Reviewer's report

Title: Modelling optimal location for pre-hospital helicopter emergency medical services

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Reviewer: Stephen H Thomas

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I find the paper well-written, and that it makes an excellent case for systematic approach to placement of HEMS assets. This is a direction that everyone involved in HEMS (and prehospital/systems) care should follow. I do have some concerns that the important messages about methodology, may be mired in a debate (which is also appropriate, but in another place) as to whether autolaunch is indeed a good/cost-effective idea.

Some consistent points: The word spellings "autolaunch" and "fixed-wing" are my personal preferences, but whatever is preferred by the authors, there should be consistency in spelling (including hyphenation/capitalization) for these terms and others.

Specific comments

Background/First paragraph: The authors' background is in my opinion well-presented, and makes a good case for the importance of the article. I do believe that the "inclusive" trauma system is best for regional healthcare resource allocation, but I also believe this remains an area of intense debate. Thus, I suggest the authors consider adding some text to the effect that while the assessment of optimal trauma system configuration continues, they and their region have judged that inclusive trauma systems are preferable...and that inclusive trauma systems must include access to rapid (ground and/or air) transport for patients requiring secondary transfer.

Background/Second paragraph: In the systems where HEMS has been demonstrated to be cost-effective (as cited by the authors), is there auto-launch? Don't those systems dispatch HEMS only after evaluation by ground EMS? The sentence beginning with "Time delays..." could possibly benefit from rewording - I agree with the authors' point though.

Background/Second paragraph: We begin to arrive at the crux of an issue that has the potential to confound the understanding of this well-written paper. I personally find that the article - including methodology and presentation - is excellent. However, I am concerned that the considerable controversy over the
practice of autolaunch will cast a shadow over the authors' cogent presentation of how to implement autolaunch. The citations supporting autolaunch in this paragraph (#12 and #13) are not primary research. In fact, only Dan Hankins' (excellent) work as cited on the next page (#14) gives any evidence in favor of autolaunch. What we know, is that the Mayo system described by Hankins et al works well - but that this system is characterized by an amazing level of medical oversight and utilization review, etc. Even so, the Minnesota group reports only a 21% completion rate (Hankins DG. Air medical transport of trauma patients. Prehosp Emerg Care. Jul-Sep 2006;10(3):324-327). Furthermore, another group (Diaz M, Hendey G, Bivens H. When is the helicopter faster? A comparison of helicopter and ground ambulance transport times. J Trauma. 2005;58:148-153), writing that simultaneous HEMS and ground EMS dispatch becomes time-beneficial when patients are at least 10 miles from the receiving trauma center, points out that their autolaunch scheme results in a frequency (55%) of cancelled flights that would stretch the financial resources of any HEMS program.

Background/Third paragraph: I suspect many readers would like to see some comment as to the functionality of the current BCAS autolaunch program. I understand that's not what this paper is about, but by way of background, perhaps the authors could at least provide info on triage guidelines, and for summary statistics such as % cancellations, and % flights judged as "appropriate" by whatever standards they use. For triage guidelines, for example, is appropriateness defined as ISS at least 12 or 3-day hospitalization (as outlined on page 5 top)?

Methods/Defining the question first paragraph: The last sentence may benefit from rewording, and/or some clarification as to how frequently these other options are actually available/unavailable.

Methods/Defining the question second paragraph: Who makes the estimate as to the 20-minute driving time? Is this defined by GIS? (Forgive me if this is explained in the other detailed section of the Methods).

Methods/Remaining paragraphs after second paragraph: I believe the authors do a good job of explaining a complicated system, as succinctly as possible. There are enough datapoints about the system given, to allow the reader to get a real sense of how the authors' methods were/are applied. I found that some of this bordered on going a bit over my head, but this is not an area of my expertise and I appreciate the authors' attention to detail.

Two points as to the remaining Methods writing, one minor and one major:

Minor point is that some readers feel (strongly) that Emergency Department (ED) is preferred over Emergency Room (ER). My suggestion is that the authors simply avoid rubbing anyone the wrong way, and change things to ED.
Major point is that, as elegant as these computer models are, many folks who work in the field are highly suspicious of their accuracy. It's not at all a case of "garbage in, garbage out" - these models are far too well-thought to have the word "garbage" applied. However, even the most well-executed computer models will be better accepted if they are tested. Sometimes the results about these time estimates are surprising. For instance, Svenson et al found that many "assumptions" about logistics and time savings were (way) off, in their demonstration that HEMS is associated with time savings even when most (or all) logistics assumptions would suggest otherwise (Svenson J, O'Connor J, Lindsay M.

Is air transport faster? A comparison of air versus ground transport times for interfacility transfers in a regional referral system. Air Med J. 2006;24(5):170-172). The answer is not to abandon high-end computer modeling, but could the authors consider (maybe as a next-step study) actually validating the computer-calculated driving times?

Discussion/First sentence: Given the above point about assumptions, I feel that the authors should replace the word "accurate" with the word "precise" (or some other word indicating that we don't know for certain, how close to "truth" the estimates are).

Discussion/Second paragraph: The authors' point here, about their system being a (far) step above using "crow-fly" estimates, is well-taken. Whatever imprecision or inaccuracy there may be to the computer-generated estimates for HEMS placement, those estimates are substantially more likely to approach "truth" than previous methods.

Discussion/Remainder after second paragraph: I find this section well-written, and believe that the authors should consider closing the loop, by pointing out their next potential steps. These should include, in my opinion, some sort of utilization review for autolaunch (in terms of HEMS appropriateness), and some assessment of whether their modeling proved accurate, after they institute additional HEMS coverage.