Supplementary materials

Applied Health Economics and Health Policy

The economic impact of community paramedics within emergency medical services: a systematic review
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Funding

The authors received no specific funding for this research. MWS's research is generally supported by the Westpac Scholars' fund, University of Melbourne's PhD Award, and the Australian Government's Research Training Program; these funders had no involvement in any aspect of this research.

Conflict of interest

The authors declare no competing interests.

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Appendix I: Search strategy

As per Joanna Briggs Institute recommendations, a three-stage search strategy was used. To determine a full search string, an initial search string was determined by separating the research question into PICO terms and adding terms already known to the researchers.

The first stage, an initial search for relevant articles, was undertaken prior to publication of the protocol via Medline (Ovid, 1946-present) on 6 February 2023.

Search	Query	Records retrieved
#1	emergency medical service* OR ambulance service*	N/A
#2	community paramedic* OR paramedic practitioner* OR extended care paramedic*	N/A
#3	cost	N/A
#4	#1 AND #2 AND #3	18

The second stage, reviewing relevant articles for alterative search terms, was undertaken by assessment of the titles, abstracts, and index terms of these 18 articles. The completed list (with generic wildcards) is shown below.

PICO	Key word	Alternative search terms
Population	"emergency medical service*"	OR EMS OR "jurisdictional ambulance service*" OR JAS
Intervention	"community paramedic*"	OR "general care paramedic*" OR "primary care paramedic*" OR "low acuity paramedic*" OR "local area assessment and referral unit paramedic*" OR "isolated practice area paramedic*" OR "east coast paramedic*" OR "paramedic practitioner*" OR "extended care paramedic*"
Comparison	(not necessary – implied as standard care)	N/A
Outcome	resource*	OR cost OR finance* OR economic OR money OR time* OR transport* OR convey*

The expanded search was tested on Medline (Ovid, 1946-present) on 6 February 2023 to evaluate if the results appear prima facie reasonable. With 16 results, this was considered appropriate.

Search	Query	Records retrieved
#1	emergency medical service* OR EMS OR ambulance service* OR JAS	N/A
#2	community paramedic* OR general care paramedic* OR primary care paramedic* OR low acuity paramedic* OR local area assessment and referral unit paramedic* OR isolated practice area paramedic* OR east coast paramedic* OR paramedic practitioner* OR extended care paramedic*	N/A
#3	resource* OR cost OR finance* OR economic OR money OR time* OR transport* OR convey*	N/A

#4	#1 AND #2 AND #3	16

The expanded search was run on Embase (Ovid, 1883–present) on 8 February 2023.

Search	Query	Records retrieved
#1	emergency medical service* OR EMS OR ambulance service* OR JAS	N/A
#2	community paramedic* OR general care paramedic* OR primary care paramedic* OR low acuity paramedic* OR local area assessment and referral unit paramedic* OR isolated practice area paramedic* OR east coast paramedic* OR paramedic practitioner* OR extended care paramedic*	N/A
#3	resource* OR cost OR finance* OR economic OR money OR time* OR transport* OR convey*	N/A
#4	#1 AND #2 AND #3	23

The expanded search was run on Cochrane CENTRAL Register (Cochrane Library) on 8 February 2023.

Search	Query	Records retrieved
#1	“emergency medical service” OR EMS OR “ambulance service” OR JAS	N/A
#2	“community paramedic” OR “general care paramedic” OR “primary care paramedic” OR “low acuity paramedic” OR “local area assessment and referral unit paramedic” OR “isolated practice area paramedic” OR “east coast paramedic” OR “paramedic practitioner” OR “extended care paramedic”	N/A
#3	resource* OR cost OR finance* OR economic OR money OR time* OR transport* OR convey*	N/A
#4	#1 AND #2 AND #3	7

The expanded search was run on CINAHL (EBSCO, 1961–present) on 8 February 2023.

Search	Query	Records retrieved
#1	“emergency medical service*” OR EMS OR “ambulance service*” OR JAS	N/A
#2	“community paramedic*” OR “general care paramedic*” OR “primary care paramedic*” OR “low acuity paramedic*” OR “local area assessment and referral unit paramedic*” OR “isolated practice area paramedic*” OR “east coast paramedic*” OR “paramedic practitioner*” OR “extended care paramedic*”	N/A
#3	resource* OR cost OR finance* OR economic OR money OR time* OR transport* OR convey*	N/A
#4	#1 AND #2 AND #3	99

The expanded search was run on Scopus (Elsevier, 1966–present) on 8 February 2023.

Search	Query	Records retrieved
#1	{emergency medical service} OR EMS OR {ambulance service} OR JAS	N/A
#2	{community paramedic} OR {general care paramedic} OR {primary care paramedic} OR {low acuity paramedic} OR {local area assessment and referral unit paramedic} OR {isolated practice area paramedic} OR {east coast paramedic} OR {paramedic practitioner} OR {extended care paramedic}	N/A
#3	resource OR cost OR finance! OR economic OR money OR time OR transport OR convey	N/A
#4	#1 AND #2 AND #3	194

Appendix II: Studies excluded based on full text review

Agarwal G, Angeles R, Pirrie M, Marzanek F, McLeod B, Parascandalo J, Dolovich L. Effectiveness of a community paramedic-led health assessment and education initiative in a seniors' residence building: the Community Health Assessment Program through Emergency Medical Services (CHAP-EMS). BMC Emerg Med. 2017 Mar 9;17(1):8. doi: 10.1186/s12873-017-0119-4.

> *Reason for exclusion: Ineligible population and intervention. On initial reading of title and abstract, unclear if the intervention ('CHAP-EMS program') was reactive response to emergency requests or proactive scheduled visits / clinics. Intervention is the latter (voluntary drop in to two 4-hour scheduled physical checkup sessions on site each week), and therefore ineligible for inclusion. Full text screening by DC, MWS, conflict resolution not required.*

Agarwal, G., Girard, M., Angeles, R. et al. Design and rationale for a pragmatic cluster randomized trial of the Cardiovascular Health Awareness Program (CHAP) for social housing residents in Ontario and Quebec, Canada. Trials 20, 760 (2019). https://doi.org/10.1186/s13063-019-3806-5

> *Reason for exclusion: Ineligible population and intervention. On initial reading of title and abstract, unclear if the intervention ('CHAP-EMS program') was reactive response to emergency requests or proactive scheduled visits / clinics. Intervention is the latter (voluntary drop in to two 4-hour scheduled physical checkup sessions on site each week), and therefore ineligible for inclusion. Full text screening by DC, MWS, conflict resolution not required.*

Bennett KJ, Yuen MW, Merrell MA. Community Paramedicine Applied in a Rural Community. J Rural Health. 2018 Feb;34 Suppl 1:s39-s47. doi: 10.1111/jrh.12233.

> *Reason for exclusion: Ineligible population and intervention. On initial reading of title and abstract, unclear if the intervention ('Abbeville CP program') was reactive response to emergency requests or proactive scheduled visits / clinics. Intervention is the latter ('a set number of follow-up visits'), and therefore ineligible for inclusion. Full text screening by DC, MWS, conflict resolution not required.*

Burns J. Paramedic-led acute home visiting services in primary care. Journal of Paramedic Practice. 8 June 2021; 13 (6). doi: 10.12968/jpar.2021.13.6.238

> *Reason for exclusion: Not a primary research article - review of secondary evidence. On initial reading of title and abstract, unclear if the article is a primary research article or a review. Full text review reveals the article is a review, and therefore ineligible for inclusion. As with all review articles identified in the search, the reference list will be checked for potentially relevant articles. Full text screening by DC, MWS, conflict resolution not required.*

Choi BY, Blumberg C, Williams K. Mobile Integrated Health Care and Community Paramedicine: An Emerging Emergency Medical Services Concept. Ann Emerg Med. 2016 Mar;67(3):361-6. doi: 10.1016/j.annemergmed.2015.06.005.

> *Reason for exclusion: Not a primary research article - review of secondary evidence. On initial reading of title and abstract, unclear if the article is a primary research article or a review. Full text review reveals the article is a review, and therefore ineligible for inclusion. As with all review articles identified in the search, the references will be reviewed for potentially relevant articles. Full text screening by DC, MWS, conflict resolution not required.*

Dinh MM, Muecke S, Berendsen Russell S, Chalkley D, Bein KJ, Muscatello D, Nagaraj G, Paoloni R, Ivers R. Demand for Emergency Services Trends in New South Wales Years 2010-2014 (DESTINY): Age and Clinical Factors Associated with Ambulance Transportation to Emergency Departments. Prehosp Emerg Care. 2016 Nov-Dec;20(6):776-782. doi: 10.1080/10903127.2016.1182603.

> *Reason for exclusion: Ineligible intervention, comparator and outcome. Population are all*

NSW Ambulance EMS calls during study period. The intervention is ambulance attendance. There is no comparison group - only descriptive statistics are pooled, and no segregation of ECP attendances is provided. The data is therefore ineligible for inclusion. Full text screening by MT, MWS, conflict resolution not required.

Finn, J.C., Fatovich, D.M., Arendts, G. et al. Evidence-based paramedic models of care to reduce unnecessary emergency department attendance – feasibility and safety. BMC Emerg Med 13, 13 (2013). https://doi.org/10.1186/1471-227X-13-13

Reason for exclusion: Not a primary research article – protocol. This article is not primary research, and is instead a protocol for an upcoming study. Therefore, this is ineligible for inclusion. Full text screening by MT, MWS, conflict resolution not required.

Flake, F., Schmitt, L., Oltmanns, W. et al. Das Konzept Gemeindenotfallsanitäter/in. Notfall Rettungsmed 21, 395–401 (2018). https://doi.org/10.1007/s10049-018-0426-7

Reason for exclusion: Not English language. Apart from Abstract, full text is not in English - as per Protocol, therefore ineligible for inclusion. Full text screening by MG, MWS, conflict resolution not required.

Georgiev R. The Integral Role of Nurse Practitioners in Community Paramedicine. The Journal for Nurse Practitioners. 2019 Dec; 15(10): 725-731. doi: 10.1016/j.nurpra.2019.07.019

Reason for exclusion: Ineligible population and intervention. On initial reading of title and abstract, unclear if the intervention was reactive response to emergency requests or proactive scheduled visits / clinics - intervention is the latter ('4 to 5 in-home visits are conducted.') - therefore ineligible for inclusion. Presents preliminary data. Full text screening by DC, MWS, conflict resolution not required.

Hänninen J, Kouvonen A, Sumanen H. Patients Seeking Retreatment after Community Paramedic Assessment and Treatment: Piloting a Community Paramedic Unit Program in Southwest Finland. Nurs Rep. 2020 Nov 13;10(2):66-74. doi: 10.3390/nursrep10020010.

Reason for exclusion: Ineligible comparator. On initial reading of title and abstract, unclear if there is a comparison group. There is no comparison - outcome is only descriptive statistics of the intervention group - therefore ineligible for inclusion. Full text screening by DC, MWS, conflict resolution not required.

Kant RE, Vejar M, Parnes B, Mulder J, Daddato A, Matlock DD, Lum HD. Outcomes and provider perspectives on geriatric care by a nurse practitioner-led community paramedicine program. Geriatr Nurs. 2018 Sep-Oct;39(5):574-579. doi: 10.1016/j.gerinurse.2018.04.003.

Reason for exclusion: Ineligible comparator. Population appears appropriate (000-equivalent). Intervention appears appropriate (community paramedic led by an RN). On initial reading of title and abstract, unclear if there is a comparison group. There is no comparison - outcome is only descriptive statistics of the intervention group - therefore ineligible for inclusion. Full text screening by DC, MWS, conflict resolution not required.

Wood K, Ashton C, Duffie-Ashton D. The Economic Value of Community Paramedicine Programs. Paramedic Services of Hastings-Quinte and County of Renfrew; 2017 Mar.

Reason for exclusion: Ineligible intervention. On initial reading of title and abstract, unclear if the intervention was reactive response to emergency requests or proactive scheduled visits / clinics. Intervention is the latter, and therefore ineligible for inclusion. Full text screening by SC, MWS, conflict resolution not required.

Keene T, Davis M, Brook C. Characteristics and Outcomes of Patients Assessed by Paramedics and Not Transported to Hospital: A Pilot Study. Australasian Journal of Paramedicine. 2015 May 4;12(2). doi: 10.33151/ajp.12.2.231

Reason for exclusion: Ineligible intervention and comparator. On initial reading of title and abstract, unclear if the intervention was specialist primary care paramedics or only standard paramedic response to emergency requests. Intensive care paramedics are not considered to be primary care specialists. The intervention does not include primary care specialist paramedics, and is therefore ineligible for inclusion. Full text screening by CY, MWS, conflict resolution not required.

