Rapid Response Teams

A Systematic Review and Meta-analysis

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Background: Although rapid response teams (RRTs) increasingly have been adopted by hospitals, their effectiveness in reducing hospital mortality remains uncertain. We conducted a meta-analysis to assess the effect of RRTs on reducing cardiopulmonary arrest and hospital mortality rates.

Methods: We conducted a systematic review of studies published from January 1, 1950, through November 31, 2008, using PubMed, EMBASE, Web of Knowledge, CINAHL, and all Evidence-Based Medicine Reviews. Randomized clinical trials and prospective studies of RRTs that reported data on changes in the primary outcome of hospital mortality or the secondary outcome of cardiopulmonary arrest cases were included.

Results: Eighteen studies from 17 publications (with 1 treated as 2 separate studies) were identified, involving nearly 1.3 million hospital admissions. Implementation of an RRT in adults was associated with a 33.8% reduction in rates of cardiopulmonary arrest outside the intensive care unit (ICU) (relative risk [RR], 0.66; 95% confidence interval [CI], 0.54-0.80) but was not associated with lower hospital mortality rates (RR, 0.96; 95% CI, 0.84-1.09). In children, implementation of an RRT was associated with a 37.7% reduction in rates of cardiopulmonary arrest outside the ICU (RR, 0.62; 95% CI, 0.46-0.84) and a 21.4% reduction in hospital mortality rates (RR, 0.79; 95% CI, 0.63-0.98). The pooled mortality estimate in children, however, was not robust to sensitivity analyses. Moreover, studies frequently found evidence that deaths were prevented out of proportion to reductions in cases of cardiopulmonary arrest, raising questions about mechanisms of improvement.

Conclusion: Although RRTs have broad appeal, robust evidence to support their effectiveness in reducing hospital mortality is lacking.

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clinically meaningful outcome of hospital mortality. Because a primary action of RRTs is to triage sick patients to the ICU, to demonstrate that these interventions not only reduce rates of intermediate outcomes (such as cardiopulmonary arrest outside the ICU) but also reduce hospital-wide mortality is critical before endorsing their widespread adoption. Finally, although meta-analyses of RRTs have been previously performed, they do not include a number of recent studies, have not examined the cumulative temporal trend on outcomes with inclusion of each additional study, and have not addressed the extent to which mortality benefits may be attributable to the interventions of the RRT.

Accordingly, we conducted an updated systematic review and meta-analysis to (1) assess the effect of RRT implementation in reduction of rates of cardiopulmonary arrest and hospital mortality, (2) examine the cumulative temporal trend on outcomes among published studies of RRTs, and (3) evaluate the degree to which observed mortality reductions are explained by lower rates of cardiopulmonary arrest.

**METHODS**

**DATA SOURCES AND SEARCH STRATEGY**

We conducted a systematic review of the literature for studies published from January 1, 1950, through November 30, 2008, by the use of PubMed, EMBASE, Web of Knowledge, CINAHL, and all Evidence-Based Medicine Reviews (which include the Cochrane Databases). The search was not restricted by language of articles and used both keywords and Medical Subject Headings in a Boolean search strategy (eAppendix available at http://www.archinternmed.com). In addition to these automated searches, we conducted a hand search of bibliographies of key articles and abstracts presented at major scientific conferences between 2006 and 2008, such as the annual meetings of the American Heart Association, the American College of Cardiology, the American College of Chest Physicians, and the American College of Emergency Physicians.

**STUDY INCLUSION CRITERIA AND STUDY OUTCOMES**

To be considered for inclusion in this meta-analysis, studies were required to meet 3 inclusion criteria: (1) be randomized clinical trials or prospective active intervention studies of RRTs among hospital inpatients, (2) perform comparisons with a control group or a control period, and (3) provide sufficient quantitative data on either the primary outcome of hospital-wide mortality or the secondary outcome of rates of non-ICU-treated cardiopulmonary arrest. On the basis of the initial search results, 532 titles and abstracts were independently reviewed by at least 2 of the authors (P.S.C., R.J., and/or C.S.), and 389 were immediately excluded. Two reviewers (P.S.C. and R.J.) subsequently reviewed the full text articles of the remaining 143 publications for study inclusion. Agreement between the reviewers for study eligibility was high (weighted κ = 0.91), and disagreements were resolved by discussion. The corresponding authors of 8 studies were contacted by electronic correspondence on at least 3 occasions during a 4-week period (December 2008) to provide additional data on study outcomes to allow for meta-analysis; 5 did not respond and were excluded. Additional reasons for study exclusion were nonintervention study (n = 51), lack of a control group (n = 22), lack of evaluation of a prognostic outcome (n = 20) or any of the outcomes of this study (n = 22), and duplicate studies (n = 6) (Figure 1).

