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Centre for Clinical Effectiveness

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Effectiveness of Acute Care of the Elderly (ACE) Units

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Abstract

Background: Specific wards for acute treatment of older patients were established by Southern Health at Dandenong hospital in the early 1990s and at Monash Medical Centre, Clayton in 1997. The goals of an Acute Care of the Elderly (ACE) unit are to stabilise acute medical problems, maximise the functional independence of the patient and discharge the patient to an appropriate level of care.

The Clayton unit has recently closed and the model of care at the Dandenong site is thought to have gradually eroded through time and relocation.

The Centre for Clinical Effectiveness was requested to conduct a review of the evidence of the effectiveness and cost effectiveness of this model of care.

Clinical Questions: For older patients with acute illness does treatment in an Acute Care of the Elderly Unit, as compared to usual care, result in improved clinical outcomes?

For older patients with acute illness is treatment in an Acute Care of the Elderly Unit, as compared to usual care, cost-effective?

Methods: We included all relevant studies published in English since January 1990.

We searched The Cochrane Library, including The Cochrane Database of Systematic Reviews, DARE, CENTRAL and HTA. We also searched Medline, EMBASE and CINAHL. Searches were conducted in August 2008.

Studies were selected and appraised by one reviewer in consultation with colleagues, using inclusion, exclusion and appraisal criteria established a priori.

Results: Three randomised controlled trials have been included in this review, those by Landefeld,¹ Counsell² and Saltvedt.³

The included studies have been assessed to be of moderate quality. Some quality criteria, particularly those regarding blinding of patients and care providers, are more difficult to apply in a model of care situation than they would be in a simpler randomised controlled trial (RCT). Thus, although the studies have been assessed as of moderate quality we are mindful of how difficult it would be to implement a RCT in a model of care setting.

Two of these studies found no significant difference in mortality or length of stay (LOS) however the third study found a significantly lower mortality and significantly longer LOS in the intervention group.

One study showed significant benefit of the intervention in several measures of functional ability and overall health status at discharge. At three months post discharge there was no difference between the intervention and control patients in the mean number of activities of

daily living (ADLs) the patient could perform independently or the mean number of instrumental ADLs. This study also showed that significantly more patients were discharged to home rather than to long term care in the intervention group.

Another study showed no change between groups from baseline (two weeks prior to admission) to discharge or admission to discharge in the number of independent ADLs performed. Generalised Estimating Equation (GEE) analysis showed differences between groups for ADL decline from baseline to 12 months follow-up significantly favoured the intervention group. Significantly fewer intervention patients experience the composite outcome of either ADL decline from baseline or nursing home placement at discharge by GEE analysis.

Individual studies showed process of care and drug profiles to differ significantly between ACE units and usual care units and significantly more patients in the intervention group to have psychiatric diagnoses at discharge.

Neither of the studies that examined costs found any significantly increased costs for in hospital patient care for those in the intervention group when costed from a hospital perspective. Only one study found any difference in resource use after the initial hospital admission showing that significantly fewer intervention patients lived in long-term institutions at any time in the three months post-discharge.

There were no other significant differences found in readmission to acute care at one, three or six months, patients receiving home healthcare services at one, three or six months, admission to long-term institutions during the first three months of those initially discharged to home, nursing home admission in the following year or times spent in hospitals or nursing homes at six months.

Patient, care-giver, physician and nurse satisfaction was significantly higher in the intervention group.

From a hospital perspective, treatment of older patients in ACE units was no more or less expensive than treatment in a usual care ward. No comprehensive cost-effectiveness study of the ACE model of care was found.

Discussion: This evidence report has attempted to review the best available evidence for the use of Acute Care for Elders Units (ACE units) as a model of acute care for older patients in hospital.

There were several limitations to this review including the difficulties involved in searching for and assessing the quality of models of care, applying the concept of 'usual care' across health services and countries and applying appraisal criteria to a model of care study.

The outcomes of this review need to be considered in light of the noted limitations, in particular as a study of a Geriatric Evaluation and Management Unit (GEMU) has been included in this review. This GEMU met the criteria for our definition of an ACE unit however many other studies of GEMUs were excluded as they did not meet these criteria.

Results of some outcomes should be interpreted with caution, particularly those of patients and provider satisfaction as there is not enough information regarding the outcome measures to assess the validity of these results. Caution should also be taken in interpreting 'no difference' results for those outcomes for which studies were not powered.

Conclusions: There is some moderate quality evidence to suggest that an ACE model of care may reduce mortality, improve functional ability and increase the number of patients discharged to home rather than long-term care in selected populations of older patients. ACE units result in significant differences in process of care and drug profiles of the patients.

A comprehensive cost-effectiveness study of ACE units was not found however those that analysed costs from a hospital perspective found that there was no significant difference in the costs of treating patients in an ACE unit as compared to a usual care unit.

Background

Specific wards for acute treatment of older patients have been established by Southern Health (SH) with an Acute Medical Unit for the Aged (AMUA) at Dandenong hospital created in the early 1990s followed in 1997 by a unit at Monash Medical Centre, Clayton.

This model of care for older patients is also known in the literature as an Acute Care for Elders unit (ACE unit) or an Acute Care for Elders Program¹ and has also been described as very similar to Geriatric Evaluation and Management Unit.⁴

The goals of an ACE unit are to stabilise acute medical problems, maximise the functional independence of the patient and discharge the patient to an appropriate level of care.⁵

Key features of ACE units, as described by Southern Health staff are:

1. Defined physical environment.
2. Admission processes which identify the problem and target the appropriate assessment processes.
3. Assessments which incorporate the principles of comprehensive geriatric assessment.
4. Management to avoid unnecessary environmental or physiological stresses which may precipitate delirium.
5. Discharge arrangements which function across the hospital/community interface and begin early in admission.

The Clayton unit has recently closed and the model of care at the Dandenong site is thought to have gradually eroded through time and relocation.

The Centre for Clinical Effectiveness was requested to conduct a review of the evidence of the effectiveness and cost effectiveness of this model of care.

Clinical Questions

For older patients with acute illness does treatment in an Acute Care of the Elderly Unit, as compared to usual care, result in improved clinical outcomes?

For older patients with acute illness is treatment in an Acute Care of the Elderly Unit, as compared to usual care, cost-effective?

Methods

Study Selection Criteria

Patient

Inclusion: Older people with acute illnesses

Exclusion: Older people admitted to intensive care or critical care units

Intervention

Inclusion: Acute Care of the Elderly units (ACE units), Acute Medical Units for the Aged (AMUA), or any interdisciplinary model of acute care directed specifically at older people that includes at least 4 of the following five key features of ACE units as described by SH:

1. Defined physical environment.
2. Admission processes which identify the problem and target the appropriate assessment processes.
3. Assessments which incorporate the principles of comprehensive geriatric assessment.
4. Management to avoid unnecessary environmental or physiological stresses which may precipitate delirium.
5. Discharge arrangements which function across the hospital/community interface and begin early in admission.

Exclusion: Residential, hostel or nursing home care. Inpatient rehabilitation.

Comparison

Inclusion: Care in a general medical ward

Exclusion: Residential, hostel or nursing home care. Inpatient rehabilitation.

Outcomes	Inclusion: All outcomes			
	Exclusion: None			
Study Type	Any comparative study*	Publication Date	1990 onwards	Language English

* Only studies for which full text has been published were included.

Search Strategy

Evidence Source	Date of Search or Issue searched
All EBM (Ovid) *	1 st August 2008
Medline (Ovid)	1 st August 2008
CINAHL (Ovid)	1 st August 2008
EMBASE	1 st August 2008

*(including The Cochrane Database of Systematic Reviews, DARE, CENTRAL and HTA)

Search Terms in Medline

Patient	1. (aged or geriatric or elder\$ or older or ACE or AMUA or GAU).ti.
Intervention	2. (unit\$ or service\$ or model\$).mp. or Hospital Units/ 3. (acute\$ and (care\$ or manag\$ or treat\$)).mp.
Comparison	-
Outcomes	-
Strategy	1 and 2 and 3

Data Collection & Analysis

Studies were selected and appraised by one reviewer, in consultation with colleagues, using study selection and appraisal criteria established a priori.

We did not search guideline websites due to the difficulties in applying inclusion and exclusion criteria to guidelines potentially addressing this topic.