Mackey KE, Qiu C. Can Mobile Integrated Health Care Paramedics Safely Conduct Medical Clearance of Behavioral Health Patients in a Pilot Project? A Report of the First 1000 Consecutive Encounters. Prehosp Emerg Care. 2018 Aug 23:1-10. doi: 10.1080/10903127.2018.1482390.

Reason for exclusion: Ineligible comparator. Population are all mental health EMS calls responded to by a specialist mental health paramedic during study period. There is no comparison group - only descriptive statistics are provided from the intervention group. The data is therefore ineligible for inclusion. Full text screening by MT, MWS, conflict resolution not required.

Mason S, Knowles E, Colwell B, Dixon S, Wardrope J, Gorringe R, Snooks H, Perrin J, Nicholl J. Effectiveness of paramedic practitioners in attending 999 calls from elderly people in the community: cluster randomised controlled trial. BMJ. 2007 Nov 3;335(7626):919. doi: 10.1136/bmj.39343.649097.55.

Reason for exclusion: Duplicate study. This paper meets all inclusion criteria; however, it is a duplication of the data set used in two other papers by the same authors. The three studies are "Effectiveness of paramedic practitioners in attending 999 calls from elderly people in the community: cluster randomised controlled trial", "Is it cost effective to introduce paramedic practitioners for older people to the ambulance service? Results of a cluster randomised controlled trial", and "Safety of Paramedics with Extended Skills". The papers use the same methods on the same population in the same period and have the exact same results. The papers differ in their analysis of the data only, one being an economic evaluation, another a safety evaluation, and a third a subgroup (elderly falls) evaluation. Inclusion of all papers would result in the same data being included thrice, over-weighting that evidence. Instead, it is proposed that the economic evaluation paper ("Is it cost effective to introduce paramedic practitioners for older people to the ambulance service? Results of a cluster randomised controlled trial") be formally included, and the results carefully compared across all three to identify any inconsistencies. Authors contacted to confirm this on 1/3/23; they confirmed all three papers are different analysis on the same data set on 3/3/23. Full text screening by CY, MWS, conflict resolution by MG, DC, MT.

Mason S, Knowles E, Freeman J, Snooks H. Safety of paramedics with extended skills. Acad Emerg Med. 2008 Jul;15(7):607-12. doi: 10.1111/j.1553-2712.2008.00156.x.

Reason for exclusion: Duplicate study. This paper meets all inclusion criteria; however, it is a duplication of the data set used in two other papers by the same authors. The three studies are "Effectiveness of paramedic practitioners in attending 999 calls from elderly people in the community: cluster randomised controlled trial", "Is it cost effective to introduce paramedic practitioners for older people to the ambulance service? Results of a cluster randomised controlled trial", and "Safety of Paramedics with Extended Skills". The papers use the same methods on the same population in the same period and have the exact same results. The papers differ in their analysis of the data only, one being an economic evaluation, another a safety evaluation, and a third a subgroup (elderly falls) evaluation. Inclusion of all papers would result in the same data being included thrice, over-weighting that evidence. Instead, it is proposed that the economic evaluation paper ("Is it cost effective to introduce paramedic practitioners for older people to the ambulance service? Results of a cluster randomised controlled trial") be formally included, and the results carefully compared across all three to

identify any inconsistencies. Authors contacted to confirm this on 1/3/23; they confirmed all three papers are different analysis on the same data set on 3/3/23. Full text screening by CY, MWS, conflict resolution by MG, DC, MT.

Myers LA, Carlson PN, Krantz PW, Johnson HL, Will MD, Bjork TM, Dirkes M, Bowe JE, Gunderson KA, Russi CS. Development and Implementation of a Community Paramedicine Program in Rural United States. West J Emerg Med. 2020 Aug 24;21(5):1227-1233. doi: 10.5811/westjem.2020.7.44571

Reason for exclusion: Ineligible population and intervention. On initial reading of title and abstract, unclear if the intervention was reactive response to emergency requests or proactive scheduled visits / clinics - intervention is the latter - therefore ineligible for inclusion. Full text screening by MT, MWS, conflict resolution not required.

O'Meara P, Ruest M, Martin A. Integrating a Community Paramedicine program with local health, aged care and social services: An observational ethnographic study. Australasian Journal of Paramedicine. 2015 Nov 1;12(5). doi: 10.33151/ajp.12.5.238

Reason for exclusion: Ineligible study design - qualitative design only. Method is interview of community paramedics. No outcomes relevant to this study provided. Study is not suitable for inclusion. Full text screening by CY, MWS, conflict resolution not required.

Patterson DG, Coulthard C, Garberson LA, Wingrove G, Larson EH. What Is the Potential of Community Paramedicine to Fill Rural Health Care Gaps? J Health Care Poor Underserved. 2016;27(4A):144-158. doi: 10.1353/hpu.2016.0192.

Reason for exclusion: Ineligible population and comparator. Ineligible methods for inclusion - study presents descriptive statistics of multiple CP programs, rather than any comparative data on the outcomes of those programs themselves. Not suitable for inclusion. Full text screening by SC, MWS, conflict resolution not required.

Quatman-Yates CC, Wisner D, Weade M, Gabriel M, Wiseman JM, Sheridan E, Garvin JH, Bridges JFP, Santry HP, Panchal AR, Fernandez S, Quatman CE. Assessment of Fall-Related Emergency Medical Service Calls and Transports after a Community-Level Fall-Prevention Initiative. Prehosp Emerg Care. 2022 May-Jun;26(3):410-421. doi: 10.1080/10903127.2021.1922556.

Reason for exclusion: Ineligible population and intervention. On initial reading of title and abstract, unclear if the intervention was reactive response to emergency requests or proactive scheduled visits / clinics - intervention is the latter - therefore ineligible for inclusion. Full text screening by MT, MWS, conflict resolution not required.

Ruest, M.R., Ashton, C.W. & Millar, J. (2017). Community Health Evaluations Completed Using Paramedic Service (CHECUPS): Design and Implementation of a New Community-Based Health Program. Journal of Health and Human Services Administration. 40(2), 186-218.

Reason for exclusion: Ineligible population and intervention. On initial reading of title and abstract, unclear if the intervention was reactive response to emergency requests or proactive scheduled visits / clinics - intervention is the latter - therefore ineligible for inclusion. Full text screening by SC, MWS, conflict resolution not required.

Seeger, I., Klausen, A., Thate, S. et al. Gemeindenotfallsanitäter als innovatives Einsatzmittel in der Notfallversorgung – erste Ergebnisse einer Beobachtungsstudie. Notfall Rettungsmed 24, 194–202 (2021). https://doi.org/10.1007/s10049-020-00715-6

Reason for exclusion: Not English language. Apart from Abstract, full text is not in English - as per Protocol, therefore ineligible for inclusion. Full text screening by MT, MWS, conflict resolution not required.

Stainthorpe A.Hart J.Ahern A. Pih43 Can Using Point Of Care Blood Tests Help Inform Decision Making For Patients With Acute Frailty Syndrome? A Budget Impact Analysis. Value In Health (2019);

22(Supp 3); S635. doi: 10.1016/j.jval.2019.09.1224

Reason for exclusion: Ineligible intervention and comparator. On initial reading of title and abstract, does not appear to meet inclusion criteria - intervention is POCT rather than paramedic type. Due to uncertainty progressed to full text review. Both intervention and comparison group are ECP, with no standard care paramedics available for comparison - therefore ineligible for inclusion. Full text screening by MT, MWS, conflict resolution not required.

Swain AH, Al-Salami M, Hoyle SR, Larsen PD. Patient satisfaction and outcome using emergency care practitioners in New Zealand. Emerg Med Australas. 2012 Apr;24(2):175-80. doi: 10.1111/j.1742-6723.2011.01525.x.

Reason for exclusion: Duplicate study. The publication meets inclusion criteria. However, the population were attended between "8 November to 13 December 2010"... "These patients were surveyed retrospectively by phone or home visit (according to patient preference) 3–10 days after being attended by paramedics." This overlaps with the sample in Hoyle et al., which was attended between May 2010 and March 2011. Consequently, with the same metrics being presented, inclusion of both studies would result in duplication of data. With Hoyle et al. being the larger and more reliable study (1000 patients and a focus on resource use metrics, compared to 50 patients and a focus on satisfaction in this study), having a focus directly on the topic being investigated in this review, and additionally having a lower outcome rate for the comparison group (reducing the risk of overstating effect size), inclusion of Hoyle et al. and exclusion of Swain et al. appears the best option. This is further discussed below under Appendix VII. Full text screening by SC, MWS, conflict resolution by MG, DC, MT, CY.

Swain AH, Hoyle SR, Long AW. The changing face of prehospital care in New Zealand: the role of extended care paramedics. N Z Med J. 2010 Feb 19;123(1309):11-4.

Reason for exclusion: Duplication of data and not a primary research article - this article is a commentary / news piece - while grey literature is eligible for consideration under the Protocol, in this instance the data presented here is also presented in a formal peer-reviewed publication already included in this review (Hoyle et al.). Inclusion of this grey literature would lead to duplication of data and skew results. Full text screening by MWS, confirmed by MG, DC, MT, SC, CY.

Tavares W, Drennan I, Van Diepen K, Abanil M, Kedzierski N, Spearen C, Barrette N, Mercuri M. Building Capacity in Healthcare by Re-examining Clinical Services in Paramedicine. Prehosp Emerg Care. 2017 Sep-Oct;21(5):652-661. doi: 10.1080/10903127.2017.1311391.

Reason for exclusion: Ineligible study design - theoretical model only. The study analyses all EMS transports to ED to determine the proportion theoretically avoidable, and then models a set of interventions that could be used by specialist paramedics to achieve this. As a theoretical model, this study is not suitable for inclusion in the review. Full text screening by SC, MWS, conflict resolution not required.

Todd VF, Swain A, Howie G, Tunnage B, Smith T, Dicker B. Factors Associated with Emergency Medical Service Reattendance in Low Acuity Patients Not Transported by Ambulance. Prehosp Emerg Care. 2021 Jan 25:1-17. doi: 10.1080/10903127.2020.1862943.

Reason for exclusion: Ineligible intervention and comparator. Paramedics in this study were normal paramedics, no extended scope of practice. Study comparator was patients left at home vs left at home with EMS reattendance in 48 hours. Full text screening by SC, MWS, conflict resolution not required.

Appendix III: Studies discovered from reference lists of included studies, excluded based on full text review

Abrashkin KA, Poku A, Ramjit A, et al Community paramedics treat high acuity conditions in the home: a prospective observational study BMJ Supportive & Palliative Care. 04 April 2019. doi: 10.1136/bmjspcare-2018-001746

> *Reason for exclusion: Ineligible comparator. On initial reading of title and abstract, unclear if there is a comparison group. There is no comparison - outcome is only descriptive statistics of the intervention group - therefore ineligible for inclusion. Full text screening by SC, MWS, conflict resolution not required.*

Adams A, Wright K, Cooke M. Evaluation of the NHS Changing Workforce Programme's Emergency Care Practitioners Pilot Study in Warwickshire. Coventry: Division of Health in the Community, Medical School, University of Warwick; 2005.

> *Reason for exclusion: Ineligible comparator and outcome. On initial reading of title and abstract, unclear if intervention / comparison / outcome criteria met. Intervention criteria appears met, however, comparison and outcome criteria are not - no information provided on outcomes of interest for intervention or comparison groups. Full text screening by SC, MWS, conflict resolution not required.*

Bernard P, Corcoran G, Kenna L, O'Brien C, Ward P, Howard W, Hogan L, Mooney R, Masterson S. Is Pathfinder a safe alternative to the emergency department for older patients? An observational analysis. Age Ageing. 2021 Sep 11;50(5):1854-1858. doi: 10.1093/ageing/afab095.

> *Reason for exclusion: Ineligible comparator. On initial reading of title and abstract, unclear if there is a comparison group. There is no comparison - outcome is only descriptive statistics of the intervention group - therefore ineligible for inclusion. Full text screening by SC, MWS, conflict resolution not required.*

Castillo DJ. Mobile integrated healthcare: Preliminary experience and impact analysis with a Medicare advantage population. Journal of Health Economics and Outcomes Research. 2016;4(2):172–187.