**DATA EXTRACTION**

Data extraction was independently performed by 2 reviewers (P.S.C. and C.S.) by the use of a standardized form. From each study, the following variables were abstracted: study outcomes (rates of cardiopulmonary arrest and hospital mortality in control and intervention periods); age group (pediatric or adult); type of study (randomized trial, interrupted time series design with adjustment for preintervention trends, or observational studies with no adjustment for preintervention trends); year of publication; number of sites; academic status of hospitals; duration of control, educational rollout, and intervention periods; sample sizes of control and intervention groups; control for confounders (eg, demographics or case mix); control for preintervention time trends; and frequency of activations per 1000 admissions. We also collected information as to whether a study included or excluded patients with do not resuscitate (DNR) status in its recording of outcomes of hospital mortality. Authors were contacted to clarify study definitions of mortality when it was not clearly defined. Finally, if several estimates for study outcomes were reported, the most fully adjusted estimate was chosen.

**CRITERIA USED TO ASSESS QUALITY**

Studies that met our inclusion criteria were further evaluated for quality. Studies were considered high quality if they adjusted for confounding (age, sex, ethnicity, and case mix) between the control and intervention periods and for time trends by the use of either contemporaneous control groups (eg, randomized clinical trials and observational studies with concurrent controls) or an
interrupted time series design with at least 3 data time points before and after intervention. Studies that adjusted only for confounding but not for time trends were categorized as fair quality, whereas studies that did not adjust for either were categorized as low quality.

DATA SYNTHESIS
Outcome data for hospital mortality and non-ICU–treated cardiopulmonary arrest were summarized by the use of basic descriptive statistics (simple counts and proportions). Because of differences in the origin and incidence of cardiopulmonary arrest between adults and children, we stratified our quantitative analyses by the study population. Meta-analyses for each outcome were conducted by means of a random-effects model. Between-study heterogeneity was evaluated with the I² statistic, and publication bias was evaluated by means of the Begg test. We also examined the cumulative influence of each study on the pooled estimate over calendar time. Finally, we performed sensitivity analyses to examine the influence of each study on the overall pooled estimate by omission of each estimate at a time.

Meta-regression was conducted to explore the heterogeneity in risk ratios between studies based on predefined study criteria, which included differences in the definition of study outcome (eg, included or excluded patients with DNR status), study quality, and the frequency of RRT use (≥10 vs ≤10 activations per 1000 admissions). Significant variables identified in meta-regression were further explored with subgroup analyses.

To examine the mechanistic plausibility of mortality gains with RRT implementation, we evaluated the extent to which lower hospital mortality rates were fully attributable to reductions in cardiopulmonary arrest by the RRT intervention. We accomplished this by examination of whether the number of codes averted exceeded the number of deaths prevented among those studies that reported a significant reduction in hospital mortality. For those studies that also reported case-fatality rates for cardiopulmonary arrest before and after RRT intervention, we further determined whether the number of expected deaths prevented (derived from reductions in and improved survival from cardiopulmonary arrest) was comparable to the actual reported number of fewer deaths after RRT intervention.

All statistical tests were 2-sided and were evaluated at a significance level of .05. We used STATA statistical software, version 10.0 (StataCorp, College Station, Texas) to conduct all analyses.

RESULTS
SEARCH RESULTS
A total of 17 articles (of which one study was treated as 2 unique studies) met our inclusion criteria (Figure 1). Original data from 1 study were obtained from study authors to calculate event rates and risk ratios. For a second study, an intervention hospital was separately compared with 2 control hospitals (hence its treatment as 2 distinct studies). A third study reported outcomes separately for medical and surgical patients, and we combined event rates from both groups for this review.