Results

A search of Medline, CINAHL, EMBASE and All EBM resulted in 3,213 returns. Removal of obvious duplicate results left 2,103 titles for review. 1,886 articles were excluded based on title leaving 217 abstracts for review.

Of the remaining 217 titles, abstracts, and where necessary full text, were reviewed and inclusion and exclusion criteria applied. Three randomised controlled trials have been included in this review, two from the United States (US) and one from Norway.

The first trial in the United States was conducted in a teaching hospital in Cleveland, Ohio, between 1990 and 1992. The results of this trial are reported in two papers, Landefeld et al who report functional outcomes¹ and Covinsky et al who report cost outcomes.⁶ These two papers are appraised together and will be referred to as the Landefeld study in this review.

This same research group replicated and evaluated their model in a larger study population in a community hospital in Cleveland, Ohio, between 1994 and 1997. This study is reported in the paper by Counsell et al.²

The Norwegian trial was conducted in the University Hospital of Trondheim between 1994 and 1995 and is described as a Geriatric Evaluation and Management Unit (GEMU) however the description of the intervention meets the criteria that were set for an ACE unit. Saltvedt et al have reported different trial outcomes in at least four papers.^{3, 7-9} One of these papers, on pharmacy prescribing, was not retrieved through our search strategy but was picked up through reference lists. These papers have been appraised as a group.

Appraisals of the quality of the included studies can be found in Appendix 1.

Mortality

Neither the Landefeld or Counsell studies found any significant difference in mortality between the intervention and control groups during the hospital stay^{1, 2} however the Saltvedt study found a decrease in mortality in patients treated in their intervention ward (6% vs 13%, p=0.002). This difference was sustained at three and six months follow-up but no longer significant at 12 months follow-up.³

Length of Stay (LOS)

There was no significant difference in LOS between intervention and control groups reported in the Landefeld or Counsell studies.^{1,2} Saltvedt found that patients in the intervention unit had a significantly longer median length of stay (19 days, interquartile range 13-30 vs 13 days, interquartile range 7-18, $p<0.001$) and suggested this was a outcome of the more comprehensive assessment process.³ The US studies reported much shorter LOS in general (six to eight days).

Functional ability and well-being

The Landefeld study showed significant differences between the intervention and control groups in several measures of functional ability¹:

- At discharge 21% of the intervention group were classified as better at performing ADLs compared to 13% of the control group ($p=0.009$). This difference remained significant in a multivariable model controlling for potentially confounding baseline characteristics ($p=0.04$). The intervention group also had a higher level of function in ADLs than they did two weeks before admission ($p=0.05$).
- More patients in the intervention group improved in the individual ADLs of ability to bathe and dress themselves performed between admission and discharge ($p=0.006$) and fewer became worse ($p=0.02$). Differences in ability to move from a bed to a chair and ability to use a toilet were not significant.
- Subgroup analysis showed an improvement in ADLs at discharge amongst patients under 80 years of age ($p=0.03$), those who performed < five basic activities two weeks prior to admission ($p=0.04$) and patients with an APACHE II score of 0-14 ($p=0.02$). The difference in ADLs at discharge for patients over 80 years, those who performed 5 ADLs two weeks prior to admission and those with APACHE II scores of ≥ 15 were not significant.
- Intervention patients rated their overall health status at discharge as better ($p<0.001$) and this remained significantly better after controlling for health status at admission ($p=0.01$).

However several of these comparisons are to the level of functional ability two weeks prior to admission that patients or their family members recollected at the time of admission. These measures are very subjective and open to recall bias.

There was no significant difference in change from admission to discharge in instrumental activities performed, ability to walk or mental status score and at three months post discharge there was no difference between the intervention and control patients in the mean number of basic ADLs the patient could perform independently or the mean number of instrumental ADLs.¹

Counsell et al did not show a difference between groups in change from baseline (two weeks prior to admission) to discharge or admission to discharge in the number of independent ADLs performed. Changes in self-reported mobility were also similar between groups.² Generalised Estimating Equation (GEE) analysis showed differences between groups for ADL decline from baseline to 12 months follow-up which favoured the intervention group ($p=0.04$). Fewer intervention patients experience the composite outcome of either ADL decline from baseline or nursing home placement at discharge by GEE analysis ($p=0.03$). Estimates of effect size were not provided.²

Saltvedt et al did not find a significant difference between groups in measures of functional ability, depression and general well being at three, six or 12 months follow-up. This was a direct comparison between groups and not a comparison of change from baseline. Baseline results of these assessments are not presented and though randomisation is expected to distribute characteristics roughly equally we cannot be sure if any differences existed between the groups at the beginning of the intervention.⁷

Process of care

The Counsell paper reports that many processes of care differed between the intervention and control units. There were significant differences, in favour of the intervention groups, in the number of nursing care plans initiated (1.5 ± 1.2 vs 0.7 ± 0.8 , $p<0.001$), the days till first mention of discharge planning (2.4 ± 3.6 vs 3.5 ± 4.1 , $p<0.001$), the number of days till social work consult (3.2 ± 3.9 vs 3.9 ± 5.1 , $p=0.05$), the days to new activity order (1.7 ± 1.7 vs 2.5 ± 2.2 , $p=0.001$), the days to physical therapy consult (2.6 ± 3.4 vs 3.2 ± 3.4 , $p=0.03$), the number of patients restrained (2% vs 6%, $p=0.001$) and the number of shifts where restraints were used (3.1 ± 4.1 vs 10.5 ± 17.5 , $p=0.01$), the number of patients receiving high-risk medication in the first 24 hours (6% vs 10%, $p=0.008$). There was no difference in the number of patients receiving high risk medication at discharge or the number of patients or shifts where urinary catheterisation was used.²

Discharge diagnoses

Saltvedt et al found that significantly more patients in the intervention group had psychiatric diagnoses at discharge (38% vs 7%, $p<0.001$) and suggested that this would be a function of the more comprehensive assessments carried out in the intervention ward as compared to the control ward.³

Discharge Destination

The Landefeld study found that more intervention patients were discharged to home rather than to long term care (86% vs 78%, $p=0.01$).¹ Saltvedt et al found that comparable numbers of patients were discharged to their homes, to

rehabilitation institutions and to nursing homes. They also reported that the hazard ratio of living at home versus living in a nursing home or having died was 2.1 (95%CI 1.3, 3.4) at three months and 1.6 (95%CI 1.1, 2.5) at six months showing that intervention patients had significantly more chance of living at home after discharge.⁸ This outcome however is largely due to the reduced mortality in the intervention group and so should be interpreted with caution. Additionally 12 month data is not reported for this outcome though the study had 12 months of follow-up as shown in functional outcomes.

Drug profile

Analysis of medication regimes of patients in the Saltvedt study, as extracted from medical records, showed differing drug profiles. At discharge no difference was found between median numbers of drugs used per patient or with number of patients with polypharmacy (defined as daily use of five or more drugs).

In the time from admission to discharge the median number of scheduled drugs withdrawn per patient was reported to be significantly higher in the intervention group (median 2 [range 0 to 6] vs median 1 [range 0 to 6], $p=0.005$) and the median number of scheduled drugs started was also reported to be higher in the intervention group (median 1 [range 0 to 5] vs median 1 [range 0 to 6], $p=0.03$).

There was no difference found between the groups in the number of potential drug-drug interactions (DDIs) at admission, however at discharge the intervention group had a lower number of potential DDIs (28% vs 43%, $p=0.009$).

Though there was no difference in prescribing of particular classes of drugs at admission, at discharge more patients in the intervention group had been withdrawn from drugs with anticholinergic effects (14 vs 2, $p=0.003$), cardiovascular drugs (60 vs 13, $p<0.001$), digitalis glycosides (14 vs 0, $p<0.001$), beta receptor antagonists (8 vs 1, $p=0.02$), psychotropic drugs (33 vs 9, $p=0.005$) antipsychotic drugs (14 vs 2, $p=0.009$). Patients in the intervention group were also started on significantly more psychotropic drugs (35 vs 16, $p=0.02$) and antidepressants (20 vs 2, $p<0.001$).⁹

Cost of care and health resource utilisation after discharge

Neither the Landefeld nor Counsell studies found any significantly increased costs for in hospital patient care for those in the intervention group. Both studies included intervention costs including increased staff salaries and the costs of renovating a ward. This was based on the intervention being implemented for a minimum of five years in the Landefeld study and though a time is not given in the Counsell study as this is the same research group use of the same time period is likely.^{1,2} Landefeld conducted a sensitivity analysis which showed that the cost of the intervention program would need to be 220% greater before the mean cost of intervention patients began to exceed the mean cost of control patients.¹ Saltvedt did not report costs for hospital care.