> *Reason for exclusion: Ineligible population and intervention. On initial reading of title and abstract, unclear if intervention was 911 calls or scheduled in home visits - intervention is both - as latter is likely to confound the impact of the former, ineligible for inclusion. Full text screening by MG, MWS, conflict resolution not required.*

Cooper S. Collaborative practices in unscheduled emergency care: role and impact of the emergency care practitioner—qualitative and summative findings. Emergency Medicine Journal: EMJ. 2007;24(9):625–629.

> *Reason for exclusion: Ineligible intervention and comparator. On initial reading of title and abstract, unclear if intervention was 911 calls or GP clinic visitors - intervention is both - as latter is likely to confound the impact of the former and segregated data not available, inelgible for inclusion. No comparison group. Full text screening by MG, MWS, conflict resolution not required.*

Cooper S, O'Carroll J, Jenkin A, Badger B. Emergency care practitioners (ECP): practice and performance in the UK West country--a case study. Int Emerg Nurs. 2008 Jul;16(3):180-4. doi: 10.1016/j.ienj.2008.04.004.

> *Reason for exclusion: Ineligible study design - qualitative design only. On initial reading of title and abstract, unclear if there is a comparison group. There is no comparison - outcome is only descriptive statistics of the intervention group - comparison stated in paper is from a different study, therefore ineligible for inclusion. Full text screening by MG, MWS, conflict*

resolution not required.

Department of Health. Right Skill, Right Time, Right Place. The ECP Report. London: Department of Health Publications; 2004.

Reason for exclusion: Not a primary research article - grey literature of unreported data. Basic data provided under subheading '999 response'. However, no information on sample, characteristics, geography, selection/randomisation, and no references provided. With no information suitable for evaluation, inclusion of this grey literature would be inappropriate. Full text screening by MWS, confirmed by MG, DC, MT, CY, SC.

Evashkevich M, Fitzgerald M. A Framework for Implementing Community Paramedic Programs in British Columbia. Richmond, BC: Ambulance Paramedics of British Columbia, 2014. doi: 10.13140/RG.2.1.1611.5445

Reason for exclusion: Not a primary research article - grey literature of unreported data. Full text screening by MG, MWS, conflict resolution not required.

Gray JT, Walker A. Avoiding admissions from the ambulance service: a review of elderly patients with falls and patients with breathing difficulties seen by emergency care practitioners in South Yorkshire. Emerg Med J. 2008 Mar;25(3):168-71. doi: 10.1136/emj.2007.050732.

Reason for exclusion: Ineligible comparator. Data presented for comparison group has insufficient detail to determine benefit. Authors contacted 9/3/2023, replied 10/3/2023 and confirmed data not available. Further details provided in Appendix VII. Study for exclusion. Full text screening by MG, MWS, conflict resolution by DC, MT, CY.

Halter M, Marlow T, Tye C, Ellison GT. Patients' experiences of care provided by emergency care practitioners and traditional ambulance practitioners: a survey from the London Ambulance Service. Emerg Med J. 2006 Nov;23(11):865-6. doi: 10.1136/emj.2005.032912.

Reason for exclusion: Ineligible comparison. Comparison group includes both standard paramedics and EMTs, with no segregation of data. EMT data is unsuitable for inclusion. Authors contacted twice to request segregated data on 9/3/2023 and again on 29/3/2023 - no reply received. Full text screening by MG, MWS, conflict resolution not required.

Iezzoni LI, Wint AJ, Cluett WS 3rd, Ajayi T, Goudreau M, Blanchfield BB, Palmisano J, Tripodis Y. Early experiences with the Acute Community Care Program in eastern Massachusetts. Am J Manag Care. 2018 Sep 1;24(9):e270-e277.

Reason for exclusion: Ineligible comparator and outcome. On initial reading of title and abstract, unclear if comparison / outcome criteria met. Comparison and outcome criteria are not - no comparison group, and outcomes are descriptive statistics of the specialist paramedics only. Note: the reduction in non-transporation rate reported is based off patient opinion, and ineligible for inclusion. Full text screening by MG, MWS, conflict resolution not required.

Mason S CP. A national evaluation of emergency care practitioners. Phase one: Report to the Department of Health. Sheffield, UK: School of Health and Related Research, University of Sheffield; 2004.

Reason for exclusion: Paper unable to be found by either professional librarians or authors themselves. Contacted authors on 20/2/23 - they replied on 27/2/23 to confirm that they have been unable to locate a copy of the report themselves. Full text screening by CY, MWS, conflict resolution not required.

Mason S, Coleman P, O'Keeffe C, Ratcliffe J, Nicholl J. The evolution of the emergency care practitioner role in England: experiences and impact. Emerg Med J. 2006 Jun;23(6):435-9. doi: 10.1136/emj.2005.027300.

Reason for exclusion: Ineligible intervention and comparator. On initial reading of title and abstract, unclear if intervention / comparison criteria met. Intervention includes 999 attendances, along with other specialist roles such as GP clinics, and minimal segregated data. There is no comparison group - only descriptive statistics of the intervention are provided. Ineligible for inclusion. Full text screening by CY, MWS, conflict resolution not required.

Roeper B, Mocko J, O'Connor LM, Zhou J, Castillo D, Beck EH. Mobile Integrated Healthcare Intervention and Impact Analysis with a Medicare Advantage Population. Popul Health Manag. 2018 Oct;21(5):349-356. doi: 10.1089/pop.2017.0130.

Reason for exclusion: Ineligible population and intervention. On initial reading of title and abstract, unclear if intervention was 911 calls or scheduled home visits - intervention is both, as well as scheduled telephone calls, education, and others - as latter is likely to confound the impact of the former and segregated data not available, ineligible for inclusion. Full text screening by MT, MWS, conflict resolution not required.

Ruest M SA. Evaluating the impact of 911 calls by an in-home programme with a multidisciplinary team. International Paramedic Practice. 2012;1(4):125–32.

Reason for exclusion: Ineligible population, intervention, and comparator. Population/intervention is pre-enrolled patients receiving scheduled visits, with 911 calls monitored after. No comparison group. Full text screening by SC, MWS, conflict resolution not required.

Xie F, Yan J, Agarwal G, Ferron R. Economic Analysis of Mobile Integrated Health Care Delivered by Emergency Medical Services Paramedic Teams. JAMA Netw Open. 2021 Feb 1;4(2):e210055. doi: 10.1001/jamanetworkopen.2021.0055.

Reason for exclusion: Ineligible intervention. The first of the three different interventions is the only one that meets our primary synthesis criteria. The second and third both include a proactive component and do not meet inclusion criteria. Segregated data requested from authors on 9/3/2023 and again on 29/3/2023. Authors replied on 29/3/2023 and confirmed segregated data is not available; as the number of ineligible visits is unknowable, for exclusion. Full text screening by CY, MWS, conflict resolution by MG, DC, MT.

Appendix IV: Methodological quality assessment

Critical appraisal of economic evaluation studies

Citation	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q11	Overall evaluation	Evaluators	Conflict resolution
Dixon 2008	Y	Y	Y	N	Y	Y	N	Y	N	Y	N	Moderate	CY, MWS	MT
%	100%	100%	100%	0%	100%	100%	0%	100%	0%	100%	0%	64%		

N = No, N/A = Not applicable, U = Unclear, Y = Yes

Critical appraisal of quasi-experimental studies

First author	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Overall evaluation	Evaluators	Conflict resolution
Mason 2012	Y	Y	Y	Y	N	N/A	Y	Y	Y	Moderate	CY, MWS	MT
%	100%	100%	100%	100%	0%	N/A	100%	100%	100%	88%		

N = No, N/A = Not applicable, U = Unclear, Y = Yes

Critical appraisal of cohort studies

First author	Q1	Q2	Q3	Q4	Q5	Q6	Q7	Q8	Q9	Q10	Q11	Overall evaluation	Evaluators	Conflict resolution
Abrashkin 2016	N	Y	Y	N	N	Y	Y	Y	N/A	N/A	Y	Moderate	SC, MWS	MG
Breyre 2021	Y	Y	Y	N	N	Y	Y	N/A	N/A	N/A	Y	Moderate	MG, MWS	DC
Cooper 2004	U	U	Y	U	U	Y	Y	U	U	U	Y	Low	MG, MWS	DC
Hoyle 2012	U	Y	Y	N	N	Y	Y	Y	N/A	N/A	Y	Moderate	SC, MWS	MG
Jensen 2015	N	Y	Y	N	N	Y	Y	Y	N/A	N/A	N	Moderate	DC, MWS	MT
Jensen 2013	U	Y	Y	U	N	Y	Y	Y	N/A	N/A	N	Moderate	DC, MWS	MT
Mason 2007	U	Y	Y	Y	N	Y	Y	Y	U	U	Y	Moderate	MT, MWS	DC
Thompson 2014	U	U	U	N	N	Y	Y	Y	N/A	N/A	Y	Moderate	MT, MWS	CY
Widiatmoko 2008	Y	Y	Y	U	N/A	Y	Y	N/A	N/A	N/A	Y	Moderate	CY, MWS	MT
%	22%	78%	89%	11%	0%	100%	100%	75%	0%	0%	78%	50%		

N = No, N/A = Not applicable, U = Unclear, Y = Yes

Appendix V: Additional information requested from authors

As per JBI protocols, corresponding authors were contacted to request further information where appropriate. Three authors replied, with their replies summarised below:

Study	Breyre A, Taigman M, Salvucci A, Sporer K. Effect of a Mobile Integrated Hospice Healthcare Program on Emergency Medical Services Transport to the Emergency Department. Prehospital Emergency Care. 2022 May 4;26(3):364–9.
Information requested	Asked to confirm the number of patients seen and not transported by MIHH paramedics. Asked to clarify Table 1. The 167 in the abstract / text doesn't align with Table 1. The Results state that from 8/2016 to 07/2017, 63/190 patients were seen/transported. Table 1 shows those results just from 02/2017 to 07/2017, with the first half of that study year excluded. The study states there are 523 patients treated by the MIHH paramedics during the period, with 167 transported However, on Table 1, the totals for the intervention period are 629 patients seen and 194 transported. The difference of 106 and 27 is exactly equal to the Feb 2017-July 2017 period All other numbers align perfectly (e.g. the reported 68/188 equals the 99+89 and 33+35 of the first year) Reviewers unsure if the 106 patients calling 911 in the first half of 2017 were excluded (and if so, for what reason), if Table 1 actually represents all calls (standard paramedics + MIHH) and the numbers aligning is coincidence or if it was an addition error (and the 106 was left off by accident). Asked to confirm a minor addition error on Table 1 - column 2 and 3 in row 4 (Aug 2016-Jan 2017) state 190 and 63 - they should be 84 and 36. 190 and 63 are the totals for that whole year (Aug 2016-Aug 2017). Asked to confirm if the MIHH paramedics were working solo or in a team, and if they were in an ambulance or a non-transport-capable vehicle. (Given that EMS were already on scene.)
Summary of reply	Raw data for Table 1 provided. Confirmed minor addition error in Table 1 as above. 'The MIH paramedics were typically trained supervisors in fly cars not used for patient transport. However, EMS with transport capabilities was usually already on scene.'
How the data was handled by the reviewers	Suitable for inclusion.

Study	Cooper S. The emerging role of the emergency care practitioner. Emergency Medicine Journal. 2004 Sep 1;21(5):614–8.
Information requested	Asked to confirm training, additional medications, and additional skills provided to specialist paramedics. Asked to confirm if the MIHH paramedics were working solo or in a team, and if they were in an ambulance or a non-transport-capable vehicle.
Summary of reply	'Paramedics (from the Westcountry in the UK) enrolled in an Advanced Healthcare Practice masters degree at the University of Plymouth. It was a generic program to take nurses, physios, midwives, paramedics etc to advanced 'practitioner' level The two year program was taught by a range of academics but physicians taught the advanced clinical skills… Note that as the program was generic across the professions the skills focussed around history taking, differentials, referrals etc as opposed to say rapid sequency induction. Having said this we did teach all to intubate etc.' Confirmed specialist paramedics worked solo, and were in a non-transport-capable vehicle.
How the data was handled by the reviewers	Suitable for inclusion.