SYSTEMATIC REVIEW OF RRTs
Description of Studies
The RRT studies had a total sample size of 1 271 864 admissions (580 776 during the intervention period). All RRT studies were published during or after 2000, with 9 (50.0%) published since 2007 (Table 1). A randomized trial design was used in 2 studies (11.1%). Fourteen studies were single-institution studies, 3 involved 2 hospitals, and 1 involved 23 hospitals. Evaluation was performed among adults in 13 studies and among children in 5 studies.

Physicians participated in the RRT assessment of patients in 13 (81.3%) of the 16 studies that reported on team composition. Activation criteria, when reported, were similar across studies. Use of the RRT varied substantially across hospitals, with a median of 15.1 activations (range, 2.5-40.3) per 1000 admissions in adult studies and a median of 7.5 activations (range, 2.8-12.8) per 1000 admissions in pediatric studies.

Quality Assessment and Definition of Outcomes
Of the 18 RRT studies, 6 studies (5 adult and 1 pediatric) were categorized as high quality. These studies included 2 randomized clinical trials, 2 observational studies with contemporaneous control populations, and 2 observational studies that adjusted for preintervention trends.

Sixteen RRT studies reported on rates of cardiopulmonary arrest. Seven studies defined cardiopulmonary arrest as both respiratory and cardiac arrest, another defined cardiopulmonary arrest as either respiratory or cardiac arrest, whereas 1 study included all cardiac arrest calls. Fifteen RRT studies reported on hospital mortality. Ten studies included patients with DNR status in their mortality outcome, whereas 3 studies excluded patients with DNR status. One study did not define mortality or respond to queries for additional information.

META-ANALYSIS OF RRT STUDIES
Cardiopulmonary Arrest
In adults, 7 studies showed a significant reduction in rates of cardiopulmonary arrest, whereas 4 studies did not. Collectively, implementation of an RRT in adults was associated with a 33.8% reduction in rates of non-ICU–treated cardiopulmonary arrest (pooled relative risk [RR], 0.66; 95% confidence interval [CI], 0.54-0.80) (Figure 2). No evidence of publication bias was seen (P = .30 for the Begg test). In subgroup analyses, adult studies identified as high quality reported a more modest 21.1% reduction in rates of non-ICU–treated cardiopulmonary arrest compared with a 47.8% reduction in other studies (Figure 1).

Among the 5 pediatric RRT studies, 4 studies reported a significant reduction in rates of cardiopulmonary arrest outside the ICU. Pooled analyses found that implementation of an RRT was associated with a 37.7% reduction in rates of non-ICU–treated cardiopulmonary arrest (Figure 2) and was robust to subgroup analyses (Figure 1).
Two studies reported a significance, 1 study reported a decreased significant reduction in hospital mortality. Taken together, these studies showed no overall effect on hospital mortality (pooled RR, 0.96; 95% CI, 0.84–1.09).

The cumulative effect of each additional study over calendar time on the pooled mortality estimate in adults is shown in Figure 4. Although the pooled estimate from initial studies suggested a benefit, with the inclusion of recent studies, the cumulative pooled estimate has trended toward the null and was not associated with lower mortality rates. Collectively, there was no evidence of publication bias (P = .92 for the Begg test). Moreover, systematic omission of studies one at

**Hospital Mortality**

Significant heterogeneity was found among the 11 adult studies that examined the effect of RRT implementation on hospital mortality ($P = .92$). Two studies reported a significant reduction in hospital mortality, 1 study reported a decreased trend, 6 studies reported no effect, 1 study reported a trend toward increased mortality, and 1 study reported an increase in hospital mortality. Taken together, these studies showed no overall effect on hospital mortality (pooled RR, 0.96; 95% CI, 0.84–1.09).

The cumulative effect of each additional study over calendar time on the pooled mortality estimate in adults is shown in Figure 4. Although the pooled estimate from initial studies suggested a benefit, with the inclusion of recent studies, the cumulative pooled estimate has trended toward the null and was not associated with lower mortality rates. Collectively, there was no evidence of publication bias ($P = .92$ for the Begg test). Moreover, systematic omission of studies one at
### Figure 2. Pooled relative risks (RRs) of cardiopulmonary arrest outside the intensive care unit for adults and children after rapid response team (RRT) implementation. CI indicates confidence interval. ∗Number owing to rounding error for each of the individual pediatric studies.