Only one study found any difference in resource use after the initial hospital admission. Landefeld found that fewer intervention patients lived in long-term institutions at any time in the three months post-discharge (67 vs 90, $p=0.03$).

Otherwise there were no significant differences found in:

- Numbers readmitted to acute care at one month,² three months¹ or six months.⁸
- Receiving home healthcare services at one month,² three months¹ or six months.⁸
- Admission to long-term institutions during the first three months of those initially discharge to home.¹
- Nursing home admission in the following year.²
- Times spent in hospitals or nursing homes at six months.⁸

Patient and provider satisfaction

The Counsell study measured patient, care-giver, physician and nurse satisfaction through interviews and survey. The study found that satisfaction was significantly higher in the intervention group. Given the lack of blinding and the lack of information about methods of measuring satisfaction, this result should be interpreted with caution.

Discussion

This evidence report has attempted to review the best available evidence evaluating the use of Acute Care for Elders Units (ACE Units) as a model of acute care for older patients in hospital.

We found several systematic reviews of varying quality reviewing various forms of acute care for the elderly, however as we were unsure if all trials included in the identified systematic reviews met the criteria that we had defined for an ACE unit, we decided to assess the articles individually for inclusion in our review, rather than appraise the reviews and meta-analysis.^{4,10,11} Additionally, these reviews sometimes combined different models of care, for example, Parker et al combined ACE Units with Geriatric Assessment Units (GAUs) due to their 'similarity of focus, philosophy and design'.⁴ However the GAUs they included in their discussion did not meet our inclusion criteria. These documents were used, in addition to our database searches, to identify potentially relevant studies.

A major limitation of this review is the difficulty in searching for and assessing models of care. While we are confident that any randomised studies involving designs called Acute Care for the Elders or ACE Units will have been picked up in our search strategy, though we searched broadly, it is possible that differently named models that may have fit the

criteria but been poorly described, have been missed.

Parker et al, when reviewing the best place of care for older people after acute and during sub-acute illness, spent over six months scoping the review.⁴ While our topic is narrower, the shorter timeframe for this review has limited our exploration of papers that may not have explained their model of care in sufficient details.

Some papers compared acute geriatric units to acute units not specified as geriatric however, lack of description of any differences in the model of care between these units meant the studies could not be included. For example, Flamer et al described a Melbourne study where a hospital with three acute units had one acute unit staffed by geriatric specialists however there is no description of any other differences between the model of care in the unit staffed by geriatric specialists and the other two acute units.¹²

The suite of papers by Saltvedt reporting on a GEMU was included in this review as the description of the model of care met the criteria for inclusion as an ACE unit.^{3, 7-9} In fact the authors of this study mention Landefeld¹ and Counsell² when discussing their results, suggesting that they see their model of care as similar to the ACE units. However, many other trials of GEMU units were excluded and potentially, if models of care were described in more detail, there may be more GEMUs that meet our criteria for an ACE unit.

Our goal is to review the best available evidence. This means that if a particular outcome is not reported from RCTs we would examine a lower level of evidence such as a cohort study, however if we find reports of an outcome in both a RCT and a lower level of evidence such as a cohort or a case-control study, only the RCT will be appraised.

As a result of this process there may be studies of ACE units that the reader may expect to find in this review which have been excluded.

For example, a cohort study by Edwards et al examining potentially inappropriate drug prescribing in geriatric inpatients comparing an ACE unit to a general medicine service¹³ has not been appraised as in their RCT Saltvedt et al reported on drug prescribing.⁹ Similarly, a costing study by Jayadevappa¹⁴ was also excluded as it was a case-control study reporting costs and not as rigorous a study design as the study whose costs were reported in Covinsky et al.⁶

Two final limitations to consider when examining the evidence for ACE units are the use of 'usual care' as a comparator and the quality of the studies.

Most studies take some care to describe the intervention model of care, which as discussed previously, may still not be detailed enough for an appropriate assessment of inclusion. The comparator for this review is 'usual care'. This is often not reported well. Studies may say that the intervention was not implemented in the usual care or general medical wards but lack of description of usual care makes it difficult to generalise the outcomes of these studies to an Australian setting.

The included studies have been assessed to be of moderate quality. The criteria we use to appraise RCTs are a standard set of criteria used at the Centre for Clinical Effectiveness developed over time and in consultation with other critical appraisal resources. The fulfilment of criteria is then used to assess the overall quality of the study. Some criteria, particularly those regarding blinding of patients and care providers, are more difficult to apply in a model of care situation than they would be in another RCT such as a RCT of a drug treatment. Thus, although the studies have been assessed as of moderate quality we are mindful of how difficult it would be to implement a high quality RCT in a model of care setting.

Only three studies met the inclusion and exclusion criteria defined for this report. Two of the three studies were reported in more than one paper and in these cases the papers have been appraised together. Appraisals in the appendix therefore refer to studies rather than individual articles.

Reports of functional abilities in patients treated in ACE units have varied. Two key studies of ACE units by the same research group showed different effects of the intervention on functional ability.^{1, 2} Landefeld et al showed an improvement in functional ability at discharge and this improvement was used to calculate the sample size required for the study reported by Counsell et al who did not show an improvement in functional ability at discharge. Counsell et al suggest that this could be explained by a healthier study population (better overall health status on admission, lower APACHE II scores and lower hospital mortality), decreased hospital length of stay between the two studies, improvements in usual care over the four years between studies and different organisation of attending physicians (limited resident coverage). The study by Counsell et al did however show an improvement in the intervention group in the composite outcome of either ADL decline from baseline or nursing home placement at discharge. It is likely that this composite outcome was not defined a priori but rather examined when the predetermined outcome of number of independent ADLs at discharge did not show any difference. Counsell also showed that differences between groups for ADL decline from baseline to 12 months follow-up favoured the intervention group.

Length of stay of patients in ACE units was shown to be no longer or shorter than in usual care according to the Landefeld¹ and Counsell² studies however the Saltvedt study showed a significantly longer LOS in the intervention group.³ Additionally, where Saltvedt showed a decrease in mortality in the intervention group neither the Landefeld nor Counsell studies showed any difference in mortality. This should be considered in conjunction with the difficulties in assessing models of care of studies to include in this review and the fact that although Saltvedt compare their study to the Landefeld and Counsell studies they also call themselves a Geriatric Evaluation and Management Unit (GEMU)

rather than an ACE unit.

Process of care and drug profile was significantly different in the ACE units^{2,9}. These results seem unsurprising given that the model is designed to improve management of patients in these areas however the information is useful as a measure of process evaluation of the intervention. Patient and provider satisfaction were measured by Counsell et al and found to be significantly better amongst patient and staff in the ACE unit however more information on the validity of these measures is required before making any judgement on this aspect of the ACE model.²

Costs of health care, from a hospital perspective, showed treatment of older patients in ACE units to be no more or less expensive than treatment in a usual care ward.^{2,6} No comprehensive cost-effectiveness study of the ACE model of care was found.

Though many of the reported study outcomes showed no difference between intervention and control groups it is important to take note of outcomes for which the studies were powered. It is unclear what outcome the Landefeld study was powered to detect.¹ The Saltvedt study was powered to detect a difference in mortality³ and the Counsell study was powered to detect a difference in the mean number of independent activities of daily living at discharge.² When no difference is found in a secondary outcome for which the study was not powered, as is the case in several outcomes of the studies included in this review, then we cannot be certain if there really is no difference or if the study was insufficiently powered to detect such a difference, and we cannot rule out that a difference, in either direction, really exists.

Conclusions

There is some moderate quality evidence to suggest that an ACE model of care may reduce mortality, improve functional ability and increase the number of patients discharged to home rather than long-term care in selected populations of older patients. ACE units show significant differences in process of care and drug profiles of the patients.

A comprehensive cost-effectiveness study of ACE units was not found however those studies that analysed costs from a hospital perspective found that there was not significant difference in the cost of treating patients in an ACE unit as compared to usual care.