Study	Gray JT, Walker A. Avoiding admissions from the ambulance service: a review of elderly patients with falls and patients with breathing difficulties seen by emergency care practitioners in South Yorkshire. Emergency Medicine Journal. 2008 Mar 1;25(3):168–71.
Information requested	Asked to confirm the number of patients who were attended by standard paramedics in the comparison group. The comparison group data provided in the paper lists the number of patients attended by standard paramedics and transported, but does not provide any data on the number of patients attended by standard paramedics and not transported. Without a comparison group of the transportation rate from standard paramedics, the paper is ineligible for inclusion. Similarly, asked to confirm the ECP ward admission rate, control group 72 hour representation rate, and control group 28 day representation rate. 'For hospital ward admissions (rather than just ED presentations), we can see 1902 (1130+772) in the control group, with 1251 (855+396) admitted. We need to confirm the number of patients seen by ECPs, transported to hospital, who were admitted. We can see that 419 (186+233) were seen by ECPs and 290 (119+171) not transported. Of the 129 transported, were 100% admitted to a ward? Without the ECP ward admission rate we are unable to tell if there is any meaningful difference from the normal admission rate. For 72 hour representations, we can see 39 (18+21) of the 290 (119+171) patients seen by ECPs and not transported represented. For this data to be included, we need a comparison rate (i.e. to tell if this is superior or inferior to standard care). We can't see how many of the 1902 (1130+772) in the control group represented to ED in 72 hours. Is this data available? Without the usual service representation rate we are unable to tell if there is any meaningful difference from the ECP representation rate. Similarly, for 28 day representations, we can see an unknown number (roughly 46.7% and 56%, equal to 55.5 and 95.7 individuals) of the 290 (119+171) patients seen by ECPs and not transported represented. For this data to be included, we need a comparison rate (i.e. to tell if this is superior or inferior to standard care). We can't see how many of the 1902 (1130+772) in the control group represented in 28 days. Is this data available? Without the usual service representation rate we are unable to tell if there is any meaningful difference from the ECP representation rate.' Asked to confirm if the specialist paramedics were working solo or in a team, and if they were in an ambulance or a non-transport-capable vehicle.

Summary of reply	'We looked at attendance or admission as the focus was whether they were able to stay at home in this period so I cannot give clarity on what proportion attending the ED were then admitted… We did not look at that because that was not the focus of the study as any attendance to ED was considered a "failure"… Again as per the comment above this was not looked at due to it being considered a failure once the patient got to the ED' 'Working solo in a car - potentially could transport if patient suitable but mostly would not.'
How the data was handled by the reviewers	As intervention/outcomes do not align with review (the total number of patients attended by specialist paramedics is unavailable), excluded.

Study	Halter M, Marlow T, Tye C, Ellison GTH. Patients' experiences of care provided by emergency care practitioners and traditional ambulance practitioners: a survey from the London Ambulance Service. Emergency Medicine Journal. 2006 Nov 1;23(11):865–6.
Information requested	Asked to confirm if the comparison group includes both EMTs and paramedics. Asked to confirm the segregated data for the transportation rate of only paramedics, as EMTs are ineligible for inclusion in the review.
Summary of reply	No reply
How the data was handled by the reviewers	As unable to confirm paper's eligibility for inclusion, the paper was excluded.

Study	Hoyle S, Swain AH, Fake P, Larsen PD. Introduction of an extended care paramedic model in New Zealand. Emergency Medicine Australasia. 2012 Dec;24(6):652–6. Swain AH, Al-Salami M, Hoyle SR, Larsen PD. Patient satisfaction and outcome using emergency care practitioners in New Zealand. Emerg Med Australas. 2012 Apr;24(2):175-80. doi: 10.1111/j.1742-6723.2011.01525.x.
Information requested	Asked to confirm the number of patients seen by regular paramedics in the same region and period (i.e. the source of the 74% transportation rate provided). Asked to confirm the training ECPs undertook at that time. Asked to confirm if the specialist paramedics were working solo or in a team, and if they were in an ambulance or a non-transport-capable vehicle.
Summary of reply	"Unfortunately I cannot help you with the absolute numbers now as it is too long ago and the database is well archived. However, I can tell you that there was ECP-focused teaching for five days per week over two weeks for the group. We did not have a diploma until much more recently" "1. Yes, once training complete. 2. No. Fast-response vehicles in case needed as first response for triage 1 or 2 calls (Pro-Qa purple or red)."
How the data was handled by the reviewers	Significant discussion among reviewers as to best course of action. This paper's data set overlaps with Swain 2012. Swain 2012 provides absolute numbers, and the comparison group are drawn from similar calls (i.e. non-urgent calls) that are less likely to require transportation and therefore less likely to skew data. However, the sample size for Swain 2012 is far smaller at n = 50 in each of the intervention and comparison group, and that paper is focussed on a different topic (patient satisfaction) and incidentally happens to present transportation rate data. By contrast, Hoyle 2012 has a far more reliable sample size (n = 1000) that is twenty times larger than Swain 2012, provides absolute numbers for the intervention group's outcome (402/1000) and the paper is directly focussed on the topic of interest for this review (and includes an additional relevant outcome, 7-day ED representation rate). However, Hoyle 2012's comparison group ED transportation rate (74%) is only provided as a rate without absolute numbers, and additionally appears to include emergency calls (i.e. it is not only limited to calls suitable for an ECP, potentially leading to an increased transportation rate and skewing the results to make the benefit appear greater than it is). The review group ultimately decided to include Hoyle 2012, and to exclude Swain 2012. This decision was based on three factors. Firstly, the control group ED transportation rates was higher in Swain 2012, meaning that using that study may overstate the benefit of ECPs. In Hoyle 2012, there was 402/1000 (40%) seen by ECPs transferred to the ED, and an unknown amount (74%) seen by paramedics transferred to the ED, providing a difference of 34%. In Swain 2012, there was 11/50 (22%) seen by ECPs transferred to the ED, and 42/50 (84%) seen by paramedics transferred to the ED, providing a difference of 62%. Secondly, the sample size in Hoyle is twentyfold larger than Swain, providing less risk of sampling error. Thirdly, the reviewers noted that Hoyle was directly focussed on the outcomes of interest for the review, while Swain was on a different topic and happened to incidentally present relevant results. Therefore, the reviewers determined to include Hoyle 2012's data, and exclude Swain 2012's data.

Study	Jensen JL, Travers AH, Bardua DJ, Dobson T, Cox B, McVey J, et al. Transport outcomes and dispatch determinants in a paramedic long-term care program: a pilot study. CJEM. 2013 Jul 4;15(04):206–13.
Information requested	Asked to confirm the segregated data on just the 911 calls attended by ECPs compared to regular paramedic attendances. (I.e. not including the scheduled follow-up visits, which aren't in the eligibility criteria.) Asked to confirm if the specialist paramedics were working solo or in a team, and if they were in an ambulance or a non-transport-capable vehicle. Asked to confirm ECP training. 'We can see a quote "These ECPs received additional specialized training in the following "extended care" roles: 1) geriatric assessments and management, 2) end-of-life care, 3) primary wound closure techniques (suturing, tissue adhesive), and 4) point-of-care testing." but wasn't sure on how detailed this is - other programs vary from nothing to a Master's degree.'

Summary of reply	Attempted to retrieve raw data, however, the Excel spreadsheet is password protected and no remaining staff have the password. 'ECPs work alone (single responder SUV) – typically no lights and sirens.' 'No – as above. They use an SUV. They will call for a transport unit if required.' 'It has evolved since inception. They use an in-house program with subject matter experts discussing particular issues (e.g. I provided a lecture on frailty). It has since transitioned to be online directed learning modules, in person peer education from other Extended Care Paramedics, as well as subject matter expert lectures. Additionally, there are skill competencies through labs, followed by mentorship shifts aka "OJT". Successful completion is competency based and length is based on needs of the individuals. To estimate times: 5-10 hours online directed learning modules 40 hours peer education 5-10 hours of labs 60-120 hours of mentorship shifts Estimated total: 110-180 hours.'
How the data was handled by the reviewers	As the number of scheduled follow-up visits by ECPs is explicitly discussed (n = 19, per Table 2), it is possible to discuss sensitivity analysis on the low and high rates if the scheduled visits are assumed to all be in the transport group or non-transport group respectively. This analysis is provided below in Appendix IX. Suitable for inclusion.

Study	Dixon S, Mason S, Knowles E, Colwell B, Wardrope J, Snooks H, et al. Is it cost effective to introduce paramedic practitioners for older people to the ambulance service? Results of a cluster randomised controlled trial. Emergency Medicine Journal. 2009;26(6):446–51. Mason S, Knowles E, Colwell B, Dixon S, Wardrope J, Gorringe R, et al. Effectiveness of paramedic practitioners in attending 999 calls from elderly people in the community: Cluster randomised controlled trial. Br Med J. 2007;335(7626):919–22. Mason S, Knowles E, Freeman J, Snooks H. Safety of Paramedics with Extended Skills. Academic Emergency Medicine. 2008 Jul;15(7):607–12.
Information requested	Asked to confirm that three papers draw on the same data set (Dixon 2008, 'Is it cost effective to introduce paramedic practitioners for older people to the ambulance service? Results of a cluster randomised controlled trial'; Mason 2008, 'Effectiveness of paramedic practitioners in attending 999 calls from elderly people in the community: cluster randomised controlled trial'; Mason 2010, 'Safety of paramedics with extended skills').
Summary of reply	Confirmed that all three papers use the same data set. All report on the paramedic practitioner in older people's support (PPOPS) scheme trial.
How the data was handled by the reviewers	Discussed among the review team. The reviewers determined to include Dixon 2008, 'Is it cost effective to introduce paramedic practitioners for older people to the ambulance service? Results of a cluster randomised controlled trial' as this paper is the closest to an economic evaluation, and to cross-check the data against Mason 2008 and 2010 and follow up any inconsistencies with authors for clarification. No inconsistencies found.

Study	Thompson C, Williams K, Morris D, Lago L, Kobel C, Thompson C;, et al. HWA Expanded Scopes of Practice program evaluation: Extending the Role of Paramedics sub-project: final report [Internet]. 2014.
Information requested	Asked to confirm if the study included scheduled proactive attendances. ("Frequently if ECPs were concerned about a patient they would ensure the ECP on the following shift was aware and if necessary a follow-up call or visit would be made", and "On occasions an emergency response crew would ask the ECP to follow-up a case several hours later to ensure the patient's status was unchanged".) Asked to confirm if any scheduled proactive attendances are included in the data in Table 25. Asked to confirm if the data from any follow-up visits is included in the outcomes calculated (e.g. Economic Evaluation, transport to ED rates, etc). Asked to confirm the number of follow-up visits there were, and what the outcomes of these were.
Summary of reply	Confirmed that a small number of scheduled follow-up calls did occur, largely by telephone. 'we could estimate that between 1 – 1.4% of ESOP cases received a follow-up call, I would stress this was unlikely to be a visit from the ECP however, it is possible a very small number occurred. It appears that follow-up was predominantly by telephone.'
How the data was handled by the reviewers	Discussed among the review team. The reviewers determined that, as the number of scheduled calls was extremely low, to conduct high and low sensitivity analysis if the scheduled visits are assumed to all be in the transport group or non-transport group respectively. This analysis is provided below in Appendix IX. Suitable for inclusion.

Study	Xie F, Yan J, Agarwal G, Ferron R. Economic Analysis of Mobile Integrated Health Care Delivered by Emergency Medical Services Paramedic Teams. JAMA Netw Open. 2021 Feb 24;4(2).
Information requested	Asked to confirm the data for only the specialist paramedic service. Asked to confirm the segregated data excluding scheduled follow-up calls. "Our inclusion criteria only involves reactive models, not proactive ones. We can see that your intervention group included 3 different programs, 2 of which (OT and MH RN) had a proactive element. In the published article, they are presented cumulatively. However, without the segregated data, we wouldn't be able to include this. Do you happen to have the data available for just the first service (CARE community paramedics)?"
Summary of reply	'Unfortunately we don't have that data.'
How the data was handled by the reviewers	Discussed among the review team. As unable to determine the number of scheduled visits, the impact of these on outcomes is unknowable. For exclusion.

Appendix VI: Data extraction instrument

Data was extracted directly into a shared Google Drive Spreadsheet (Google, San Paolo) accessible to all reviewers, with version history enabled to track changes. It is presented here as a table for ease of interpretation.

