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A case study of eight Partnership for Older People Projects (POPP)

An evaluation of the impact of community-based interventions on hospital use

Research report

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Acknowledgements

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Contents

In recent years, the Department of Health has encouraged efforts to deliver more care in community settings, with the joint aims of avoiding unplanned admissions to hospital and reducing net costs. Interventions that prevent such admissions can, in theory, both improve the quality of care delivered and help address the financial challenges currently faced by the NHS. This research summary outlines the findings of an evaluation conducted by researchers at the Nuffield Trust that examined whether eight such interventions achieved a reduction in hospital use. The evaluation was conducted using a person-based, risk-adjusted approach.

Key points

- We examined eight carefully selected interventions that formed part of the wider Partnership for Older People Projects (POPP) initiative, funded by the Department of Health. Of these, four were thought to have a high likelihood of reducing hospital admissions.
- In the absence of a randomised controlled trial, we compared participants to matched controls. Our research method ensured that participants and controls were similar in terms of a very wide range of characteristics. However, it is possible that our findings could be driven by other, unknown differences between the groups that we were unable to observe.
- When compared to matched control patients, we did not find evidence of a reduction in emergency hospital admissions associated with any of the POPP interventions studied. In some instances, there were more admissions in the intervention group than in the control group. One intervention reduced the number of bed-days, but overall we found that the interventions we studied did not appear to be associated with a reduction in the use of acute hospitals.
- One possible explanation for our findings is that the process of 'case finding' identified unmet need. In other words, when patients first entered into the interventions, the professionals may have identified problems that necessitated hospital admission.
- The impact of hospital-avoidance interventions should be monitored in as close to real-time as possible. If they are not effective, it might be possible to refine the intervention or connected services in order to improve its effectiveness.
- NHS commissioners should consider using person-based risk-adjusted evaluation (PBRE) to test whether preventive care interventions are effectively avoiding hospital admissions. The impact on the NHS of local authority interventions can also be evaluated using NHS datasets in this way.
- The evaluation approach we developed using matched control groups is novel and has several advantages over traditional methods. The approach is relatively inexpensive due to the use of existing data sources, and predictive modelling controls for the natural tendency that some patients have fewer admissions over time.
- The potential to improve the quality of care while reducing net 'downstream' costs is substantial. Further innovation is therefore essential, both in terms of refining the case finding process and in the design of interventions.

1. Background

The costs associated with complex health and social care needs in the UK are expected to rise considerably over the coming years. This is largely due to two linked phenomena: an ageing population and the increasing number of people who will be living live with long-term medical conditions.[1] In an effort to improve the quality of care, while at the same time addressing the financial strain on the NHS and local authorities, efforts are being made across the UK to deliver more health and social care in community settings, with the aim of preventing or delaying admission to hospital or residential care.[2] Emergency hospital admissions are undesirable for the individual patient concerned and are expensive to the NHS, costing over £1,000 per admission on average.[*] It is commonly accepted that many admissions to hospital can be prevented if the right interventions are put in place, at the right time for the right people.

One recent initiative to address this issue was the Partnership for Older People Projects (POPPs). These were a series of innovative projects run by 29 local authorities in partnership with their local PCTs and representatives of the voluntary, community and independent sectors. The aim of the POPP initiative was to:

> *"shift resources and culture away from institutional and hospital-based crisis care for older people towards earlier, targeted interventions within their own homes and communities"*[3].

POPP sites received dedicated funding from the Department of Health over a two-year period (some ran from 2006 to 2008, and some from 2007 to 2009). The projects varied considerably, both in terms of the circumstances in which they operated and in how they were targeted. They ranged from projects that worked with the general older population to projects that focused on a subset of highly-complex users.

The POPP initiative as a whole has been subject to a national evaluation. The Department of Health also commissioned the Nuffield Trust to evaluate a small but carefully selected set of eight POPP interventions.

The impact of these projects could be assessed in many different ways, and a comprehensive evaluation would consider these different dimensions. For example, it may consider the health benefits to individuals; changes in functional status or quality of life; impact on user satisfaction; or impact on the organisations involved with service provision.

This study was specifically focused on the effects of selected POPP projects on the utilisation of hospital care. It was specifically designed to examine the experiences of those individuals who received a POPP intervention, rather than examining aggregated effects on populations at the PCT level. The use of individual-level data, rather than aggregated data, is one of the main differences between the current study and other evaluations of POPP. The other difference is that it used

[*] Nuffield Trust calculation of the median tariff for an emergency inpatient admission in 2008/09 under Payment by Results.

control groups to take account of the natural drop in admissions that occurs when high-risk cases are selected for an intervention.

Choice of POPP interventions for this evaluation

Overall, the 29 POPP sites operated 146 core interventions. The Nuffield Trust was commissioned by to evaluate eight of these interventions. The interventions were selected by the Department of Health on the basis that they met the following criteria:

- the nature of the intervention involved face-to-face individual contact over a reasonable period of time
- the site was able to generate a list of the individuals receiving the intervention, as required by the evaluation approach
- the sites had not already been subject to 'above average' quality of local evaluation.

The eight POPP interventions studied are described in Table 1.1. Four of the interventions were chosen because there was felt to be a strong possibility of impact on hospital use. These were:

- a programme of support workers who worked alongside community matrons with people with long-term conditions
- an intermediate care scheme supporting people on discharge from hospital
- multi-dimensional integrated health and social care teams
- daytime and out-of-hours response services.

The other four POPP interventions were short-term assessment and signposting services (E, F, G and H). These aimed to improve access to low-level preventive services through visiting older people in their own homes, conducting assessments, and referring to (or commissioning) appropriate specialist support. Because of their low-level focus, there was little expectation that these would produce clear evidence of an impact on emergency hospital admissions in the short term. However, they were included in case this approach to evaluation might detect some effects that might elude other more traditional approaches.

The interventions differed in terms of the number of users they saw; ranging from 500 users for intervention A to over 7,000 for intervention C . The interventions operated over different time periods but they all represented a sustained period of investment, with new users brought in to receive the interventions over a protracted period of time (see Figure 1.1).

Figure 1.1: Number of new people receiving POPP interventions per month in four sites studied

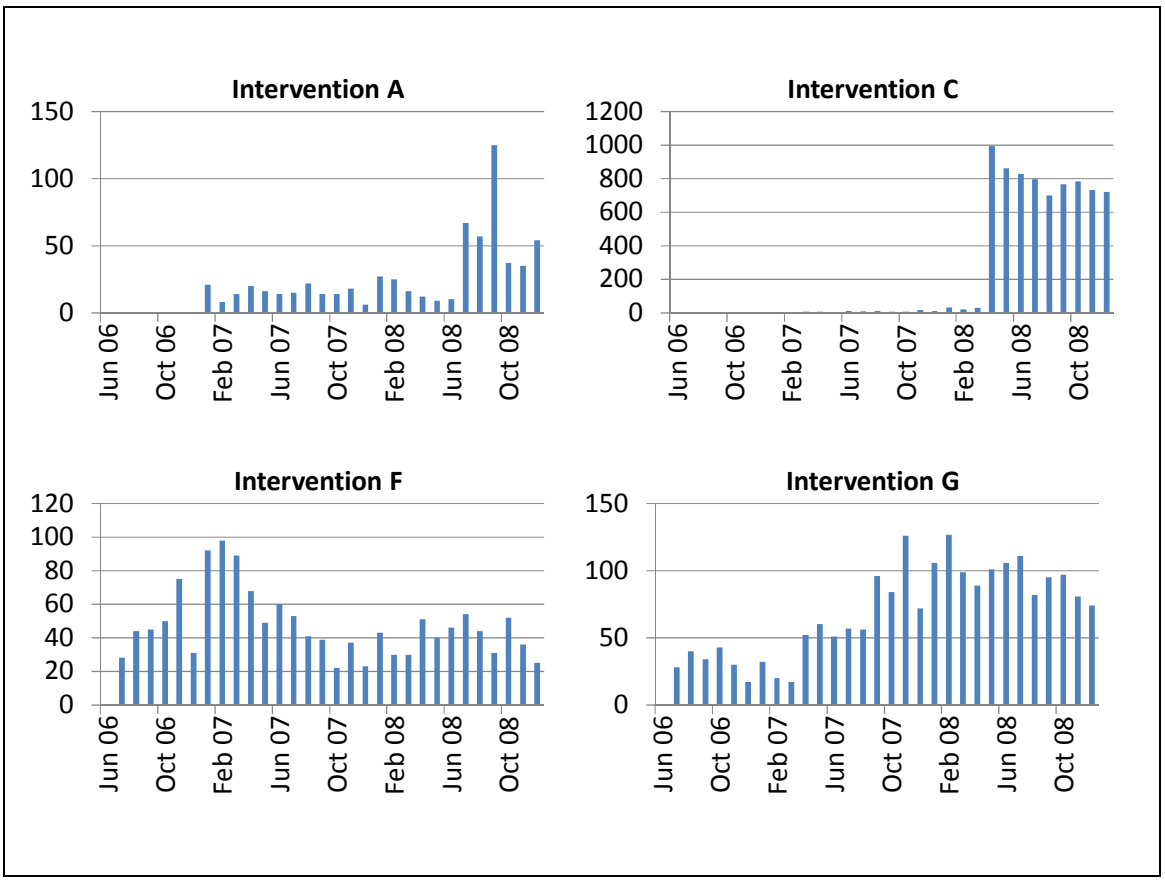


Table 1.1: The eight POPP interventions examined in this study

Intervention		Number of users[†]
A	Support workers working under the direction of community matrons with people with one or more long-term conditions who were felt to be at risk of deterioration or were unstable. Support workers provide personal nursing and social care.	500
B	Intermediate care service with generic workers, which supported people on discharge from hospital.	700
C	Integrated health and social care teams configured around primary care teams, which focused on people with one or more long-term conditions.	7,400
D	Out-of-hours response service and daytime response service, both consisting of an integrated team comprising community alarm services, mobile wardens, generic workers, district nurses, paramedics and community psychiatric nurses.	1,100
E	Volunteer-run assessment and signposting service. Volunteers made contact with older people, carried out a home-based 'check-up', and provided advice on benefits entitlement, housing, community transport, education and leisure activities. If necessary the volunteer acted as a personal navigator through the range of services available.	700
F	Short-term assessment and signposting service, which targeted older people in some of the most deprived areas. Multi-agency team signposting to a range of health, housing, social care, benefits, and community development services.	900
G	Short-term assessment and signposting service, which involved staff visiting clients in their own environment. The initiative used the single assessment process to signpost and commission from a pre-agreed menu of community services, or referred clients to specialist services.	1,500
H	Short-term assessment and signposting service, which aimed to improve access to low-level preventive services by establishing a single point of access. Joint prevention teams consisted of health advisers, health trainers, social care workers, link workers, a team coordinator, and volunteers.	1,300

[†] As identified by the sites for this evaluation (includes people seen by the service up to 31 December 2008)

2. Methods

The POPP interventions may potentially have had a wide range of effects, for example on the utilisation of primary and secondary health care, the utilisation of social care, the up-take of social security benefits, and on individuals' independence, well-being and quality of life. Some of the interventions could have also benefited people besides those individuals receiving a POPP intervention, for example, formal and informal carers. Furthermore, POPP could have had an impact on the organisations involved in delivering care, or may have shifted the local culture away from institutional and hospital-based crisis care for older people towards earlier, targeted support.

The aim of this particular study was to assess the effect of selected POPP interventions on rates of emergency (unplanned) admissions to hospital for people receiving the intervention. Specifically, we sought to measure the mean number of emergency hospital admissions per individual over various time periods. Our secondary aim was to investigate the impact of the POPP interventions on the number of emergency hospital bed-days, elective admission rates and rates of outpatient attendance. These observations were then compared to a matched set of controls.

Control group

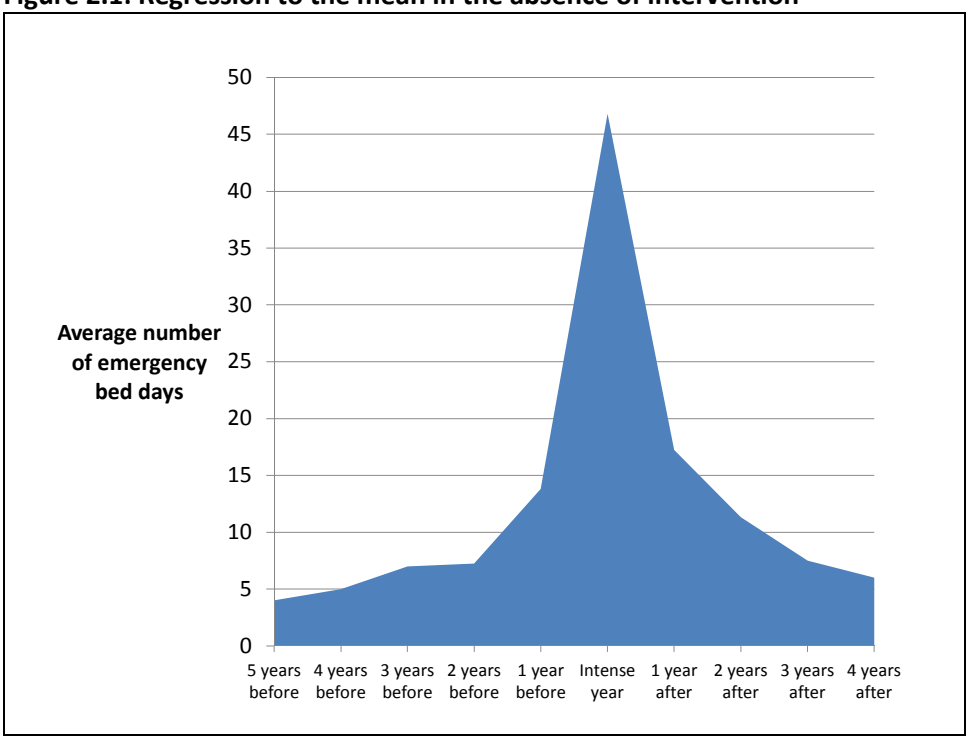
Ideally, any evaluation of the effectiveness of a health or social care intervention should be compared to what would happen to an otherwise identical control. For example, the Department of Health is currently sponsoring a large randomised control trial of telehealth and telecare, where randomisation is used to select an unbiased control group.[4] However, randomised controlled trials can be difficult and costly to undertake and may be slow to reveal results, so alternative methods are needed for many innovations. Without a robust control group, the evaluation of hospital avoidance interventions can be misleading. More specifically, controls ensure that:

- Any reductions seen in hospital utilisation are not simply due to an intervention shifting its focus towards lower-risk patients over time. For example, if an intervention was being offered to a declining number of higher-risk patients (perhaps because of the success of the intervention), then it is possible that the proportion of lower-risk patients being offered the intervention could increase. Without an appropriate control group, the evaluation would over-estimate the impact of the intervention on hospital admissions because the impact on higher-risk patients would be obscured by the increase in services offered to lower-risk patients.