References

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Appendix 1 – Appraisal of Included Studies

Articles:

Landefeld CS, Palmer RM, Kresevic DM, Fortinsky RH and Kowal J. A randomized trial of care in a hospital medical unit especially designed to improve the functional outcomes of acutely ill older patients. *New England Journal of Medicine* 1995; 332: 1338-44.

Covinsky KE, King JT, Quinn LM, Siddique R, Palmer R, Kresevic DM, Fortinsky RH, Kowal J and Landefeld CS. Do acute care for elders units increase hospital costs? A cost analysis using the hospital perspective. *Journal of the American Geriatrics Society* 1997; 45(6): 729-34.

Note: These two articles report the results of one randomised controlled trial. Landefeld et al describe the intervention fully and report patient outcomes while Covinsky et al report on the costs of the intervention. As these report the same trial they have been appraised together.

Description of study

Patient/population	Patients aged 70 years or older admitted for general medical care
N	Intervention n=327, Control n=324
Setting	University Hospitals of Cleveland, Ohio, United States.
Intervention/indicator	<p>Treatment via admission to an Acute Care for Elders Unit. Features of the unit were well described as:</p> <ul style="list-style-type: none"> • A prepared environment, including carpeting, handrails, uncluttered hallways, large clocks and calendars, elevated toilet seats and levered door handles. • Patient-centred care including daily assessment of physical, cognitive and social function, protocols to manage geriatric syndromes and daily rounds by a multidisciplinary team. • Early, ongoing discharge planning with an emphasis on returning home and involvement of social worker and home care nurse if necessary. • Medical care review daily, including protocols to minimise the adverse effects of selected procedures such as urinary catheterisation and certain medications. <p>Funding allowed for increased hours of medical and nursing directors, social worker, physical therapist, occupational therapist and dietician.</p>
Comparison/control	Usual care in a general medical ward. The four elements described in the intervention were not implemented in the control ward.
Outcomes	<p>Main outcome was change from admission to discharge in the number of basic activities of daily living the patient could perform independently (bathing, dressing, toileting, eating and moving from bed to a chair).</p> <p>Other outcomes reported included:</p> <ul style="list-style-type: none"> • Instrumental activities of daily living (shopping, cooking, performing household chores, using transportation, managing money, managing medication and using the telephone) • Length of stay • Hospital charges • Outcomes after discharge <p>Subgroup analysis was performed according to:</p> <ul style="list-style-type: none"> • Age • Ability to perform basic and instrumental activities of daily living two weeks before admission • Charlson comorbidity score at admission • APACHE II score at admission <p>Costs for patient care were obtained from the standard health care financing form and compared with the hospital billing system. Costs for establishment of the unit and the ongoing maintenance costs were estimated. A cost-analysis was undertaken from a hospital perspective.</p>
Inclusion Criteria	Patients 70 years or older admitted for general medical care.
Exclusion Criteria	Patients under 70 years of age and those admitted to a specialty unit such as intensive care, cardiology or oncology.

Study Validity

Were there any conflicts of interest in the writing or funding of this study?	Not reported	Funding sources for the study are stated as being from a range of philanthropic, Government and professional association sources
Does the study have a clearly focused question?	Yes	The aim of the study is to examine whether an Acute Care for Elders model of care in a hospital can improve overall outcomes for a heterogeneous group of older patients. The patient population, intervention, comparison and outcomes are clearly described in the methods section.
Is a RCT the appropriate method to answer this question?	Yes	Yes, a RCT is an appropriate method for examining this intervention.
Does the study have specified inclusion/exclusion criteria?	Yes	Inclusion and exclusion criteria are clearly specified in the methods section.
Were the criteria used the select patients for inclusion appropriate?	Yes	The inclusion and exclusion criteria appear to be appropriate; however elderly patients were defined as 70 years and older. The definition of elderly may differ between health service and cultures. Also of the 1794 eligible patients, 1143 were not enrolled as a bed was not available in both the intervention and control units at the time of their admission.
Did the study have an adequate method of randomisation?	Yes	Randomisation was according to computer-generated random numbers. If there was not a bed available in both the intervention and control ward at the time of randomisation the patient was not enrolled in the study.
Was allocation to intervention group concealed?	Not reported	There is insufficient detail regarding the process of allocation to determine if allocation was concealed. Though the methods state that computer-generated random numbers were used we do not know whether the randomisation sequence was readable prior to allocation.
Were patients blind to intervention group?	Not reported	Blinding or otherwise of patients is not reported and given the nature of the intervention, is unlikely. Patients (or their proxies) gave informed consent and it is not reported how much they were told about the models of care.
Were investigators and care providers blind to intervention group?	No	The methods state that "The staff of the intervention unit was not involved in the care of the patients receiving usual care" however they also state that "attending physicians and resident physicians provided care to patients in both the intervention and usual-care groups". There is no suggestion in the paper that any staff were blinded to the intervention, and given the nature of the intervention it is unlikely that this could have been achieved.
Were outcome assessors blind to intervention group?	No	Medical record data was recorded by clinicians who were not blinded. Interviews were conducted by research assistants who were not blinded to patients' group assignment. Outcomes consisted of data from medical records and interviews covering sociodemographic characteristics, ability to perform various activities and items from a geriatric depression scale and the Mini-Mental State Examination. Though some outcomes such as mortality and destination at discharge are objective, others such as functional ability are more subjective.
Aside from the experimental intervention, were the groups treated the same?	Not applicable.	The intervention forms the entire model of care for this study, so all treatment forms part of the intervention. There may be a weakness in the degree to which the details of all aspects of care can be documented, particularly as this may vary between staff members, etc.

Was there sufficient duration of follow-up?	Yes	Measures and interviews were conducted at admission at discharge and at three months after discharge, however only 60-70% of participants were included in each round of interviews. Three months after discharge further utilisation of medical services (readmission to hospital, home care and long-term care) was measured as was basic activities of daily living and instrumental activities of daily living.
All outcomes were measured in a standard, valid and reliable way?	Partial	<p>Outcomes were measured from hospital records and by interview.</p> <p>Interrater reliability for data obtained by interview was assessed for 1.5% of the sample. The mean kappa statistics were 0.98 for basic activities of daily living, 0.94 for the instrumental activities of daily living, 0.96 for the mental-status items and 0.99 for depression items. Kappa is not necessarily the most appropriate method to measure interrater reliability.</p> <p>At admission, patients and their families were asked to recall the patient's functional status and overall health 2 weeks prior to admission. This data is very subjective and open to recall bias and was used for many comparisons.</p> <p>Cost data was obtained for both groups from hospital financial records and included direct costs (ie. nursing salaries) and indirect costs (ie. building maintenance) and fixed and variable costs (ie. costs that may or may not be dependent on volume). These costs are an objective measure of costs. The costs of developing and maintaining the intervention (for example modification of a ward) were estimated and added to the interventions patients' hospital costs.</p>
Were outcomes assessed objectively and independently?	Partial	<p>Some outcomes were assessed on standardised scales (ie. the Mini-Mental Status Exam) and information such as mortality and data taken from hospital records was assessed objectively.</p> <p>Assessment of basic activities of daily living and instruments of daily living were assessed as a number items performed independently. This may be subjective.</p> <p>Self report of overall health status as excellent, good, fair or poor is subjective.</p> <p>The lack of blinding and fact that many of these tests were undertaken by the same researcher may mean that the results of the tests are not independent.</p>
Was the study sufficiently powered to detect any differences between the groups?	Yes	<p>No sample size calculation is reported, however there was a significant difference reported in the main outcome so the study was sufficiently powered for this outcome.</p> <p>Ability of the study to detect changes in other outcomes is uncertain. Though several outcomes show a significant difference between intervention and control groups, where there is no difference observed we can not rule out a change.</p>

