

The case report: Level 5 evidence, Level 1 medicine

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The case report as a journal manuscript is becoming less popular and many medical journals have chosen to cease publishing them.¹ This is most likely a consequence of the evidence-based medicine (EBM) movement, and while I do not intend to discourage the use of EBM, I would argue that the loss of case reports among many peer-reviewed medical journals is detrimental to medical scholarship and practice. So what is the place of the case report in the modern health care system?

Case reports do not deserve the poor reputation they have received in recent years. Rather than being seen as weak evidence, they should be viewed as strong lessons in education and cognitive development of professionalism. Rather than being seen as Level 5 at the bottom of the ladder, they should be viewed as Level 1 on a different ladder. They are complementary rather than competitive. This argument resonates closely with the argument between quantitative and qualitative methods, which I will return to later.

Case reports are often the first piece of academic writing that doctors will attempt, and having a peer-reviewed article published in a medical journal is a significant stepping stone in the career of many doctors. Aside from the pride that many feel about their first publication, it also allows them to develop the art of academic writing, which is seldom part of the medical school curriculum.² Case report writing requires a certain talent. The author needs to make the manuscript precise, concise, relevant and interesting. It can take several drafts and it is this revision of drafts and reflection on the previous content that allows doctors to develop their argument. The characteristics of a good case report make excellent attributes when instilled in the developing clinician. To limit the publication of case reports in medical journals is to stifle the professional development of many junior clinicians, and potentially the future researchers of the medical community.

What is interesting for doctors is usually not good for patients. It's an old phrase but one that rings true. Interesting cases usually have uncommon elements, are usually less researched and therefore less well known. Treatment may not be so established, which can lead to uncertainty for both doctors and patients. However as clinicians we do find rare cases interesting, and why would we not? The television series *House MD* is based on *The New York Times* column "Diagnosis", which discusses rare cases that baffled medical teams until a diagnosis was finally made. While the medical teams in "Diagnosis" did

not exhume the neighbours dead cat to make a diagnosis, they did struggle with an array of symptoms and signs they were unable to connect, until they finally made the breakthrough diagnosis.

A recent case of a patient at my own institution, who was diagnosed with placental abruption due to thyroid storm, was notable for the number of times a senior clinician commented "That's odd". Her presentation on admission, looking unwell with severe tachypnoea, clear chest x-ray and normal pH; her hypertension and arrhythmia on induction of anaesthesia; her placental abruption with minimal blood loss; and her sudden fever and cardiovascular decompensation postoperatively with an echocardiogram showing minimal cardiac contractility were all described as "odd". But it is when things are "odd" that we need to be cognisant of this fact, to appreciate that we do not know the answer and that we need to think again. The danger here is confirmation bias,³ where a clinician starts a process of cognitive cherry-picking to make the signs fit the diagnosis, rather than let the signs lead to the diagnosis. Most clinicians can recall an instance where they failed to think again and missed a diagnosis, possibly because they had momentum bias³ and were fixed to their initial diagnosis. While experienced clinicians use heuristic thinking to arrive at a diagnosis that the junior staff have struggled to see, they also use it to come to the conclusion that the pattern in front of them does not make sense. In other words, they avoid attribution and momentum bias, go back to the start and think again. And this process is a valuable process for the junior clinician to develop. Case reports are a timely reminder that rare diseases do present and, like *House MD*, somebody has to diagnose them.

So what is the argument against case reports? Jerome Hoffman argues that case reports take one of three forms: a new or innovative treatment or approach to particular disease, a rare condition, or a very unusual manifestation of a common problem; and that none of these forms are beneficial to the clinician.⁴ I shall sequentially consider the three forms of his argument.

Hoffman's first argument is that the description of a new or innovative treatment cannot be extrapolated to treatment based on $N=1$. However, such descriptions are hypotheses-generating. When Michael Rubenstein used plasmapheresis for a young patient with thrombotic thrombocytopenic purpura (TTP) in 1959,⁵ his approach was considered radical and dangerous, and his case report was criticised severely. Many years later, plasma exchange

became the standard treatment for TTP. The counterargument that most theories postulated in case reports do not lead to useful intervention is a weak one, since many randomised controlled trials (RCTs) lead to negative studies. However, many negative studies lead to a change in practice by finally laying to rest perpetuated dogma, the ANZICS (Australian and New Zealand Intensive Care Society) dopamine study being an example.⁶

Hoffman's second argument is that the description of a rare condition is limited in its utility because the low numbers of subjects do not allow it to be studied adequately, and it is therefore unlikely that many clinicians will ever be exposed to the condition in their careers. However, case reports arise from different sources discussing the same phenomenon, which leads to a rethinking of the original theory and understanding. Takotsubo cardiomyopathy was first reported in the 1990s: a reversible cardiomyopathy precipitated by acute emotion stress in postmenopausal women.⁷ However, it became apparent that the descriptive echocardiographic pattern of "apical ballooning" occurred in other demographics, and then it became clear that it occurred in different clinical states, and there were other echocardiographic descriptions. We now view it as stress-induced cardiomyopathy. This has not been established by specific clinical trials, but by clinical observations and case reports.