<table>
<thead>
<tr>
<th>Study</th>
<th>RR (95% CI)</th>
<th>Weight, %</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control Group</td>
<td>Intervenion Group</td>
<td></td>
</tr>
<tr>
<td>Bristow et al (hospital 1 vs 2)</td>
<td>0.88 (0.62-1.23)</td>
<td>1.00 (0.73-1.37)</td>
</tr>
<tr>
<td>Bristow et al (hospital 1 vs 3)</td>
<td>0.50 (0.35-0.73)</td>
<td>0.52 (0.32-0.85)</td>
</tr>
<tr>
<td>Buist et al</td>
<td>0.92 (0.72-1.17)</td>
<td>1.03 (0.84-1.24)</td>
</tr>
<tr>
<td>Bellomo et al</td>
<td>0.99 (0.71-1.37)</td>
<td>1.03 (0.84-1.24)</td>
</tr>
<tr>
<td>Kenward et al</td>
<td>0.81 (0.71-0.93)</td>
<td>0.91 (0.71-1.16)</td>
</tr>
<tr>
<td>DeVita et al</td>
<td>0.70 (0.50-1.00)</td>
<td>0.70 (0.50-1.00)</td>
</tr>
<tr>
<td>Hillman et al</td>
<td>0.54 (0.35-0.84)</td>
<td>0.54 (0.35-0.84)</td>
</tr>
<tr>
<td>Jones et al</td>
<td>0.60 (0.50-0.76)</td>
<td>0.60 (0.50-0.76)</td>
</tr>
<tr>
<td>Dacey et al</td>
<td>0.91 (0.72-1.18)</td>
<td>0.91 (0.72-1.18)</td>
</tr>
<tr>
<td>Baxter et al</td>
<td>0.62 (0.50-0.80)</td>
<td>0.62 (0.50-0.80)</td>
</tr>
<tr>
<td>Chan et al</td>
<td>0.59 (0.40-0.88)</td>
<td>0.59 (0.40-0.88)</td>
</tr>
<tr>
<td>Overall Adult (I²=80.5%, P&lt;.001)</td>
<td>0.65 (0.54-0.80)</td>
<td>0.65 (0.54-0.80)</td>
</tr>
</tbody>
</table>

### Figure 3. Pooled relative risks (RRs) of hospital mortality for adults and children after rapid response team (RRT) implementation. CI indicates confidence interval.

<table>
<thead>
<tr>
<th>Study</th>
<th>RR (95% CI)</th>
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</thead>
<tbody>
<tr>
<td>Control Group</td>
<td>Intervenion Group</td>
<td></td>
</tr>
<tr>
<td>Bristow et al (hospital 1 vs 2)</td>
<td>0.93 (0.77-1.12)</td>
<td>0.93 (0.77-1.12)</td>
</tr>
<tr>
<td>Bristow et al (hospital 1 vs 3)</td>
<td>0.72 (0.60-0.87)</td>
<td>0.72 (0.60-0.87)</td>
</tr>
<tr>
<td>Buist et al</td>
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<td>0.70 (0.50-1.00)</td>
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<tr>
<td>Bellomo et al</td>
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</tr>
<tr>
<td>Kenward et al</td>
<td>0.90 (0.71-1.13)</td>
<td>0.90 (0.71-1.13)</td>
</tr>
<tr>
<td>Priestley et al</td>
<td>0.94 (0.75-1.20)</td>
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</tr>
<tr>
<td>Hillman et al</td>
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<td>0.65 (0.50-0.85)</td>
</tr>
<tr>
<td>Jones et al</td>
<td>0.86 (0.71-1.05)</td>
<td>0.86 (0.71-1.05)</td>
</tr>
<tr>
<td>Baxter et al</td>
<td>0.96 (0.83-1.12)</td>
<td>0.96 (0.83-1.12)</td>
</tr>
<tr>
<td>Chan et al</td>
<td>0.95 (0.81-1.11)</td>
<td>0.95 (0.81-1.11)</td>
</tr>
<tr>
<td>Overall Adult (I²=91.4%, P&lt;.001)</td>
<td>0.90 (0.84-1.00)</td>
<td>0.90 (0.84-1.00)</td>
</tr>
</tbody>
</table>

