

## PEER REVIEW HISTORY

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## ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Directing research funds to the right research projects: a review of criteria used by research organisations in Australia in prioritising health research projects for funding
<b>AUTHORS</b>	Tuffaha, Haitham; El_Saifi, Najwan; Chambers, Suzanne; Scuffham, Paul

## VERSION 1 – REVIEW

<b>REVIEWER</b>	Bruce Currie-Alder International Development Research Centre, Canada
<b>REVIEW RETURNED</b>	08-Sep-2018

<b>GENERAL COMMENTS</b>	<p><b>Comments on bmjopen-2018-026207</b></p> <ul style="list-style-type: none"><li>• Discussion mentions ‘purpose of scheme and the organisation objectives’. This passing reference hiding substantial first principles regarding the purpose of research and how ‘success’ is viewed; which are intimately tied to the mandate of the funding organisation and the purpose of particular funds or programs. Different funders and funds place different emphasis on advancing knowledge versus real-world application in their considerations of ‘relevance’; which in turn can explain the observed variation in criteria and scoring weights. The analysis could be enriched by digging beyond selection criteria to group funders by the outcome sought under each scheme.<ul style="list-style-type: none"><li>○ There is multi-decade shift away from exclusive merit-review within academia, towards relevance of research funding as judged by multiple stakeholders (including users and beneficiaries). Such fora embed assumption on application and real-life problem-solving that challenge and widen notions of ‘research quality’ (see <a href="https://doi.org/10.1093/reseval/rvy026">https://doi.org/10.1093/reseval/rvy026</a>)</li><li>○ There are known concerns with merit review, particularly when identities of applications are visible and past performance is a selection criteria. These include the so-called ‘Matthew effect’ where more established and prolific teams receive a disproportionate share of funding, and a tendency towards conformity privilege incremental research over more novel and potentially innovative approach (recall the logic</li></ul></li></ul>
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	<p>behind launching ‘grand challenges’ see <a href="#">Varmus et al 2003</a>).</p> <ul style="list-style-type: none"> <li>○ Research funders can seek a ‘portfolio’ that intentionally includes diversity rather than merely selecting the best ‘project’ under selection criteria. Even were transparent application of merit review is used in the first instance to recommend certain projects, funders can include considerations of equity in their final decision in order to provide opportunities to more remote or less established locations.</li> </ul> <ul style="list-style-type: none"> <li>● ‘Value of money’ and cost-effectiveness are quite problematic concepts in application. The manuscript should touch upon the debates beyond their application, and the ties to ‘research impact’ (see <a href="https://dx.doi.org/10.1186%2Fs12916-016-0620-8">https://dx.doi.org/10.1186%2Fs12916-016-0620-8</a> and work by <a href="#">CFHI</a> and <a href="#">Claire Donovan</a>) <ul style="list-style-type: none"> <li>○ The manuscript implies the economic choice is between a dollar invested in health research versus a dollar invested in healthcare. This decision is at a higher level than the empirical data examined, which takes for grant a research budget and decides investing a dollar in one project versus another.</li> <li>○ The introduction mentions ‘maximise value and reduce waste’ yet beyond scrutiny of cost effectiveness in project proposals these goals are also pursued through requirements to publish in open access, adopt measure of open data and open science, and fostering collaboration (rather than competition) within the research community.</li> <li>○ Table 2 does not consider the amount of funding available under each scheme. One would expect greater rigor and more complete assessment of submissions to larger value schemes or grants. Put simply, a million dollar grant should in principle receive greater scrutiny than a \$100,000 grant. If so, there might be patterns between the completeness of selection criteria and size of funding available. Exploring this relation offers an opportunity to deepen the analysis.</li> </ul> </li> <li>● The discussion argues “cost effectiveness of research projects was largely overlooked”, yet one alternative is that these considerations are addressed at a higher level, by the funding organization before launching a call for proposals. <ul style="list-style-type: none"> <li>○ In designing a program or fund, the sponsoring organization may scope the state of particular research fields and have a sense of what resources might be needed for the outcomes sought. This decision may occasionally be based on political consideration or simply budget available, yet that sponsoring organisations often need to make a business case before launching</li> </ul> </li> </ul>
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	<p>a call: some assessment of existing knowledge and potential opportunities to contribute to it. This means some of the authors call for 'systematic review' is addressed a priori by funders.</p> <ul style="list-style-type: none"> <li>○ Once the funder has determined the scale of resources required to likely achieve the results sought, these are codified in the eligibility criteria. For example, what are the minimum and maximum budget and duration for individual project applications, and even budget lines within such. A funding call for clinical trials should have different eligibility criteria from a call on climate modeling, given the difference in scale required and cost structures involved.</li> <li>○ Similarly 'ethical considerations' may be deferred to a lower level, as the funding organisations rely on the ethical review processes within the recipients' home institutions, such as universities and research centres. Thus rather than overlooked, ethical considerations can be simply embedded into checking the existence of such procedures as part of the feasibility criteria of team quality and the institutions support they receive from their home institution.</li> </ul>
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<b>REVIEWER</b>	Eva Hummers Dept. of General Practice, University Medical Center Goettingen
<b>REVIEW RETURNED</b>	17-Sep-2018