Was the statistical analysis appropriate?	Yes	<p>Statistical analysis was well reported in the methods section.</p> <p>Differences between groups in ability to perform ADLs independently were examined using a chi-square test for linear trend. The Wilcoxon rank sum test was used to evaluate continuous variable and the chi-square test was used for categorical variables.</p> <p>Subgroup analysis was performed based on age, ability to perform ADLs two weeks prior to admission, comorbidity score and APACHE II score at admission. It is unclear whether these subgroups were established a priori.</p> <p>Logistic regression was used to control for potential confounders.</p> <p>Where data was missing or where patients were deceased, analysis was performed only for those patients where an outcome was reported for the variable. Though no patients dropped out of the study there is substantial missing data for outcome variables.</p> <p>Missing data is reported in Tables three and four of the Landefeld paper on page 1342. Missing data ranged from 0% for some outcomes to 14% of intervention and 16% of control patients for overall health status at discharge to over 40% for depression score and mental status score at discharge. It is not reported what percentage of intervention and control groups had missing depression and mental-status scores at discharge.</p>
Were the groups similar at baseline with regards to key prognostic variables?	Yes	<p>The patients in the intervention and control groups were similar at baseline. Baseline characteristics are reported in Table 2 on page 1340 of the Landefeld paper. Characteristics reported include age, sex, living situation, overall health and activities being performed prior to admission, main reason for admission, coexisting conditions, Charlson comorbidity score, APACHE II score, mental-status score and depression score.</p> <p>Patients assigned to the intervention group reported a better overall health status at admission. 44% of control patients reported this as poor compared to 36% of intervention patients.</p>
What percentage of the individuals recruited into each arm of the study dropped out?	6% of all patients	<p>At three month follow up of resource use 14% of intervention patients and 13% of usual care patients were deceased and 6% of patients were lost to follow up. Loss to follow up is not reported according to intervention or control group. It is not clear whether resource use of deceased patients and patients lost to follow up was examined through hospital records or via interviews with proxies.</p>
Were all the subjects analysed in the groups to which they were randomly allocated (ie intention to treat analysis)?	Yes	ITT analysis is not mentioned, however there is no indication that patients were swapped between wards
Is the paper free of selective outcome reporting?	Partial	We do not know what outcomes were specified in the original protocol so we cannot be certain that all outcomes were reported.
Other		
What is the overall risk of bias?	Moderate	This study has a moderate risk of bias due to the lack of blinding of health care providers and outcome assessor and the potentially subjective nature of some of the outcome assessments. However, it should be considered that when examining something complex like a model of care as opposed to a more simple drug intervention, it would be harder to fulfil some of the criteria of a rigorous RCT.

Results

At discharge:

Twenty-four patients in each group died. There was no difference in mortality between those cared for in the ACE unit and in usual care.

Mean length of stay (LOS) was one day shorter for intervention patients (7.3 vs 8.3 days) though this difference was not statistically significant. Mean LOS reported in the two papers differs slightly but the results are not affected by this reporting discrepancy.

The change in performance of activities of daily living was classified as better if the number of activities performed at discharge was more than the number performed at admission and worse if this number decreased. At discharge the 21% of the intervention group were classified as better at performing ADLs compared to 13% of the control group ($p=0.009$). This difference remained significant in a multivariable model controlling for potentially confounding baseline characteristics ($p=0.04$). The intervention group also had a higher level of function in ADLs than they did two weeks before admission ($p=0.05$).

In regards to changes in individual ADLs from admission to discharge more patients in the intervention group improved in ability to bathe and dress themselves ($p=0.006$) and fewer became worse ($p=0.02$). Differences in ability to move from a bed to a chair and ability to use a toilet were not significant.

Subgroup analysis showed an improvement in ADLs at discharge amongst patients under 80 years of age ($p=0.03$), those who performed less than five basic activities two weeks prior to admission ($p=0.04$) and patients with an APACHE II score of 0-14 ($p=0.02$). The difference in ADLs at discharge for patients over 80 years, those who performed 5 ADLs two weeks prior to admission and those with APACHE II scores of ≥ 15 were not significant.

Improvements were also seen in secondary outcomes in the intervention group:

- More intervention patients were discharged home rather than to long-term care ($p=0.01$).
- Intervention patients rated their overall health status at discharge as better ($p<0.001$) and this remained significantly better after controlling for health status at admission ($p=0.01$).

There was no significant difference in change from admission to discharge in instrumental activities performed, ability to walk or mental status score.

Three months post discharge:

Less intervention patients used long-term care in the three months post-discharge ($p=0.03$)

At three months post discharge there was no difference between the intervention and control patients in the mean number of basic ADLs the patient could perform independently or the mean number of instrumental ADLs.

Of those patients discharged to private homes there was no difference between intervention and control patients in numbers admitted to long-term institutions during the three months after discharge however, overall fewer intervention patients lived in long-term institutions at any time in the three months post-discharge (67 vs 90, $p=0.03$)

There was no difference between intervention and control groups in numbers readmitted to acute care or receiving home healthcare services.

Cost analysis:

Development and maintenance costs of the intervention added US\$38.43 per bed day to the intervention patients' hospital costs.

Cost per day per patient was US\$876 in the intervention group and US\$847 in the control group (not a statistically significant difference). When combined with a non-statistically significant reduction in LOS for intervention group patients, total costs to care for intervention group patients were US\$6608 compared to US\$7240 for usual care patients.

Subgroup analysis according to age, ADL status on admission, functional status at admission and functional change between admission and two weeks prior did not show a difference in costs between the groups.

Sensitivity analysis showed that the cost of the intervention program would need to be 220% greater (or US\$123) before the mean cost of intervention patients began to exceed the mean cost of control patients.

Author's Conclusions

"Specific changes in the provision of acute hospital care can improve the ability of a heterogeneous group of acutely ill older patients to perform basic activities of daily living at the time of discharge from the hospital and can reduce the frequency of discharge to institutions for long-term care"

"Three months after discharge the groups did not differ in their ability to perform basic or instrumental activities of daily living".

"Caring for patients on an intervention ward designed to improve functional outcomes in older patients was not more expensive to the hospital than caring for patients on a usual-care ward even though the intervention required a commitment of hospital resources"

Our comments/summary

This article examined a well defined model of acute care for elders and showed that the intervention resulted in significant improvements in several patient outcomes. Though the rigour of the study could have been improved by blinding of outcome assessors, implementing a different model of care within a hospital has understandable difficulties in terms of allocating staff resources. It is unlikely an intervention such as this could be implemented with blinding of healthcare providers.

The cost analysis is from a hospital perspective. From a more societal view a full cost-effectiveness analysis would take into account health resources post-discharge including long-term care, other medical bills and community services that may be required. The study was not designed to test cost effectiveness.

Articles:

Saltvedt I, Opdahl Mo E, Fayers P, Kaasa S and Sletvold O. Reduced mortality in treating acutely sick, frail older patients in a geriatric evaluation and management unit. A prospective Randomized Trial. *Journal of the American Geriatric Society* 2002; 50: 792-798.

Saltvedt I, Saltnes T, Opdahl Mo E, Fayers P, Kaasa S and Sletvold O. Acute geriatric intervention increases the number of patients able to live at home. A prospective randomized study. *Aging Clinical and Experimental Research* 2004; 16: 300-306.

Saltvedt I, Spigset O, Ruths S, Fayers P, Kaasa S and Sletvold O. Patterns of drug prescription in a geriatric evaluation and management unit as compared with the general medical wards: a randomised study. *European Journal of Clinical Pharmacology* 2005; 61: 921-928.

Saltvedt I, Jordhoy M, Opdahl Mo E, Fayers P, Kaasa S and Sletvold O. Randomised trial of in-hospital geriatric intervention: Impact on function and morale. *Gerontology* 2006; 52: 223-230.

Note: These four articles report different outcomes of a single trial and thus have been appraised as one study.

Description of study

Patient/population	Acutely sick, frail elderly patients, 75 years or older who were admitted as emergencies to the Department of Internal Medicine of the hospital.
N	Intervention n=127, Control n=127
Setting	University Hospital of Trondheim, Norway.