Study	Lead author/s: Year: Location/s:	
Design	Prospective or retrospective: Experimental or observational: Is it a full Economic Evaluation, partial EE, & what perspective is taken:	Economic evaluation criteria: 1. Is there a comparative incremental analysis? 2. Are costs measured? 3. Are consequences measured? *If not measured but known to be equivalent, CMA (partial EE); if not measured and not known to be equivalent, Costing Study (not an EE)* If so, meets economic evaluation definition. 4. Are outcomes measured in natural units (e.g. mmHg)? *If so, CEA* 5. Are QALYs / DALYs used? *If so, CUA (type of CEA)* 6. Is everything, including health, measured in $s? *If so, CBA* 7. Is everything presented separately (i.e. costs and consequences not combined)? *If so CCA*
	Is there incremental analysis: Is there marginal analysis: Methodology: Methods: JBI Evaluation Tool to be used:	
Data collection	Data collection dates: Data collection duration:	
Intervention	Title used for specialist paramedics: Training for specialist paramedics: Were specialist paramedics working alone or in a crew: Were specialist paramedics in an ambulance or non-transport vehicle: Additional notes on intervention:	
Population and participants	Population description: Population size: (*If relevant/knowable*) Sample size: Sample characteristics: Intervention size: Intervention characteristics: Comparison size: Comparison characteristics:	
Outcomes	Outcomes measured: Outcome methods: For each outcome: (*As applicable*)	Sample (n=): Intervention (n=): Intervention outcome (n=): Intervention rate: Control (n=): Control outcome (n=): Control rate: Difference in rates: Relative risk: Absolute risk ratio: Odds ratio: 95% confidence intervals: Probability value: Sensitivity analysis: Other outcome information:
Conclusions	Limitations: Author conclusions:	
Reviewer commentary	Reviewer comments:	

Appendix VII: Summary of included studies

This is a brief summary of extracted data, and is not a comprehensive list of all data extracted.

Study	Abrashkin KA, Washko J, Zhang J, Poku A, Kim H, Smith KL. Providing Acute Care at Home: Community Paramedics Enhance an Advanced Illness Management Program-Preliminary Data. J Am Geriatr Soc. 2016 Dec;64(12):2572–6.
Country	USA
Study design	Prospective observational cohort study. Costs are not measured. The study is not an economic evaluation.
Setting / context	Suburban setting. Provided with a 24/7 healthcare number to call as an alternative to 911. This goes to an RN, who can send an ambulance, CP, or provide telephone advice. The AIM program has a staff of 21 (5 doctors) and provides in-home primary care. Data collection from 1 January 2014 - 30 April 2015, 16 months.
Participant Characteristics	Have an advanced illness (dementia 44%, decubitus ulcers 29%, diabetes mellitus 26%, CCF 24%, COPD 15%). Enrolled in an 'Advanced Illness Management (AIM) program'. Median age 83, ADL assistance mean 5 (on 6 point scale), 2/3 female.
Groups	• 1602 individuals in total enrolled in the AIM program • 1755 total responses (664 CP, 1091 EMS) • 773 individuals responded to (mean 2 responses per individual; 404 CP, 369 EMS)
Intervention	Community Paramedics. All ICPs with an additional 40 hours of training in geriatrics and primary care. Working alone in a non-transport vehicle. All cases overseen by physician via teleconsultation.
Outcomes measured	ED transportation rate, admission to hospital, number of ED transports, most common CP medications, most common CP reasons for dispatch 24 hour representation by CP patients not transported, survey on satisfaction. Method: comparison of routinely collected data extracted directly from patient's medical record, univariate analysis, X2 categorical, t continuous.
Results & author conclusion	Authors stated that CPs are safe - determined using 24 hour representation as a proxy. CPs can identify sicker patients - determined using higher CP admission rate. Authors speculate that this will be cost effective, however, this is not evaluated in this study.
Reviewer comments	Consulted with physicians for all patients - not independent - 'paramedics as physician extenders' - this is due to state regulations that appear to prohibit EMS from providing primary care - exclude from primary synthesis.
Bias risk	Selection bias; patients not randomised to intervention or control group and clinical judgement guided CP deployment. As there was no blinding, it is plausible that dispatch of intervention or control may have been consciously or unconsciously influenced by this. Intervention group appear sicker pre-intervention (significantly more likely to be older, worse ADLs), and therefore may understate true effect benefit. Low survey return rate.
Synthesis	Secondary synthesis (physician oversight).
Data extraction	MWS SC

Study	Breyre A, Taigman M, Salvucci A, Sporer K. Effect of a Mobile Integrated Hospice Healthcare Program on Emergency Medical Services Transport to the Emergency Department. Prehospital Emergency Care. 2022 May 4;26(3):364–9.
Country	USA
Study design	Prospective observational before-after study. Costs are not measured. The study is not an economic evaluation.
Setting / context	After 911 was called, standard paramedics arrived on scene first, and then referred to the intervention team via a phone call with a hospice RN and a Palliative Paramedic. Data collection from Before period: January - June 2015 (Training period: July 2015), After period: August 2015-July 2018, Before period of 6 months, After period of 3 years.
Participant Characteristics	Palliative care patients calling 911 for a reason related to their illness.
Groups	• 645 sample • 523 intervention attended by CPs • 122 comparison attended by standard EMS (routine data gathered in 6 months prior to program implementation)
Intervention	Community Paramedics with extra training and an RN. Additional 30 hours in palliative care / grief counselling (16 hours classroom, 14 hours with hospice RNs), pre-requisites of 200 hours of CP training beforehand. Working alone in a non-transport vehicle.
Outcomes measured	ED transportation rate, reason for transportation, use of equipment. OR & ARR with 95% CI and p. Comparison with routine data gathered in 6 months prior to program implementation. Cases reviewed by a committee of a physician, 3 paramedics, and 2 hospice RNs.
Results & author conclusion	Palliative care paramedics reduce ED transportation; extended scope of practice is not necessary for this to occur. ED transportation OR 0.125, 95% 0.077-0.201, p<0.0001, ARR 46.6%, 95% 38.53-54.72.
Reviewer comments	Standard paramedics on scene first - would increase cost overall and reduce any potential cost benefit. Specific palliative care population.
Bias risk	Unclear if before period includes calls unrelated to palliative illness (these were excluded in after period). If it does include 'high acuity' calls that are more likely to require transportation to hospital, this is likely to skew data towards overstating the benefit of the intervention group for the outcome of ED transportation rate.
Synthesis	Secondary - special population
Data extraction	MWS MG

Study	Cooper S. The emerging role of the emergency care practitioner. Emergency Medicine Journal. 2004 Sep 1;21(5):614–8.
Country	UK
Study design	Prospective observational before-after study. Costs are not measured. The study is not an economic evaluation. Note: the full study is mixed methods, but the qualitative components are not relevant to this review and are excluded.
Setting / context	All 999 calls. An average of 13000 per month (65000 approximately over study period) in region. The paper also includes paramedics working in a minor injury clinic - segregated data is provided, and only the EMS cases included in this review.
Participant Characteristics	51% male, 44% female, aged <1 – 99. Note: this data is pooled and includes minor injury clinic patients. Data collection from October 2002 - March 2003, 6 months.
Groups	• 501 sample • 170 intervention • 331 comparison
Intervention	Emergency Care Practitioner, completed a Master's degree. Working alone in a non-transport vehicle. Dispatched directly 34% of the time, self-activated from available jobs 45%, requested by regular paramedics 18%.
Outcomes measured	ED transportation rate, representation rate (24 hours). For the quantitative component, reporting of routinely collected data was analysed with univariate analysis, X2 nominal (Yates correction), Spearman for ordinal.

Results & author conclusion	Investment in the ECP role could be beneficial, however, more work is required to evaluate the development of practice, the quality of care, and cost benefits. A major reported benefit of the ECP role was the reduction in unnecessary trips by emergency ambulance to A&E and an ability to re-grade incidents appropriately.
Reviewer comments	The full study is mixed methods, but the qualitative components are not relevant to this review and are excluded. All the practitioners were based in a local minor injury unit (MIU) for half their working week and their base ambulance station for the remainder.
Bias risk	Comparison group data appears to include all EMS calls (i.e. it includes 'high acuity' calls that are more likely to require transportation to hospital). This is likely to skew data towards overstating the benefit of the intervention group for the outcome of ED transportation rate. Selection bias; patients not randomised to intervention or control group and clinical judgement guided CP deployment. As there was no blinding, it is plausible that dispatch of intervention or control may have been consciously or unconsciously influenced by this. This is acknowledged by the authors. 24 hour representation rate is used as a proxy for 'quality', however, in the reviewer team's opinion, 24 hours is likely too short a time period to capture meaningful rates of representation. Additionally, representation is noted to be a poor proxy for quality.
Synthesis	Primary
Data extraction	MWS MG

Study	Dixon S, Mason S, Knowles E, Colwell B, Wardrope J, Snooks H, et al. Is it cost effective to introduce paramedic practitioners for older people to the ambulance service? Results of a cluster randomised controlled trial. Emergency Medicine Journal. 2009;26(6):446–51.
Country	UK
Study design	Prospective experimental cluster randomised-controlled trial with an in-built cost-utility analysis (EQ-5D, UK tariff, TTO method). 'Cluster randomisation to reduce the risk of contamination (practice in the control group being influenced by the presence of the paramedic practitioner in the community) and to allow service level, rather than individual patient level, evaluation of the intervention. Weeks were randomised before the start of the study (to allow for rostering of the paramedic practitioners) to the paramedic practitioner service being active (intervention) or inactive (control), when the standard 999 service was available. The forward roster was concealed from other members of the emergency services. During inactive weeks, the paramedic practitioners were removed from operational duties.'
Setting / context	Urban area. 56 weeks allocated to intervention or normal service. During intervention weeks, practitioners were available 0800-20000, 7 days, to all 999 callers aged >60 years within a particular geographical area. Limited to complaints "that fell within the scope of practice of the paramedic practitioner", including falls, lacerations, epistaxis, minor burns, foreign bodies, local anaesthesia, wound care, suturing, neuro, cardiac, pulmonary, additional medications. Data collection from 1 September 2003 to 26 September 2004, 13 months.
Participant Characteristics	72.6% female, mean age 82.6 years, 77.8% living in own home.
Groups	• 3018 sample • 1549 intervention (as low as 1510 for some outcomes due to incomplete data) • 1469 comparison (as low as 1437 for some outcomes due to incomplete data)
Intervention	Paramedic practitioner (n=7). Training (taken from Mason 2008, confirmed to be the same) was 3 weeks of full-time theory with lectures from specialists in emergency medicine or care of the elderly, followed by 45 days in supervised practice. Working alone in a non-transport vehicle.
Outcomes measured	ED transportation rate, ED attendance within 28 days, admission with 28 days, mortality at 28 days, total episode time, specialist paramedic minutes, regular paramedic minutes, number of ambulance activations, length of time in ED, same day admission length of stay, hospital social referrals, community social referrals, cost, EQ-5D, QALY. CEAC calculated. Both marginal and incremental analyses are presented. Incremental cost per QALY ratio calculated. This was from an estimated baseline compared to actual 28-day EQ-5D (UK tariff, TTO method). Sensitivity analysis based on unit cost of intervention. Training costs calculated over 20 years with 3.5% discount rate. Costs calculated from normal routinely-collected data, with some costs (community care) provided by patients during 28-day questionnaire.
Results & author conclusion	"Given these economic results in tandem with the clinical, operational and patient-related benefits, we would recommend consideration of the wider implementation and evaluation of similar schemes." >95% chance of being cost effective using usual $20,000 pound QALY. Total costs 140 pounds lower in intervention group (not statistically significant). Reductions in the episode length, admissions to ED and hospital, and increased patient satisfaction for those patients in the intervention group. "Paramedics with extended skills can provide a clinically effective alternative to standard ambulance transfer and treatment in an emergency department for elderly patients with acute minor conditions." (Mason, 2007). "The service conveyed considerable benefits for patients and the NHS in terms of reduced over-all attendances at an emergency department and hospital, shorter episode times, and higher levels of satisfaction among patients." (Mason, 2007) "Clinical records were rated independently by two senior ED clinicians to identify related episodes, avoidable subsequent episodes, and suboptimal care… Of the 2,025 patients included in this analysis, there were 16 agreed upon episodes related to suboptimal care (0.80%). This study suggests that appropriately trained paramedics with extended skills treating older people with minor acute conditions in the community are as safe as standard EMS transfer and treatment within the ED." (Mason 2008) ED transportation RR 0.72 (95% CI 0.68-0.75), p<0.001.
Reviewer comments	Three studies use the same data set from the paramedic practitioner in older people's support (PPOPS) scheme. The other two papers were excluded to avoid duplication of data: Dixon S, Mason S, Knowles E, Colwell B, Wardrope J, Snooks H, et al. Is it cost effective to introduce paramedic practitioners for older people to the ambulance service? Results of a cluster randomised controlled trial. Emergency Medicine Journal. 2009;26(6):446–51.