Hoffman's third argument is that the description of a rare manifestation of a common problem is more likely to do harm than good, since it is most likely that the description is incorrect or that it is a once-in-a-lifetime occurrence. This is not a third reason, but an extension of the second reason; since it is an argument that suggests documenting rarities is dangerous, whether it is actual rare disease or rare manifestations of common disease. Another counterargument to this is that rare manifestations can often become observations that produced benefit. The observation during Phase I clinical trials that sildenafil had no effect on hypertension or angina, but caused a marked increase in penile erections, subsequently led to sildenafil becoming the mainstay medical treatment in male sexual dysfunction.⁸

This latter argument also implies that case reports represent the bizarre and the rare, whereas what they actually represent is recognising the unexpected.⁹ Recognising the unexpected is analogous with inductive thinking and scientific discovery, and this context enters the realm of many other scientific arguments — inductive versus hypothetico-deductive thinking, the context of discovery versus the context of justification, qualitative methods versus quantitative methods. And whereas these arguments can divide advocates of each team into bitterly opposed sides, there are merits of both arguments.

In his sentinel work *The structure of scientific revolutions*,¹⁰ Thomas Kuhn plays down the distinction between the context of discovery and the context of justification. Kuhn's theory brings discovery and justification closer together as he discusses the pattern of scientific change. The historical development of a mature science shows a pattern: normal science, crisis, extraordinary science, new phase of normal science, etc. Normal science is conservative, with scientists building on current ideas rather than questioning them. Normal science is solving the puzzles raised by the current paradigm. Unsolvable puzzles are classed as anomalies. Crisis occurs when anomalies mount up, and cause scientists to question the current thinking. Extraordinary science is revolutionary; a significant component of the existing tradition is replaced, and the revised practice solves many of the previous anomalies and provides a platform for future research. Many case reports personify this tradition of scientific discovery that should not be dismissed because it was not conducted in the manner of an RCT.

My argument is that EBM and case reports should exist together and be appreciated for their different but significant contributions to the medical literature, just as qualitative and quantitative methods should also be seen as complementary. The argument between qualitative and quantitative methods is tiresome, and the arguments can be countered. The argument that quantitative methods discover the truth is overinterpreted, since quantitative methods are based on the principles of postpositivism, which states that there is a truth, but it can be known only imperfectly and probabilistically. The argument that qualitative methods find a deeper truth is also misinterpreted, since qualitative methods answer different questions to quantitative.

Just as there is no reason to accept poor RCTs, there is equally no reason to accept poor case reporting. A good case report should deliver a clear message to the reader and make interesting reading. It should make practitioners critically reflect on their own practice, and promote personal internal discussion about how they can use this evidence or argument in their professional development. As for being a poor form of evidence, it may be seen as the weakest or lowest form of evidence, but in the world of medicine and especially intensive care medicine, it remains the first line of evidence.¹¹

The case report is truly struggling to find its niche in modern medical practice, but its place has been long established. Science and medicine are continually shifting paradigms, and there is even a suggestion that we should move away from EBM to personalised medicine.¹² However, personalised medicine is simply EBM being interpreted and used correctly. Personalised medicine is all about being

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astute and contextualised, it is about good professionalism. And it is these qualities that sit hand-in-hand with good case reporting. The case report is far from dead, it is awaiting its appropriate revival.

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Competing interests

None declared.

References

- 1 Elsevier. *Resuscitation*: guide for authors. <http://www.elsevier.com/journals/resuscitation/0300-9572/guide-for-authors> (accessed Apr 2014).
- 2 McNeill A, Parkin CK, Rubab U. Using a case report to teach junior doctors about medical publishing. *Med Teach* 2007; 29: 511.
- 3 Groopman J. How doctors think. Boston: Houghton Mifflin, 2007.
- 4 Hoffman JR. Rethinking case reports. *West J Med* 1999; 170: 253-4.
- 5 Rubinstein MA, Kagan BM, Macgillviray MH, et al. Unusual remission in a case of thrombotic thrombocytopenic purpura syndrome following fresh blood exchange transfusions. *Ann Intern Med* 1959; 51: 1409-19.
- 6 Bellomo R, Chapman M, Finfer S, et al; Australian and New Zealand Intensive Care Society Clinical Trials Group. Low-dose dopamine in patients with early renal dysfunction: a placebo-controlled randomised trial. *Lancet* 2000; 356: 2139-43.
- 7 Tsuchihashi K, Ueshima K, Uchida T, et al; Angina Pectoris-Myocardial Infarction Investigations in Japan. Transient left ventricular apical ballooning without coronary artery stenosis: a novel heart syndrome mimicking acute myocardial infarction. *J Am Coll Cardiol* 2001; 38: 11-8.
- 8 Boolell M, Allen MJ, Ballard SA, et al. Sildenafil: an orally active type 5 cyclic GMP-specific phosphodiesterase inhibitor for the treatment of penile erectile dysfunction. *Int J Impot Res* 1996; 8: 47-52.
- 9 Vandenbroucke JP. In defense of case reports and case series. *Ann Intern Med* 2001; 134: 330-4.
- 10 Kuhn T. The structure of scientific revolutions. Chicago: University of Chicago Press, 1962.
- 11 Jenicek M. Clinical case reporting in evidence-based medicine. Oxford: Butterworth-Heinemann, 1997.
- 12 Berezcki D. Personalized medicine: a competitor or an upgrade of evidence-based medicine? *Future Medicine* 2012; 9: 211-21. □