Intervention/indicator	<p>Transfer to a Geriatric Evaluation and Management (GEMU) ward. This intervention fits criteria of an ACE unit as it includes</p> <ul style="list-style-type: none"> • A defined physical environment and a communal activity/dining room. • Admissions processes identify the problem, ie. the inclusion criteria specifically targeted frail elderly patients. • A multidisciplinary team including a geriatrician, resident, nurses and allied health staff comprehensively assess all relevant illnesses and disabilities in order to provide appropriate treatment • Management to avoid precipitation of delirium through visible clocks and calendars, a focus on communication and access to news and prevention of iatrogenic conditions and complications which may include conditions and complications related to delirium. • Early discharge planning in collaboration with caregivers and home services.
Comparison/control	Continued treatment in the medical ward (MW) to which the patient had been admitted within the Department of Internal Medicine.
Outcomes	<ul style="list-style-type: none"> • Mortality and causes of death. • Destination after discharge. • Hospital and nursing home readmission after discharge. • Health care utilisation after discharge. • Impact on function, depression and general well being. • Appropriateness of drug prescribing.
Inclusion Criteria	<p>Patients aged 75 years or older with at least one of: acute impairment of single ADL, imbalance/dizziness, impaired mobility, chronic disability, weight loss, falls during last 3 months, confusion, vision or hearing impairment, depression, malnutrition, mild or moderate dementia, urinary incontinence, social or family problems, polypharmacy or prolonged bed rest.</p> <p>Acute stroke patients were admitted if the stroke unit was full.</p> <p>Eligible patients who had been recently admitted to the department of internal medicine were preferred over those who had been there longer.</p>
Exclusion Criteria	Patients in need of specific treatment in the unit to which they were already admitted, patients with cancer with metastasis, other disease with expected survival less than 6 months, known severe dementia prior to hospital admission, nursing home patients, those previously fully independent, those who seemed to recover quickly from the acute illness and those for whom discharge was planned in less than 3 days were all excluded.

Study Validity

Were there any conflicts of interest in the writing or funding of this study?	No	This research was funded by the Norwegian Ministry of Health and Social Affairs and the Research Council of Norway. One of the articles specifically states that no conflicts of interest have been declared.
Does the study have a clearly focused question?	Partial	Each paper presents a different outcome measure, however as the study was powered to detect a reduction in mortality we assume that the main study question was whether treatment in a GEMU unit as compared to care in a usual medical ward (MW) reduced mortality. We cannot be sure how many secondary outcome measures were proposed.
Is a RCT the appropriate method to answer this question?	Yes	RCT is an appropriate study design to examine this model of care.

Does the study have specified inclusion/exclusion criteria?	Partial	Though the study has listed inclusion and exclusion criteria the exclusion criteria of 'those who seemed to recover quickly from the acute illness' is potentially subjective and might introduce selection bias. In addition, for practical reasons stroke patients were admitted to the study if the stroke ward was full and a small number of patients under the age of 75 were admitted to the study as the hospital was unable to keep empty beds when faced with demand. Authors state that they have selected patients expected to benefit most from treatment in a GEMU.
Were the criteria used to select patients for inclusion appropriate?	No	Patients were screened for inclusion when a bed became available in the GEMU ward and then eligible patients were randomised. Eligible patients who had been recently admitted to the department of internal medicine were preferred over those who had been there longer, allowing the possibility of manipulating the inclusion of patients. Also 1167 admitted patients ≥ 75 years old were either not suitable for enrolment or were not enrolled as a bed was not available in the GEMU ward. The number who were suitable for enrolment is not reported separately.
Did the study have an adequate method of randomisation?	Yes	Permuted block randomisation with unknown and varied block size was performed by an independent research office to produce sealed, serially numbered opaque envelopes.
Was allocation to intervention group concealed?	Yes	Envelopes were opened in sequence with at least one independent witness present.
Were patients blind to intervention group?	Not reported	Blinding or otherwise of patients is not reported. Patients (or their proxies) gave informed consent and it is not reported how much they were told about the models of care. Blinding of patients is unlikely given the nature of the intervention.
Were investigators and care providers blind to intervention group?	No	Nurses, occupational therapists and physiotherapists who staffed the GEMU ward performed study related assessments in both study and intervention groups. It is unclear whether doctors worked on more than one ward but it is unlikely that they could be blinded to a different model of care within the hospital.
Were outcome assessors blind to intervention group?	No	Nurses, occupational therapists and physiotherapists performed study related assessments in both study and intervention groups. Research assistants extracting data on drug prescriptions are described as independent but it is not reported whether they were blind to intervention group.
Aside from the experimental intervention, were the groups treated the same?	Not applicable.	The intervention forms the entire model of care for this study, so all treatment forms part of the intervention. There may be a weakness in the degree to which the details of all aspects of care can be documented, particularly as this may vary between staff members, etc.
Was there sufficient duration of follow-up?	Yes	Assessment of drug use was at discharge and length of follow up was at least six months for other variables.
All outcomes were measured in a standard, valid and reliable way?	Partial	Some outcomes were measured in a reliable way (ie. date of death from death certificates, drug prescriptions from hospital records) however other outcomes, even though measured using validated scales, were not measured reliably. For example, the Mini-Mental Status Exam and Barthel Index for ADLs were found to have been applied differently in the control and intervention wards and therefore baseline data could not be used. The Mini-Mental Status Examination (MMSE) was used to assess mental status, the Barthel Index was used to assess ADLs and the Lawson Instrumental Activity of Daily Living instrument was used for instrumental ADLs. Montgomery and Asberg's Depression Rating Scale (MADRS) was used as a screening tool for symptoms of depression and the Philadelphia Geriatric Centre Morale Scale (PGCMS) was used to assess general well being.

Were outcomes assessed objectively and independently?	Partial	<p>Date and cause of death were recorded from death certificates. While mortality is objective, cause of death may not be.</p> <p>Readmissions were determined from hospital records and nursing home stays were determined from municipal records. Rehabilitation stays were determined from a combination of rehabilitation institution records and from the National registry.</p> <p>The hospital is both the regional hospital from Central Norway and the local hospital for 200,000 residents. It is unclear how many other hospitals, if any, service the area.</p> <p>The authors do not comment on the comprehensiveness of these records and it is unclear whether patients may have entered nursing homes, rehabilitation services or hospitals outside of the area.</p> <p>Information on drug regimes was extracted from medical records by two independent research assistants after the study was finished.</p> <p>The Norwegian Drug Information Database and the American Drug Interactions Analysis and Management Handbook were used to look for potential drug-drug interactions which were then classified as 1) should be avoided, 2) should be avoided but if used should entail specific precautions, 3) can be combined but should be monitored or 4) no action is needed. Inappropriate prescriptions were identified using Beer's criteria. This was conducted independently of the study.</p> <p>The lack of blinding and fact that some tests may have been undertaken by the same researcher may mean that the results of the tests are not independent.</p>
Was the study sufficiently powered to detect any differences between the groups?	Yes	<p>Based on a 30% mortality in the medical wards the study was powered to detect a 50% reduction in mortality in the GEMU ward with $\alpha=0.05$ and a power of 80%. This required 113 patients in each group. This sample size was attained as the study recruited 127 participants in each group.</p>
Was the statistical analysis appropriate?	Partial	<p>Analysis included the chi-square test for all categorical data. The Mann-Whitney U test for comparison of age, time in institutions and causes of death and functional scales (MMSE, ADLs, etc). Kaplan-Meier plots and the log rank test were used for survival analysis and to compare time to readmission to hospital and nursing home placement. The Breslow test was used for survival analysis in the first 12 months. To estimate the magnitude of treatment effect Cox's proportional hazard model was used for hazards ratios and to adjust for covariates.</p> <p>However, analysis of some variables is inappropriate. For example, presentation of functional scores such as MMSE and ADLs is a comparison of the two groups at various time points when a change in score from baseline would be more appropriate, particularly given that mortality rates were different in the two groups. Similarly, examining living arrangements after intervention and including nursing home residents and deceased together is questionable.</p>
Were the groups similar at baseline with regards to key prognostic variables?	Yes	<p>Reporting of baseline characteristics showed groups to be fairly similar at baseline. Slightly more patients in the GEMU lived alone and had had previous hospital diagnoses of infectious disease, gastrointestinal disease and cerebrovascular disorder. More patients in the control group had previous diagnoses of heart disease. Percentages of each group who met various selection criteria (ie. impairment of ADL, mobility issues, falls history, depression, confusion, polypharmacy) varied slightly but the median number and interquartile range of targeting criteria were the same.</p>

What percentage of the individuals recruited into each arm of the study dropped out?	0.8% treatment 1.5% control/ comparison	One patient in the intervention group and two patients in the control groups withdrew consent after the hospital stay and were lost to follow up.
Were all the subjects analysed in the groups to which they were randomly allocated (ie intention to treat analysis)?	Yes	Patients were analysed in the groups to which they were randomised.
Is the paper free of selective outcome reporting?	Partial	We do not know what outcomes were specified in the original protocol so we cannot be positive that all outcomes were reported. Of note, the results of the study have been reported in four different papers, published in 2002, 2004, 2005 and 2006. The 2002 paper reports mortality to two years post-discharge, the 2004 paper reports resource use and living arrangements to six months only and the 2006 paper reports ADLs and well-being to 12 months.
Other		
What is the overall risk of bias?	Moderate	This study has a moderate risk of bias as several of the quality criteria have not been fulfilled. Although it would be difficult to blind investigators and health care providers to treatment group, outcome assessors could potentially be blinded. Additionally the inclusion criteria for entry into the study could potentially be manipulated. The multiple publications and the varied length of reported follow-up of some variables suggest that we cannot rule out selective outcome reporting.