	Mason S, Knowles E, Colwell B, Dixon S, Wardrope J, Gorringe R, et al. Effectiveness of paramedic practitioners in attending 999 calls from elderly people in the community: Cluster randomised controlled trial. Br Med J. 2007;335(7626):919–22. Mason S, Knowles E, Freeman J, Snooks H. Safety of Paramedics with Extended Skills. Academic Emergency Medicine. 2008 Jul;15(7):607–12. Data cross checked against all three papers for inconsistencies. RR for ED transportation rate and participant characteristics was not reported in Dixon and was taken from Mason 2007 and Mason 2008 (confirmed to align with Dixon). All other outcomes (28 day admission, episode time, 28 day mortality) were reported identically in both papers.
Bias risk	Selection bias; as dispatcher (a paramedic practitioner) was not blinded to if it was an intervention or control week, it is plausible that inclusion of patients in the study may have been consciously or unconsciously influenced by this. This is acknowledged by the authors. To mitigate this, the study used service-wide weeks of activation / inactivation with costs calculated on a service-wide basis. Reporting bias. Some costs (including community care) were provided by patients during a 28-day questionnaire. The paper likely understate the true effect size due to use of ITT. 'More than a quarter (29.6%, n=459) of patients in the intervention group did not receive the paramedic practitioner service. These patients therefore received the "normal service" but were still included in the "intervention" group as the results were analysed on a pragmatic intention to treat basis, reflecting the outcomes that could be expected were the intervention to be introduced more widely, and standard for the reporting of the results of health services research. This had the effect of considerably weakening the impact of the intervention.'
Synthesis	Primary
Data extraction	MWS CY

Study	Hoyle S, Swain AH, Fake P, Larsen PD. Introduction of an extended care paramedic model in New Zealand. Emergency Medicine Australasia. 2012 Dec;24(6):652–6.
Country	NZ
Study design	Retrospective observational cohort study. Costs are not measured. The study is not an economic evaluation.
Setting / context	The first 1000 consecutive clinical presentations attended by ECPs in the Kapiti region. 'The district is a popular retirement area, and as a result the proportion of residents over the age of 65 is twice the national average.' Data collection from May 2009 - February 2010, 10 months.
Participant Characteristics	Mean age 62.2 years (2 months - 101 years), 58.7% female, 6 deceased on arrival
Groups	• Sample size unknown • 1000 presentations in intervention • 797 patients in intervention • Comparison size unknown
Intervention	Extended Care Paramedic. 10 day training course. Working alone in a non-transport vehicle.
Outcomes measured	ED transportation rate, 7 day representation rate. Comparison of routinely collected data extracted directly from patient's medical record – no comparison data for 7 day representation rate, which was consequently excluded. Representation to the local hospital included (did not include presentations to other hospitals) and reviewed by physician to determine if related to original condition.
Results & author conclusion	ECPs "Reduce hospital ED attendances by treating more patients in the community, and this is associated with a low rate of subsequent ED presentations." 'Although treating patients within the community offers advantages to the health system by reducing pressure on EDs, it also offers advantages to the patient. Avoiding transport to the hospital and lengthy delays associated with acute treatment when this is not required has been seen as a very positive aspect of ECP programmes.'
Reviewer comments	The study is primarily descriptive statistics of the intervention group, however, comparative statistics from the emergency paramedic group are provided in the instance of ED transportation rate. Possibly understates true effect size. 'Because this study describes the first 1000 clinical presentations attended by ECPs in a model that was developing, some of these patient management pathways were not fully developed and integration into the local health setting was still occurring. For these reasons it is probable that a slightly higher rate of treatment in the community could be achieved in a more developed model.' This paper's data set overlaps with Swain 2012. Swain 2012 provides absolute numbers, and the comparison group are drawn from similar calls (i.e. non-urgent calls) that are less likely to require transportation and therefore less likely to skew data. However, the sample size for Swain 2012 is far smaller at n = 50 in each of the intervention and comparison group, and that paper is focussed on a different topic (patient satisfaction) and incidentally happens to present transportation rate data. By contrast, Hoyle 2012 has a far more reliable sample size (n = 1000) that is twenty times larger than Swain 2012, provides absolute numbers for the intervention group's outcome (402/1000) and the paper is directly focussed on the topic of interest for this review (and includes an additional relevant outcome, 7-day ED representation rate). However, Hoyle 2012's comparison group ED transportation rate (74%) is only provided as a rate without absolute numbers, and additionally appears to include emergency calls (i.e. it is not only limited to calls suitable for an ECP, potentially leading to an increased transportation rate and skewing the results to make the benefit appear greater than it is). The review group ultimately decided to include Hoyle 2012, and to exclude Swain 2012. This decision was based on three factors. Firstly, the control group ED transportation rates was higher in Swain 2012, meaning that using that study may overstate the benefit of ECPs. In Hoyle 2012, there was 402/1000 (40%) seen by ECPs transferred to the ED, and an unknown amount (74%) seen by paramedics transferred to the ED, providing a difference of 34%. In Swain 2012, there was 11/50 (22%) seen by ECPs transferred to the ED, and 42/50 (84%) seen by paramedics transferred to the ED, providing a difference of 62%. Secondly, the sample size in Hoyle is twentyfold larger than Swain, providing less risk of sampling error. Thirdly, the reviewers noted that Hoyle was directly focussed on the outcomes of interest for the review, while Swain was on a different topic and happened to incidentally present relevant results. Therefore, the reviewers determined to include Hoyle 2012's data, and exclude Swain 2012's data.

Bias risk	Comparison group data appears to include all EMS calls (i.e. it includes 'high acuity' calls that are more likely to require transportation to hospital). This is likely to skew data towards overstating the benefit of the intervention group for the outcome of ED transportation rate. Selection bias; patients not randomised to intervention or control group and clinical judgement guided ECP deployment. As there was no blinding, it is plausible that dispatch of intervention or control may have been consciously or unconsciously influenced by this. This is acknowledged by the authors. Cannot exclude possibility that patients presented to a different hospital and were not captured in 7-day representation risk. As there is no comparison group for representation rate this data is excluded, and therefore not considered problematic by the review team.
Synthesis	Primary
Data extraction	MWS SC

Study	Jensen JL, Marshall EG, Carter AJE, Boudreau M, Burge F, Travers AH. Impact of a Novel Collaborative Long-Term Care – EMS Model: A Before-and-After Cohort Analysis of an Extended Care Paramedic Program. Prehospital Emergency Care. 2016 Jan 2;20(1):111–6.
Country	Canada
Study design	Retrospective observational cohort study. Costs are not measured. The study is not an economic evaluation. Note: the full study is prospective before-after cohort study, however, only After period data will be used in this review, segregated by intervention, to avoid skewing data; therefore effectively a cohort study for the review.
Setting / context	RACF residents at 10 different sites, called 911 with 'low acuity dispatch' or if ECP specifically requested by RAC, available 0900-2100 daily, all cases overseen by physician via teleconsultation. Data collection from 1 September 2010 to 31 January 2011 and 1 September 2011 to 31 January 2012, 5 months. Note: full study includes 2 x 5 month periods, however, only After period included, as discussed earlier.
Participant Characteristics	Median age 85, 66% female.
Groups	• 224 sample (After period only) • 128 intervention ECP group • 96 comparison EMS group
Intervention	Extended Care Paramedic, undertaking a further 2 weeks training in in wound care (suture, adhesive), imaging, ED referrals, consultation, and palliative care. Working alone in a non-transport vehicle.
Outcomes measured	ED transportation rate, response time, on scene time, admissions, ED length of stay, 48 hour relapse rate. 'Relapse' within 48 hours only if for the same condition; this was determined by consensus among 9 paramedics and physicians as a committee. Comparison of routinely collected data extracted directly from patient's medical record. Univariate analysis (ANOVA), X2 categorical, t for continuous.
Results & author conclusion	"In this cohort analysis, it was found that the addition of the ECP program significantly reduces the number of patients transported to the ED and number of admissions, without any significant risk to patient safety." "The addition of ECP to the LTC model of care appears to be beneficial and safe, with few relapse calls identified."
Reviewer comments	Paramedic specialists are not operating autonomously, and the study therefore does not directly examine their effect on outcomes. Exclude from primary synthesis and include in the secondary synthesis. The population are RACF residents, and therefore not generalisable to broader population. Exclude from primary synthesis and include in the secondary synthesis. Before period are only EMS; after period are both EMS and CP - therefore cannot use before-after data as the combination of intervention and control in the after period will skew data - can only using the after period data, segregated by EMS and ECP
Bias risk	Both groups include all categories of EMS calls (i.e. 'high acuity' calls that are more likely to require transportation to hospital). However, the comparison group include more category 1-3 calls; 76.1% of comparison responses were category 1-3, while 28.9% of intervention responses were category 1-3 (canadian 5-point triage scale). Comparison group includes 2 (2.1%) triage category 1 cases. This is likely to skew data towards overstating the benefit of the intervention group for the outcome of ED transportation rate. Selection bias; patients not randomised to intervention or control group and clinical judgement guided ECP deployment. As there was no blinding, it is plausible that dispatch of intervention or control may have been consciously or unconsciously influenced by this. This is acknowledged by the authors. 48 hour representation rate is used as a proxy for 'quality', however, in the reviewer team's opinion, 48 hours is likely too short a time period to capture meaningful rates of representation. Additionally, representation is noted to be a poor proxy for quality. Some missing data. Admission not reported for 11; dispatch time missing for 3; departed scene time missing for 73; time patient left ED missing for 8; time patient placed in ED bed missing for 8). Authors state they do not believe this affected outcomes.
Synthesis	Secondary – physician oversight and special population
Data extraction	MWS DC

Study	Jensen JL, Travers AH, Bardua DJ, Dobson T, Cox B, McVey J, et al. Transport outcomes and dispatch determinants in a paramedic long-term care program: a pilot study. CJEM. 2013 Jul 4;15(04):206–13.
Country	Canada
Study design	Retrospective observational cohort study. Costs are not measured. The study is not an economic evaluation.
Setting / context	RACF at 15 sites over 3 months, available from 0900h to 2100h 7 days per week. 'ECPs were dispatched through a variety of mechanisms: 1) emergency medical dispatchers may dispatch the ECP based on a predetermined list of low-acuity dispatch determinants; 2) ECPs may be dispatched as a result of specific request from the LTC facility staff (based on their clinical judgment, for any clinical reason or acuity); 3) emergency paramedics may recognize a LTC call as appropriate for the ECP to manage and may request the ECP after consultation with the OLMOP; 4) the ECP may arrange a follow-up visit; and 5) the ECP may be added to a call as an extra resource after emergency paramedics were already assigned.' In all cases when feasible, the ECP was required to consult with the OLMOP and the LTC physicians. Data collection from 15 March 2011 - 15 June 2011, 3 months.