- Any reductions seen in hospital utilisation are not simply a statistical artefact caused by selecting high-risk patients for treatment. By selecting high-risk patients, there is a natural tendency for subsequent measurements on those patients to show reductions in use; a statistical phenomenon called 'regression to the mean'. This effect is illustrated in Figure 2.1, which is based on the Hospital Episode Statistics for England. The chart spans a ten-year

period and illustrates hospital admissions for a cohort of frequent hospital users identified in the central intense year. Hospital admissions were tracked for this cohort of people for five years beforehand and five years afterwards. The chart illustrates that, if patients are chosen for an intervention based on their current high rates of hospital admissions, we would expect their rates of hospital admissions to reduce over time, even in the absence of a specific intervention. This would mean that and evaluation without an appropriate control group would tend to overestimate the effectiveness of the intervention on hospital use, since some or all of the observed reductions would have happened anyway.

Figure 2.1: Regression to the mean in the absence of intervention



Source: Department of Health for England analysis of Hospital Episode Statistics

Basic datasets compiled for analysis

This project has been innovative in its use of routine, person-level data relating to service utilisation. Compared to area-level analyses, person-level analyses are able to examine the particular individuals who received the intervention. This avoids the risk that the evaluation results might be distorted by what happens to people who are registered in the local area but who did not actually receive the intervention under evaluation.

We were able to access person-level information while maintaining the highest standards in information governance and protecting the confidentiality of the individuals who received the interventions. New data linkage techniques developed with the NHS Information Centre allowed us to obtain person-level data about hospital activity without compromising confidentiality. The eight POPP sites were asked to send identifiable data about the people who had received the interventions between the start of their initiative and December 2008.[‡] These lists were sent securely to the NHS Information Centre for Health and Social Care, who then linked this information to the Hospital Episode Statistics (HES) dataset. The evaluation team at the Nuffield Trust received pseudonymous data which meant that we were unable to identify any personal information (such as names and addresses), but that we could identify the individuals' records in HES. Our approach was scrutinised by the Ethics and Confidentiality Committee of the National Information Governance Board, who confirmed that individual consent was not required from participants for us to use pseudonymous data in this way.

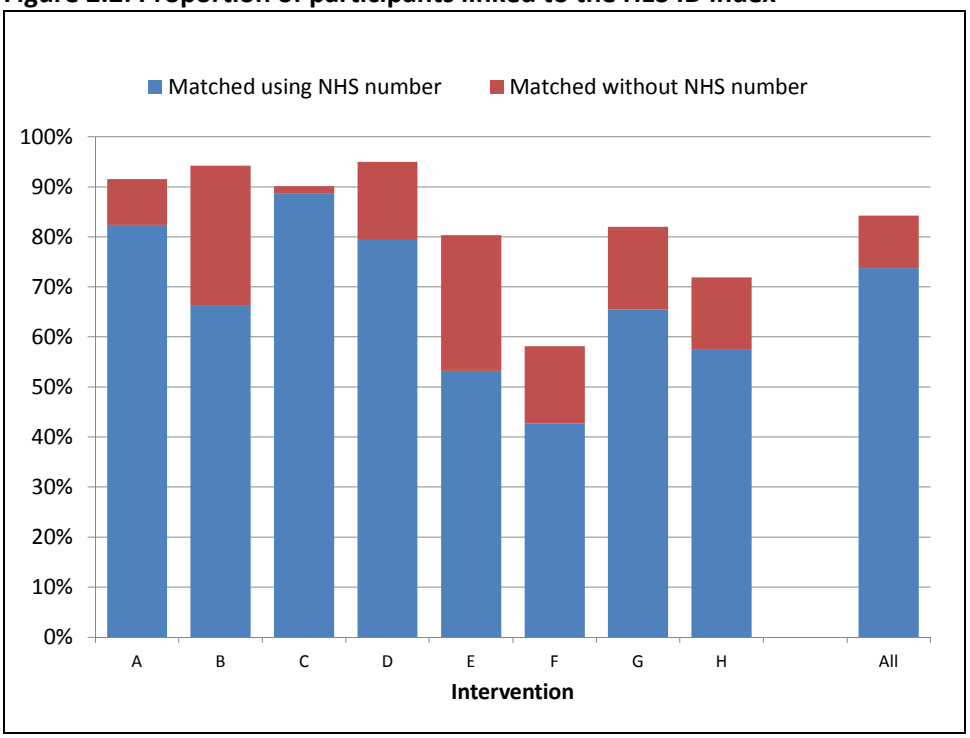
Our two approaches to linking pseudonymous data with HES are described in more detail in Appendix A. Generally the HES data linkage performed well. As can be seen in Figure 2.2, we found that 84 per cent of the records identified by the sites could be linked to HES (15,568 out of 18,472). The HES linkage rate was higher for the four more intensive interventions (A-D) than for the four less intensive ones (E-H). Only two interventions (F and H) had a HES linkage rate of less than 80 per cent. While the lower linkage rates for interventions F and H were a concern, the majority of records that were not linked were found to contain incomplete data (1,752 out of 2,902 records (60 per cent) had missing data). For example, sites had not recorded either the NHS number or the date of birth of the individual concerned, meaning it was not possible to link the record.

Records that were not linked to HES were discarded for this study. There was no evidence that the cases that were not linked to HES differed in any systematic way from the cases that were linked to HES, for all but one intervention.[§] This exception was intervention E, for which there was some evidence that the cases that were not linked to HES were older than the cases that were linked to HES (mean age 75.4 compared to 73.6, p-value = 0.01).

[‡] There are two known exceptions. For intervention A, data were only available from January 2007, and for intervention D, electronic records were only available for an estimated 70 per cent of the people receiving the intervention.

[§] Across the eight sites as a whole, the cases not linked to HES had a mean age of 78.1, in comparison to 77.9 for the cases linked to HES (analysis restricted to those with a recorded date of birth). P-value = 0.64. The proportion of females was 65 per cent and 67 per cent, respectively (analysis restricted to those with a recorded gender).

Figure 2.2: Proportion of participants linked to the HES ID index



Definition of the study cohorts

We received data on people who received an intervention on or before 31 December 2008, but typically only included those who received an intervention on or before 31 March 2008 in our study cohorts (Table 2.1). This allowed us to track hospital utilisation for 12 months following the start of interventions A, B, F and G, since the available HES data ran up until 31 March 2009. However, interventions C, D and H started later than the rest and we were only able to track hospital utilisation for six months post-intervention, and for intervention E we were limited to nine months.

The study cohorts were usually selected to exclude people who received the interventions in the first few months of their operation. This helped focus on the steady-state impacts of the interventions, as it seemed reasonable to suppose that, in the early months, the sites were still developing and refining the interventions as they were made operational. The two exceptions were interventions B and H. Table 2.1 shows, for each site, the first month of the intervention[**], together with the start date we chose to define the cohort under study. Of the 18,472 records received from the sites, 10,790 were for people who belonged to our study cohort. Of these, 9,080 linked to HES.

[**] As defined as the first month in which more than five people are recorded as receiving the intervention

Table 2.1: Cohorts selected for evaluation

	First month of the intervention	Cohort selected - those receiving intervention between:		Number of months follow-up
		Start date	End date	
A	January 2007	1 April 2007	31 March 2008	12
B	June 2006	1 June 2006	31 December 2007	12
C	September 2007	1 April 2008	30 September 2008	6
D	November 2007	1 January 2008	30 September 2008	6
E	May 2007	1 June 2007	30 June 2008	9
F	July 2006	1 September 2006	30 April 2008	12
G	July 2006	1 January 2007	31 December 2007	12
H	October 2007	1 October 2007	30 September 2008	6

In addition to selection based on the time of intervention, we also had to exclude some cases where it was not possible to fit the predictive risk models used for deriving controls. Modelling was performed on the subgroups of the study cohorts who were aged 70 or over (65 for interventions E and F), and had not been resident in more than one of the POPP areas. Further, we decided to focus on those participants who had experienced a hospital admission during the two years before the start of the intervention. This was for two reasons:

- There is very limited scope to prevent hospital admissions in the short term for people who have not recently had a hospital admission. For example, fewer than five per cent of 65-year-olds who have not had a hospital admission in the last two years will have an admission in the next 12 months. By focusing on people with a history of hospital admissions, we were concentrating out analysis on those patients more likely to benefit from the intervention in the short term.
- More information is available about people who have recently had a hospital admission, since medical diagnoses are routinely recorded within HES. We therefore concentrated our analysis on these people to ensure that our control group selection was more robust.

After applying these restrictions, we were left with a group of 5,146 participants across the eight POPP interventions studied, ranging from 131 for A to 2,557 for C (Table 2.2). This represented just under half (47 per cent) of the total number of people who received these interventions, but it was the half for whom the interventions were most likely to have an effect on in the short term.

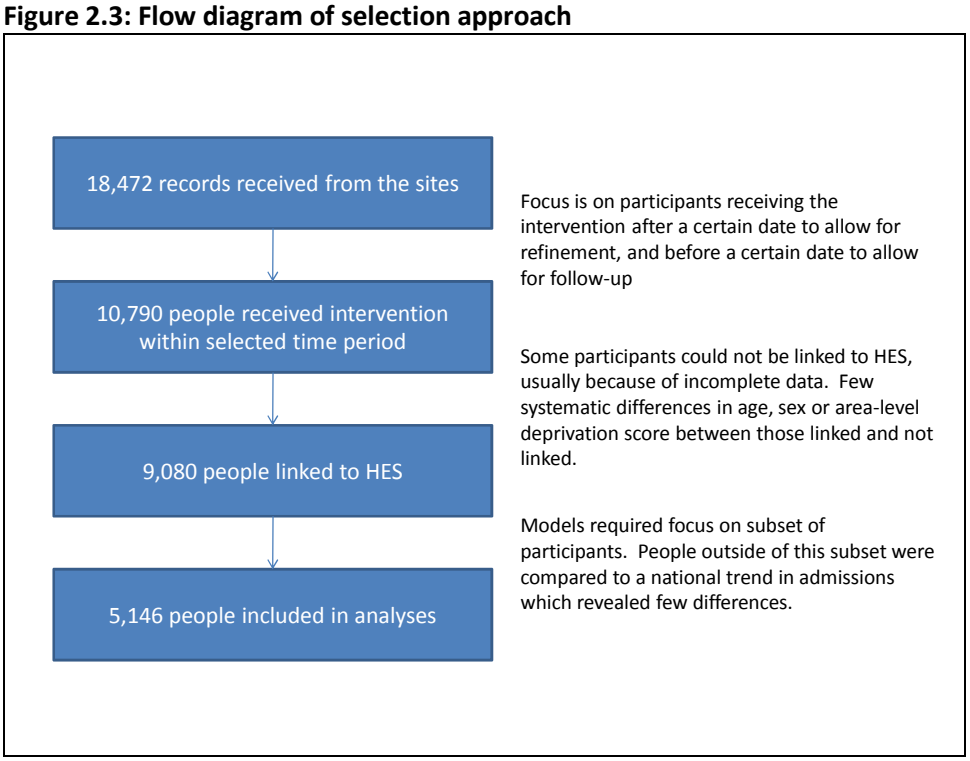
Table 2.2: Number of records included in principal analyses

	Number of records in study cohort	Number in study cohort and linked to HES	Breakdown of study cohort			Per cent of study cohort included in analyses
			Under age 70*	Over age 70* and no inpatient admission In previous two years	Included in analyses	
A	208	192	47	14	131	63.0
B	722	673	27	90	556	77.0
C	4,988	4,533	814	1,162	2,557	51.3
D	814	780	98	191	491	60.3
E	974	795	111	372	312	32.0
F	1,405	823	137	329	357	25.4%
G	768	622	77	181	364	47.4%
H	911	662	43	241	378	41.5%
Total	**10,790**	**9,080**	**1,354**	**2,580**	**5,146**	**47.7%**

* 65 in E and F

The selection process is summarised in the flow diagram below. Although we were not able to derive controls for everybody in the study cohort, we were able to compare the numbers of admissions before and after the intervention for everybody who was linked to HES. The people that were not included in the controlled study had very few emergency admissions before the intervention and, following the intervention, the number of emergency admissions increased (Appendix B). We think it is unlikely that our selection process biased the results towards showing no reduction in admissions.

Figure 2.3: Flow diagram of selection approach



Method for deriving the matched controls

To avoid the problems associated with regression to the mean (see Figure 2.1), we constructed a matched control group at person level. This technique is often used in clinical observational studies. There are several methods for constructing a control group, but the aim is always for the control group to have the same distribution of relevant characteristics as the intervention group in the time period prior to the start of the intervention. Methods include:

- Matching several of the underlying characteristics at once, without attempting to summarise them into a single figure, using Mahalanobis metric matching or genetic matching.[5]
- Deriving a propensity score. This score summarises as a single figure those characteristics that reflect the likelihood that a given person received the intervention.[6] A control group is then determined by selecting people with similar propensity scores to those in the intervention group.[7]
- Matching according to a prognostic score. The prognostic score is a summary of the characteristics relevant to determining whether someone would experience the outcome event of interest, in the absence of the intervention.[8]

Although we implemented and compared all three of these approaches for this evaluation, our preferred approach was a variant to the prognostic scoring technique, since we found that it optimised the performance of the underlying predictive models. To derive our prognostic score, we developed predictive models focused on emergency hospital admissions. These models were similar to the Patients At Risk of Re-hospitalisation (PARR) model that is used widely by the NHS in England.[9] The models attribute a number between 0 and 100 for every person with a recent inpatient admission that reflects their probability of having an emergency hospital admission within 12 months. We calibrated these models based on people who did not receive the POPP intervention at any point. This was done in order to derive an estimate of the probability of emergency hospital admission in the absence of receiving the POPP intervention. The method used is described in more detail in Appendix C, along with a summary of the models' performance.

We had a choice of areas from which to select controls. Three options were considered: controls selected only from within the intervention area ; controls from similar areas across England; or controls from all of England. Our preferred approach was to select controls from similar areas across England. A list of the areas chosen is given in Appendix C. We performed sensitivity analysis to test that this choice did not impact upon our final conclusions.

Characteristics of the matched controls

The matching process identified a control person for each person in the study cohort in each site. When compared to the intervention group, the characteristics of the matched controls appeared similar throughout the pre-intervention period.

Table 2.3 compares the intervention and control groups according to eight characteristics. It shows how similar the matched controls were to the intervention group prior to the intervention based on the following factors: predictive risk score, prior emergency hospital utilisation, number of chronic health conditions, age, sex and area-level deprivation. The prevalence of common health diagnoses was also similar between the intervention and control groups (Figures 2.4 and 2.5).

Note that because of random fluctuations, we would never expect absolutely exact matches on these quantities, even in a randomised control trial. The closeness of the match was assessed using the standardised difference.[10,11] In most cases the standardised difference between these groups was less than five per cent (see appendices D to K).

