



## Editorial

## In search of the “Holy Grail”: Will we ever prove the efficacy of Rapid Response Systems (RRS)?



It is hard to think of an intervention in the last two decades that has been implemented in such a widespread fashion, and so effectively, but with so little evidence as Rapid Response Systems.<sup>1</sup> No one from an administrative, clinical or research perspective can question the underlying assumptions that underpin RRS utilization. A plethora of research articles in the 1990s and 2000s have described the risk factors for unexpected patient clinical deterioration, that eventually results in either in-hospital cardiac arrest (IHCA), unplanned Intensive Care Unit (ICU) admission from the general ward or unexpected hospital mortality.<sup>2–10</sup> The executive summary of this basic epidemiological work is that such patients do not on the whole suddenly deteriorate, arrest, die, or if they are lucky get an ICU admission with an associated high mortality rate; rather they deteriorate slowly over hours and even days, with the clinical deterioration beautifully documented in the observation chart and the written medical record. The obvious solution is to intervene in some way during this patient deterioration, with appropriate, and timely, diagnosis, treatment and if necessary, resuscitation.<sup>6,11,12</sup> Alternatively an earlier decision about the appropriateness of resuscitation can be made with the patient and palliative options considered.

Based on this logic, the case for the RRS is overwhelming. However, demonstrating this in practice has been more difficult. In the early to mid-2000s a number of single centre historically controlled studies, demonstrated quite significant benefit with the introduction of RRS with up to a 50% reduction IHCA.<sup>13–16</sup> However, there have been only two prospective randomised studies that have drawn data from more than a single hospital.<sup>17,18</sup> There is only the cluster-randomised work of the MERIT group that has extended this type of analysis to more than three sites.<sup>17</sup> Neither of these studies could demonstrate superiority of the RRS with respect to the traditional endpoints of IHCA arrest, unplanned ICU admission from the ward, or unexpected hospital mortality as defined as being a death that occurred with no “do not resuscitate” order in place. The MERIT study, despite being ambitious in its scope, was underpowered and suffered from intervention contamination in both the control and intervention hospitals. However, not deterred, by the lack of superiority of this study, the MERIT investigators performed several post-hoc analysis of the data to demonstrate that the incidence of and timing of the emergency call to a RRS were what really mattered, not the randomised allocation to the RRS intervention or control system.<sup>19,20</sup>

With that background, we come to yet another piece from this group, in their further pursuit of the answer to the efficacy of RRS's.<sup>21</sup> In this study, with Chen again as first author they compared rates of IHCA, IHCA in-patient mortality, hospital mortality and one-year post-ICHA mortality between four metropolitan Sydney teaching hospitals: one hospital with a long-established RRS and three other hospitals with newly instituted RRS. Using multinomial logistic regression, they analysed coding data from over 1.5 million patient separations, collected from 2002 to 2009. Key findings were that IHCA and hospital mortality declined at all three sites without established RRS over the eight year study period, but that this decline was most marked in the twelve months following implementation of each hospital's RRS. There was no reduction in one year post-discharge mortality. Throughout the study, the hospital with an established RRS exhibited a lower incidence of IHCA and lower all-cause mortality. A significant advantage of this study is that it involved multiple hospitals. Moreover, data from each site were gathered concurrently, thus partly addressing one limitation inherent in the single-centre before-and-after study design that predominates in this field; that is, difficulty separating improvements in mortality attributable to RRS from improvements achieved through other local interventions implemented within the study period. A further strength of this study is that the authors have taken care to minimise reporting bias. By using overall hospital mortality as an outcome, rather than simply ICHA incidence or ICHA-mortality, the authors forestall another criticism levelled at a number of previous RRS studies; namely, that rapid response teams increase the frequency with which do-not resuscitate orders are documented and also trigger transfer of patients from ward environments to the ICU, thereby introducing reporting bias and overestimating the effect of RRS. One potential limitation of this study is the limited period of data collection following implementation of RRS in the three ‘non-RRS’ hospitals. It appears that each of these hospitals had experienced periods in which the rate of IHCA and all-cause mortality had declined sharply several years prior to implementation of an RRS.

The whole issue with proving RRS efficacy is quite simply that the RRS is not an intervention amenable to most traditional forms of analysis.<sup>22</sup> This is because the intervention is not clean. A RRS is very much a cultural system of change that is superimposed on a system of hospital care that is meant to be homogenous, but in fact has tremendous variation with respect to time, staff and geography.

Even within our own hospitals, we can recognise this variability, let alone across different hospitals in different settings. If we accept, this, it is maybe time to consider, that we will never have the perfect study that will prove RRS efficacy. Maybe instead, we need to go back to the basic epidemiological work, and reflect on the current insatiable desire for RRS, and ask the more basic question as to why do we need the RRS at all? Maybe we need to think about how and what, we are teaching our medical students and junior doctors about acute medicine, talking to patients, and practising competent clinical care.

### Conflict of interest

MB is a share holder and Clinical advisor to Patientrack. AM has no conflict of interest.

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