Participant Characteristics	Not reported.
Groups	• 238 sample • 140 intervention • 98 comparison It was noted that "the ECP may arrange a follow-up visit", which would not be eligible data for the review. Segregated data requested from authors (see Appendix VII). This was not available. However, as the number of scheduled follow-up visits by ECPs is explicitly discussed (n = 19, per Table 2), it is possible to discuss sensitivity analysis on the low and high rates if the scheduled visits are assumed to all be in the transport group or non-transport group respectively. This analysis is provided below under Outcomes measured.
Intervention	Extended Care Paramedic with 110-180 hours additional training. 'These ECPs received additional specialized training in the following "extended care" roles: 1) geriatric assessments and management, 2) end-of-life care, 3) primary wound closure techniques (suturing, tissue adhesive), and 4) point-of-care testing.' Additional information provided by authors: 5-10 hours online directed learning modules, 40 hours peer education, 5-10 hours of labs, 60-120 hours of mentorship shifts. Working alone in a non-transport vehicle.
Outcomes measured	Dispatch determinants, ED transportation rate, 48 hour representation rates. Comparison of routinely collected data extracted directly from patient's medical record. For the ED transportation rate, sensitivity analysis was conducted to find the range of the effect size if scheduled follow-up visits are excluded. Combined data: 140 cases, 98 left at home, 42 transported. 30% transportation rate. Lowest possible effect size: 121 cases, 79 left at home, 42 transported. 35% transportation rate. Highest possible effect size: 121 cases, 98 left at home, 23 transported. 19% transportation rate. Non-transportation rate therefore ranges from 65% to 81%.
Results & author conclusion	"Reduced transports to the ED with a low rate of relapse."
Reviewer comments	Paramedic specialists are not operating autonomously, and the study therefore does not directly examine their effect on outcomes. Exclude from primary synthesis and include in the secondary synthesis. The population are RACF residents, and therefore not generalisable to broader population. Exclude from primary synthesis and include in the secondary synthesis. ECPs could leave at scene, transport immediately, or organise an ED presentation the next day. The ED presentations the next day have been included in the ED transportation group, as this does not reduce ED use. Prior to implementation, 100% of RACF callers were transported.
Bias risk	Both groups include all categories of EMS calls (i.e. 'high acuity' calls that are more likely to require transportation to hospital). However, the ECPs attended more 'low acuity' calls while the EMS attended more 'high acuity' calls (Table 3). This is likely to skew data towards overstating the benefit of the intervention group for the outcome of ED transportation rate. Selection bias; patients not randomised to intervention or control group and clinical judgement guided ECP deployment. As there was no blinding, it is plausible that dispatch of intervention or control may have been consciously or unconsciously influenced by this. 48 hour representation rate is used as a proxy for 'quality', however, in the reviewer team's opinion, 48 hours is likely too short a time period to capture meaningful rates of representation. Additionally, representation is noted to be a poor proxy for quality.
Synthesis	Secondary – physician oversight and special population
Data extraction	MWS DC

Study	Mason S, O'Keeffe C, Knowles E, Bradburn M, Campbell M, Coleman P, et al. A pragmatic quasi-experimental multi-site community intervention trial evaluating the impact of Emergency Care Practitioners in different UK health settings on patient pathways (NEECaP Trial). Emergency Medicine Journal. 2012 Jan 20;29(1):47–53. Note: further data, including incomplete cost and QALY data, available in the accompanying report, Mason et al., A multi centre community intervention trial to evaluate the clinical and cost effectiveness of emergency care practitioners. Report for the National Co-ordinating Centre for NHS Service Delivery and Organisation R&D (NCCSDO), 2009.
Country	UK
Study design	Prospective pragmatic quasi-experimental multi-site trial. The study is described by the authors themselves as a cost-effectiveness analysis. However, in the reviewer opinion, this study does not meet the definition of cost-effectiveness analysis. A cost-effectiveness analysis requires that both costs and health consequences are measured, and that health consequences are measured in natural units. The review team assumes that the authors have chosen this classification on the basis that ED transportation rate (and the other outcomes measured) can be described as 'natural units' that represent health. However, the review team instead prefer the stricter view that ED transportation is likely unrepresentative of health status (being affected by other factors such as patient preference that have no link to objective health status). As there is no data presented to verify the assumption that the outcomes chosen represent health outcomes, a partial economic evaluation such as a cost-minimisation description is also inapplicable. Therefore, the review team prefer the view that this study does not capture health outcomes, and cannot be described as a health economic evaluation. The full study included five matched pairs of intervention (ECP) and control services (usual care providers): ambulance, care home, minor injury unit, urgent care centre and GP out-of-hours. Only the ambulance data has been included in this review.
Setting / context	All 999 calls except 'ineligible conditions': chest pains, severe road collisions, obstetrics, overdoses, assaults and anything involving alcohol. 'Control' trust sites that did not employ ECPs, but were in close geographical proximity (within the same or in a neighbouring county). Data collection from May 2006 and August 2007, 15 months.
Participant Characteristics	Intervention Mean age 69.2 years (SD 23.2) 61.7% female, control Mean age 62.5 years (SD 24.2) 51.8% female.
Groups	• 1107 sample (911) • 593 intervention group • 514 comparison group

Intervention	Emergency Care Practitioner (n=23, 15 paramedics and 8 nurses). Training is a nationally developed core competency programme, 13 weeks' classroom learning, 6 months ED and GP placements. Includes supplying medications, ordering relevant investigations and tests, such as x-rays. Working alone, unclear if in an ambulance or a non-transport vehicle.
Outcomes measured	Cost per patient, ED transportation rate, referral to primary care, investigations ordered, treatment given, time. Univariate analysis, X2 categorical, t for continuous. For the analysis of episode times, a log-transform was applied to the data and the results presented as time ratios (which can be interpreted as the ratio of the median episode time in the ECP compared to control). OR presented. Some outcome data taken from Mason et al., A multi centre community intervention trial to evaluate the clinical and cost effectiveness of emergency care practitioners. Report for the National Co-ordinating Centre for NHS Service Delivery and Organisation R&D (NCCSDO), 2009. That study also QALY and cost per patient. However, due to the incomplete data, an CEA was not completed 'cost-effectiveness analysis was not possible due to high rates of missing data.' This data found no significant difference in the rate of QALY or Cost for the subset of patients with sufficient data to allow computation. The review team agreed with the decision of the authors, and also excluded this data.
Results & author conclusion	'ECPs can have clear benefits to patients within certain emergency and urgent healthcare settings. However, ECP working is not a panacea and a beneficial impact of the role on patients may not transfer equally across all settings or health professionals. ECP training and skills are intended to equip practitioners to see a selected but limited range of clinical complaints and patient groups. The extended role should be considered a way of supplementing existing roles by releasing doctor and other resource time, (such as unnecessary ambulance journeys, or ED attendances) for more appropriate targeting of these services to patient need.' 'Further research is also needed to establish the cost-effectiveness of ECPs in comparison to usual care providers.'
Reviewer comments	Geographically different areas in comparison and control Some data on training taken from Appendix I of Mason et al., A multi centre community intervention trial to evaluate the clinical and cost effectiveness of emergency care practitioners. Report for the National Co-ordinating Centre for NHS Service Delivery and Organisation R&D (NCCSDO), 2009.
Bias risk	Due to limited information reported within the study, determination of possible biases is difficult. Possible selection bias. It is unclear how the intervention paramedics were dispatched. Presumably, this was done purposefully rather than blinded, and the dispatcher was not blinded to if it was an intervention or control paramedic. If this is true, it is plausible that inclusion of patients in the study may have been consciously or unconsciously influenced by this. The corresponding report specifies that both groups include all categories of EMS calls (i.e. 'high acuity' calls that are more likely to require transportation to hospital). However, the number and category of calls attended is not provided. If the ECPs attended more 'low acuity' calls while the EMS attended more 'high acuity' calls, then this would be likely to skew data towards overstating the benefit of the intervention group for the outcome of ED transportation rate.
Synthesis	Primary
Data extraction	MWS CY

Study	Mason S, O'Keeffe C, Coleman P, Edlin R, Nicholl J. Effectiveness of emergency care practitioners working within existing emergency service models of care. Emergency Medicine Journal. 2007 Apr 1;24(4):239–43.
Country	UK
Study design	Observational costing study. Unclear if prospective or retrospective. Note: the full study is mixed methods. This review will only consider the quantitative aspects. While the study provides EQ-5D findings ('found no differences'), the actual outcome data is not presented. Without this data, a cost-utility analysis is not possible (and there is no report of one being performed by the authors). The authors themselves describe the results as a costing study, and the review team agrees with this description.
Setting / context	999 callers who were eligible for an ECP, March to May 2005. Data collection from March 2005 - May 2005, 3 months.
Participant Characteristics	Median age 60, 63% female.
Groups	• 233 sample • 125 intervention • 108 comparison
Intervention	Emergency care practitioner, training 'formal training and extended clinical skills' (no further explanation found). Unclear if responding solo or as a crew, and unclear if working in an ambulance or a non-transport vehicle.
Outcomes measured	ED transportation rate, cost, EQ-5D. All outcomes were adjusted for age, sex, presenting complaint, and service model, which did not affect results. Costs are measured in 2004 UK pounds. Comparison of routinely collected data extracted directly from patient's medical record, and patient-reported data from a survey completed at 28 days.
Results & author conclusion	'Care provided by ECPs appears to reduce the need for subsequent referral to other emergency and unscheduled care services in a large proportion of cases. We found no evidence that the care provided by an ECP was less appropriate than the care by the usual providers for the same type of health problem.' 'This saving comes primarily from lower staff costs at incident, avoided ED attendances and lower use of non-inpatient follow-up services.' 'The potential for avoiding ambulance journeys, ED attendances and hospital admissions appears to be present when ECPs are operating through the 999 setting.' 'Using the EQ-5D,6 we found no differences between patients seen by ECPs and those seen by the usual providers in self-reported health status at 3 and 28 days after their initial contact.' There is a citation in the statement that ECPs are associated with a cost saving of 291 pounds; however, the citation is for the reference that was unable to be found by our research team, librarians, or the authors of that study themselves. Therefore, we are unable to confirm this data. OR of being left at home 32.1
Reviewer comments	Economic evaluation very poorly described - unclear what costs were included, or exactly what method was used (study mentions EQ-5D earlier on). Significant concerns about lack of clarity in costing component. Not randomised, 28 day

	representation is self-reported by patients via a survey, economic findings unlikely to be generalisable, large amounts of missing data for costing. Conclusion that 'Costs information (based on one site only) indicated that ECP care may be cost effective in that model of ECP working' is unsupported – there is no cost utility analysis performed – can only conclude that costs are lower, not that this is effective.
Bias risk	No baseline EQ-5D. Assumption that intervention and control groups are identical at baseline for EQ-5D and that changes are linear. This assumption is considered by our review team as unlikely to hold if tested, especially as the authors have reported that there were significant differences between the control (younger, more likely to live at home) and intervention groups (older, less likely to live at home). Large missing data. Costing data based on just 56 patients (24%). Survey bias. Some cost data (subsequent health resource use) was calculated based on patient-reported outcomes at 28 day survey. While all patients completed the survey, their responses may have been influenced by survey bias. Selection bias; patients not randomised to intervention or control group and clinical judgement guided ECP deployment. As there was no blinding, it is plausible that dispatch of intervention or control may have been consciously or unconsciously influenced by this. ECP patients were older, and therefore arguably more likely to have comorbidities and a worse baseline status resulting in worse outcomes. This would potentially skew the study towards a finding of non-significance. 'Our economic findings have to be regarded with caution. The savings of approximately £291 (US$566) per patient seen by an ECP in service one are based on limited data in one operational setting, and may not be generalisable. Also, within a 999 ECP model, the finding may be affected by whether or not the practitioner was formerly employed as a nurse, as a paramedic or from another background. Assuming identical performance, the additional training cost required for equivalent competence training may affect the overall cost-effectiveness reported.'
Synthesis	Primary
Data extraction	MWS MT