Table 2.3: Prior characteristics of intervention and matched control groups

	Intervention	Control	Intervention	Control	Intervention	Control	Intervention	Control
	A		B		C		D	
Size (N)	127	127	556	556	2,557	2,557	491	491
Proportion aged 85+	20%	18%	46%	46%	48%	48%	49%	49%
Proportion female	50%	50%	65%	65%	65%	65%	55%	55%
Mean area-level deprivation score	20.1	20.2	17.0	16.4	17.7	17.1	18.1	17.7
Mean number of emergency admissions in previous year	2.2	2.4	1.4	1.4	1.2	1.1	1.1	1.0
Mean number of emergency admissions in previous 30 days	0.5	0.6	0.7	0.6	0.3	0.3	0.2	0.2
Mean emergency length of stay in previous year	22.0	20.6	9.4	10.2	13.0	11.4	12.2	10.8
Mean number of chronic conditions	2.9	2.9	1.4	1.4	1.3	1.4	1.5	1.4
Mean predictive risk score	0.4	0.4	0.34	0.34	0.20	0.20	0.24	0.24

	E		F		G		H	
Size (N)	312	312	357	357	364	364	378	378
Proportion aged 85+	21%	21%	15%	15%	37%	37%	47%	47%
Proportion female	67%	67%	70%	70%	67%	67%	68%	68%
Mean area-level deprivation score	33.2	32.9	46.2	45.8	19.2	18.1	16.6	16.2
Mean number of emergency admissions in previous year	0.5	0.5	0.8	0.7	1.0	1.0	1.0	0.9
Mean number of emergency admissions in previous 30 days	0.1	0.0	0.1	0.1	0.2	0.2	0.3	0.3
Mean emergency length of stay in previous year	4.2	3.7	7.1	6.0	10.3	9.7	8.6	8.7
Mean number of chronic conditions	1.5	1.5	1.5	1.4	1.3	1.2	1.6	1.5
Mean predictive risk score	0.19	0.19	0.28	0.28	0.27	0.27	0.25	0.25

Figure 2.4

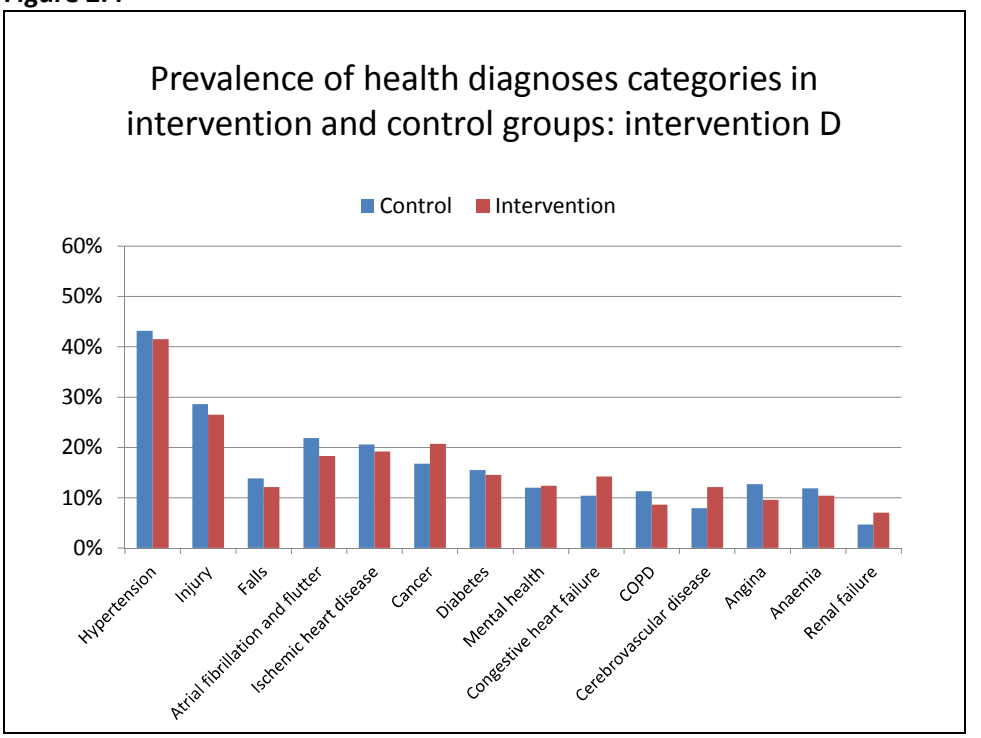
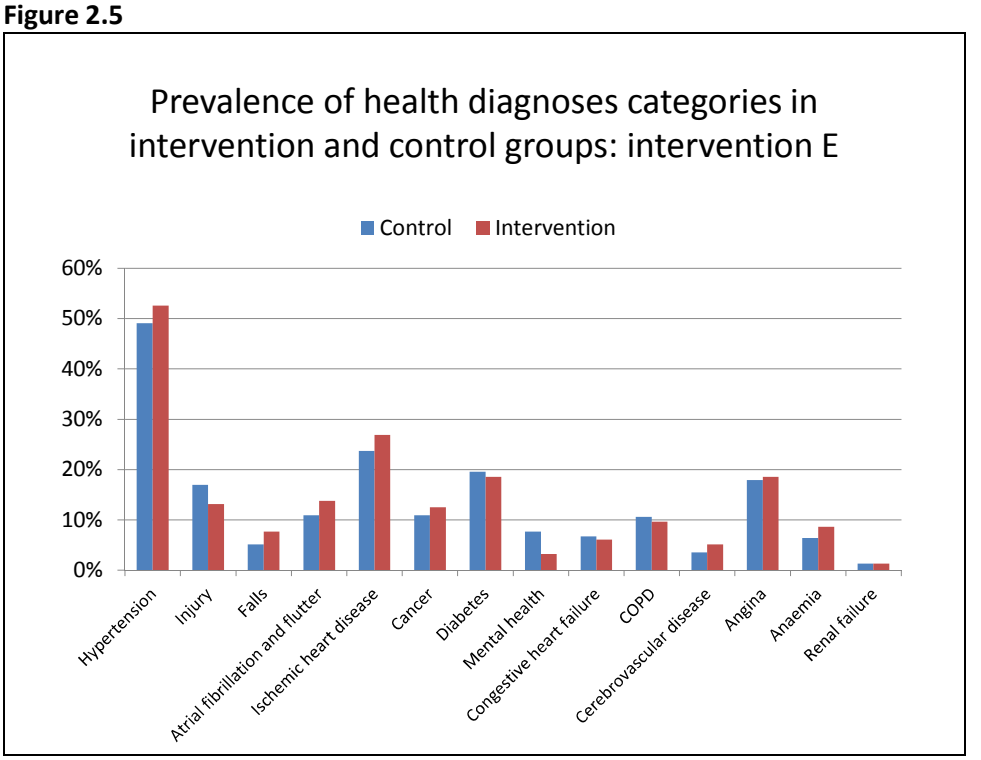


Figure 2.5



Discussion with sites

Using these methods, we made a series of assumptions and choices. Towards the end of this project we had the opportunity to visit four of our eight sites (B, C, D and H) and test how reasonable these were. The particular aims of our visits are set out below, along with our conclusions.

Check our understanding of the intervention and that reducing rates of emergency hospital admission was a key objective.	Reducing emergency hospital admissions was a key objective for all four sites, although one (intervention H) had a focus on admissions over two to five years. All had a variety of other aims, such as improving quality of life and improving partnership working between professionals.
Check that the nature of the intervention remained relatively stable over the period used to define our study cohort.	The key elements of the interventions appear to have been stable, and continuity and sustainability were explicit aims of the Department of Health funding. In one site the intervention had been rolled out across a wider geographic area, and in two sites the interventions had been rolled out across more client groups. Members of staff and staff types involved in the interventions had changed.
Discuss our choice of comparison areas and check that these seemed natural choices to people more familiar with the local area.	Our chosen comparison areas generally seemed appropriate, although sometimes they differed from the 'statistical neighbours' used regularly for internal analysis. An individual at one site said he would have preferred to see controls drawn from within the local area, to benchmark the POPP intervention against usual local care. At another site one individual was pleased to see that we had drawn controls from outside of the local area because of the perceived lack of availability of local controls.
Talk through how we had used the basic datasets and any limitations.	Three of the sites were confident that the data were complete and accurate. We are missing data for 30 per cent of participants from one site (D), as to identify them would have meant interpreting an ambiguous free-text field. However, the 70 per cent that were matched were felt to be representative. This issue had already been reported at the data-gathering stage.
Discuss whether we had matched on the key variables associated with recruitment into the intervention and hospital admissions.	Recruitment into the interventions was usually by referral from a social care or health care professional; referral from friends or relatives; or self-referral. As such, it was difficult to gauge whether we had matched on all of the attributes associated with recruitment. One site would like to see controls matched on social care characteristics, as well as the impact of the interventions on social care.
Test the reasonableness of evaluation findings.	Local evaluations had been conducted which typically found reductions in emergency hospital admissions. At least one site had also found reductions in admissions at the pilot stage. However, typically sites were not surprised at our findings; reducing admissions was seen as a challenging and long-term endeavour.

3. Impact on hospital utilisation

This chapter describes the observed differences in hospital utilisation between the intervention groups and the matched controls.

Assessment of hospital utilisation

The tracking of hospital use was in some cases limited by the range of data available. The aim was to cover a period of 12 months after the intervention start date. For most of the sites, data were available to monitor hospital utilisation for the whole of this period. However, for interventions C, D and H the available data were restricted to six months after the intervention date, and for intervention E they were restricted to nine months. We acknowledge that, even where we were able to monitor hospital utilisation for a full year, the interventions could still have had an effect over a longer time period.

Our base comparisons assess changes from 12 months before the intervention date to 12 months after (six for C, D and H, and nine for E). Later analyses looked at the sensitivity of results to this choice of timescales.

Our primary measure was based on changes in the rate of emergency admissions per month in the intervention and control groups. Other analyses also looked at other measures of utilisation including the number of bed-days following emergency admissions, elective admissions and outpatient attendances. In what follows, the intervention effect is calculated using the standard difference-in-differences approach:

Intervention effect = D(intervention) – D(control)

Where:

D(intervention) = Outcome in Year 2 for the intervention group
less outcome in Year 1 for the intervention group

D(control) = Outcome in Year 2 for the matched control group
less outcome in Year 1 for the matched control group

Changes in emergency hospital admissions

Figures 3.1 and 3.2 are examples of the patterns observed in emergency admissions per month for the intervention group. There are broadly two patterns. In most areas there was a clear peak in emergency admissions around the start time of the intervention. This may be a sign that the selection of patients for the intervention was linked with their use of hospitals. For two of the interventions (E and F) there was not such a pronounced increase at the start of the intervention.

Figure 3.1

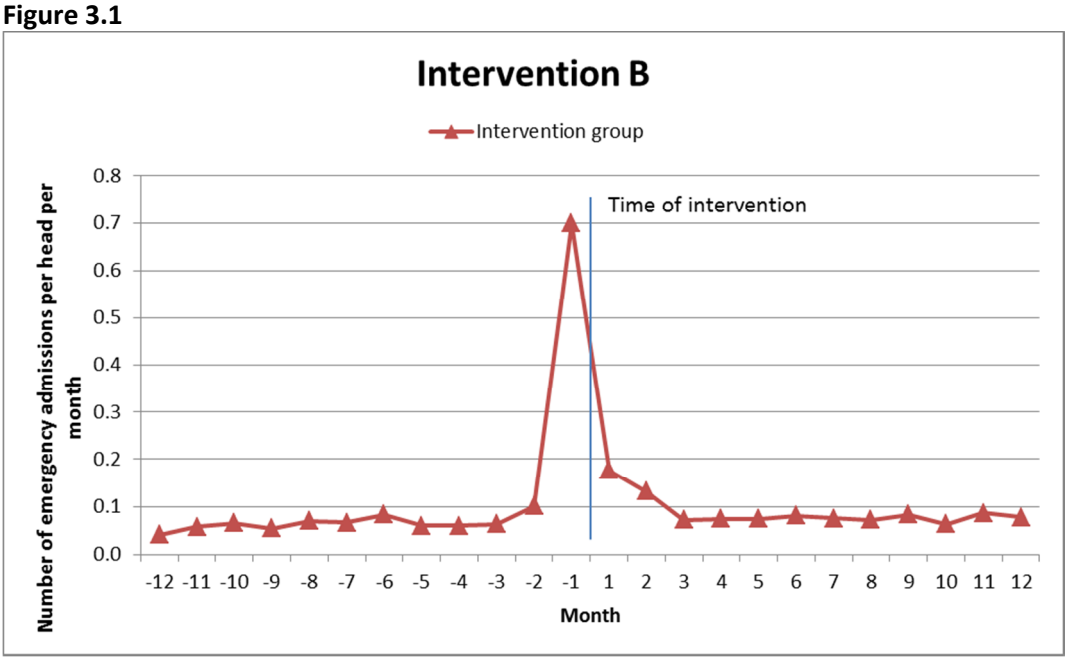


Figure 3.2

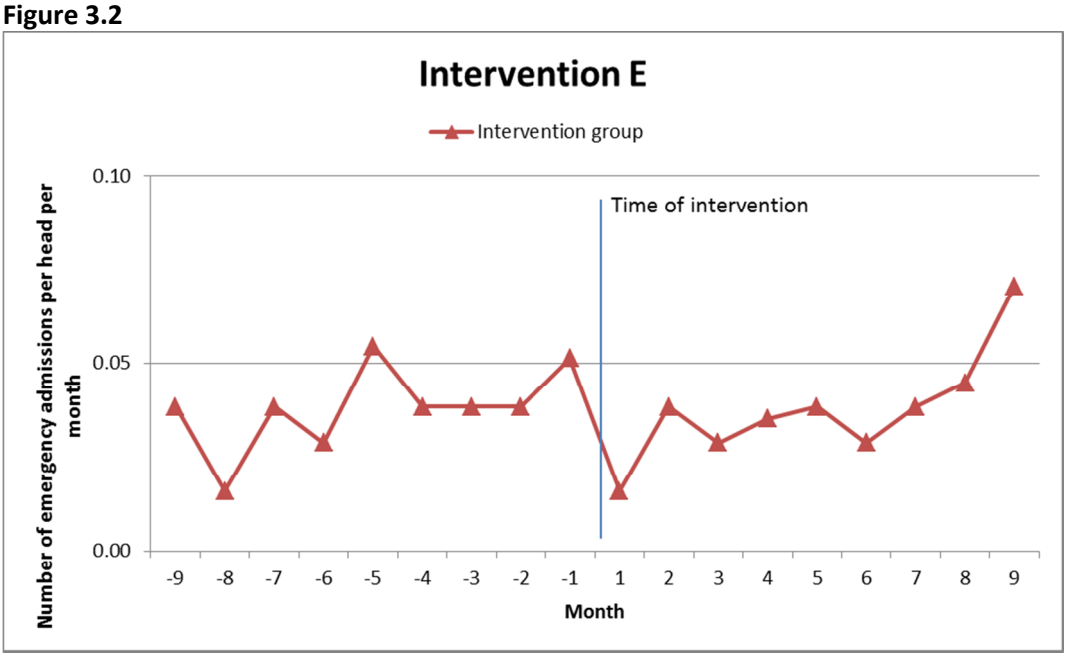


Table 3.1 summarises the changes observed in emergency admission rates for the intervention groups. In every case the rate of emergency hospital admission reduced for the intervention group following the intervention. These reductions were statistically significant at the one per cent level for four of the interventions (A, B, C and H). Following intervention C, admission rates dropped by 46 per cent and following intervention A they dropped by 35 per cent.

