Author's response to reviews

Title: Why Technology Matters as Much as Science in Improving Healthcare

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Author's response to reviews: see over
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Dear Mr Aldcroft

Thank you very much for the opportunity to revise our article in light of the constructive and valuable comments and concerns expressed by the referees.

Dr Szcerba and I are delighted to take this opportunity, and have prepared a point by point response overleaf.

We trust that the accompanying revision has fully addressed the reviewers’ concerns and correctly incorporated the changes recommended and required by them.

Thank you again for the invitation to re-submit the revised article.

Sincerely,

Marco D. Huesch
Reviewer: Andrew Cook

I found this an interesting read, in an area - the translation of scientific knowledge into usable technologies of benefit to patients - which is an essential part of the development of health care, and should be of interest to every practicing clinician and to those who manage health services.

Thank you for your kind comments, and the opportunity to address your concerns below.

**Major Compulsory Revisions**

(1) The authors have a very USA focus- and consequently miss the significant body of government reports and research funders, which have recognised the various translational gaps in medical research - and the governments and organisations which have acted upon that recognition. Given the audience for this journal is international, I'd have expected to see some acknowledgement or consideration of how at least one other country has addressed these issues. The obvious reference for me would be the UK set up, influenced by the 2006 review by Cooksey (A review of UK health research funding - http://www.hm treasury.gov.uk/d/pbr06_cooksey_final_report_636.pdf ) which led to a significant reconfiguration of health research funding in the UK to address identified translational gaps. It's entirely possible that the UK approach would be inappropriate in the USA, but I'd expect to see the authors at least identify, and preferably consider, models which have already been tried to address the problem they identify. Add some consideration of how this problem has been tackled outside the USA, and some reflection on how those approaches may be applicable to the US context.

Thank you for this important comment. We agree entirely that this was a weakness of our original revision. In reading Sir David’s report in light of your comments, we were struck by how similar issues of translation were identified in the Cooksey Report. Indeed, Sir David’s diagnosis of an absence of overarching strategy to translate ideas from basic and clinical research into the development of new products and approaches to treatment of disease and illness, and the absence of a strategy to implement those new products and approaches into clinical practice, is very much the point we seek to make in this country. To a more limited extent the Report’s recommendations on prioritizing projects in view of potential downstream significance are also in line with our recommendation to insist on an implementation plan for all basic research, tying them more closely to applied research. To a large extent, the degree of non-market control of research by centralized decision-makers is arguably much stronger in an essentially nationalized delivery system with centralized cost and comparative effectiveness functions.

In our revision, we have added the UK as one exemplar of how another nation has wrestled with similar issues, and we reflect on the potential for similar solutions here in the USA. Due to our lack of familiarity with other healthcare systems, we are careful not to generalize our observations.

(2) Recognise that in almost all cases in this paper, consideration is limited to the
USA context, or explain how these US based observations and problems are of more general applicability.

We agree our focus is almost exclusively the USA – and remains so even after incorporation of the UK exemplar (per the point above). However, we are not convinced that we can argue that these US-based observations and problems are of much more general applicability. That is, the case of the USA is so specialized that other national health systems are generally unlikely to face the same setting characterized by the absence of a universal healthcare insurance system, serious weaknesses in primary and preventive care, serious obstacles to access. Compounding the limited generalizability is the leading role that US federally funded basic science research plays in stimulating global accumulation of scientific knowledge in medicine, pharmacy and the life sciences. We believe that the more general applicability of our US-based observations and recommendations lies in the potential positive spillover effects that applied research and US technology solutions could have globally, were they to be increasingly focused on, developed and implemented here. In the revision we have noted your concern and fleshed out the relatively specialized nature of the problem better.

(3) At the end of the first paragraph go the background there is an appeal to 'conventional wisdom'. Sitting outside the USA I can believe this statement, but would like to see a reputable reference, as 'conventional wisdom' is often incorrect - and is this statement is incorrect the need for the rest of the piece falls down. Add references or discussion to justify that this 'conventional wisdom' is correct - or at least correct enough to make the rest of this paper relevant.

Thank you for pointing out the concerns inherent in such an appeal. We agree completely. We had omitted the standard references for space, given the saliency of these issues here in the USA Following your recommendation we’ve fleshed out the issues more, and provided several recent references in support of each of the sub-points of cost, access, safety, quality and value.