Study	Thompson C, Williams K, Morris D, Lago L, Kobel C, Thompson C;, et al. HWA Expanded Scopes of Practice program evaluation: Extending the Role of Paramedics sub-project: final report. 2014.
Country	Australia
Study design	Prospective observational costing study. While the full study is mixed methods, only the quantitative components relative to this review have been extracted. While the study states that it includes an 'economic evaluation' and refers to 'cost-effectiveness' (noting that the term 'cost-effectiveness analysis' is not used at any point), the review team consider that the effectiveness outcome (ED or GP transportation-referral) does not reflect health outcomes. Multiple factors may influence ED or GP transportation unrelated to ECP treatment, and there is no convincing evidence available to suggest this is a reliable metric. Therefore, the review team have determined that this study does not meet the formal criteria of an economic evaluation, and is best characterised as a costing study.
Setting / context	The intervention was set across 5 different sites in four States, including metro and rural areas. 000 call or direct referral from RACF/GP within geographic region. Data collection from 1 January 2013 and 31 March 2014, 15 months.
Participant Characteristics	Not provided. The patient-reported survey demographics on page 33, at less than 10% of the sample, was not able to be assumed as representative.
Groups	• 2,118 intervention • 388,774 comparison (all 000 calls in that entire jurisdiction for the year, including the intervention group, taken from the Report on Government Services 2012-13)
Intervention	Extended Care Paramedic (n = 17). Prior to additional training, 88% were ICPs (n = 15), and 35% also qualified as RNs (n = 6). Experience ranged from 3 – 35 years. Training varied across clinicians. Most commonly this included 111 hours of lectures, 23 hours of tutorials, 18 hours of simulation training, and 232 hours of clinical placement. This was provided as four weeks of theory taught in modules, a two-week clinical placement in the major city with two additional weeks in a regional centre or home base for the ECPs, and four weeks of internship or supervised practice. Mixed model of working alone or in a crew, and mixed ambulance or non-transport response vehicle.
Outcomes measured	ED transportation rate, representation (any time during the study) rate, and cost. For the ED transportation rate, the intervention group was compared with the mean ED transportation rate for that entire State (including the ECP group). Representation rate was only presented for the intervention group, and as no comparison is available was excluded. Costs include the sum of: salary (including superannuation and benefits), equipment (5 year life for vehicle and medical equipment, 3 year life for IT equipment), GP visits (using the MBS <20 minutes rate), hospital costs (using the IHPA data), and ECP initial training (totalling approximately $30,000 AUD). Costing took healthcare system perspective – gap fees, patient time, etc are all not costed. The economic model presented under 5.2 does not calculate the cost of usual care, which is crudely estimated by dividing total service expenditure by total number of patients.
Results & author conclusion	'Potential cost savings for the health care system through the use of ECPs but only where volume and diagnostic accuracy is high enough. The crucial factors for successful implementation are sufficient levels of ECP activity and high ED avoidance rates through correct identification at the time of call.' 'The ambulance service bears the cost of setting up and running it while other stakeholders of the health care system realise some of the benefits.'
Reviewer comments	Appears likely but uncertain that intervention calls that came from a source other than 000 (primarily RACFs and GPs) would otherwise have had to call 000. Appears that program may have potentially included proactive calls that are ineligible for inclusion: "It gives us the ability for the ECPs to book appointments. We can actually anticipate jobs and make contact with them. So the ECP program's given us the opportunity for our staff to ring up patients, which is unheard of in the ambulance industry; we don't call patients, they call us." Authors were contacted - they confirmed that they estimate that 1-1.4% of patients had a follow up telephone call, and "almost none" had a follow up visit. As this is unlikely to have impacted outcomes the study is nonetheless considered suitable for inclusion, with sensitivity analysis to test the change in outcomes if 1.4% change in

	sample or intervention group outcomes. This found that the outcome of ED transportation rate, calculated at 28%, may be as low as 26%. Explicitly did not seek to capture reduced pressure on local hospital EDs and enhanced continuity and quality of care. Comparison data is not drawn from an equivalent group but from jurisdiction average data for all patients. While the study states that 'only patients who are, or would be seen by an ECP in treatment and control arms are considered', this only refers to data collection for the intervention group. Assumes patients not transported to the ED are seen by a GP ('Na x [ECPa + alpha x GP]') at a rate of 'alpha' – this was estimated at the average number of GP referrals per ECP site. However, some sites reported referrals, some reported patient-reported GP visits, and others provided no data. Consequently, the number of patients referred to the GP is likely to be unreliable.
Bias risk	Patient transportation cost was noted to be highly unreliable in reviewer opinion. This was crudely calculated by dividing total ambulance service expenditure by the total number of patients (Table 28). This is highly unlikely to be accurate, as this cost includes pre-arrival costs and post-transport costs. The study also did not directly capture any hospital information. Hospital expenses were calculated using the Independent Hospital Pricing Authority average jurisdictional cost, and then weighted by triage acuity and rurality. It was assumed that all ECP patients were category 4 or 5 and were admitted at the usual rate, both assumptions that have previously been shown not to be true. For the comparison group, the average cost per patient was calculated by dividing the total patients by the total expenditure of each service, as reported in the Productivity Commission ROGS data. Comparison rate of transport data was taken from ROGS 2012-13 ('Total patients'). This is highly likely to be unreliable, as incremental cost per patient changes significantly as economies of scale effect overheads. Costing was noted to include multiple assumptions, such as estimations for time of GP visit (all assumed to be <20 minutes), estimation on the number of GP visits hospital cost, and training cost. The reliability of these assumptions is unknowable.
Synthesis	Primary
Data extraction	MWS MT

Study	Widiatmoko D, Machen I, Dickinson A, Williams J, Kendall S. Developing a new response to non-urgent emergency calls: Evaluation of a nurse and paramedic partnership intervention. Prim Health Care Res Dev. 2008;9(3):183–90.
Country	UK
Study design	Observational costing study. Unclear if prospective or retrospective. While the study states that 'cost effectiveness of the pilot service was assessed using an incremental cost-effectiveness analysis, where the difference in costs of introducing such a service is compared with the difference in outcomes/effectiveness of the service', the review team consider that the effectiveness outcome (ED transportation-referral) does not reflect health outcomes. Multiple factors may influence ED transportation unrelated to model treatment, and there is no convincing evidence available to suggest this is a reliable metric. Therefore, the review team have determined that this study does not meet the formal criteria of an economic evaluation, and is best characterised as a costing study.
Setting / context	999 caller, Friday to Monday (inclusive), 0800-0000, with a non-urgent call (non urgent defined as triaged as priority C on the A-B-C triage system). Data collection from 5 December 2003 - 15 March 2004, 15 weeks. Data analysed is limited to 13 weeks, as first fortnight used to allow adjustment to new model.
Participant Characteristics	Not presented.
Groups	• 2781 requests to 999 during period (Friday-Monday, during shift times, same area) coded as priority B or C • 198 intervention • 2583 comparison
Intervention	Paramedic working with an RN (either a district nurse or emergency nurse). Unclear if additional training was provided for the paramedic or what this entailed. Ambulance vehicle used.
Outcomes measured	Costs, measured in 2004 UK pounds with unit costs recovered from University of Kent and Department of Health (UK). Incremental analysis used by comparing costs of introducing the service with changes in cost. ED and admission costs were calculated by standard costs per age segment (</>65), and all admissions assumed to be for 1 day. Sensitivity analyses were directly performed to factor uncertainty in the analyses. These were done by manipulating some related variables to reflect differences that might occur and also to provide a safe margin of error to support policy decisions.
Results & author conclusion	'The pilot service was very cost effective for NHS when the wider system was considered. Although it actually costs more to the Ambulance Trust to provide such a service, the pilot service saved money when considered in terms of overall NHS expenditure.'
Reviewer comments	It is unclear what additional training, if any, the paramedic undertook. It is possible that none was provided and the paramedic was simply reassigned to low acuity patients. While overall there was a significant benefit to the healthcare system, with a saving $29260, the program shifted costs onto the ambulance service (who it cost $20,657 to run) and savings were exclusively felt in the ED and hospital admissions ($49,917 saved). The paper states that "no evidence was found to indicate any adverse effects on patient outcomes". However, it is unclear how this conclusion was reached, and it does not appear supported by the data presented in the paper.
Bias risk	Rather than measuring costs from societal perspective, the study used only healthcare provider perspective (ambulance and hospital trusts). Authors considered this suitable during study design. Selection bias; patients not randomised to intervention or control group and clinical judgement guided ECP deployment. As there was no blinding, it is plausible that dispatch of intervention or control may have been consciously or unconsciously influenced by this. The comparison group otherwise appears reliable, being drawn from the pool of calls on the same shift, time, geography, and coding (priority B or C).

	Possibly understates true effect size: 'The economic modelling has used very conservative assumptions about costs in the calculations; therefore, it is possible that the actual cost effectiveness of the pilot service is higher than the calculated figures presented in this paper.'
Synthesis	Secondary – multidisciplinary team
Data extraction	MWS CY

Appendix VIII: Inflation and currency conversion

Cost outcomes have been adjusted for inflation and converted to a single comparable currency (AUD).

The table below summarises key raw data extracted from the articles.

Dixon	2008	• 3,966 GBP / attendance for intervention • 4,116 GBP / attendance for normal EMS • 140 GBP cheaper, insignificant (note – discrepancy with reported group means, which are 150 GBP different; author's figures used)
Mason	2012	• 675 GBP / attendance for intervention • 723 GBP / attendance for normal EMS • 114 GBP cheaper, significant
Mason	2007	• 291 GBP cheaper
Thompson	2014	• Between 707 and 10,020 dollars / attendance for intervention • Between 660 and 1,204 dollars / attendance for intervention* • Between 380 dollars cheaper and 8,816 dollars more expensive • If 6 patients seen per shift: Between 411 and 998 dollars cheaper
Widiatmoko	2008	• 20,657 GBP cost to the ambulance service to introduce the service • 49,917 GBP saving to the whole healthcare system (entirely in hospital) • 29,260 GBP saving to the healthcare system

* Calculated manually by the review team

Inflation is taken from the CPI data from the Office of National Statistics for the UK studies and from the Australian Bureau of Statistics for the Australian study and calculated from the main year of data collection in the study to the most recently released data (2023). The adjusted costs are provided in the table below.

Dixon	2008	• 6,893 GBP / attendance for intervention • 7,154 GBP / attendance for normal EMS • 261 GBP cheaper, insignificant
Mason	2012	• 1,064 GBP / attendance for intervention • 1,139 GBP / attendance for normal EMS • 180 GBP cheaper, significant
Mason	2007	• 480 GBP cheaper
Thompson	2014	• Between 848 and 12,025 dollars / attendance for intervention • Between 792 and 1,445 dollars / attendance for intervention* • Between 456 dollars cheaper and 10,580 dollars more expensive • If 6 patients seen per shift: Between 506 and 1,227 dollars cheaper
Widiatmoko	2008	• 35,249 GBP cost to the ambulance service to introduce the service • 85,177 GBP saving to the whole healthcare system (entirely in hospital) • 49,928 GBP saving to the healthcare system

Currency conversion is taken from the Reserve Bank of Australia (at as 19 May 2023). The converted costs are provided in the table below.

Dixon	2008	• 11,994 AUD / attendance for intervention • 12,448 AUD / attendance for normal EMS • 454 AUD cheaper, insignificant
Mason	2012	• 1,996 AUD / attendance for intervention • 2,137 AUD / attendance for normal EMS • 338 AUD cheaper, significant
Mason	2007	• 900 AUD cheaper
Thompson	2014	• Between 848 and 12,025 AUD / attendance for intervention • Between 792 and 1,445 AUD / attendance for intervention* • Between 456 AUD cheaper and 10,580 AUD more expensive • If 6 patients seen per shift: Between 506 and 1,227 AUD cheaper
Widiatmoko	2008	• 66,121 AUD cost to the ambulance service to introduce the service • 159,777 AUD saving to the whole healthcare system (entirely in hospital) • 93,656 AUD saving to the healthcare system

Appendix IX: Additional outcomes

Several additional outcomes were excluded from the main synthesis as there are either investigated by only a single study or are not effectiveness outcomes. The table below summarises key raw data extracted from these articles.

First author	Year	Outcome			
		Intervention	Comparison	Difference	Calculated Relative Risk
24-hour re-presentation rate					
Cooper	2004	1%	0%	1% increase	RR 5.82 (95%CI 0.24 to 142.23, p=0.280)
28-day re-presentation rate to EMSs					
Mason	2007	'No differences between services'; no further information provided			
28-day re-presentation rate to ED					
Dixon	2009	63%	88%	25% decrease	RR 0.72 (95%CI 0.68 to 0.75, p<0.001)
28-day admission rate					
Dixon	2009	40%	47%	6% decrease	RR 0.87 (95%CI 0.81 to 0.94, p<0.001)
Clinician time with patient (minutes)					
Mason	2012	67	48	19% decrease	Unable to calculate[1]
EMS arrival at ED to EMS clear time (minutes)					
Jensen	2016	113	116	3% decrease	Diff. -3 (95%CI -73 to 79, p=0.938)
Any investigation					
Mason	2012	9%	0.2%	9% increase	RR 35.57 (95%CI 4.92 to 257.32, p<0.001)
Any treatment					
Mason	2012	40%	1%	39% increase	RR 31.17 (95%CI 11.65 to 83.40, p<0.001)
Dixon[2]	2009	50%	68%	18% decrease	RR 0.82 (95%CI 0.76 to 0.88, p<0.001)
28-day mortality					
Dixon	2009	4%	5%	1% decrease	RR 0.87 (95%CI 0.63 to 1.21, p=0.41)

ED = Emergency Department, EMS = Emergency Medical Service, RR = Relative Risk. Exact percentages are used in calculations, but rounded percentages are presented here; therefore, there may be slight inconsistencies across columns. 1) Unable to calculate as IQR provided instead of SD due to skew 2) Taken from Mason 2007, Mason 2008, confirmed data the same; for reasoning, see Appendix VII.