Table 3.1: Changes in emergency hospital admissions per head observed for the intervention group – means (standard deviations)

Intervention	N	Before	After	Change	% Change
A	127	2.36 (1.77)	1.54 (2.15)	-0.82** (2.37)	-35%**
B	556	1.42 (1.40)	1.06 (1.54)	-0.35** (1.78)	-25%**
C (1)	2,557	0.84 (1.10)	0.46 (0.86)	-0.38** (1.30)	-46%**
D (1)	491	0.72 (0.95)	0.64 (0.99)	-0.08 (1.32)	-11%
E (2)	312	0.34 (0.68)	0.34 (0.68)	0.00 (0.90)	-1%
F	357	0.80 (1.18)	0.71 (1.25)	-0.09 (1.43)	-11%
G	364	1.04 (1.39)	0.94 (1.34)	-0.10 (1.58)	-10%
H (1)	378	0.74 (1.09)	0.56 (1.03)	-0.18** (1.17)	-24%**

(1) Admissions rates are for six months before/after intervention
(2) Nine months following intervention
* denotes statistically significant at the 5% level
** denotes statistically significant at the 1% level

However, given that many of the interventions seem to have targeted patients who had recently had an emergency hospital admission, a subsequent fall in hospital use is to be expected. Individuals with a recent hospital admission have a natural tendency to show a subsequent reduction in hospital use (regression to the mean). So, for example the matched control group for intervention B also shows a reduction in emergency admissions, from 1.38 to 0.80 per head: a reduction of 0.58 per head (Figure 3.3). Overall, for intervention B, admissions reduced in the intervention group by a smaller amount than in the control group. The difference-in-difference approach using a matched control group yields a net increase in admissions of 0.23 admissions per head, which is statistically significant at the one per cent level.

Figure 3.3

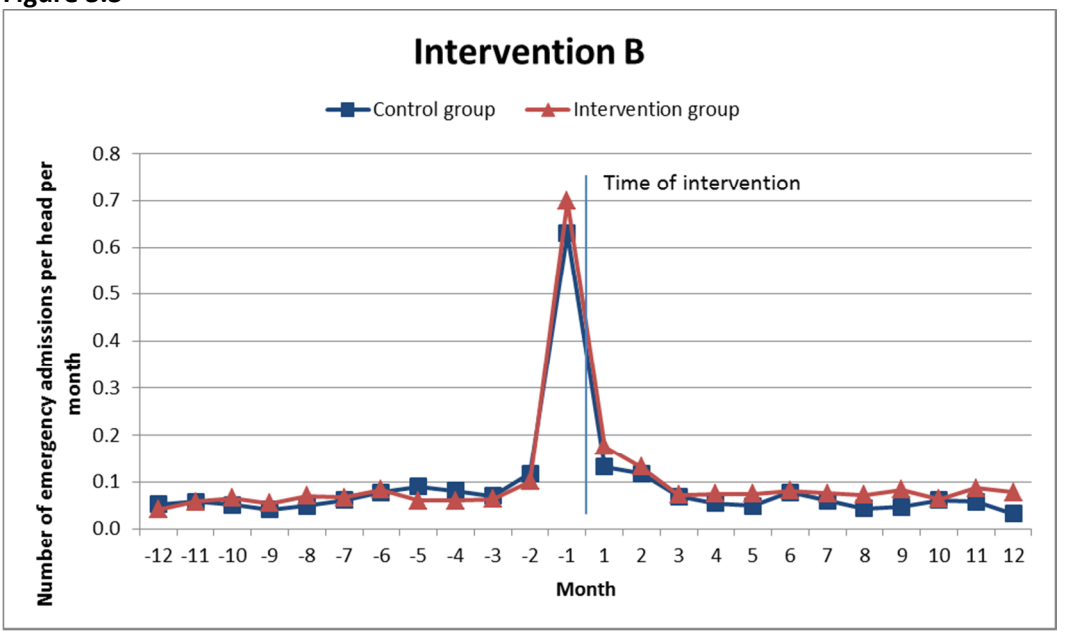


Table 3.2 summarises the estimated intervention effect on emergency hospital admissions for all eight interventions. The intervention group appears to have slightly increased emergency hospital admissions compared to the control group for two of the interventions (B and G). Although these differences are not great, there is no indication using our methods that the intervention groups had lower rates of emergency hospital admission compared to the matched control group.

Table 3.2: Comparisons between intervention and matched control groups for emergency hospital admissions per head – means (standard deviations)

Intervention	Intervention group			Control group			Intervention effect
	Before	After	Change	Before	After	Change	
A	2.36 (1.77)	1.54 (2.15)	-0.82* (2.37)	2.15 (1.66)	1.05 (1.64)	-1.10* (1.79)	0.28 (2.83)
B	1.42 (1.40)	1.06 (1.54)	-0.35** (1.78)	1.38 (1.32)	0.80 (1.30)	-0.58** (1.47)	0.23** (1.95)
C (1)	0.84 (1.10)	0.46 (0.86)	-0.38** (1.30)	0.75 (0.99)	0.38 (0.80)	-0.36** (1.10)	-0.02 (1.40)
D (1)	0.72 (0.95)	0.64 (0.99)	-0.08 (1.32)	0.62 (0.83)	0.46 (0.88)	-0.16** (1.11)	0.08 (1.48)
E (2)	0.34 (0.68)	0.34 (0.68)	0.00 (0.90)	0.34 (0.65)	0.34 (0.75)	0.00 (0.87)	0.00 (1.10)
F	0.80 (1.18)	0.71 (1.25)	-0.09 (1.43)	0.75 (1.10)	0.52 (1.11)	-0.23** (1.38)	0.14 (1.43)
G	1.04 (1.39)	0.94 (1.34)	-0.10 (1.58)	0.96 (1.26)	0.68 (1.19)	-0.29** (1.50)	0.18* (1.57)
H (1)	0.74 (1.09)	0.56 (1.03)	-0.18** (1.17)	0.63 (0.92)	0.43 (1.20)	-0.20** (1.23)	0.02 (1.53)

(1) Admissions rates are for six months before/after intervention. (2) Nine months following intervention
* denotes statistically significant at the 5% level. ** denotes statistically significant at the 1% level.

Other hospital utilisation measures

Although the rate of emergency hospital admission was the primary outcome measure, further checks were undertaken using a range of alternative metrics. Table 3.3 shows the changes observed in emergency bed-days, elective admissions and outpatient attendances for the intervention group, compared to the matched control group. Participants of intervention C appear to have fewer emergency bed-days than the corresponding matched control group; at around one day per person. On the other hand, the participants of intervention B appear to have more emergency bed-days than the corresponding matched control group, at around eight days per person.

Table 3.3: Estimated intervention effect on other aspects of hospital utilisation – means (standard deviations)

Intervention	Emergency bed-days	Elective admissions	Outpatient attendances
A	0.66 (42.82)	0.03 (1.24)	-0.02 (6.55)
B	8.11** (34.45)	-0.05 (1.41)	-0.62** (4.40)
C (1)	-1.08** (23.94)	-0.11** (1.40)	-0.24** (3.44)
D (1)	1.08 (26.21)	-0.10 (1.52)	-0.42** (2.88)
E (2)	1.07 (16.43)	0.03 (1.77)	0.73** (5.57)
F	-0.07 (23.11)	-0.05 (1.21)	-0.33 (7.70)
G	1.19 (26.22)	-0.05 (2.04)	0.20 (5.00)
H (1)	-0.62 (23.80)	0.10 (1.16)	0.22 (2.98)

(1) Utilisation figures are for six months before/after intervention (2) Nine months following intervention
* denotes statistically significant at the 5% level. ** denotes statistically significant at the 1% level.

Sensitivity analysis

The following sections summarise the work we did to test the robustness of our observations.

1. Differences in observed mortality rates

To check on the comparability of the intervention and control groups, we compared the frequency of deaths in hospital. Only data on deaths that occurred in hospital were available for this study. We did not expect the types of interventions studied to alter the mortality rates in the short term and indeed, for most of the sites, we found similar in-hospital mortality rates between the intervention and control groups. . However, participants of interventions B, C and D were significantly more likely than the corresponding control groups to die in hospital following the interventions (Table 3.4). In the absence of linked data relating to deaths outside of hospital, it is impossible to know whether these differences in the in-hospital mortality rate are indicative of an impact on mortality, or differences in the proportion of people dying in hospital as oppose to at home or in the community.

Table 3.4: Proportion of control and intervention groups dying in hospital within one year following the time of the intervention

Intervention	Intervention group	Control group	Difference
A	11.0%	13.4%	-2.4%
B	22.1%	14.9%	7.2%**
C (1)	8.7%	5.6%	3.1%**
D (1)	15.9%	8.1%	7.7%**
E (2)	2.2%	2.2%	0.0%
F	6.4%	5.9%	0.6%
G	8.2%	8.5%	-0.3%
H (1)	5.0%	5.8%	-0.8%

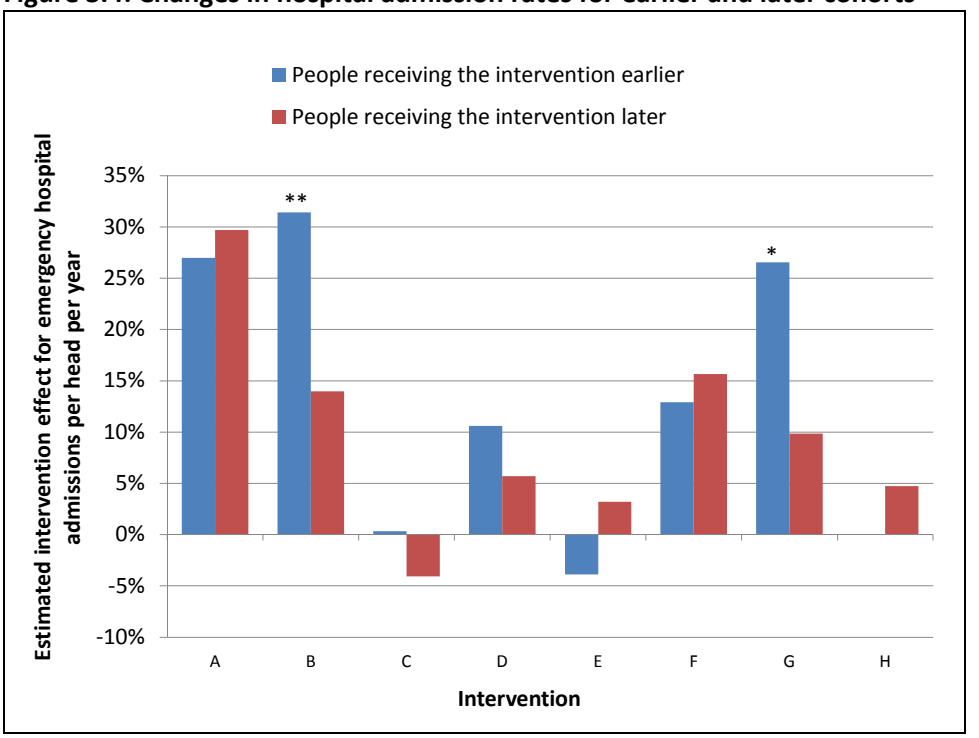
(1) Mortality rates are for six months following intervention (2) Nine months following intervention
* denotes statistically significant at the 5% level. ** denotes statistically significant at the 1% level.

It is possible that differences in the in-hospital mortality rate are an artefact of the control group matching. We have tested the possible implications of this for our analysis of emergency hospital admissions by controlling for subsequent death. Nothing has been found to suggest that the interventions have had a reduction in admissions over and above the reductions shown above.

2. Earlier and later cohorts

It is possible that the impact of the interventions on rates of emergency hospital admission had changed over time. We separated our study cohort for each site into two approximately equal-sized subgroups, depending on whether they received the intervention relatively early or relatively late in the programme. We then compared the estimated intervention effects for the two groups. Figure 3.4 shows some marked differences between the earlier and later cohorts. Intervention G appears to have increased emergency admissions for the early cohort by 0.27 admissions per head per year according to our method, which is statistically significant. For the later cohort, emergency admissions increased by only 0.10 admissions per head per year, which is not statistically significant. A similar reduction was observed in intervention B.

Figure 3.4: Changes in hospital admission rates for earlier and later cohorts



Note: Figures for interventions C, D and H are for emergency admissions per head over a six month period, while figures for intervention E are for a nine month period.
* denotes statistically significant at the 5% level ** denotes statistically significant at the 1% level

The differences shown in Figure 3.4 could be due to a number of factors, including changes in the types of individuals seen, as well as changes in the operation of the interventions themselves. Table 3.5 compares the characteristics of the individuals in the earlier and later cohorts of interventions B and G. In both cases, the later cohorts appear to be lower risk than the earlier ones, with a lower average predictive risk scores, fewer chronic conditions and lower prior emergency hospital utilisation.

Table 3.5: Characteristics of the earlier and later cohorts

	Intervention G		**Intervention B**	
	Earlier cohort	Later cohort	Earlier cohort	Later cohort
Period for interventions	January 2007 to August 2007	August 2007 to December 2007	June 2006 to May 2007	May 2007 to December 2007
Size (N)	181	183	277	279
Average age	82.2	81.8	83.4	84.1
Proportion female	66%	67%	64%	66%
Mean number of emergency admissions in previous year	1.18	0.91	1.51	1.33
Mean emergency length of stay in previous year	11.32	9.39	10.81	8.09
Mean number of chronic conditions	1.4	1.2	1.5	1.3
Mean predictive risk score	0.28	0.25	0.36	0.32

3. Other subgroup analysis

Within all of the intervention groups, there is a distribution of case types, for example some patients are at higher risk of a hospital admission than others. It is possible that the intervention had greater impact on one or other subgroup of patients. We performed a range of subgroup analyses. The full analyses are not presented here but the following observations were made (Table 3.6):

- Intervention C appears to have reduced emergency hospital admissions for the 179 people with a predictive risk score of greater than 0.4, by almost 0.5 admissions per head on average over six months.
- For some other interventions, the high-risk people in the intervention group experienced more admissions than their controls (B, F or G).
- There were no systematic differences across the sites in whether the interventions were more effective at reducing admissions for older or younger people.
- Besides the high-risk people receiving intervention C, in no other cases did the analysis uncover subgroups for which an intervention appears to have reduced emergency admissions.

Table 3.6: Estimated intervention effect for rates of emergency hospital admission – means (standard deviations and counts)

Intervention	All	Women	Men	Aged 85 or above	Aged under 85	Predictive risk score 0.4 or greater	Predictive risk score less than 0.4
A	0.28 (Std=2.83 N=127)	0.81* (Std=2.97 N=64)	-0.25 (Std=2.60 N=63)	0.30 (Std=1.96 N=23)	0.28 (Std=3.00 N=104)	0.28 (Std=3.56 N=58)	0.29 (Std=2.06 N=69)
B	0.23** (Std=1.95 N=556)	0.19* (Std=1.83 N=363)	0.30 (Std=2.16 N=193)	0.11 (Std=1.85 N=257)	0.33** (Std=2.03 N=299)	0.23 (Std=2.61 N=163)	0.23** (Std=1.60 N=393)
C (1)	-0.02 (Std=1.40 N=2,557)	-0.03 (Std=1.32 N=1,669)	0.00 (Std=1.54 N=888)	-0.02 (Std=1.42 N=1,234)	-0.02 (Std=1.39 N=1,323)	-0.46* (Std=2.91 N=179)	0.01 (Std=1.21 N=2,378)
D (1)	0.08 (Std=1.48 N=491)	0.07 (Std=1.31 N=270)	0.10 (Std=1.66 N=221)	0.07 (Std=1.35 N=238)	0.09 (Std=1.59 N=253)	0.04 (Std=2.18 N=55)	0.09 (Std=1.37 N=436)
E (2)	0.00 (Std=1.10 N=312)	-0.06 (Std=1.09 N=209)	0.12 (Std=1.12 N=103)	-0.03 (Std=1.17 N=65)	0.00 (Std=1.08 N=247)	-0.85 (Std=2.44 N=13)	0.03 (Std=1.00 N=299)
F	0.14 (Std=1.43 N=357)	0.04 (Std=1.43 N=249)	0.38** (Std=1.42 N=108)	0.50* (Std=1.70 N=54)	0.08 (Std=1.37 N=303)	0.09 (Std=2.17 N=47)	0.15* (Std=1.29 N=310)
G	0.18* (Std=1.57 N=364)	0.14 (Std=1.53 N=242)	0.25 (Std=1.66 N=122)	0.43** (Std=1.72 N=133)	0.04 (Std=1.47 N=231)	-0.28 (Std=2.90 N=43)	0.24** (Std=1.30 N=321)
H (1)	0.02 (Std=1.53 N=378)	0.04 (Std=1.62 N=257)	0.00 (Std=1.31 N=121)	0.03 (Std=1.40 N=176)	0.01 (Std=1.63 N=202)	-0.21 (Std=3.22 N=48)	0.06 (Std=1.09 N=330)

(1) Utilisation figures are for six months before/after intervention (2) Nine months following intervention

* denotes statistically significant at the 5% level. ** denotes statistically significant at the 1% level.

