not relevant, yet I thought I should flag my interest in this data while reading the paper just in case.

In the discussion “comparison with other studies” section, the authors note a new study indicating more prompt publication of NIH-funded trials in recent years. In light of this, the authors need to be more cautious about how they compare their results to NIH-funded trials. Throughout, it would be more accurate to nuance claims by saying HTA-funded research is prompt “in comparison with other public sector funders” and that there is a possibility that newer trials/reviews overall (i.e., HTA funded or otherwise) may be improving publication practices in light of CONSORT, AllTrials, PROSPERO, etc.

In the discussion, the authors should also comment on how NIHR HTA funding and Health Technology Assessment guidelines might lead to the observed results: e.g., the requirement to submit final reports within 14 days of project completion, need to retain copyright (which other journals might not like), average time from submission to publication = 40 weeks, etc., are important to know in order to contextualise results and provide possible mechanisms for the findings that other groups (e.g., NIH, Cochrane) can adopt. The researchers note in the “conclusion and recommendations” section that “researchers are contractually obliged to publish their findings in full”: are there any other HTA policies worth noting? E.g., do researchers have to publish in HTA before other journals? I am also not sure of the authors claim that the HTA journal series “provides a means of publication for those projects that would not otherwise reach the public domain”, as authors may not be publishing as much in external journals because they have to publish in HTA. A valid question for the authors to address here is: if HTA didn't exist, would the authors publish elsewhere because they want a publication for their

research and didn't already have to publish a full monograph in HTA? As the authors themselves note in the introduction, many authors see their duty to the public as ending with the final report. Due to the lengthiness of HTA reports, some authors might stop with the HTA journal when they otherwise would have published elsewhere. I whole-heartedly agree that the HTA monograph series is valuable, particularly in providing open access and more comprehensive reporting than the vast majority of external journals allow, but I am not sure the claim that "projects … would not otherwise reach the public domain" is accurate.

In the discussion "implications" section, given the general audience of BMJ, the authors might consider the audience-dependent definitions of "laudable"/"prompt" given the lack of an objective or a priori standard for time-to-publication and publication rates. As a researcher, I agree that the data for NIHR HTA-funded research is laudable and prompt compared to the data about NIH-funded trials (from the Ross 2012 study) and Cochrane reviews, though policy-makers and practitioners may think that (in trials for example) waiting over two years from project completion to publication is a long time for these data to reach the public. Moreover, the Ross 2013 study indicates that NIH-funded trials may be published more quickly now. I think it is worth noting this context throughout the discussion section and perhaps commenting on whether the data from this study further indicate that health research in general needs to work on quickening the processes of making scientific data available to the public.

Overall I think that this study could provide new, important empirical data for a general health research audience. The above points about study methods should be addressed before publication of this information is further considered.

Stylistic comments:
- In the section "What is already known in this topic" (and the rest of the article), please report the median time to publication in Cochrane reviews in months rather than years, as the NIH data is reported in months, and the data this study adds are also reported in months. There should be consistency in reporting of measurement units throughout the paper to facilitate comparisons.

- In the abstract "results" section, please report the number of trials, as is done for primary research projects and evidence syntheses. Please report all "months" data consistently to one decimal point (this should also be done for the rest of the article: this inconsistency occurs in the introduction, results, and discussion). Also provide the exact percent for evidence syntheses published in an external journal rather than stating "fewer than half did".

- In the introduction, Reference 3 is from 1987, which is outdated considering the vast changes in publication practices over the last 15 years. Is there a more up-to-date reference to use instead?

- In the methods "data source" section, please update the data about the HTA journal to reflect the 2012 JCR, which is now out and was available when the paper was submitted (study submitted 5 July 2013; 2012 JCR published 20 June 2013).

- In the methods "cohort sample" section, could the authors please add a note explaining how one knows whether projects planned to submit their draft final report on/before 9 December 2011? Is there a section about this in the NIHR research programmes database?

- In the methods "data analysis" section, there is a run on sentence: "compared to other peer reviewed journals, time to publication".

- In the results "primary research" section, please provide the full data for the Mann-Whitney U test rather than just the p-value, for example "U(df) = u value, Z = z value, p value".

- In the results “evidence synthesis” section, I think it would help clarify why Table 2 has a cell missing by adding a clause like “fewer than 50% of evidence synthesis projects publish in other peer-reviewed journals, so it was not possible to test for statistical significance.” I was confused at first as to why Table 2 had that missing cell.

- In the discussion “comparison with other studies section”, there are a few typos: "Sixty-eight per cent" should be "68%" to be consistent with rest of paragraph, and “Cochrane reviews have a median time to publication is 29 months” should change “is” to “of”.

- In the discussion, should the statement “The median time to publication in the monograph and an external journal could only be compared for primary research (as over half of the evidence syntheses do not have a recorded external publication)” be moved from the “implications” to “strengths and limitations” section? As the purpose of the article (according to the title) is to investigate time-to-publication, this strikes me as a limitation that requires further discussion rather than an implication.

- Throughout the manuscript, could the authors please italicise HTA when they are referring to the journal to avoid confusion?

- In Figure 1, the authors should add two more boxes for clarification: "Total Primary Research in Cohort = 184" and "Total Evidence Syntheses in Cohort = 274".

- In Table 1, a bit of re-formatting could make the table easier to scan. For example, the authors could remove the row that says "Number of studies (in the cohort)" and instead change the top row to "Primary research (n = 155)" and "Trials (n = 126)". Glancing over the first row as it stands, it is easy to confuse that all 155 studies have been published in the HTA and an external journal. The authors could also collapse the second and third rows to read "Number of studies published (%)", and columns to therefore read "144 (92.9%)", "137 (88.4%)", etc—same change for bottom two rows as well. I also ask for consistent use of decimal places (some months don't have a decimal place and others do). If 23.0, then say 23.0 not 23, or change the months with decimal places to be rounded up. If rounding is done in some cells and not others, it can make differences look bigger than they actually are, particularly for the “months” data.

- In Table 2, a bit of re-formatting could also occur. For example, the top row can be removed and add to the end of the title “Publication characteristics of HTA Programme-funded evidence syntheses with a protocol online date (n = 223)”. Otherwise there is the same “confusion problem” noted for Table 1 Row 1 above. The authors could also collapse the two "published" rows, and collapse the two "published at 30 months" rows, as suggested for Table 1.

Additional Questions:
Please enter your name: Sean Grant

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Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

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