<b>GENERAL COMMENTS</b>	<p>bmjopen manuscript 2018-026207 - review</p> <p>I was looking forward to reading this paper, which I think is of potential interest for other researchers or officers of funding agencies.</p> <p>However, the paper falls short of my expectations and needs major amendments in order to become suitable for publication at all. While it promises to critically review funding criteria, it actually mostly only lists the criteria and their relative frequency across the schemes included in the analysis. While some discussion is attempted, there are no reference standards, calibration or comparators, for example from other countries or fields of science. Overall, the paper lacks a theoretical background or reference to the theory/theories of research or knowledge translation used by the founding organism. This would probably explain where the "five representative domains" come from. I would also expect the theoretical reference framework to clarify the construction /definition of concepts as "quality", "relevance", or "value" (what exactly does "high value" mean). For example, on p 4 line 74ff authors seem to presume that the principal aim of healthcare research in their field of interest is "to optimise health and economic benefit". Where does this come from? Do the grant schemes or funding organisms specify this? In my country, the main aim of some research grant schemes would be to advance knowledge in a given field, others focus on optimising health, but economic benefit would not be the primary goal of research. In their discussion and conclusion, the authors put a lot of emphasis</p>
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	<p>on “value for money”. While poorly defined in the first place, I would also like to know the theory/background behind this. I am surprised that the relative weight of each criterion was specified in many schemes, and would be interested in learning whether this always the case, or how many grant schemes dropped out of the analysis because this was not given. Overall, the paper is disappointingly superficial.</p> <p>Methods: It is unclear whether the authors included grant schemes focussing on or including basic biomedical research, for example using human biomaterials or animals to study mechanisms of disease or potential treatments. This kind of research would be included in some of the grant schemes focussing on health/medicine in my country. It looks as if they are not included here – which also implies a clear separation of funding schemes in Australia – but the authors should specify this.</p> <p>Legends should be provided for tables and figure. The flowchart/figure 1 states that 15 schemes were excluded during data extraction. Please specify why. Table 2 is not referred to in the text.</p> <p>Both table 2 and 3 are lengthy and very difficult to read. The authors should attempt to condense the information, and put the fully detailed tables into an appendix.</p> <p>The second figure (bar diagram) is almost illegible, being very small and of poor quality. Please provide a better quality image.</p> <p>The discussion must address limitations of the study, for instance effects of including only a small number of grant schemes, or also the lack of/lack of transparency of a clear theoretical framework used by funding organisms, if this should be the case.</p>
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### VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Bruce Currie-Alder

Institution and Country: International Development Research Centre, Canada

Please state any competing interests or state ‘None declared’: None declared

A good beginning: the analytical core of the paper is fine, but deserves a more complete discussion that examines more critically some of the points raised. Some specific suggestions below would help strengthen the text, yet the authors might also consider convening a roundtable with representatives of funding organizations to verify findings and understand reasons behind the observed patterns. We thank the reviewer for commending the manuscript and the analysis. We would like to take this opportunity to clarify that this paper, as we indicated in the introduction, is the first attempt to understand the criteria considered by various funding organisations in Australia in selecting research projects for funding. This work is part of a larger project to develop a comprehensive framework to guide research prioritisation in the country. The next step in this process, in agreement with what the reviewer suggested, is to directly engage with the leadership of funding organisations to obtain their further insights. We will add this to the discussion as a future direction.

(1) Discussion mentions 'purpose of scheme and the organisation objectives'. This passing reference hiding substantial first principles regarding the purpose of research and how 'success' is viewed; which are intimately tied to the mandate of the funding organisation and the purpose of particular funds or programs. Different funders and funds place different emphasis on advancing knowledge versus real-world application in their considerations of 'relevance'; which in turn can explain the observed variation in criteria and scoring weights. The analysis could be enriched by digging beyond selection criteria to group funders by the outcome sought under each scheme.