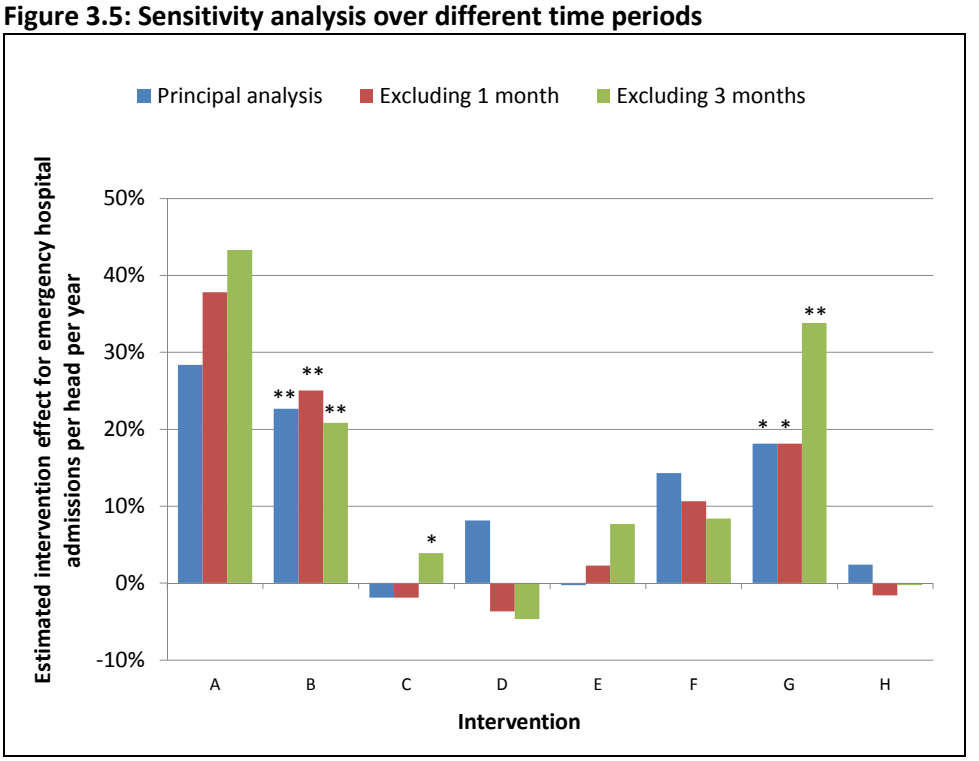
4. Comparisons over different time periods

It is not always clear over which time period the impact of an intervention is best measured. As the interventions did not start and finish on a single day, but lasted for a period of time, there is an argument for not looking for an impact from the first day, but after a period of time has elapsed.

Figure 3.1 showed the number of emergency hospital admissions per head for the months leading up to and following intervention B. The intervention group was characterised by a large peak in hospital admissions in the month before intervention, which was to be expected for a hospital discharge scheme. Following the intervention, emergency hospital admissions declined rapidly. Our sensitivity analysis aimed to remove the impact of the more turbulent patterns of hospital use seen in the months around the intervention taking place. The first set of sensitivity analysis excluded all hospital admissions observed in the month immediately before and after the intervention; that is, we compared utilisation in months 1-11 with utilisation in months 14-24. Our second set of sensitivity analysis excluded three months either side of the intervention.

As Figure 3.5 shows, the scale of the differences between the intervention and matched control groups were not consistent over time. However, in all but five combinations (which are not statistically significant), the fall in emergency admissions in the intervention groups was less than the matched controls.

Figure 3.5: Sensitivity analysis over different time periods



Note: Figures for interventions C, D and H are for emergency admissions per head over a six month period, while figures for intervention E are for a nine month period.

* denotes statistically significant at the 5% level ** denotes statistically significant at the 1% level

5. Effects of alternative controls

In testing the robustness of the conclusions it is important to ensure that random errors in the selection of the matched control group do not result in misleading results. Therefore our analyses used a number of different options to select control cases.

As mentioned above, we preferred to select controls from similar areas across England. However, we performed a variant of the analysis selecting controls from within the area undertaking the intervention. The results using alternative controls did not produce different conclusions. We do not think our conclusions are dependent on our particular choice of control areas.

We found that it was possible to improve the performance of the predictive modelling by deriving separate models for distinct subgroups of participants. Although the evaluation results are sensitive to the choice of predictive modelling, we have not found evidence using our methods that the interventions have reduced rates of emergency hospital admission compared to a matched control group.

4. Conclusions and suggested next steps

This study used person-level data and matched control groups to estimate the impact on hospital use of eight of the POPP interventions. These approaches are innovative and offer a number of potential advantages.

Principal findings

The purpose of this short-term study was to determine whether a subgroup of POPP interventions had a statistically significant effect on rates of emergency hospital admissions, compared to matched control groups. For the eight interventions in this study, we found that we were able to construct control groups that matched the intervention groups very well in terms of age, sex, area-level deprivation, medical diagnoses, predicted risk of hospital admission (PARR scores) and prior health care use. When compared to these controls, we did not find evidence of a reduction in emergency hospital admissions, and in some instances there were more admissions in the intervention group than in the control group. In one site emergency bed-days were reduced, while in another the intervention group had more bed-days than the control group. Overall we found that the POPP interventions we studied did not appear to have reduced use of acute hospitals. However, there were signs that one of the interventions reduced emergency hospital admissions for a high-risk subgroup.

One of the principal strengths of this study is that we were able to work using individual-level data rather than aggregated data. Hospital use in any given area varies for a great many reasons, some being local factors (local need), others being wider effects (for example national policy). By studying individual data rather than site data, we have been able to avoid falsely attributing findings at the site level to the subset of individuals who received the intervention.

Another key strength of this study is that we were able to take account of the natural drop in admissions that occurs when high-risk cases are selected for an intervention. Our control groups were matched closely for a large number of factors, such that there were few observable differences between our intervention and control groups. We constructed two control groups for several of the interventions: one group by selecting controls from similar areas of the country, and one by selecting controls from within the area undertaking the intervention. We undertook many analyses and tested that our findings were the same regardless of the control group we used.

It is worth reiterating that we only looked at hospital utilisation in eight interventions. There were 29 POPP sites, operating 146 core projects between them. It is likely that the eight interventions we studied differed systematically from the rest, although the interventions that we did study were heterogeneous and represented a range of different types. We also note that we were only able to analyse data from a certain proportion of the people who received the interventions; 16 per cent of participants were excluded because we were unable to match to their HES record based on NHS number, name, post code and date of birth. In all but one of the sites, there was no evidence that the cases that were not linked to HES differed in any systematic way from the cases that were linked. We think the less than 100 per cent linkage rate is largely due to incomplete or inaccurate data held by the sites.

Other individuals (43%) were excluded from our analysis since we were unable to derive a control. This set of excluded people had very few emergency admissions before the intervention and, following the intervention, the number of emergency admissions increased. We think it is unlikely that our selection process biased the results to show no reductions in admissions in the intervention group.

Validity of controls

We matched our controls accurately on the basis of age, sex, area-level deprivation, recorded medical diagnoses, predicted risk of hospital admission and prior health care use. Although we selected controls from specific areas, we do not think our conclusions are dependent on the particular choice we made.

However, the matching process was constrained by the information we had available in the HES history, so it is possible that our intervention groups and our control groups differed systematically from each other according to some other unknown factors that we were unable to observe. This is known as residual confounding (that is, confounding on the basis of an unknown characteristic or variable) and the only way to avoid it completely would be to conduct a sufficiently large randomised controlled trial.

We are confident about our matching in the majority of cases, since standardised differences in the covariates are low and there was not a difference in subsequent mortality rates. For three interventions we found higher mortality rates for the intervention group than for the corresponding control group. Since we only had data on deaths that occurred in hospital, it is possible that this finding reflects differences in the location of death rather than in the total mortality rate. However, it seems unlikely, given that, at the aggregate level, the proportion of deaths occurring in hospital was similar between these three POPP sites and their corresponding control areas.[12] The differences are therefore still a concern. On the assumption that the interventions did not genuinely have an impact on mortality rates, it may be that they are indicative of some systematic, unobserved imbalance between the intervention and control groups. Such an imbalance could have led to biases in estimating the impact of the interventions on hospital utilisation for those three interventions.

A second difficulty associated with the limited amount of mortality data relates to the selection of controls. A small percentage of our controls may have in fact died before the intervention began. We removed known deaths from the pool of potential controls, but could not remove people who had died outside of hospital. This may have suppressed the post-intervention admission rate for the control group slightly, and made admission rates for the intervention group appear high.

It is noteworthy that some of the interventions were targeted at individuals who were already receiving particular services. For example, one intervention was targeted at a subgroup of people already receiving support from a community matron, and aimed to provide additional support. Ideally our control group for this intervention would have also been receiving support from a community matron; however, we cannot be sure of this in the absence of community health data for our control areas. We may have conflated the impact of community matrons and the additional support provided by this POPP intervention, and perhaps overestimated the impact of this POPP intervention as a result.

Our analysis examined aspects of health care use over six months at least, and over a year for four of the interventions. It may be that the interventions had longer-term impacts that we could not detect. This possibility was raised by a number of the sites when we visited them, and is particularly pertinent where the intervention itself happens over a period of time. We could update our analysis as more HES data become available. Moreover, a key objective of the POPP projects was to improve older people's independence, wellbeing and quality of life but our study did not measure these for the individuals or their carers. Therefore it may be that there were important benefits from POPP that this study could not detect.

The findings could be driven by changes in the definition of 'usual care' for the matched control group. Although the matched controls had not taken part in a similar POPP intervention, other active hospital reduction programmes may have been in place in the comparison areas. The findings are best interpreted as being relative to other measures being taken elsewhere for similar patients.

Implications for clinicians and policymakers

We have not found evidence that the eight POPP interventions studied reduced rates of emergency hospital admissions. In fact, in some cases there were increases in hospital use. This phenomenon has been observed previously in other hospital avoidance initiatives including the UK Evercare[13]. One hypothesis is that the process of 'case finding' identifies new problems which result in patients being referred into the health care system. In other words, when patients first began the interventions, the professionals may have identified problems that necessitated hospital admission.

Extra data would allow quantification of the impact of the interventions on the number of GP visits and the intensity of social care use. Both of these were identified by our sites as possibilities for reductions in utilisation. The hospital-based analysis could be updated as more HES data become available to look for impact over longer time periods.

This study has provided a model for other evaluations of complex interventions in the community where changes in hospital utilisation are a key outcome measure. We were able to access individual-level data and construct control groups. This strategy avoids the potential of falsely attributing findings at the site level to the subgroup of individuals who received the intervention, and meant that we could take account of the natural drop in admissions that occurs when high-risk cases are selected for an intervention. The analysis was retrospective and could be applied to a range of interventions and pilot projects in health and social care, even when these happened some time ago.

About the authors

Adam Steventon

Adam is a Senior Research Analyst at the Nuffield Trust. He has a background in mathematical modelling and policy analysis and is leading the Nuffield Trust's work in evaluating the success of a number of initiatives designed to prevent avoidable admissions to hospital and care homes. Previously he worked at the Pensions Policy Institute where he wrote widely on the UK Government's recent reforms to state and private pensions.

Martin Bardsley

Martin is Head of Research at the Nuffield Trust and leads a specialist team undertaking a range of quantitative research projects. He previously worked at the Healthcare Commission, leading on the use of information to support risk based regulation. He has also undertaken research into the measurement of outcomes and case mix, and led a public health information group for London.

John Billings

John is currently working with the NHS and the Nuffield Trust to develop techniques for analysing barriers to care in London and the UK, and to develop programmes to improve chronic care services. He is also Director of New York University's Health Policy and Management Program at the Robert F Wagner Graduate School of Public Service and a Senior Associate at the King's Fund.

Theo Georghiou

Theo joined the Nuffield Trust in 2008 from the King's Fund where he'd helped develop the Patients at Risk of Readmissions tool. He has since undertaken work on extending such predictive models to use a wider range of data sources and to predict other adverse events. He has recently worked on projects including the development of Person Based Resource Allocation models and has studied variations in hospital and social care at the end of life.

Geraint Lewis

Geraint is a Senior Fellow at the Nuffield Trust. He is a consultant public health physician and previously worked as a junior doctor in London and Sydney. He was a policy advisor at the Cabinet Office and visiting fellow at the King's Fund, before spending the 2007–08 academic year as a Commonwealth Fund Harkness Fellow in New York. He is a Fellow of the Royal College of Physicians of London.

Appendix A: HES data linkage

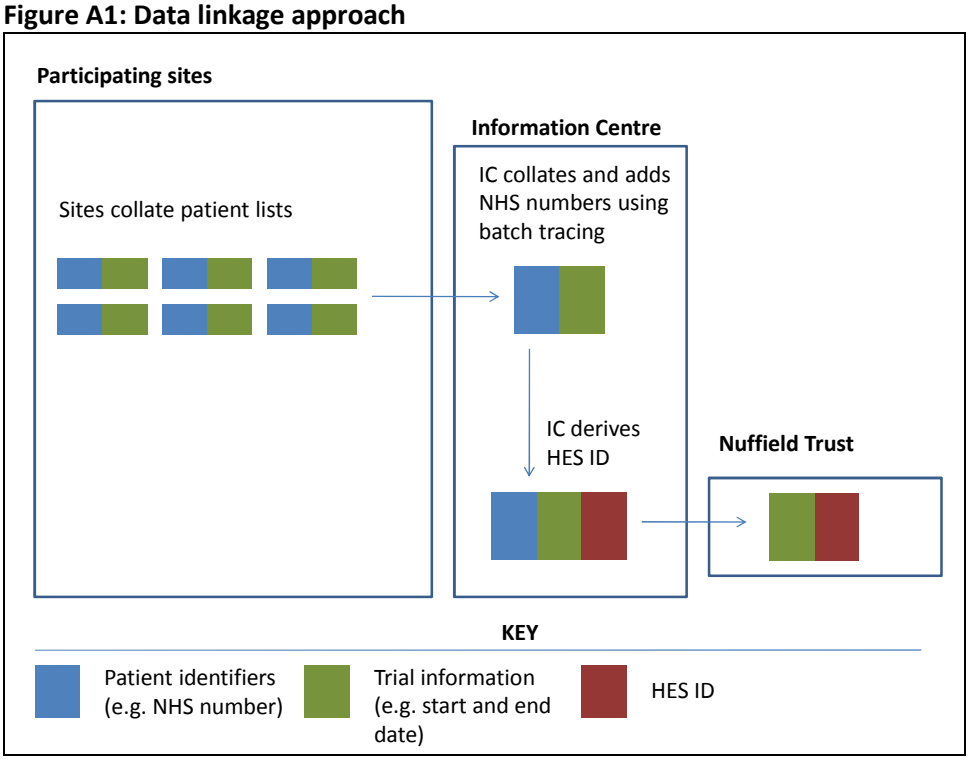
Description of data requested from sites

This study exploited existing operational information streams, rather than requiring significant new data collection on hospital utilisation. It was retrospective, looking at patient histories throughout the course of the selected POPP interventions. All of the data used by the research team were effectively anonymised.