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<th>Study</th>
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<tbody>
<tr>
<td>Control Group</td>
<td>Intervenion Group</td>
<td></td>
</tr>
<tr>
<td>Brilli et al</td>
<td>0.55 (0.00-2.10)</td>
<td>0.55 (0.00-2.10)</td>
</tr>
<tr>
<td>Sharek et al</td>
<td>0.70 (0.50-0.95)</td>
<td>0.70 (0.50-0.95)</td>
</tr>
<tr>
<td>Zenker et al</td>
<td>0.70 (0.50-0.95)</td>
<td>0.70 (0.50-0.95)</td>
</tr>
<tr>
<td>Tibballs and Kinney et al</td>
<td>0.90 (0.70-1.15)</td>
<td>0.90 (0.70-1.15)</td>
</tr>
<tr>
<td>Overall Pediatric (I²=66.0%, P=.03)</td>
<td>0.70 (0.50-0.95)</td>
<td>0.70 (0.50-0.95)</td>
</tr>
</tbody>
</table>

| Overall (I²=90.3%, P<.001)               | 0.90 (0.82-1.04) | 0.90 (0.82-1.04) |
a time revealed no study whose removal would have meaningfully changed the pooled mortality estimate. Finally, pooled mortality estimates were not different between studies characterized as high vs low quality, studies with high vs low rates of RRT activation, or studies that excluded or included patients with a DNR designation (eFigure 2).

Among the 4 pediatric RRT studies, 2 studies reported a significant reduction in mortality, whereas 2 others found no effect. Implementation of an RRT was associated with lower hospital mortality rates in pediatric patients (pooled RR, 0.79; 95% CI, 0.63-0.98) (Figure 3), but significant heterogeneity was observed (I2 = 66.6%, P = .03). Importantly, the pooled mortality estimate was sensitive to omission of any 3 of the 4 individual studies (ie, would result in no association with hospital mortality when omitted), which suggests that the association of RRT implementation with lower mortality rates in pediatric hospitals was not robust (eFigure 3). Lastly, when data from adult and pediatric studies were aggregated, implementation of RRTs was not associated with lower hospital mortality rates (pooled RR, 0.92; 95% CI, 0.82-1.04) (Figure 3).

Mechanistic Plausibility

For the 5 studies that reported lower mortality rates after implementation of an RRT, we examined the extent to which this was owing to the interventions of the RRT with regard to cardiopulmonary arrest. One study did not report on rates of cardiopulmonary arrest. In the 4 remaining studies, the number of fewer deaths observed after RRT implementation exceeded by 1.4-fold to more than 100-fold the number of cases of cardiopulmonary arrest averted (Table 2). For the 2 studies that also reported case-fatality rates of cardiopulmonary arrest before and after intervention, the number of averted deaths attributable to the RRT intervention (determined from the lower rate of and improved survival from cardiopulmonary arrest) could not account for the actual number of fewer deaths observed during the postintervention period (Table 3). This was particularly the case for the study with the greatest weight in the pooled mortality estimate in children, in which 10 deaths were prevented through the direct interventions of the RRT on cardiopulmonary arrest, whereas 214 fewer deaths were reported in the postintervention period. For the other 2 studies, we similarly found that even if we assumed a best-case scenario (100% case-fatality rate before intervention and 0% case-fatality rate after intervention), the number of prevented deaths attributable to the RRT intervention could not account for the actual number of fewer deaths reported during the postintervention period (Table 3).

This systematic review found that implementation of RRTs was associated with substantial reductions in non-ICU–treated cardiopulmonary arrest rates of 33.8% in adults. However, these reductions were not associated with lower overall hospital mortality rates in this group of patients. Notably, the effect of RRT implementation on hospital mortality in adults has shifted toward the null during the past decade, which raises questions about the effective dissemination of relevant information about RRTs or the possibility of initial publication bias. In children, implementation of an RRT intervention was associated with a pooled reduction in non-ICU–treated cardiopulmonary arrest rates of 37.7% and an overall 21.4% reduction in hospital mortality, but this latter finding was not robust to sensitivity analyses.

In the adult and pediatric studies that reported lower hospital mortality rates after RRT implementation, we also found a disconnect between these improvements and lower rates of or improved survival from cardiopulmonary arrest (see Tables 2 and 3). It is likely that the mortality benefit associated with the RRT intervention was overestimated in these studies. The excess deaths prevented may have been owing to the overall improvement in hospital care quality from RRT training and education, unmeasured secular trends, other quality improvement initiatives during the intervention period, or residual confounding (eg, inadequate control for case mix and preintervention time trends). Collectively, the findings from this review raise questions about the effectiveness and generalizability of RRT implementation, given the lack of a sustained, robust, and plausible mortality benefit.

