Author's response to reviews

Title: Direct hospital costs of chest pain patients attending the emergency department: a retrospective study

Authors:

Jakob L Forberg (jakob@hansen.net)
Louise S Henriksen (louise_henriksen@hotmail.com)
Lars Edenbrandt (lars.edenbrandt@klinfys.mas.lu.se)
Ulf Ekelund (ulf.ekelund@med.lu.se)

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Author's response to reviews: see over
Sweden, February 2006

Dear BioMed Central Editorial Team

We were very thankful to receive the reviewer comments and have done the best we can to accomplish the recommended changes. In principle, we have made all the changes suggested by the referees. Below please find our replies to the comments and a point-by-point description of the changes (blue italics).

The manuscript adheres to the formatting checklist.

We would be grateful if the manuscript could be considered for publication in BMC Emergency Medicine.

We are looking forward hearing from you.

On behalf of all co-authors –
Yours sincerely,

Jakob Lundager Forberg, MD
Reviewer: Steve Goodacre

Minor Essential Revisions

Results, paragraph 1: I presume the Euro and $ equivalents are 1000’s, i.e. 2,900 Euros / US$3,200.

This is correct. The text concerning the mean cost of all patients in results, paragraph 1 has been changed from 2.9 EUR or 3.2 USD to 2.9 kEUR or 3.2 kUSD

Discretionary Revisions

The limitations section should probably also note that the cost-effectiveness analysis did not take into account potential uncertainty in the parameters used, due to sampling variability and the assumptions used.

These important limitations have been added at the end of the limitations section (page 13).

Table 3: Was the unit cost for an ED visit really only 10 Euro?

Dr Goodacre is completely right; the cost for a ED visit should be 100 EUR. This has been corrected.
Reviewer: Thomas Allison

General
This is a retrospective study to document hospital costs in 1000 patients who presented to the emergency department for chest pain at a single hospital in Sweden in 1997. The authors provide little in the way of insight into factors affecting the decision to admit or discharge—and ultimately cost for visit. There is a cost-effectiveness analysis which is based on an assumption with limited documentation that a patient inappropriately discharged from the ED with ACS will experience twice the mortality of a patient with similar characteristics who is admitted to the hospital.

1. Is the question by posed by the authors new and well defined? The aim of the study was purely descriptive – that is, to describe direct hospital costs for unselected patients with chest pain who present to the emergency department (ED). The authors did attempt to answer some implicit questions not stated in the Background including which type of patient accounted for the largest portion of costs and whether or not the (unstated and undefined) strategy used for admission of ED patients with chest pain at this particular hospital was “cost-effective”.

At the end of Background we have added the aim of estimating the cost per additional life-year saved by hospital admission compared to theoretical strategy of discharging all patients home. During the inclusion period (1997), there was no general strategy in use for admission of ED patients, and this is now stated at the end of Methods, Subjects.

The problem of chest pain diagnosis in the ED and the issue of costs associated with this group of patients – and how to appropriately reduce these costs – has been previously well studied both observationally and in randomized clinical trials, so this paper cannot be described as breaking new ground.

We agree, but believe that our results could still be of interest.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work? There are several deficiencies in the methods. “Costs” are not precisely defined. It sounds from the description as if these were truly costs of operating the ER and hospital rather than itemized charges to the patient. If so, how were these costs assigned to the chest pain patients – as a simple fraction of the total number of patients using the ED and hospital over the same time period or was there some adjustment for the amount of resources consumed by a chest pain patient versus other types of patients?

Lund University Hospital is fully publicly financed, and the patients are not charged for medical services. The unit costs described were the yearly calculated average costs of all patients that presented to the ED, that were admitted to a specified ward or underwent a diagnostic test or a procedure. We have slightly rewritten the end of Methods, Data collection, in order to clarify the cost calculation further, and have added “The total cost for each included patient was calculated as the sum of the cost of the ED and hospital admission (when applicable) and the costs of performed procedures and diagnostic tests (laboratory tests, x-ray, exercise test etc.)”.

Another problem with the methods is that we are not told that these were consecutive patients presenting to the ED with chest pain. If so, the authors should state. If not, then the authors should explain how they were selected from all the patients presenting with chest pain during the study period. If there was any selection bias in terms of which patients ended up in the study,
then results are really arbitrary and not necessarily representative of the practice being described.

*The study included consecutive patients presenting to the ED with chest pain, and this is now also stated in Methods, Subjects, first sentence.*

3. Are the data sound and well controlled? The reader must more or less take the cost figures presented on faith. The tables provide unit costs for hospital beds and various tests and procedures, but not ED costs and items such as lab work.