Results

Length of stay:

Median total length of stay for patients (including time before randomisation) was higher in the GEMU group at 19 days (interquartile range 13-30) compared to 13 days in the usual medical ward (MW) (interquartile range 7-18), $p < 0.001$.

Mortality:

Mortality was significantly reduced in the GEMU group during the hospital stay (6% vs 13%, $p = 0.002$), at 3 months (12% vs 27%, $p = 0.004$) and 6 months (16% vs 29%, $p = 0.02$) follow-up. At 12 months follow-up mortality was 28% in the GEMU group and 34% in the MW group ($p = 0.06$) and at two years mortality was approximately 50% in each group.

At 3 and 12 months heart disease as a cause of death was higher in the MW group but not significantly higher. Infectious disease as cause of death at 12 months was significantly higher in the GEMU group ($p = 0.04$). At baseline the MW had a higher proportion of previous diagnoses of heart disease and the GEMU groups had slightly higher previous diagnoses of infectious disease.

Discharge diagnoses:

At discharge significantly more patients in the GEMU group had a psychiatric diagnoses ($p < 0.001$) though this could be a function of the more comprehensive assessment in the GEMU ward as compared to the MW. No other differences were found in diagnoses at discharge though the study may not have been powered to detect such differences.

Discharge destination and resource utilisation after discharge:

After the index hospital stay no significant difference was found between groups in the proportions discharged to home or discharged to rehabilitation institutions or nursing homes. At six months follow-up no difference was found in hospital readmissions or time spent in hospitals or nursing homes or in the proportion of patients receiving assistance at home from community nurses.

The hazard ratio of living at home versus living in a nursing home or having died was 2.1 (95%CI 1.3, 3.4) at three months post-discharge and 1.6 (95%CI 1.1, 2.5) at six months. This is related to the reduced mortality in the intervention group.

Function, depression and well being:

No difference was found between groups in MMSE, MADRS, ADL, instrumental ADL, PGCMS or Barthel scores at follow-up at 3, 6 or 12 months. However, results reported are not expressed as change from baseline and do not take into account the effect of differing mortality rates in the groups and are therefore difficult to interpret.

Drug profile:

Other than more patients in the MW using cardiovascular drugs ($p=0.02$), there was no significant difference found in drug profile at study inclusion.

At discharge there was no difference found between median number of drugs used per patient or number of patients with polypharmacy (defined as daily use of five or more drugs).

In the time from admission to discharge the median number of scheduled drugs withdrawn per patient was reported to be significantly higher in the intervention group (median 2 [range 0 to 6] vs median 1 [range 0 to 6], $p=0.005$) and the median number of scheduled drugs started was also reported to be higher in the intervention group (median 1 [range 0 to 5] vs median 1 [range 0 to 6], $p=0.03$).

There was no difference found between the groups in the number of potential drug-drug interactions (DDIs) at admission, however at discharge the intervention group had a lower number of potential DDIs (28% vs 43%, $p=0.009$).

Though there was no difference in prescribing of particular classes of drugs at admission, at discharge more patients in the intervention group had been withdrawn from drugs with anticholinergic effects (14 vs 2, $p=0.003$), cardiovascular drugs (60 vs 13, $p<0.001$), digitalis glycosides (14 vs 0, $p<0.001$), beta receptor atagonists (8 vs 1, $p=0.02$), psychotropic drugs (33 vs 9, $p=0.005$) antipsychotic drugs (14 vs 2, $p=0.009$). Patients in the intervention group were also started on significantly more psychotropic drugs (35 vs 16, $p=0.02$) and antidepressants (20 vs 2, $p<0.001$).

Author's Conclusions

“Treatment of acutely sick, frail, older patients in a GEMU substantially reduced mortality”

“The results indicate the overall positive treatment effect of acutely sick, frail elderly in a GEMU, ie. patients treated in the GEMU had increased possibilities of living in their own homes, an effect that was mainly related to considerable reduced mortality in the GEMU group”.

“Treatment in the GEMU had no measurable beneficial impact on function, morale or symptoms of depression. Taking the previously shown mortality reduction into consideration an additional effect on function was less likely and the overall treatment effect was considered to be positive”.

“Drug treatment in the GEMU as compared with the MW was more appropriate in terms of prescription of fewer drugs with anti-cholinergic effects and fewer potential DDIs. There were distinct differences in treatment patterns of cardiovascular and psychotropic drugs”.

The intervention “increased survival with no extra need of home care services, rehabilitation, readmissions to hospital or nursing home stays”.

Our comments/summary

This study showed reduced mortality in the GEMU as compared to the control wards. It also showed the presence of inappropriate drug prescribing in the elderly, frail population which was less likely to be rectified in the control ward. The study was powered to show a difference in mortality, no difference was found in most of the other reported outcomes and the authors suggest that this shows a positive effect of the intervention as prolonged life may lead to higher healthcare utilisation and decreased function and quality of life if the increased survival in the GEMU group prolonged life in those at high risk of death and morbidity. However, the study does not appear to have been powered for these outcomes and although a lack of improvement in the GEMU group has been interpreted as a positive outcome, given the reduced mortality, we cannot rule out that a bigger sample size would not have shown a different result.

Article: Counsell SR, Holder CM, Liebenauer LL, Palmer RM, Fortinsky RH, Kresevic DM, Quinn LM, Allen KR, Covinsky KE and Landefeld CS. Effects of a multicomponent intervention on functional outcomes and process of care in hospitalized older patients: A randomized controlled trial of Acute Care for Elders (ACE) in a community hospital. Journal of the American Geriatrics Society 2000; 48(12): 1572-81.

Description of study

Patient/population	
N	Intervention n=767, Control n=764
Setting	Akron City Hospital, Ohio, United States
Intervention/indicator	<p>Treatment via admission to an Acute Care for Elders Unit. Features of the unit were well referenced and described and include:</p> <ul style="list-style-type: none"> • A prepared environment, including carpeting, handrails, uncluttered hallways, large clocks and calendars, elevated toilet seats and levered door handles. • Patient-centred care including daily assessment of physical, cognitive and social function, protocols to manage geriatric syndromes and daily rounds by a multidisciplinary team. • Early, ongoing discharge planning with an emphasis on returning home and involvement of social worker and home care nurse if necessary. • Medical care review daily, including protocols to minimise the adverse effects of selected procedures such as urinary catheterisation and certain medications. <p>Funding allowed for increased hours of medical and nursing directors, social worker, physical therapist, occupational therapist and dietician.</p>
Comparison/control	Usual care in a general medical ward.
Outcomes	Ability to perform Activities of Daily Living (ADLs)
Inclusion Criteria	Community dwelling persons aged 70 years or older admitted to the Akron City Hospital for acute care
Exclusion Criteria	Patients transferred from a nursing facility or another hospital or who required specialty unit admission such as intensive care, those who were admitted electively, had a length of stay of less than two days or had previously been enrolled in the study.

Study Validity

Were there any conflicts of interest in the writing or funding of this study?	Not reported	The authors received funding from the Summa Health System Foundation, Akron, Ohio, US.
Does the study have a clearly focused question?	Yes	The aim of the study was to examine whether an Acute Care for Elders model of care in a hospital can improve overall outcomes for a heterogeneous group of older patients. The patient population, intervention, comparison and outcomes are clearly described in the methods section.
Is a RCT the appropriate method to answer this question?	Yes	A RCT is the appropriate study design to answer this question.
Does the study have specified inclusion/exclusion criteria?	Yes	Inclusion and exclusion criteria are clearly specified in the methods section.
Were the criteria used the select patients for inclusion appropriate?	Yes	The patient selection criteria appear to be appropriate. Patients were aged 70 years and older. As mentioned in the other appraisals, the definition of elderly may vary between health services and cultures.