We have elaborated further on this point and discussed how the purpose of the scheme and its success may vary across funders. However, it would be hard to fully analyse the selection criteria conditional on the overall objectives and purpose of each scheme because these objectives were not publically available for the majority of the grants included.

o There is multi-decade shift away from exclusive merit-review within academia, towards relevance of research funding as judged by multiple stakeholders (including users and beneficiaries). Such fora embed assumption on application and real-life problem-solving that challenge and widen notions of 'research quality' (see <https://doi.org/10.1093/reseval/rvy026>)

We have included this aspect in the discussion and cited the work by Cartier and colleagues. We have also referred to the recently published papers on PCORI's experience in the US with stakeholders involvement in research prioritisation.

o There are known concerns with merit review, particularly when identities of applications are visible and past performance is a selection criteria. These include the so-called 'Matthew effect' where more established and prolific teams receive a disproportionate share of funding, and a tendency towards conformity privilege incremental research over more novel and potentially innovative approach (recall the logic behind launching 'grand challenges' see Varmus et al 2003).

We agree with the reviewer. We have added this important point to the discussion and included the Grand Challenges initiative and the recent NHMRC scheme of 'Ideas Grants' as examples of possible solutions to reduce this bias..

o Research funders can seek a 'portfolio' that intentionally includes diversity rather than merely selecting the best 'project' under selection criteria. Even were transparent application of merit review is used in the first instance to recommend certain projects, funders can include considerations of equity in their final decision in order to provide opportunities to more remote or less established locations. o Similarly 'ethical considerations' may be deferred to a lower level, as the funding organisations rely on the ethical review processes within the recipients' home institutions, such as universities and research centres. Thus rather than overlooked, ethical considerations can be simply embedded into checking the existence of such procedures as part of the feasibility criteria of team quality and the institutions support they receive from their home institution.

We have amended the text on the discussion to indicate that these considerations can be considered at a lower level (institution) or considered during the discussions before a final decision is made.

Nevertheless, we call for clarity about where the responsibility for ethical and equity considerations lies.

(2) 'Value of money' and cost-effectiveness are quite problematic concepts in application. The manuscript should touch upon the debates beyond their application, and the ties to 'research impact' (see <https://dx.doi.org/10.1186%2Fs12916-016-0620-8> and work by CFHI and Claire Donovan)

We have touched based on the definition and evaluation of research impact as an element of value. We have also referred to the works of Greenhalgh and Donovan in the sense of the need for broader dimensions of research impact.

o The manuscript implies the economic choice is between a dollar invested in health research versus a dollar invested in healthcare. This decision is at a higher level than the empirical data examined, which takes for grant a research budget and decides investing a dollar in one project versus another. Actually, we did not aim to imply that the economic choice is between a dollar invested in health research versus a dollar invested in healthcare. We agree with the reviewer that this is often done at a higher level when choices are made between investing in direct health care and investing in research. We would like to clarify that the objective of the manuscript is to review the criteria considered by

research organisations in prioritising research projects for funding (not setting overall priorities). We have clarified this point in the introduction and differentiated between two levels: 1) selecting strategic research areas or topics (e.g., indigenous health or cancer) to guide overall research activity and commissioning, and 2) selecting specific research projects for funding from proposals put forward by researchers. For level 2 (selecting among proposals), the budget has already been allocated; however, the decision maker should aim, with this finite budget, to maximise benefits by selecting a portfolio of proposals that maximise return on investment. We have elaborated on this in the discussion.

o The introduction mentions 'maximise value and reduce waste' yet beyond scrutiny of cost effectiveness in project proposals these goals are also pursued through requirements to publish in open access, adopt measure of open data and open science, and fostering collaboration (rather than competition) within the research community.

We agree with the reviewer on these important domains to reduce waste. In refereeing to the recommendations from the Lancet series about research wastage, we focused on the recommendations from the paper by Chalmers and colleagues on how to increase value and reduce waste when research priorities are set (namely engage stakeholders, avoid duplication, research on research). We have also made it clear in the discussion that research value for money should consider broader domains of benefits beyond economic benefits.

o Table 2 does not consider the amount of funding available under each scheme. One would expect greater rigor and more complete assessment of submissions to larger value schemes or grants. Put simply, a million dollar grant should in principle receive greater scrutiny than a \$100,000 grant. If so, there might be patterns between the completeness of selection criteria and size of funding available. Exploring this relation offers an opportunity to deepen the analysis.