*We agree that more cost examples could be added to table 3, and as mentioned by Dr Allison, especially cost of tests performed in the ED could be of interest. However, like the reviewer Dr Goodacre (see his previous report for BMC Medicine), we believe that too much data will distract the attention of the reader from our main results and conclusions. As this revision is not compulsory we have chosen not to change the table.*

We know the mean number of days spent in the hospital, but this is not broken down by ward. It would be difficult (perhaps impossible) to reconstruct the total cost figures given in the Results from data given in Tables 2 and 3 plus information provided in the test. However, the reviewer is not certain that questioning the accuracy of the cost data or being able to reconstruct it is necessary. A mean cost of 3.2 kUSD (currency with which this reviewer is most familiar) seems low, even at 1997 levels, considering that 66% of patients were hospitalized with an average stay of 5.3 days and that numerous cardiac tests and procedures were performed. However, we cannot rule out that this may be due to significant differences in how US and Swedish hospital are managed and how costs are established.

*As Dr Allison mentions, and as we briefly describe in the Discussion there are probably cost differences between countries due to different reimbursement systems, triage routines and resource utilization patterns.*

4. Does the data adhere to the relevant standards for reporting and data deposition? Standard deviations might be included with the means and interquartile ranges with the medians.

*We think it is very informative and in this study relevant to describe both the mean and the median. If both the mean and median is described it should not be necessary to describe standard deviation and interquartile ranges.*

5. Are the discussion and conclusions well balanced and adequately supported by the data? Though not stated as an aim in the Background, the authors employ several simplistic assumptions (which are described in the Methods) to come up with a statement that the current practice of admitting 661 of 1000 patients presenting to the ED with chest pain is “cost effective” despite the fact that only 207 of these patients were diagnosed with acute coronary syndrome (ACS) and only 83 patients had some type of cardiac procedure (CABG, PCI, or thrombolysis). The results of the cost-effective analysis depend on the assumption that mortality from ACS would be twice as high if patients were discharged from the ER rather than being admitted. Many factors likely play into that assumption (such as the type of therapy that would be offered in the hospital and characteristics of the patients such as age and sex distribution), and there is little discussion of how this figure of twice the cost was determined. Two (2) references are cited, but these are not primary references that collected the data upon which the assumption of twice the hospitalized mortality was based, but rather they are studies which used the same
assumption in their analyses. The original references cited in the 2 papers subsequently referenced in the study under review were published in 1987 and 1993. One study (Lee et al, Am J Cardiol 1987;60:219-224) involved 35 patients with a missed diagnosis of acute MI. Follow-up for mortality was only 72 hours, though one might assume that not getting appropriate care for acute MI might continue to disadvantage a patient beyond 3 days. The second study (McCarthy et al, Ann Emerg Med;1993;22:579-582) on which the assumption of 2-fold mortality was based included 20 patients with missed MI. Their characteristics were slightly different from 1030 patients with acute MI that were admitted, so direct comparisons are difficult. The mortality rate in missed MI was 10%, actually lower than the 15% mortality rate for patients admitted with MI. If this assumed figure of twice the mortality is not accurate and/or not applicable for the time frame of 1997 when the cohort for the study under review was drawn, then all of the cost-effectiveness conclusions of the paper under review are inaccurate.

We absolutely agree that our estimation of life-years saved is very crude, and that it is important that this is clearly described. In the manuscript, we referenced the study by Pope et al. (NEJM 2000;342:1163-70) who found an 1.9 /1.7 risk-adjusted mortality rate for patients discharged from the ED with myocardial infarction/ unstable angina compared with those admitted. We have now added the reference recommended by Dr Allison (Lee et al, Am J Cardiol 1987;60:219-224) which also found an two fold increase in mortality for MI patients discharged from the ED. As Dr. Allison states, the study by McCarthy et al. (Ann Emerg Med;1993;22:579-582) did not find this mortality increase in discharged patients with MI, but a risk-adjusted mortality rate was not calculated in this study.

We are aware that many factors may influence the estimated cost of life-year saved. The group of patients included in our study differs slightly from the patients in the study of Lee et al. and Pope et al. Compared to the study by Lee et al, our study patients were on average 3-4 years older, had about the same prevalence of previous MI, but fewer cases of diabetes (11% in our study compared to 21% in Lee et al). Further, the referenced studies only looked at the short term mortality (3 days and 30 days) and new treatments and procedures have been implemented since these studies were done.

In the revised manuscript, the background (Methods, Assumptions) and the limitations (Discussion, Cost effectiveness and Discussion, Study limitations) of this estimation are discussed in more detail.

