Critical Review Form

Therapy

Compression-Only CPR or Standard CPR in Out-of-Hospital Cardiac Arrest, *NEJM* 2010; 363:434-442

Objective: **"To compare the efficacy (measured as the 30-day survival rate) of compression-only CPR and standard CPR, as performed on the basis of instructions from an emergency medical dispatcher, before the arrival of EMS personnel, in witnessed cases of out-of-hospital cardiac arrest." (p.435)**

Methods: **Swedish study conducted nationally across 18 Emergency Medical Dispatch Centers from February 2005 to January 2009. Inclusion criteria included witnessed collapse in non-breathing patient >8 years old. Exclusion criteria included cardiac arrest caused by trauma, airway obstruction, drowning, or intoxication; age <8 years, difficulty of the dispatcher in communicating with the caller, CPR already initiated at the scene, caller unwilling to perform dispatcher assisted CPR, and caller already trained in conventional CPR.**

Bystander – callers meeting inclusion criteria were randomized to compression-only CPR (COCPR) or standard CPR (2-ventilations for every 15 compressions) on the basis of the next available data-collection sheet for each dispatcher. Dispatchers were given detailed written instructions to use for COCPR and standard CPR, but they could deviate from this script if necessary. Dispatchers recorded if EMS arrived and whether EMS arrival interrupted their CPR instructions.

The primary endpoint was 30-day survival and secondary endpoints were 1-day survival (survival until midnight of the day of hospital admission) as well as the first detected cardiac rhythm and survival to discharge from the hospital. The trial sample size was originally designed to detect a 2% absolute difference in 30-day survival with an alpha of 0.05 and 80% power with 2213 patients in each arm, but a change in AHA CPR guidelines and practical difficulties forced the investigators to reduce their sample size to 1000 patients per arm yielding a 78% power to detect a 3% absolute difference.

Guide		Comments
I.	**Are the results valid?**	
A.	**Did experimental and control groups begin the study with a similar prognosis (answer the questions posed below)?**	
1.	Were patients randomized?	Yes. "The type of CPR on which the caller was instructed was determined on the basis of the next available data-collection sheet for each dispatcher, who removed a paper strip covering the treatment assignment on the sheet after determining that the inclusion criteria had been met. Data collection sheets were distributed in blocks of 1000 sheets, 50 for each treatment assignment. The order of sheets within each block was unique and was based on the random-number generator in SPSS Software." (p. 435)
2.	Was randomization concealed (blinded)?	Yes – see above.
3.	Were patients analyzed in the groups to which they were randomized?	Yes. "Data was analyzed according to the randomized treatment assignments, for patients who fulfilled the inclusion and exclusion criteria (the intention-to-treat population in the primary analysis) as well as according to the treatment actually received (the per-protocol analysis)." (p. 437)
4.	Were patients in the treatment and control groups similar with respect to known prognostic factors?	Yes. "The two treatment groups were similar with respect to the baseline characteristics of the patients and the episodes of cardiac arrest (Table 2)." (p. 438)
B.	**Did experimental and control groups retain a similar prognosis after the study started (answer the questions posed below)?**	
1.	Were patients aware of group allocation?	No – patients were in cardiac arrest. Survivors could have found out later.

2.	Were clinicians aware of group allocation?	Uncertain. EMS providers were aware by necessity, but uncertain whether receiving hospitals were blinded to treatment assignment.
3.	Were outcome assessors aware of group allocation?	Uncertain. The manuscript does not provide any details about who ascertained 30 day outcomes, what records are used, or how accurate this information source may be. The online supplement alluded to in the manuscript suggests that the information sources included the Swedish Dispatch organization (SOS), the National Cardiac Arrest Register, and ambulance records as well as the Public Population Register and the impatient register.
4.	Was follow-up complete?	No. Information on follow-up was unavailable for 132/1952 (6.8%). However, "we therefore performed a subgroup analysis excluding districts where more than 18% of patients were lost to follow-up." (p. 439)
II.	**What are the results (answer the questions posed below)?**	
1.	How large was the treatment effect?	• 3809 cases were randomized but only 1276 were analyzed secondary to various exclusion criteria (Table 1 page 437). • 656 (51.4%) randomized to standard CPR and 620 (48.6%) to COCPR and 149 (11.7%) did not receive the assigned treatment. • 30 day survival 8.7% COCPR vs. 7.0% in standard CPR (1.7% difference; 95% CI -1.2 to 4.6, p=0.29). • One-day survival 24.0% COCPR vs. 20.9% (3.1% difference, 95% CI to 7.7, p=0.18) • 30-day and 1-day survival did not differ in subgroups of age,

		EMS response time, or first cardiac rhythm (Fig. 2 p. 440). • No significant difference in survival when age <18 years were excluded or on per-protocol analysis. • Insignificant trend towards improved survival for COCPR when the event was in a public place (Fig. 2, p. 440). • Mean age was 67 years, 66% were male, 76% of arrests were in the home, >50% had first cardiac rhythm asystole, and the mean EMS response time was 10 minutes.
2.	How precise was the estimate of the treatment effect?	See 95% CI above
III.	**How can I apply the results to patient care (answer the questions posed below)?**	
1.	Were the study patients similar to my patient?	Uncertain. What was the ethnic distribution? Socio-economic status? What proportion of EMS teams had ALS capability? Did patients have therapeutic hypothermia after return of spontaneous circulation (ROSC)?
2.	Were all clinically important outcomes considered?	No-the authors did not assess neurological outcomes (see PGY-I paper).
3.	Are the likely treatment benefits worth the potential harm and costs?	Uncertain based upon this data which is under-powered with an incomplete description of methods.

Limitations

1) **Under-powered-might have considered a non-inferiority design since dispatcher assisted COCPR has distinct theoretical advantages over standard CPR (easier/faster to teach, more acceptable to bystanders).**
2) **Incomplete description of methods – blinding of treating clinicians and outcome assessors? Who/how were outcomes ascertained?**
3) **No assessment of neurological status on discharge.**

4) **No description of or adjustment for EMS capabilities or use of post-ROSC therapeutic hypothermia.**
5) **Significant proportion of protocol violations, but no differences were noted in the primary outcomes between intention-to-treat and per-protocol analyses.**

Bottom Line

COCPR is easier to learn and to perform and should be considered the preferred method for bystander CPR in witnessed cardiac arrest of presumed non-respiratory etiology since 1-day and 30-day survival rates do not differ significantly between COCPR and conventional CPR