Our meta-analysis significantly extends the findings of previous studies (including meta-analyses) in several ways. First, this is the most contemporary systematic review of the literature to assess the effect of an
RRT; as a result, we identified 18 studies of RRTs, many of which were published since 2007, that involved nearly 1.3 million admissions. We also evaluated studies in both adult and pediatric populations and examined studies across time to evaluate for cumulative effects. Finally, we extensively used meta-regression, subgroup, and sensitivity analyses to evaluate the robustness of the pooled estimates.

Several additional aspects of this review deserve comment. First, the studies of RRTs in this review exhibited extensive heterogeneity across their reported outcomes. Our use of random-effects models, meta-regression, and sensitivity analyses explained some of the potential sources of this heterogeneity. However, we also noted significant variation across studies in research design, study quality, and RRT activation rates, which would contribute to study heterogeneity. Moreover, although the activation criteria for RRTs appeared to be comparable among the studies, interventions that were implemented by RRTs for specific clinical scenarios have not been sufficiently described or standardized. Our study suggests that development of more rigorous study designs and standardized treatment protocols, as well as the adoption of common terms for reporting on outcomes for RRTs (similar to the Utstein criteria for cardiac arrest\(^a\)), would improve future research in this area.

Second, the discordance between a reduction in rates of cardiopulmonary arrest but not of hospital mortality after RRT implementation in adults may be owing to several factors. This may be in part owing to the establishment of DNR status of severely ill patients by the RRT, which thereby removes very ill patients from consideration for the outcome of cardiopulmonary arrest without improving overall mortality rates.\(^9,42\) Moreover, because RRTs transfer clinically deteriorating patients to the ICU, the measurement of only non-ICU–treated cardiopulmonary arrest rates (rather than hospital-wide rates) may introduce reporting bias and overestimate the effect of the RRT on cardiopulmonary arrest.\(^9\) Finally, although the RRT intervention may succeed in preventing initial cases of cardiopulmonary arrest, their short-term impact may not be sufficient to alter overall survival in severely ill patients.

In contrast to the findings in adults, implementation of an RRT in pediatric populations was associated with lower hospital mortality rates. This difference may be because respiratory conditions are more frequently the cause of cardiopulmonary arrest in children,\(^22\) and children who have cardiac arrest have fewer comorbidities and are more likely to survive than adults.\(^43\) Nevertheless, the pooled estimate in children was not robust to sensitivity analyses and was greatly influenced by studies\(^35,37\) in which the lower number of observed deaths could not be plausibly explained by the direct interventions of the RRT.

Third, our analyses had adequate power to detect a modest reduction in hospital mortality, especially in adults, but may have been limited in their ability to detect smaller improvements. It has been estimated that a sample size of 150,000 patients before and after implementation of an RRT would be required to have 80% power to detect a 5% reduction in hospital mortality rates.\(^9\) Therefore, it remains possible that an even larger study sample of RRTs than the nearly 1 million patients for the mortality

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### Table 2. Cases of Cardiopulmonary Arrest Averted and Number of Deaths Reduced After Implementation of an RRT in Studies That Show a Mortality Benefit

<table>
<thead>
<tr>
<th>Study</th>
<th>RRT Calls, No.</th>
<th>RR for Codes Before RRT, %</th>
<th>No. of Observed Codes After RRT</th>
<th>No. of Codes Averted With RRT(^a)</th>
<th>RR for Deaths Before RRT, %</th>
<th>No. of Observed Deaths After RRT</th>
<th>No. of Fewer Deaths After RRT(^b)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bellomo et al(^27)</td>
<td>99</td>
<td>0.35</td>
<td>22</td>
<td>41</td>
<td>0.74</td>
<td>222</td>
<td>78</td>
</tr>
<tr>
<td>Buist et al(^27)</td>
<td>152</td>
<td>0.50</td>
<td>47</td>
<td>47</td>
<td>0.87</td>
<td>393</td>
<td>59</td>
</tr>
<tr>
<td>Priestley et al(^28)</td>
<td>NR</td>
<td>NA</td>
<td>NA</td>
<td>NA</td>
<td>0.52</td>
<td>83</td>
<td>77</td>
</tr>
<tr>
<td>Sharek et al(^35)</td>
<td>143</td>
<td>0.29</td>
<td>5</td>
<td>12</td>
<td>0.82</td>
<td>158</td>
<td>35</td>
</tr>
<tr>
<td>Tibballs and Kinney(^37)</td>
<td>808</td>
<td>0.91</td>
<td>24</td>
<td>2</td>
<td>0.65</td>
<td>398</td>
<td>214</td>
</tr>
</tbody>
</table>

Abbreviations: NA, not applicable; NR, not reported; RR, relative risk; RRT, Rapid Response Team.