6. Do the title and abstract accurately convey what has been found? The abstract appears to accurately convey what has been found. The title does not promise a cost-effectively analysis, but that seems to be the principal conclusion of the paper.

The reviewer is correct. Because of the limitations of the cost-effectiveness analysis, we have chosen not to change the title.

7. Is the writing acceptable? The writing style is acceptable, and we do not see frequent typographical errors or misspellings.

Major Compulsory Revisions

1. Page 4, Subjects: The paper does not tell us whether these 1000 patients presented consecutively. If so, please state. If not, then the authors should explain how they were selected from all the patients presenting with chest pain during the study period.

Consecutive patients presenting to the ED were included. This is now stated in Methods, Subjects, first sentence.
2. The cost-effectiveness analysis needs to be more thoughtful, specifically that its assumptions need to be better supported with actual data rather than references which use the same assumptions.

The assumption that mortality increases two-fold if ACS patients are discharged from the ED (Methods, Assumptions) is now supported with the primary references, and the text now reads: “Previous studies suggest that failure to admit patients with myocardial infarction (Lee, Pope) or unstable angina (Pope), increases short term mortality about two-fold.”

In Discussion, Cost effectiveness, we now attempt to comment on the cost-effectiveness analysis results in a more thoughtful way. The text (2nd para) has been changed to: “Our assumption that mortality would be twice as high if ACS patients were discharged from the ED is based on two US studies from 1977-85 (ref) and 1993 (ref) which report the 3 day and 30 day mortality, respectively. The patients included in the study by Pope et al., were slightly different from our patients; they were on average 3-4 years younger and more had diabetes (21%), and this of course influences the validity of our assumption and the accuracy of our estimation of the cost of life-years saved. Further, the 95 percent confidence interval of the 2-fold mortality increase reported by Pope et al. was broad, 0.7 to 5.2. In the study by Lee et al, comparable patient characteristics and a confidence interval are not given. As the above studies only looked at short term mortality and were performed before modern reperfusion therapy such as PCI and secondary prophylactics, the true cost of a life-year saved by hospitalization in the present study may well be lower than our estimation.”

Minor Essential Revisions

1. Page 6, Ethical approval and statistics: Some countries (or states in US) require patients to approve use of their clinical data for research; is this true for Sweden? If so, was this considered in the ethical approval?

In our ethical approval (from the year 2000), the individual patients were not required to approve the use of their data.

Statistical procedures are described briefly, but nowhere in the paper are actual statistical results described other than to use the terms “higher” or “significantly higher”. In some cases there are more than 2 groups being compared (such as cost differences among CCU, ICW, and GW), so ANOVA rather than t-test should be used.

We agree that the t-test should not be used when comparing the mean of more than two groups. Several possibilities exist when doing multiple comparisons of means, and one method is the Benferroni t-test we used in the first manuscript, which is a rather conservative test. Dr Allison suggested the use of the ANOVA test. We are not aware of any consensus in the choice of method for multiple comparisons of means, but we are thankful for Dr. Allisons advice and have now used the ANOVA test for multiple comparisons. In Methods, Ethical approval and statistics, we define a difference with p<0.05 as statistically significant, and therefore we have not mentioned the p-value each time we use the term “significantly higher” in Results. We appreciate that the term “higher” is imprecise and the term “significantly higher” is now used instead (page 8).
2. Page 6, Patient characteristics and overall cost: Shouldn’t the costs translated to EUR and USD actually be in kEUR and kUSD?

This is indeed true, and has now been corrected.

3. Page 7, Costs in relation to level of care: The authors should define CCU, ICW, and GW, as not all countries use the same designations for various levels of care. For example, does the “I” in ICW stand for intermediate or intensive?

These designations have now been written out in full text.

Discretionary Revisions

1. The paper might benefit from additional information regarding the number of physicians staffing the ED and whether or not there were any differences among physicians in terms of admission practices.

Unfortunately we do not have any data regarding the number of physicians or the different physicians’ admission practice. In 1997 there was no general strategy for admitting ED patients to in-hospital care at Lund University Hospital.

2. A comparison of patient characteristics between admitted and discharged patients would be informative and add to the educational quality of the paper. Similarly, a comparison of patients admitted without a diagnosis of ACS versus those discharged from the ED might provide insight as to the decision processes used by this physician group.

After recomendations from the reviewer Dr Goodacre we have tried to make the manuscribt short and focus on the principal findings. Therefore we have choosen not to include these otherwise very interesting camparions.

