



The importance of clinical research

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SUMMARY

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'Anaesthesia, Pain & Intensive Care' has shown interest in publishing a series of manuscripts highlighting different aspects of undertaking research and then getting the results published for wider dissemination of the useful knowledge and information derived out of it. This editorial seeks to elaborate on some of the problems we face in the clinical research community in such a way as to help aspiring clinical researchers know where to direct their effort. Subsequent editorials in this series will describe the process of planning a research project as well as how to write about one's findings. In this vein, the true importance of clinical research will be elucidated.

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This editorial needs a preface, its title seems to imply that a discussion on why clinical research is important is going to take place and it should be clarified that no such discussion will occur. I believe that every reader of this journal will already believe that clinical research is important, so further convincing them of this will have no real effect and no real impact. Just like the articles that this journal publishes, this editorial, too, seeks to have an impact. Its goal is to convince more clinicians to engage in clinical research.

Given our assumed shared belief in clinical research as a worthwhile pursuit, we can hypothesize that there exists some barrier that prevents more clinicians from engaging in research. My belief is that this barrier normally occupies one of three forms. The first form would be a perceived lack of time to devote to research, for this, I can only quip that a lack of time is true for all things one could do and that the only time we ever have time for something is when we make time for it. The second form is one of uncertainty, where the lack of clarity in how to help the research community has led to the clinician's interest stalling out. The third form originates from the acknowledgement that performing clinical research is its own area of mastery and a craft that requires many skills, and this daunting perception has left the clinician unable

to make the required leap of faith into the arena of clinical research.

This editorial is set to be part of a series. Today, we will discuss what type of research is needed and try to add clarity to our shared goal. The remaining editorials of this series will be devoted to the planning, performing and writing of a research project. To start off our discussion, I'll share some words of T. S. Eliot that I have appropriated from his play, "The Rock":¹

Where is the Life we have lost in living?

Where is the wisdom we have lost in knowledge?

Where is the knowledge we have lost in information?

We live in a world where our access to information is greater than it ever has been before. And yet, even with all of this information, we know that there is still so much left for us to learn. What we need are clinicians that can take the information that is before them and interpret it within its context to produce new knowledge and other clinicians that can then take that knowledge and distill it into wisdom that can be more easily passed on. Today, I'll highlight three areas where we can improve upon our ability to process the information available to us into wisdom. It is my hope that you, my reader, will take interest in one of these problems and commit to furthering humanity's wisdom.

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The first problem relates to medical decision making being based on physician preference instead of evidence-based medicine. Now, in many cases, there are no suitable evidence-based guidelines and the only thing a physician can rely on is their own judgment and we affectionately refer to this as the “Art of Medicine.” But art can be messy and our patients are not canvases, they are the people that have placed their faith in us, a faith founded on the belief that we know what we are doing. It is impossible to know everything, but when we recognize that we don’t know something, then we owe it to our patients to start to correct that deficit. I should note that I appreciate physician preference and that I see it as a valuable source of information. An experienced local physician will often have a feeling for patterns in their patient population that a nationally-instituted guideline can’t compete with. Taking all of this into account, our true goal should be to ensure that the physician’s preference is evidence-based, as opposed to anecdote-based, and we can realize this goal through the effort of local clinicians that test, keep track of and report on the outcomes of the various local physician-preference treatment regimens. This data could then be used for the foundation of an ethically-approved randomized controlled trial.

The second problem relates to how slowly we can be to adopt advances in patient care. In many ways, our reticence for change in the medical field is understandable, given the amount snake oil cure-alls that have sought our approval. But in other ways, this same inability to change has brought harm to many of our profession’s patients. In 1747, Dr. James Lind discovered the cure for Scurvy, and 48 years later, the British Navy adopted that cure by including Lime juice in the sailors’ rations.² So for essentially five decades, sailors were dying from a completely preventable cause and, as it turns out, part of the reason for these deaths was the medical community’s disbelief that a disease as debilitating as Scurvy could be cured by something as simple as a lime. Nowadays, the time between discovery and clinical benefit is significantly shorter, in fact it’s frequently quoted as only being 17 years... That’s right, 17... years...³ Now that’s actually a difficult number to verify, for all the reasons discussed by Morris et al,³ but it’s the best estimate we’ve got. Regardless, 17 years is still too long. Moving forward, improving upon this situation is going to require the effort of people from many industries, the part I see for most of us in this will rest in our profession remaining open to well-

documented new ideas in health care, with additional care taken to ensure that we don’t stonewall ideas that conflict with that which we want to be true.

The third problem is our oversimplification of statistical significance and how we imply clinical significance from it. There is a greater truth found in statistical significance because there are robust objective methods used to determine it. Our perception is that statistical significance gives our studies validity, and that’s true, just not in as absolute of a sense as we’d like to think. What the p-value really tells us is how certain we can be that our findings are not due to random chance. The p-value does not say that our findings are important or otherwise meaningful. Instead, this determination of clinical significance is left to the clinician who will normally rely on their own expert opinion regarding how to categorize the result. At face value, this doesn’t seem bad, but this method introduces into our research a hidden reliance on what is inherently a subjective determination and this subjectivity enables bias to creep into our research findings. To mitigate this, we have to first change how we perceive the notions of statistical and clinical significance. Furthermore, we must maximize our usage of objective means to determine clinical significance using methods like the Number Needed to Treat or the Cohen’s Effect Size and we must demand to see these values in our literature, just as we do for the p-value. That said, when we consider a clinical significance in a more holistic sense, issues such as cost and adverse effects come into play and these issues are harder to handle in a systematic and objective manner, as such, some amount of subjective analysis will still occur. Nevertheless, our research’s potential for bias will be greatly reduced.

As I begin to conclude today’s editorial, the words of T.S. Eliot seem apt once again:¹

*The lot of man is ceaseless labour,
Or ceaseless idleness, which is still harder,
Or irregular labour, which is not pleasant.
I have trodden the winepress alone, and I know
That it is hard to be really useful, resigning
The things that men count for happiness, seeking
The good deeds that lead to obscurity, accepting
With equal face those that bring ignominy,
The applause of all or the love of none.
All men are ready to invest their money
But most expect dividends.
I say to you: Make perfect your will.*

*I say: take no thought of the harvest,
But only of proper sowing.*

There are many ideas regarding research that I wish to convey to you with this appropriated poetry, but I will settle for highlighting just the one with the most

importance, that is, to highlight the importance of “proper sowing.” Indeed, I want to reaffirm with you that the true importance of clinical research is found in going about it properly.

Conflict of interest: Nil declared by the author.

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I assert that the cosmic religious experience is the strongest and the noblest driving force behind scientific research. *Albert Einstein*

I believe in intuition and inspiration. Imagination is more important than knowledge. For knowledge is limited, whereas imagination embraces the entire world, stimulating progress, giving birth to evolution. It is, strictly speaking, a real factor in scientific research. *Albert Einstein*

It is not the fruits of scientific research that elevate man and enrich his nature but the urge to understand, the intellectual work, creative or receptive. *Albert Einstein*