The sites shared the details of who received the interventions with the NHS Information Centre for health and social care, who acted as a trusted third party and performed data linking that allowed the research team to look up the intervention group in the national Hospital Episode Statistics (HES).

The flow of data during the data linkage process is illustrated in Figure A1.

Figure A1: Data linkage approach



The fields requested from the sites were:

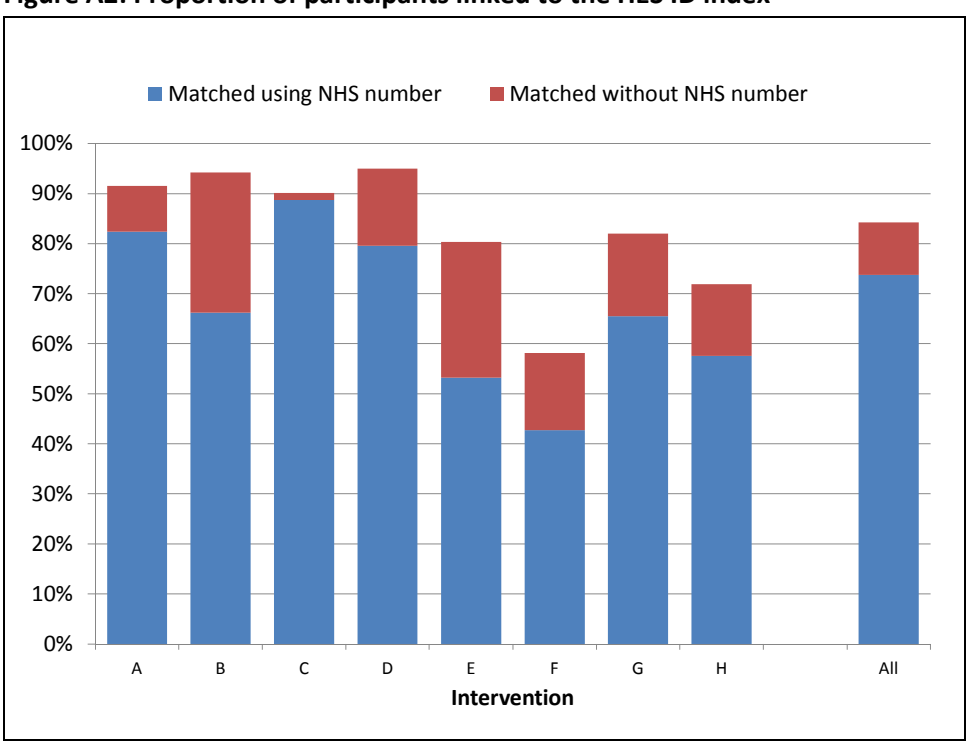
NHS number (if available)
Date of birth
Sex
Post code
First name
Last name
Start date
End date (if available)
Description of service (if relevant)

The NHS Information Centre, working with Northgate Information Solutions, linked records to the HES ID index – a file of around 170 million records that contains the NHS number, date of birth, sex, and post code of every individual in England who has had an inpatient admission, outpatient attendance or accident or emergency visit since April 1997.[††] It is assembled from data submitted on a regular basis by all acute health care providers.

The sites typically were not able to provide the NHS number of trial participants, so the NHS Information Centre attempted to obtain the NHS number from the Demographics Batch Service. This increased the proportion of participants with an NHS number from 35% to over 70%. For records with an NHS number, the Information Centre linked to the HES ID index using an algorithm that required not only an exact match on NHS number but also an exact match on sex and a partial match on date of birth. Where an NHS number could not be obtained from the Demographics Batch Service, or where the first algorithm failed to produce a match, the NHS Information Centre linked to the HES ID index using an alternative algorithm. This alternative algorithm did not use the NHS number, but required an exact match on sex, post code and date of birth.

After the HES data linkage process was complete, 84 per cent of POPP participants were linked to the HES ID index, the majority of which were linked using a combination of fields that included the NHS number. The rate varied significantly between interventions: interventions A-E had a linkage rate of 80 per cent or higher, while fewer than 60 per cent of the participants of intervention F were linked to HES (Figure A2).

Figure A2: Proportion of participants linked to the HES ID index



[††] Coverage of accident and emergency visits is partial

There are three reasons why records might not link to the HES ID index:

1. The data provided by the sites might be inaccurate or incomplete.
2. The data in the HES ID index might be inaccurate, incomplete or out of date (for example perhaps an individual has moved to a different area since the last time they went to hospital).
3. The individuals concerned may not have had a contact with hospital services in England since April 1997.

In fact, the majority (60%) of records not linked to the HES ID index were missing data (Table A1). We have assumed that inaccurate or incomplete data is a random occurrence not related to the characteristics of the individual concerned, and that no bias has resulted from not being able to work with the records not linked to HES. However, it is possible that the people not linked to HES were less intensive users of services (that data is more complete for people who engage with services more often).

Table A1: Number of records not linked to the HES ID index

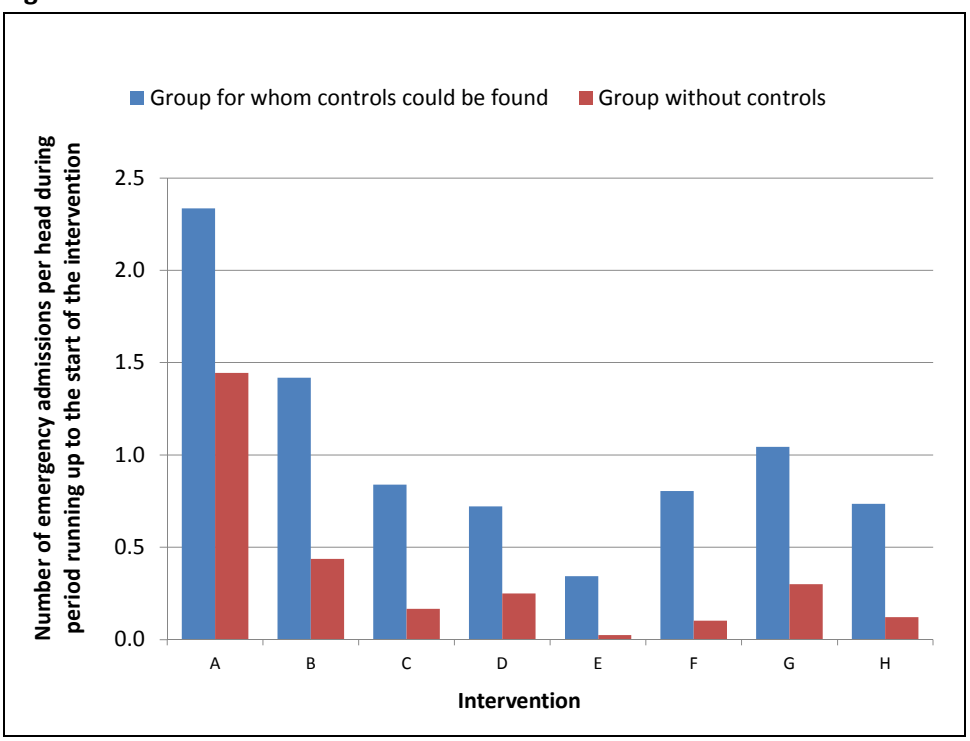
Intervention	Missing date of birth	Missing sex	Missing post code	One or more item of missing data	Complete data	Total not linked to HES ID index	% with one or more item of missing data
A	5	*	20	26	30	56	46%
B	0	*	*	5	60	65	8%
C	77	19	538	551	275	826	67%
D	9	0	*	12	56	68	18%
E	88	0	22	107	165	272	39%
F	567	*	240	680	158	838	81%
G	143	0	36	175	225	400	44%
H	66	2	158	196	181	377	52%
Total	**955**	**21**	**1,014**	**1,752**	**1,150**	**2,902**	**60%**

Appendix B: Pre-post analyses for individuals without a control

We were not able to derive controls for everybody in the study cohort. Modelling could only be performed on the members of the study cohort who were aged 70 or over (65 for interventions E and F), had an inpatient admission in the two years prior to receiving the intervention, and had not been resident in more than one of the POPP areas. We derived controls for 5,146 people, out of the 9,080 people in the study cohort.

In order to ensure that our selection did not bias the results, we undertook pre-post analyses on the people who were not included in the controlled study. The people who were not included had very low levels of hospital admission before the intervention started – usually much lower than the people for whom we found controls (Figure B1).

Figure B1



Note: Figures are for emergency admissions per head over a 12 month period, with the following exceptions: figures for C, D and H are for six months, and figures for E are for nine months.

For the group of people without a control, hospital admissions increased following the intervention (Table B1).

Table B1: Changes in emergency hospital admissions per head observed for the group of people without a control – means (standard deviations)

Intervention	N	Before	After	Change	% Change
A	61	1.44 (2.09)	1.31 (1.96)	-0.13 (1.90)	-9%
B	117	0.44 (0.87)	0.54 (0.92)	0.10 (1.12)	24%
C (1)	1976	0.17 (0.66)	0.22 (0.59)	0.06** (0.75)	33%**
D (1)	289	0.25 (0.89)	0.34 (0.64)	0.09 (1.09)	38%
E (2)	483	0.02 (0.21)	0.15 (0.56)	0.13** (0.58)	517%**
F	466	0.10 (0.43)	0.35 (0.90)	0.25** (0.89)	240%**
G	258	0.30 (0.87)	0.42 (1.25)	0.12 (1.13)	42%
H (1)	284	0.12 (0.71)	0.29 (0.76)	0.17** (0.80)	141%**

(1) Admissions rates are for six months before/after intervention (2) Nine months following intervention
* denotes statistically significant at the 5% level. ** denotes statistically significant at the 1% level.

This simple pre-post analysis does not mean that the intervention increased hospital admissions in the group of people that were not included in the controlled study: it is very difficult to determine the impact of the intervention without a control group for the reasons outlined in Chapter 2. However, the pre-post analysis does show that our controlled analyses were focused on the more intensive service users. We think it is unlikely that our selection process biased the results to show no reductions in admissions in the intervention group.

Appendix C: Methods for deriving matched controls

We derived a matched control group for each of the sites, at the person level, using predictive risk. This appendix describes the predictive risk models and the matched control groups.

The predictive risk models

The predictive risk models developed for this study are similar to the Patients At Risk of Re-hospitalisation (PARR) model that is widely in use within the NHS. The output of the models is a number between 0 and 100 for each individual in the population, representing the estimated probability of their having one or more emergency inpatient admissions to hospital in the next 12 months. In some sites, we did not have sufficient data to predict emergency admissions in the next 12 months, so we built models to predict admissions over a shorter time period.

Predictive risk models are developed based on the patterns of events (independent variables) observed running up to an emergency admission to hospital (the dependent variable). In selecting the independent variables for this project, we tested the impact of 50-100 variables derived from HES. These were similar to those selected for the PARR model, but we also included variables related to outpatient use. The variables that remained in the model varied between the sites but typically included:

- age and sex
- around 30 variables related to health diagnoses recorded in HES (for example whether someone had been diagnosed with hypertension or COPD)
- around 20 variables related to prior inpatient utilisation (for example the number of emergency inpatient admissions in the previous year)
- around ten variables related to prior outpatient utilisation (for example the number of consultant specialties).

No predictive risk model is a perfect predictor of the future, and performance is usually measured by quantities such as:

- **The Positive Predictive Value (PPV):** This is the proportion of the people who the model predicts as being likely to have an emergency hospital admission, who in reality will go on to have an emergency hospital admission (that is, the proportion of predicted cases that the model predicted correctly).
- **The sensitivity:** This is a related concept to the PPV, defined as the proportion of people who in reality will go on to have an emergency hospital admission, who the model predicts as being likely to have an emergency hospital admission (that is, the proportion of people admitted correctly identified by the model).

In building the predictive models we considered it necessary to maximise their PPV and sensitivity, so that they were the most reliable estimates possible of the probability of emergency admission. We therefore adopted a strategy of fitting separate models in each of the eight areas, which meant that the models were calibrated as closely as possible to local patterns of hospital use. When fitting the models, we did not use information related to people identified as ever receiving a POPP

intervention, because it was considered that the POPP interventions might alter the typical pattern of hospital use. After fitting the model, we applied the calculated beta coefficients to the intervention group to derive their predictive risk scores.

Individuals joined the interventions over a long period of time: for interventions F and G, for example, the first individuals started to receive the interventions in July 2006, and were still continuing to be recruited at our cut-off date of 31 March 2008. Therefore, we needed a predictive risk score calculated as close as possible to the point at which each individual received the intervention, so we fitted models and calculated risk scores on a monthly basis. In total, over the eight sites, this meant we derived over 90 predictive models in our baseline runs.

Although the performance of the models varied across time, typically we achieved a positive predictive value of 45%-55% (Figure C1). The sensitivities were lower, and varied considerably between sites. For intervention F the sensitivity was consistently above 9%, but for interventions C and D it dropped below the 2% level in the later months of the intervention (Figure C2). The predictive models are statistically significant, so much better than chance. Although the PPV and sensitivity are useful measures of the performance of a predictive model, in this context the models were used as an intermediate step to deriving controls. The validity of the control groups was assessed by comparing them with the intervention group in terms of the distribution of certain characteristics.