3. Page 9: The authors describe changes in treatment of ACS that have occurred in Sweden between 1997 and 2003, but then dismiss these changes as not affecting the current results since only 21% of patients were ACS. However, those 21% of patients with ACS accounted for 62% of all costs, so changes in care (resulting in increased costs) could significantly affect results. This discussion point should perhaps be re-thought or further supported.

We agree. The following sentence has been added (Discussion, 2nd sentence from the end of first para): “It is therefore reasonable to believe that patients with ACS now contribute with more than 62 % of the total cost.”

4. Some stratification of these patients by standard criteria according to risk of acute event would be useful. For example, what percentage of these patients presented with atypical chest pain and were at low risk for acute MI versus those at intermediate or high risk. That information would help the reader rate the effectiveness of this hospital’s admission practice

Unfortunately, we do not have data on the chest pain character in a standardized way that would allow us to make this very relevant stratification.
Reviewer: Isabelle Durand-Zaleski

General
This is a single-institution cost study of patients presenting with chest pain at the emergency department of Lund's hospital in 1997. The authors estimate form the hospital of Lund's accounting system the cost of patients with acute coronary syndrome vs patients discharged or ruled out for AMI via triage decision in the emergency department. The authors present cost information by diagnosis and a cost-effectiveness analysis of their current triage policy.

Major Compulsory Revisions

I have reservation regarding the cost-effectiveness analysis because the mortality data was obtained from a small sample and compared to the mortality in a different population and setting (USA large urban tertiary care centres). The confidence interval for the 5.3% mortality used as baseline is 2-8%. It would be interesting to know the actual mortality and confidence interval for the mortality comparator chosen. The statement that the mortality was divided by two after admitting patients is too imprecise. If a cost-effectiveness analysis is to be presented at all it should be supported with stronger mortality data and include a sensitivity analysis.

We agree that the estimation of the cost of a life-year saved is very crude and has several limitations. The assumption of a two-fold increase in mortality if ACS patients were not admitted is in the new version of the manuscript further supported by the reference Lee et al (Am J Cardiol 1987;60:219-224). The new text (Methods, Assumptions) reads: “Previous studies suggest that failure to admit patients with myocardial infarction (Lee, Pope) or unstable angina (pope), increases short term mortality about two-fold.” A more precise assumption is not possible because studies reporting the mortality of patients with missed ACS discharged from the ED are limited, due to the fact that these studies are very difficult to perform.

In Discussion, Cost effectiveness, we now attempt to comment on the cost-effectiveness analysis results in a more thoughtful way. The text (2nd para) has been changed to:

“Our assumption that mortality would be twice as high if ACS patients were discharged from the ED is based on two US studies from 1977-85 (Lee et al) and 1993 (Pope et al) which report the 3 day and 30 day mortality, respectively. The patients included in the study by Pope et al., were slightly different from our patients; they were on average 3-4 years younger and more had diabetes (21%), and this of course influences the validity of our assumption and the accuracy of our estimation of the cost of life-years saved. Further, the 95 percent confidence interval of the 2-fold mortality increase reported by Pope et al. was broad, 0.7 to 5.2. In the study by Lee et al, comparable patient characteristics and a confidence interval are not given. As the above studies only looked at short term mortality and were performed before modern reperfusion therapy such as PCI and secondary prophylactics, the true cost of a life-year saved by hospitalization in the present study may well be lower than our estimation.”

It is correct that the added study by Lee et al. was performed only in larger urban US centers, but the study sites in the work by Pope et al. included suburban and semirural sites which are probably comparable to our institution.

It is our belief that the background and the limitations of the assumptions are now clearly described. We think that the reader is thereby provided with enough information to be trusted to form his or her own opinion about the results and conclusions. Our discussion of the results should help ourselves and others to design studies with better methodology in the future.
Minor Essential Revisions

In the methods section, the authors should describe the diagnostic categories that they are going to use in the ‘results’ section.

_We have now described the ACS diagnostic category in Methods, Data collection: “A patient was considered having ACS when discharged with a diagnosis of either AMI or unstable angina”_

New possibilities in the management of patients with ACS include coronary scanner, which is not too invasive and might be a good triage test. This should be included in the discussion.

_We have now added computerized tomography in the sentence at the end of Discussion, Implications for improvement of patient management: “In the future, it may well be that modern conventional ED management supplemented by risk prediction algorithms, blood samples for new cardiac markers and/or selected investigations such as myocardial perfusion imaging, computerized tomography or magnetic resonance imaging will decrease or even eliminate the benefits of establishing dedicated chest pain units.”_