\(^a\) Determined by the following equation: \((\text{Observed Codes After RRT}) \div \text{RR for Codes})\)

\(^b\) Determined by the following equation: \((\text{Codes Averted With RRT From Table 2}) \div \text{RR for Deaths})\)

### Table 3. Case-Fatality Rates and Deaths Prevented by Implementation of a Rapid Response Team (RRT)

<table>
<thead>
<tr>
<th>Study</th>
<th>Case-Fatality Rate for Codes Before RRT, %</th>
<th>Case-Fatality Rate for Codes After RRT, %</th>
<th>No. of Deaths Prevented by Decreased Code Rate(^a)</th>
<th>No. of Deaths Prevented by Increased Code Survival(^b)</th>
<th>Total No. of Deaths Prevented by RRT Effect on Codes</th>
<th>No. of Fewer Deaths After RRT(^c)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Buist et al(^27)</td>
<td>76.7</td>
<td>55.3</td>
<td>36</td>
<td>10</td>
<td>46</td>
<td>59</td>
</tr>
<tr>
<td>Tibballs and Kinney(^37)</td>
<td>65.0</td>
<td>26.1</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>10</td>
</tr>
<tr>
<td>Bellomo et al(^27)</td>
<td>100</td>
<td>0</td>
<td>41(^d)</td>
<td>9</td>
<td>63</td>
<td>78</td>
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<tr>
<td>Sharek et al(^35)</td>
<td>100</td>
<td>0</td>
<td>12(^d)</td>
<td>5(^d)</td>
<td>17</td>
<td>35</td>
</tr>
</tbody>
</table>

\(^a\) Determined by the following equation: \((\text{Codes Averted With RRT From Table 2}) \times (\text{Case-Fatality Rate Before RRT})\)

\(^b\) Determined by the following equation: \((\text{Observed Codes After RRT}) \times (\text{Difference in Case-Fatality Rate for Codes Before and After RRT})\)

\(^c\) Derived in Table 2.

\(^d\) Rates are for best-case scenario for sensitivity analyses.
analysis in this study may have found a significant mortality reduction.

Fourth, there remains to date no formal evaluation of the costs or the cost-effectiveness of RRTs, although significant hospital staff resources are involved in their development and maintenance. It is possible that, by initiating DNR discussions with patients, RRTs may be cost-minimizing because terminally ill patients may decline aggressive treatment. Alternatively, increased triage to the ICU after RRT implementation without observable survival gains may increase hospital costs.

Our meta-analysis should be interpreted in the context of the following limitations. We did not have patient-level data, and our results were therefore analyzed at the study level. Comprehensive information on the characteristics of the hospitals that implement RRTs was not available, including the use of hospitalists. Of the 12 studies that reported on academic status in this meta-analysis, 10 studies involved academic centers and 1 study was a 23-center study of academic and community hospitals. It is possible that the greater presence of medical house staff and hospitalists in academic centers may have blunted the potential benefits of RRT implementation. Our studies did not assess the effect of RRTs on other mortality end points (e.g., 30-day or mortality of those treated in the ICU vs those not treated in the ICU). Finally, because most studies did not routinely report on outcomes other than rates of cardiopulmonary arrest and hospital mortality, we were unable to assess the effect of RRTs on issues such as satisfaction among nurses, establishment of DNR status, and prevention of in-hospital complications. Although these outcomes are important, they are not the primary goals for the establishment of RRTs and may possibly be achieved with approaches other than the use of RRTs.

Although RRTs appear to reduce rates of cardiopulmonary arrest outside the ICU, consistent and plausible evidence is not available to demonstrate that they are associated with improved survival—the primary reason for their development. Health quality organizations may need to reconsider their promotion of RRTs without robust evidence to support their use.

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