Figure C1: Positive predictive values of predictive risk models in five selected sites

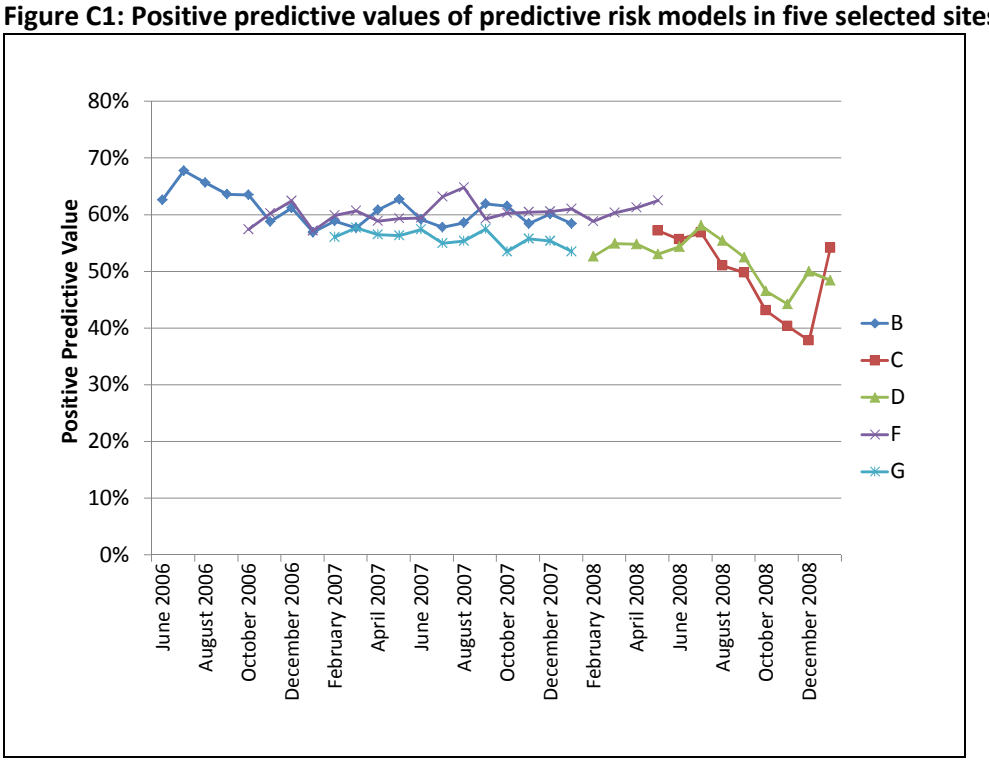
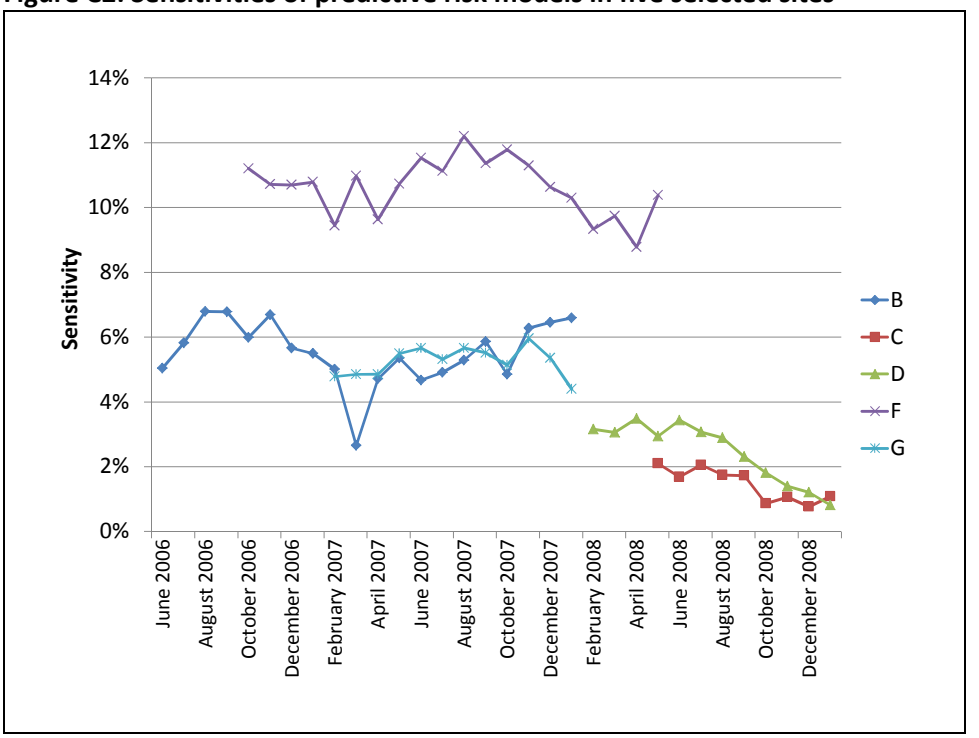


Figure C2: Sensitivities of predictive risk models in five selected sites



Source of potential controls

We aimed to select matched controls for POPP participants at an individual level. We therefore needed to decide the areas from which to select controls. Three options were considered: controls selected only from within the area undertaking the intervention, from similar areas across England, or nationally from all of England.

For our base models we chose the middle option: selecting controls from similar local authority areas across England. There are some arguments for selecting controls from within the same area. For example, it might standardise the health and social care services received by the intervention and matched control groups. However, it would also have run the risk that controls were indirectly affected by the changes in local services brought about by POPP. It would have limited the potential supply of controls and made close matches harder to find, and potentially increased selection bias.[‡‡] Selecting controls nationally was a possibility but would have been very computer-intensive and proved unnecessary because we found adequate controls from the comparison sites.

Three potential comparison sites were selected for each of our eight areas. We excluded the POPP sites analysed in this study as potential comparison sites and selected local authority areas with similar age structure, deprivation level, urban/rural nature[§§], and ethnic mix. The sites selected as potential comparators are shown in Table C1.

[‡‡] With an observational study, there is always the possibility that controls might in fact be ineligible for POPP for reasons that cannot be detected. This possibility is considered to be greater using the within-area approach, because a significant number of eligible participants (those that received the intervention) have already been removed from the pool of potential matches.

[§§] To accomplish this, we selected district local authorities as comparator areas for the POPP sites that are themselves district local authorities, and metropolitan and unitary local authorities for the remaining POPP sites

Table C1: Comparison sites (Local authorities)

	1	2	3
A	Derbyshire	Northumberland	Nottinghamshire
B	Havant	Taunton Deane	Havering
C	Lincolnshire	Somerset	Shropshire
D	Lincolnshire	Northumberland	Suffolk
E	Hyndburn	Wakefield	Bolton
F	Middlesbrough	Salford	Liverpool
G	Dorset	Suffolk	Lincolnshire
H	Gloucester	Suffolk	Worcestershire

The predictive risk models were developed in the POPP sites, and then applied to the comparison sites to produce risk sores for the potential controls (so, for example, the model for intervention C was fitted using data for people registered in the area offering intervention C, and then its beta coefficients applied to Lincolnshire, Somerset, and Shropshire to produce risk scores). In theory, we could have fitted a separate model in the control areas, but this was considered less likely to balance the matched intervention and control groups on underlying characteristics such as prior utilisation and diagnoses.

Matching approach

Having derived the predictive risk scores we were faced with choices about how to select, for each individual in the intervention group, one or more matched control. The objective was to ensure that each matched control has as similar as possible characteristics to the corresponding member of the intervention group, running up to the start of the intervention.

The predictive risk score was considered to be the most important quantity that should be balanced; indeed, matching on the risk score was found to go a long way towards matching on underlying characteristics such as prior inpatient and outpatient utilisation, health diagnoses, age and sex. However, we considered some characteristics to be particularly important, such as prior emergency inpatient utilisation, number of chronic conditions,[***] and area-level deprivation.[†††] We therefore matched on a range of characteristics, using a matching technique borrowed from other epidemiological studies: Mahalanobis metric matching.[6] For any given member of the intervention group, this technique restricts the pool of potential matches to those with a similar predictive risk score (within one-quarter of a standard deviation), and an exact match on sex and age group. It then selects the individual with a similar balance on the other variables of interest, using a multi-dimensional distance measure known as the Mahalanobis metric. We chose to select one control for each member of the intervention group. The alternative would have been to select more than one control for each. Our strategy minimised bias between the intervention and control group, although limited statistical power. Reducing bias was considered to be the more important objective and

[***] Chronic conditions here include: diabetes, hypertension, congestive heart failure, chronic obstructive pulmonary disease, ischemic heart disease, asthma, angina, cerebrovascular disease, renal failure.

[†††] Deprivation has been attributed to the lower super output area of each individual's residence using the scores available from communities and local government

resulted in a conservative strategy. We used matching without replacement so that the control group would consist of distinct individuals.

Predictive risk scores were available on a monthly basis. We had a choice, for a given member of the intervention group, whether to use the risk score calculated at the month end immediately prior to receiving the intervention, or the one calculated at the month end immediately following. Using the risk score from the month before would not have captured very recent events that occurred in the few days before joining the intervention. In many of the sites – for example the hospital discharge scheme (intervention B) – these recent events seemed to define the membership of the intervention group in an important way. Therefore we matched using the risk score at the month end immediately following joining the intervention. This means we matched using a limited amount of events that occurred after the intervention began, over a period of up to one month for some individuals; however, it results in better matches.

Appendix D: Evaluation results for intervention A

Support workers working under the direction of community matrons with people with one or more long-term conditions who were felt to be at risk of deterioration or were unstable. Support workers provide personal nursing and social care.

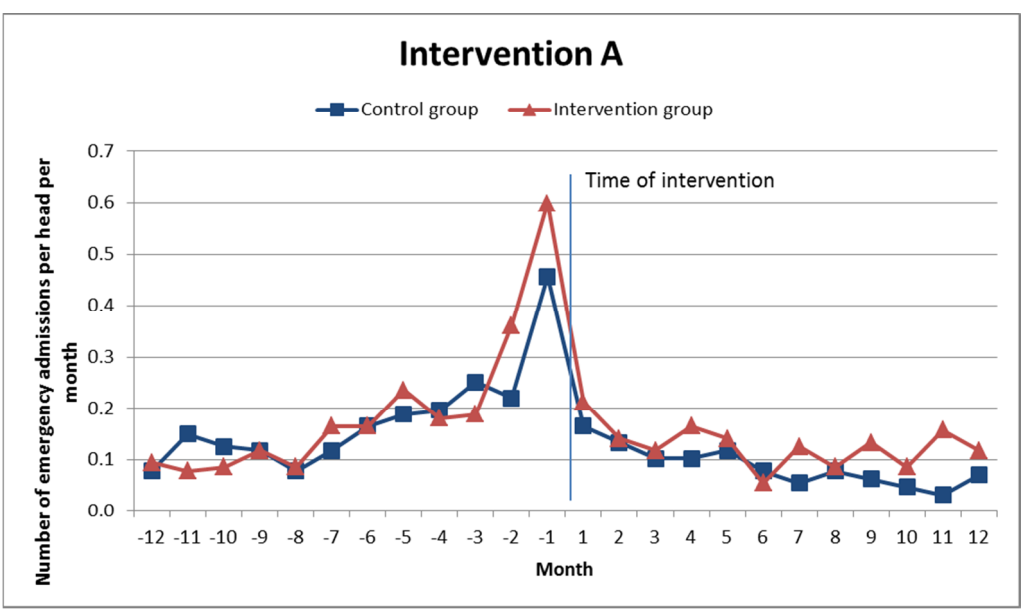
Data matching

	Intervention (N=127)	Control (N=127)	Standardised difference
Proportion aged 85+	20%	18%	4.0%
Proportion female	50%	50%	0.0%
Mean area-level deprivation score	20.1	20.2	0.5%
Mean number of emergency admissions in previous year	2.2	2.4	12.4%
Mean number of emergency admissions in previous 30 days	0.5	0.6	20.5%
Mean emergency length of stay in previous year	22.0	20.6	4.7%
Mean number of chronic conditions	2.9	2.9	4.5%
Mean predictive risk score	0.4	0.4	0.3%

Intervention effect on secondary care utilisation

	Intervention (N=127)			Control (N=127)			Intervention effect
	Before	After	Change	Before	After	Change	
Emergency admissions per head	2.36 (1.77)	1.54 (2.15)	-0.82** (2.37)	2.15 (1.66)	1.05 (1.64)	-1.10* (1.79)	0.28 (2.83)
Emergency length of stay	20.59 (21.99)	13.05 (25.82)	-7.54* (30.79)	22.02 (36.46)	13.81 (25.44)	-8.20* (37.46)	0.66 (42.82)
Elective admissions per head	0.42 (0.86)	0.39 (0.87)	-0.02 (1.06)	0.35 (0.73)	0.30 (0.68)	-0.06 (0.86)	0.03 (1.24)
Outpatient attendances/head	5.34 (6.19)	4.25 (6.14)	-1.09 (6.64)	4.06 (4.35)	3.00 (3.76)	-1.06** (4.06)	-0.02 (6.55)

Figures are based on the 6 months before/after intervention * Statistically significant at the 5% level ** at 1% level



Appendix E: Evaluation results for intervention B

Intermediate care service with generic workers, which supported people on discharge from hospital.

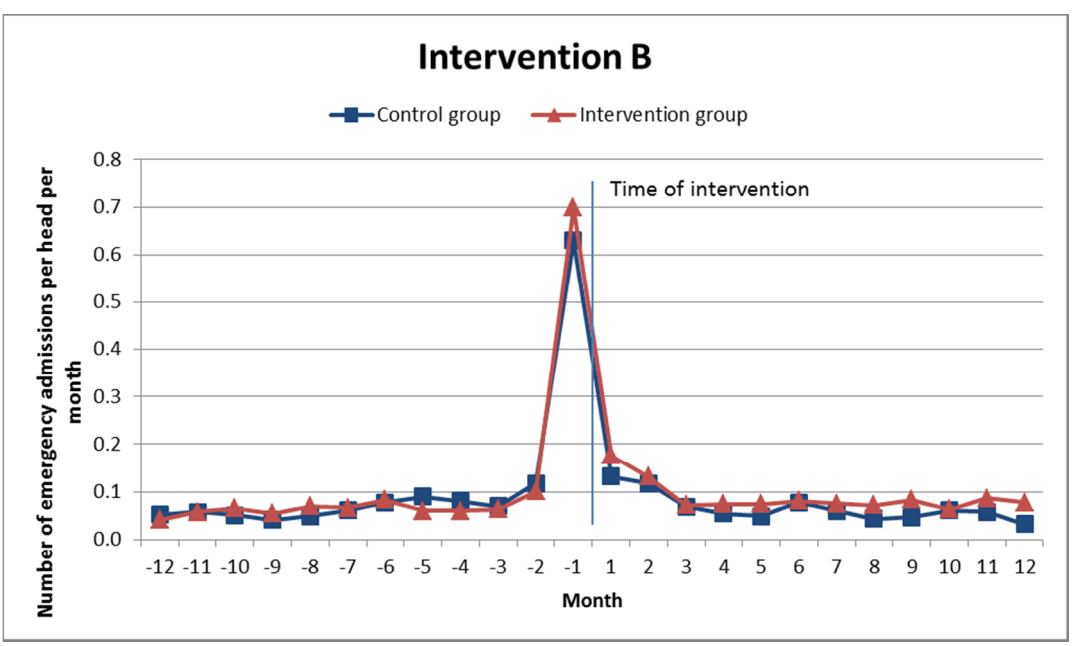
Data matching

	Intervention (N=556)	Control (N=556)	Standardised difference
Proportion aged 85+	46%	46%	0.0%
Proportion female	65%	65%	0.0%
Mean area-level deprivation score	17.0	16.4	6.2%
Mean number of emergency admissions in previous year	1.4	1.4	3.0%
Mean number of emergency admissions in previous 30 days	0.7	0.6	11.2%
Mean emergency length of stay in previous year	9.4	10.2	4.7%
Mean number of chronic conditions	1.4	1.4	2.5%
Mean predictive risk score	0.34	0.34	0.4%

Intervention effect on secondary care utilisation

	Intervention (N=556)			Control (N=556)			Intervention effect
	Before	After	Change	Before	After	Change	
Emergency admissions per head	1.42 (1.40)	1.06 (1.54)	-0.35** (1.78)	1.38 (1.32)	0.80 (1.30)	-0.58** (1.47)	0.23** (1.95)
Emergency length of stay	9.45 (16.68)	19.63 (26.52)	10.18** (30.56)	10.20 (15.55)	12.27 (22.45)	2.06 (25.44)	8.11** (34.45)
Elective admissions per head	0.50 (1.05)	0.53 (0.96)	0.03 (1.27)	0.43 (0.96)	0.51 (1.07)	0.08 (1.14)	-0.05 (1.41)
Outpatient attendances/head	2.73 (4.14)	2.04 (2.87)	-0.69** (4.23)	2.49 (3.64)	2.42 (3.53)	-0.07 (3.79)	-0.62** (4.40)

Figures are based on the 6 months before/after intervention * Statistically significant at the 5% level ** at 1% level



Appendix F: Evaluation results for intervention C

Integrated health and social care teams configured around primary care teams, which focused on people with one or more long-term conditions.