That would be a valuable addition to the analysis indeed; unfortunately, we did not have access to the amount of funds available under each scheme to conduct this analysis. We were limited by the publically available data on the websites of reviewed organisations. This can be done in the future when we engage with decision makers in these organisations.

(3) The discussion argues "cost effectiveness of research projects was largely overlooked", yet one alternative is that these considerations are addressed at a higher level, by the funding organization before launching a call for proposals. In designing a program or fund, the sponsoring organization may scope the state of particular research fields and have a sense of what resources might be needed for the outcomes sought. This decision may occasionally be based on political consideration or simply budget available, yet that sponsoring organisations often need to make a business case before launching a call: some assessment of existing knowledge and potential opportunities to contribute to it. This means some of the authors call for 'systematic review' is addressed a priori by funders.

This is absolutely possible to make the business case when the purpose is to prioritise research topic/questions for commissioning. In Australia, as we clarified in the introduction, most research funding goes to investigator-initiated proposals. In this case it would be useful to demonstrate value for money for these proposals by the investigators (i.e., it is the responsibility of the research team to make the business case), this value can be assessed/verified by funding organisation before a decision is made. We have elaborated on this point in the discussion and explained why it is important to show knowledge gaps in justifying investigator-initiated proposals.

Once the funder has determined the scale of resources required to likely achieve the results sought, these are codified in the eligibility criteria. For example, what are the minimum and maximum budget and duration for individual project applications, and even budget lines within such. A funding call for clinical trials should have different eligibility criteria from a call on climate modeling, given the difference in scale required and cost structures involved.

We agree with the reviewer, and, as we mention above, this is more suitable for commissioned research rather than investigator initiated research.

Reviewer: 2

Reviewer Name: Eva Hummers

Institution and Country: Dept. of General Practice, University Medical Center Goettingen

Please state any competing interests or state 'None declared': none declared

Please leave your comments for the authors below

bmjopen manuscript 2018-026207 - review

I was looking forward to reading this paper, which I think is of potential interest for other researchers or officers of funding agencies. However, the paper falls short of my expectations and needs major amendments in order to become suitable for publication at all. While it promises to critically review funding criteria, it actually mostly only lists the criteria and their relative frequency across the schemes included in the analysis. While some discussion is attempted, there are no reference standards, calibration or comparators, for example from other countries or fields of science.

We thank the reviewer for her comments. This manuscript is the first attempt to review the criteria used by funding organisations in Australia. In fact, as we have clarified in the revised introduction, the literature on criteria considered by funding organisations to select research proposals for funding is limited. The analysis was mainly descriptive in nature; however, and as the reviewer may appreciate, quantitative comparison across schemes or with other schemes may not be feasible given the high heterogeneity in schemes purpose, criteria used and the level of details provide.

We appreciate the reviewers concerns regarding the need for further discussion and comparisons with other jurisdictions. In response, we have made major revisions to the discussion to add more depth and breadth. We have elaborated on the key criteria reported in the review and included suggestions from both reviewers.

Overall, the paper lacks a theoretical background or reference to the theory/theories of research or knowledge translation used by the founding organism. This would probably explain where the "five representative domains" come from. I would also expect the theoretical reference framework to clarify the construction /definition of concepts as "quality", "relevance", or "value" (what exactly does "high value" mean).

We agree with the reviewer that the use of a theoretical background or a clear framework for knowledge generation and translation would be ideal. However, the theoretical background was not available in the documents/websites of the funding organisations reviewed. Nevertheless, we will add this as a limitation in the discussion. Regarding the domains used to categorise the criteria identified, these were obtained from the key comprehensive research prioritisation frameworks and tools available in the literature. We also used the definitions as reported in these frameworks and tools. Please see revised Table 1 for definitions.

For example, on p 4 line 74ff authors seem to presume that the principal aim of healthcare research in their field of interest is "to optimise health and economic benefit". Where does this come from? Do the grant schemes or funding organisms specify this? In my country, the main aim of some research grant schemes would be to advance knowledge in a given field, others focus on optimising health, but economic benefit would not be the primary goal of research.