(4) In the section 'Funding for Translational Research', the statement is made (and repeated in 'Increasing the application of scientific knowledge) that the absolute level of funding for technology development must be increased, but this is not evidenced. I’d like to see either some independent reference to support this, or the authors investigating other options (such as a redirection of current funds) and showing why they are not viable. The authors claim that $1Bn a year of public funding is available for translational research and complain about how it is focussed - they do not show why changing the focus is not a sufficient intervention. Add references or discussion to justify the requirement of more funds for translational research, and consider what the other options may be.

This is an interesting point. We had not really thought about whether we wanted the same percentage for translational research in a larger general funding pie, or a shift towards translational research funding in a constant total funding pie. To your point, it may well be that changing the focus suffices. On the other hand, for example, much of the existing change in focus such as the NIH’s very large Clinical and Translational Science Awards are essentially transformative efforts appropriate to the grantee’s
institution. Given most institutions strengths in basic science research, such research may still be far more closely linked to generation of the institution’s own new scientific knowledge than to application of existing research from wherever it originated. We admit that we are not certain what the best approach is, as long as the absolute dollar value of applied research and applications of existing knowledge increases. In this revision, we therefore present the other options and allow the reader and other scholars ad practitioners to consider their views on the best path forward.

**Discretionary Revisions**

(5) The argument about the performance landscape is a good way to portray the challenges in evolving vs designing a health care system, though I feel that the chosen reference - which discusses the evolution of RNA - is possibly carrying the analogy a bit too far. The authors may like to consider a reference to Dawkins' Climbing Mount Improbable (see http://en.wikipedia.org/wiki/Climbing_Mount_Improbable) which address the same concept in a more accessible way (In surprised the Pitt paper doesn't reference Dawkins which predates it by over 20 years). Consider whether the Dawkins reference may be more applicable than the Pitt reference for the audience likely to read this paper.

*Agreed, and replaced as per your recommendation.*

(6) In the illustrative example the authors suggest that health care system performance appears to have reached a plateau. I think they probably have the US system in mind when they say this, and in their further discussion of this phenomenon they miss the context that the US system sits in - e.g. the obesogenic US social environment, the market driven health system with poor access to care for much of the population etc. Many of the challenges facing the US health care system particularly are socially and politically driven rather than a defect in health technology. Consider exploring the limitations of how useful health technology development may be in the US health care context, given the social and political drivers and limitations.

*We agree with the weaknesses you identified in this example. We’ve added to this discussion appropriately, caveatting that only part of the solution lies in health technology. Thank you for raising this concern.*

**Reviewer: Kwan Hoong Ng**

**Minor Essential Revisions**

(1) Abstract “accumulation of knowledge (i.e. science)” : Science is also the generation and synthesis of knowledge.

*Agreed, and changed to reflect your point.*
(2) 2nd para “More generally, technology allows researchers and stakeholders to overcome the inherently complex, interacting and dynamic nature of healthcare systems.” Would it not rather be the appropriate and effective application of technology that allows....?
You are correct, we’d missed this important caveat. We’ve amended the sentence to reflect your point.

(3) 3rd para “To view this conceptually, consider a system’s performance as measured in vertical height above some rugged ‘performance landscape’.” Explain this clearly. Thanks for the opportunity to expand on this point. We’ve added more detail on how to visualize, say, the impact of two factors (say, in the x-y plane) on performance (the z dimension, vertically up) and the highly variable surface that arises due to the non-linearity of the two factors’ relationship with performance. With sufficient non-linearity, the performance surface has many peaks, valleys and ridges. Visually, this surface helps to make the point that local search is not optimal, as local improvement can lead to getting stuck on a local ‘hill’, but missing the opportunity to reach an unobserved higher performance peak representing very different combinations of factors.

(4) 4th para “Mathematically, local optimization is straightforward and requires only the plausibly causal interpretation of a significant coefficient in a regression model.” Explain this in simpler language. causal interpretation? significant coefficient? local optimization?
Agreed, and done. We’ve substantially added to the discussion here, and attempted to use simpler language.

(5) An illustrative example
1st para “On the other hand, a holistic simulation approach, using real-time data, could allow for safe experiential learning and experimentation, and thus significant improvements in the quality of such intensive care.” This is a good point. I would also argue for a multi- and trans-disciplinary approach to solving various global health issues.
Thank you for this suggestion, which we’ve implemented in the revision.

Discretionary Revisions
The title: “Why Technology Matters as Much as Science in Improving Care”, suggest changing it to “Why Technology Matters as Much as Science in Improving Health Care”.

We’ve made this change as per your suggestion to clarify the scope and content.