Did the study have an adequate method of randomisation?	Yes	Randomisation was performed using a computer generated, permuted block design. Numbers were placed in numbered, opaque, sealed envelopes that were opened at time of admission by the admitting clerk.
Was allocation to intervention group concealed?	Not reported	Although randomisation was adequate it is not reported whether there was a procedure in place to prevent manipulation of envelopes
Were patients blind to intervention group?	Not reported	Blinding or otherwise of patients is not reported and given the nature of the intervention, is unlikely. Patients (or their proxies) gave informed consent and it is not reported how much they were told about the models of care.
Were investigators and care providers blind to intervention group?	No	Nursing staff did not move between wards however physicians and residents did. Movement of allied health staff is not reported. It is unlikely that care providers could be blinded to an intervention that is a model of care.
Were outcome assessors blind to intervention group?	No	Interviews and physical performance examinations were conducted by trained research assistants who were blinded to the main outcome measures but were not blinded to group assignment. Trained medical chart data abstractors were also blinded to the main outcome measures.
Aside from the experimental intervention, were the groups treated the same?	Not applicable.	The intervention forms the entire model of care for this study, so all treatment forms part of the intervention. There may be a weakness in the degree to which the details of all aspects of care can be documented, particularly as this may vary between staff members, etc.
Was there sufficient duration of follow-up?	Yes	Follow-up of ADL function and mobility was conducted at discharge, 1, 3, 6 and 12 months.
All outcomes were measured in a standard, valid and reliable way?	Partial	Information about ADLs and mobility was gathered by interview with the patient or a proxy by trained research assistants. At discharge mobility was also assessed using the Physical Performance and Mobility Examination (PPME). Hospital records were reviewed for reason of hospitalisation, Charlson comorbidity score and APACHE II score on the day of admission, measures and processes of care, prescription of potentially inappropriate medications (as reported by Beers) and physician recognition of depression. Interrater reliability of chart abstractors had a kappa statistics of ≥ 0.60 for clinical and process of care variables. Hospital financial records were used for patient's LOS and costs. Costs for implementation of the model, including extra salaries and unit renovations were estimated to be \$28 per hospital day. Information regarding home healthcare visits, nursing home stays and hospital readmissions were collected at follow-up interviews. Interrater reliability of interviewers was assessed on 3% of study subjects; the mean kappa statistics for the items composing outcome measures at discharge was ≥ 0.84 . Physicians and nurses were surveyed midway through the study to assess their satisfaction with geriatric patient care on the intervention and control wards. The clinician satisfaction survey is unlikely to be validated and given some clinicians worked on both wards, may not be valid. Patient satisfaction was measured one month after discharge and carer satisfaction was measured at discharge. It is unclear whether these were assessed in a validated manner.

Were outcomes assessed objectively and independently?	Partial	<p>Though research interviewers and chart data abstractors were blinded to the main outcomes of the study they were not blinded to the intervention group.</p> <p>Some outcomes were assessed on standardised scales (ie. the Mini-Mental Status Exam) and information such as mortality and data taken from hospital records was assessed objectively.</p> <p>Assessment of basic activities of daily living and instruments of daily living were assessed as a number items performed independently. This may be subjective.</p> <p>Self report of overall health status as excellent, good, fair or poor is subjective.</p> <p>The lack of blinding and fact that some of these tests were undertaken by the same researcher may mean that the results of the tests are not independent.</p>
Was the study sufficiently powered to detect any differences between the groups?	Yes	The study was designed with >90% power and an alpha of 0.05 to detect a difference in the mean number of independent ADLs detected at discharge according to the difference in ADLs found in an earlier study. ¹ The required enrolment number was met.
Was the statistical analysis appropriate?	Yes	The t-test for independent samples was used to test for differences in continuous variables unless underlying assumptions could not be validated, in which case the Wilcoxon rank-sum test was used. The chi-square test was used to examine differences in categorical variables, with modification for linear trend if appropriate. Group differences in patient outcomes over follow-up were assessed using longitudinal generalized estimating equation (GEE) analysis. All statistical tests were two-sided.
Were the groups similar at baseline with regards to key prognostic variables?	Yes	Baseline characteristics are presented in Table 1 of the paper. The two groups were similar in basic demographic characteristics at baseline, including age, sex, race, education and living arrangements. They were also similar in baseline overall health, ADLs and mobility and reasons for admission. In regards to pre-existing conditions the intervention group had slightly more chronic lung disease than the control group.
What percentage of the individuals recruited into each arm of the study dropped out?	7.1% at 12 months	Functional data was obtained from 99.5% of surviving patients at discharge, 94.8% at 1 month follow-up, 92.6% at 3 month follow-up and 92.9% at 12 months follow up. This was not presented individually for intervention and control groups.
Were all the subjects analysed in the groups to which they were randomly allocated (ie intention to treat analysis)?	Yes	79 patients were not admitted to the ward to which they were randomised but they were included in an intention to treat analysis. A per protocol analysis was also performed.
Is the paper free of selective outcome reporting?	Partial	We do not know what outcomes were specified in the original protocol so we cannot be positive that all outcomes were reported.
Other		
What is the overall risk of bias?	Moderate	This study has a moderate risk of bias due to the lack of blinding of health care providers and outcome assessor and the potentially subjective nature of some of the outcome assessments. However, it should be considered that when examining something complex like a model of care as opposed to a more simple drug intervention, it would be harder to fulfil some of the criteria of a rigorous RCT.

Results

Mortality:

21 intervention group patients and 28 usual care patients died in hospital. Mortality over the follow up period was also similar between groups.

ADLs:

There was no change between groups from baseline (two weeks prior to admission) to discharge or admission to discharge in the number of independent ADLs performed. Changes in self-reported mobility were also similar between groups.

Generalised Estimating Equation (GEE) analysis showed differences between groups for ADL decline from baseline to 12 months follow-up favoured the intervention group ($p=0.04$). Fewer intervention patients experienced the composite outcome of either ADL decline from baseline or nursing home placement at discharge ($p=0.03$).

Resource Use:

There was no significant difference between the intervention and control group in LOS (6.1 vs 6.3 days), hospital costs (\$5640 vs \$5754), proportion of patients reporting one or more home healthcare visits the month after discharge, the proportion of patients readmitted to hospital within one month or admission to a nursing home in the following year.

Process of care:

The paper reports that many processes of care differed between the units however given the intervention is a model of care that includes appropriate assessment and treatment of geriatric syndromes this is unsurprising. There were significant differences, in favour of the intervention groups, in the number of nursing care plans initiated (1.5 ± 1.2 vs 0.7 ± 0.8 , $p<0.001$), the days till first mention of discharge planning (2.4 ± 3.6 vs 3.5 ± 4.1 , $p<0.001$), the number of days till social work consult (3.2 ± 3.9 vs 3.9 ± 5.1 , $p=0.05$), the days to new activity order (1.7 ± 1.7 vs 2.5 ± 2.2 , $p=0.001$), the days to physical therapy consult (2.6 ± 3.4 vs 3.2 ± 3.4 , $p=0.03$), the number of patients restrained (2% vs 6%, $p=0.001$) and the number of shifts where restraints were used (3.1 ± 4.1 vs 10.5 ± 17.5 , $p=0.01$), the number of patients receiving high-risk medication in the first 24 hours (6% vs 10%, $p=0.008$). There was no difference found in the number of patients receiving high risk medication at discharge or the number of patients or shifts where urinary catheterisation was used.

Patient and provider satisfaction:

Patient, care-giver, physician and nurse satisfaction was significantly higher in the intervention group, however given the lack of blinding and the lack of information about methods of measuring satisfaction this result should be interpreted with caution.

Author's Conclusions

"ACE in a community hospital improved the process of care and patient and provider satisfaction without increasing hospital length of stay or costs. A lower frequency of the composite outcome ADL decline or nursing home placement may indicate potentially beneficial effects on patient outcomes".

Our comments/summary

This follow-up study to Landefeld et al failed to confirm the improvements in function at hospital discharge that were reported in the first study; however decline in ADLs from baseline to 12 month follow-up significantly favoured the intervention group. The authors suggest that the lack of improvement in ADLs could be a function of a healthier study population than that of the Landefeld study or a reflection on improvements in usual care in the time between studies. It could also represent the difficulties associated with replicating models of care in different health services or comparing intervention results to the hard to define comparator of 'usual care'.

It is interesting that though functional outcomes and nursing home admissions were measured to 12 months, home healthcare visits and readmission were measured, or at least reported, to one month only.