We thank the reviewer for this important comment. The economic benefits of research is an important outcome of research funding as seen by the Australian Government. In the 2015-2016 budget, the Australian Government announced the introduction of the \$20 billion Medical Research Future Fund. The Government explicitly stated that "The MRFF will attract and retain excellent researchers, allow for the discovery and commercialisation of new medicines and technologies, and enable innovative treatments and cures. It will deliver improved health for all Australians, contribute to a sustainable health system, and provide significant economic benefits." "The government's Medical Research

Future Fund is stopping the brain drain, keeping our best and brightest minds right here in Australia. And as these researchers commercialise their ideas, Australians will benefit through a stronger health system and increased economic growth and jobs.” Further details are provided in <https://beta.health.gov.au/initiatives-and-programs/medical-research-future-fund/about-the-mrff>  
We have added this statement to the introduction to make it clearer up front for international readers how research benefits are viewed in Australia.

We appreciate that some countries do not consider research as a source of economic benefit; nevertheless, economic benefits, alongside other social and knowledge gain benefits, are considered by leading research bodies in other countries such as Canada and the UK. We have elaborated in this in the discussion when we discuss research impact and value for money.

In their discussion and conclusion, the authors put a lot of emphasis on “value for money”. While poorly defined in the first place, I would also like to know the theory/background behind this. We appreciate the ‘value for money’ means different things to different people. Traditionally, funders and researchers implicitly assumed that scientifically rigorous research that can advance knowledge or research with potentially high impact (e.g., improve health) is value for money. We have clarified this point in the methods (in Table 1 definitions) and in the discussion, that what we mean by value for money is cost-effectiveness which is the explicit comparison of the costs and benefits of research proposals competing for funding. The background behind this is deeply rooted in the economics theory; research budgets are often limited (i.e., finite) and decisions must be made about how to allocate these resources (e.g., which research proposal to fund) to maximise benefits. Failure to consider this aspect brings the risk of funding research projects where the cost of conducting research outweigh the expected benefits. This is what economists call “opportunity cost” which is the benefit forgone elsewhere by adopting suboptimal choices. These points have been added to the discussion.

I am surprised that the relative weight of each criterion was specified in many schemes, and would be interested in learning whether this always the case, or how many grant schemes dropped out of the analysis because this was not given.

We did not exclude any scheme because the weights were not reported; this was not an exclusion criterion.

Overall, the paper is disappointingly superficial.

As we have mentioned above. The purpose of this paper is to understand the current use of selection criteria by funding organisations in Australia and to critically review these. The manuscript was not written to discuss every criterion as this is a complex topic. For instance, it's hard to discuss all aspects of research impact or scientific quality and rigour. Nevertheless, we have revised our discussion in the light of the valuable comments provided by the two reviewers to improve our manuscript.

Methods: It is unclear whether the authors included grant schemes focussing on or including basic biomedical research, for example using human biomaterials or animals to study mechanisms of disease or potential treatments. This kind of research would be included in some of the grant schemes focussing on health/medicine in my country. It looks as if they are not included here – which also implies a clear separation of funding schemes in Australia – but the authors should specify this. The schemes included fund various research type including basic research. We have provided a clear definition of health research in the introduction which is Health research refers to research with human health or medical purpose, including research on the aetiology, diagnosis or management of disease, mental condition or behaviour in human.”

Legends should be provided for tables and figure.

We have provided these

The flowchart/figure 1 states that 15 schemes were excluded during data extraction. Please specify why.



Table 2 is not referred to in the text.

We have updated the figure to include the reason for excluding the 15 articles, which was the lack of publically available information on the selection criteria.

Both table 2 and 3 are lengthy and very difficult to read. The authors should attempt to condense the information, and put the fully detailed tables into an appendix.

We have moved Table 2 to appendix

The second figure (bar diagram) is almost illegible, being very small and of poor quality. Please provide a better quality image.

We have improved the resolution of the image

The discussion must address limitations of the study, for instance effects of including only a small number of grant schemes, or also the lack of/lack of transparency of a clear theoretical framework used by funding organisms, if this should be the case.

We have added this to the discussion as a limitation.

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Bruce Currie-Alder International Development Research Centre, Canada
<b>REVIEW RETURNED</b>	03-Nov-2018

<b>GENERAL COMMENTS</b>	A definite improvement over the original submission. I encourage the authors to continue this line of work and probe deeper to unpack some of the ideas presented, and how they might be better implemented into research funders' practice. Please correct the incomplete sentence in lines 223-4.
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<b>REVIEWER</b>	Eva Hummers Dept. of General Practice, University Medical Center Göttingen, Germany
<b>REVIEW RETURNED</b>	01-Nov-2018

<b>GENERAL COMMENTS</b>	Though not all of my earlier comments have been addressed, or could be addressed, most have, and the paper is considerably improved. I recommend to publish it now.
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