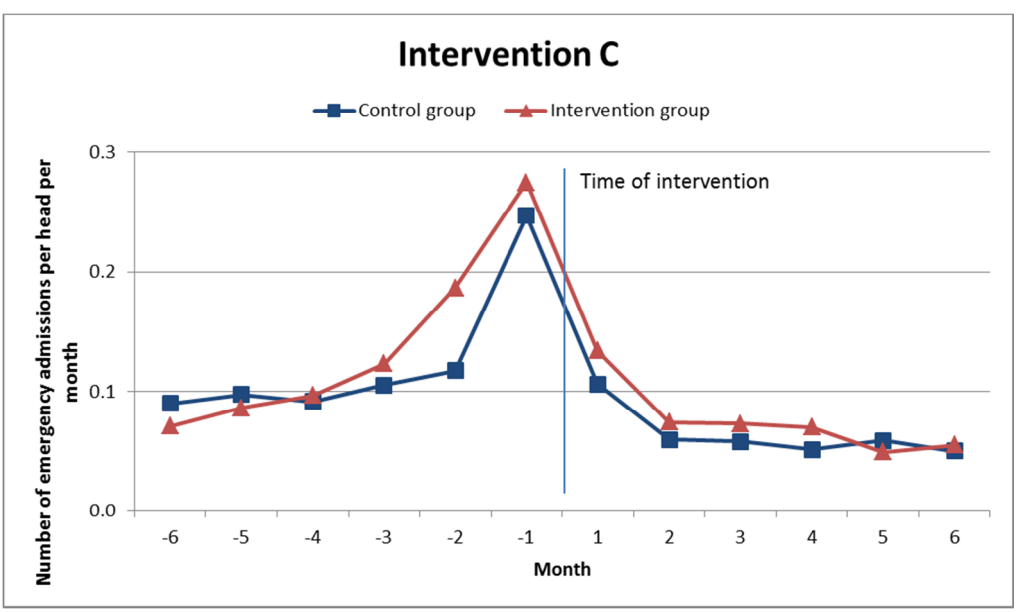
Data matching

	Intervention (N=2,557)	Control (N=2,557)	Standardised difference
Proportion aged 85+	48%	48%	0.1%
Proportion female	65%	65%	0.0%
Mean area-level deprivation score	17.7	17.1	7.9%
Mean number of emergency admissions in previous year	1.2	1.1	4.2%
Mean number of emergency admissions in previous 30 days	0.3	0.3	5.2%
Mean emergency length of stay in previous year	13.0	11.4	8.4%
Mean number of chronic conditions	1.3	1.4	1.9%
Mean predictive risk score	0.20	0.20	0.7%

Intervention effect on secondary care utilisation – means (standard deviations)

	Intervention (N=2,557)			Control (N=2,557)			Intervention effect
	Before	After	Change	Before	After	Change	
Emergency admissions per head	0.84 (1.10)	0.46 (0.86)	-0.38** (1.30)	0.75 (0.99)	0.38 (0.80)	-0.36** (1.10)	-0.02 (1.40)
Emergency length of stay	9.35 (16.69)	6.06 (14.46)	-3.29** (20.52)	7.42 (14.31)	5.21 (14.17)	-2.21** (17.98)	-1.08* (23.94)
Elective admissions per head	0.60 (1.24)	0.34 (0.97)	-0.26** (1.33)	0.41 (1.04)	0.27 (0.81)	-0.15** (1.10)	-0.11** (1.40)
Outpatient attendances/head	1.74 (2.78)	1.33 (2.24)	-0.41** (2.85)	1.50 (2.21)	1.33 (2.04)	-0.17** (2.23)	-0.24** (3.44)

Figures are based on the 6 months before/after intervention * Statistically significant at the 5% level ** at 1% level



Appendix G: Evaluation results for intervention D

Out-of-hours response service and daytime response service, both consisted of an integrated team comprising community alarm services, mobile wardens, generic workers, district nurses, paramedics and community psychiatric nurses.

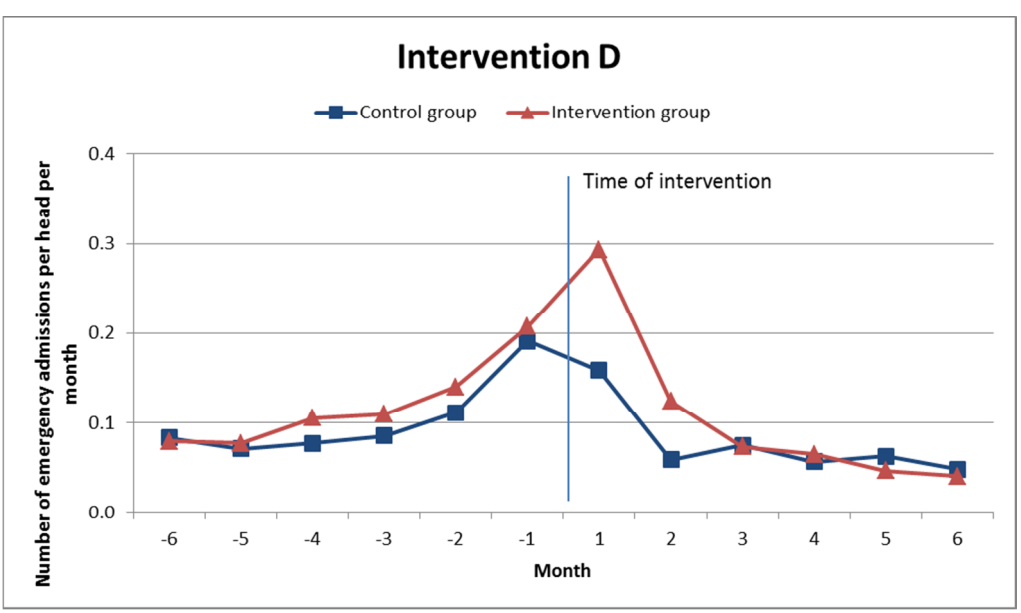
Data matching

	Intervention (N=491)	Control (N=491)	Standardised difference
Proportion aged 85+	49%	49%	0.0%
Proportion female	55%	55%	0.0%
Mean area-level deprivation score	18.1	17.7	3.9%
Mean number of emergency admissions in previous year	1.1	1.0	3.6%
Mean number of emergency admissions in previous 30 days	0.2	0.2	3.7%
Mean emergency length of stay in previous year	12.2	10.8	7.9%
Mean number of chronic conditions	1.5	1.4	2.1%
Mean predictive risk score	0.24	0.24	0.1%

Intervention effect on secondary care utilisation

	Intervention (N=491)			Control (N=491)			Intervention effect
	Before	After	Change	Before	After	Change	
Emergency admissions per head	0.72 (0.95)	0.64 (0.99)	-0.08 (1.32)	0.62 (0.83)	0.46 (0.88)	-0.16** (1.11)	0.08 (1.48)
Emergency length of stay	8.45 (15.21)	10.08 (18.55)	1.63 (23.34)	5.96 (11.30)	6.51 (14.21)	0.55 (16.37)	1.08 (26.21)
Elective admissions per head	0.40 (0.92)	0.22 (0.54)	-0.17** (0.99)	0.46 (1.20)	0.38 (1.53)	-0.08 (1.51)	-0.10 (1.52)
Outpatient attendances/head	1.81 (2.81)	1.10 (2.27)	-0.71** (2.70)	1.66 (2.50)	1.38 (1.97)	-0.28** (2.09)	-0.42** (2.88)

Figures are based on the 6 months before/after intervention * Statistically significant at the 5% level ** at 1% level



Appendix H: Evaluation results for intervention E

Volunteer-run assessment and signposting service. Volunteers made contact with older people, carried out a home-based 'check-up', and provided advice on benefits entitlement, housing, community transport, education and leisure activities.

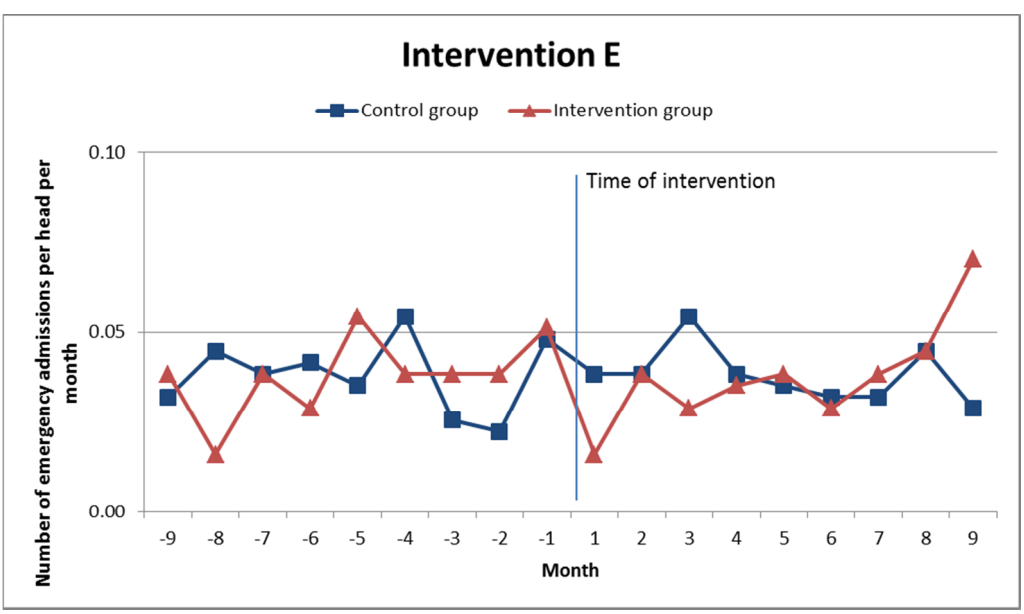
Data matching

	Intervention (N=312)	Control (N=312)	Standardised difference
Proportion aged 85+	21%	21%	0.0%
Proportion female	67%	67%	0.0%
Mean area-level deprivation score	33.2	32.9	2.0%
Mean number of emergency admissions in previous year	0.5	0.5	0.4%
Mean number of emergency admissions in previous 30 days	0.1	0.0	1.4%
Mean emergency length of stay in previous year	4.2	3.7	5.4%
Mean number of chronic conditions	1.5	1.5	4.3%
Mean predictive risk score	0.19	0.19	0.6%

Intervention effect on secondary care utilisation

	Intervention (N=312)			Control (N=312)			Intervention effect
	Before	After	Change	Before	After	Change	
Emergency admissions per head	0.34 (0.68)	0.34 (0.68)	0.00 (0.90)	0.34 (0.65)	0.34 (0.75)	0.00 (0.87)	0.00 (1.10)
Emergency length of stay	3.13 (9.06)	3.52 (13.07)	0.39 (15.54)	2.83 (8.20)	2.15 (6.76)	-0.68 (10.12)	1.07 (16.43)
Elective admissions per head	0.53 (0.76)	0.43 (0.80)	-0.10 (0.98)	0.57 (0.77)	0.44 (1.46)	-0.13 (1.65)	0.03 (1.77)
Outpatient attendances/head	3.96 (4.17)	4.10 (5.22)	0.14 (4.59)	3.56 (3.59)	2.97 (4.02)	-0.59** (3.91)	0.73** (5.57)

Figures are based on the 6 months before/after intervention * Statistically significant at the 5% level ** at 1% level



Appendix I: Evaluation results for intervention F

Short-term assessment and signposting service, which targeted all older people in some of the most deprived areas.

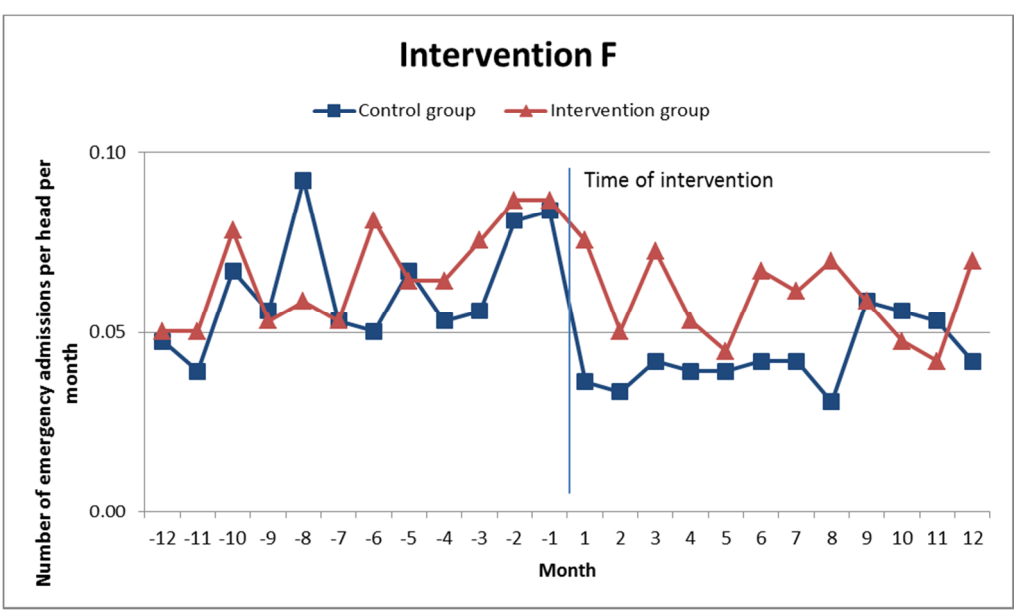
Data matching

	Intervention (N=357)	Control (N=357)	Standardised difference
Proportion aged 85+	15%	15%	0.0%
Proportion female	70%	70%	0.0%
Mean area-level deprivation score	46.2	45.8	2.1%
Mean number of emergency admissions in previous year	0.8	0.7	4.9%
Mean number of emergency admissions in previous 30 days	0.1	0.1	1.0%
Mean emergency length of stay in previous year	7.1	6.0	7.5%
Mean number of chronic conditions	1.5	1.4	2.1%
Mean predictive risk score	0.28	0.28	0.3%

Intervention effect on secondary care utilisation

	Intervention (N=357)			Control (N=357)			Intervention effect
	Before	After	Change	Before	After	Change	
Emergency admissions per head	0.80 (1.18)	0.71 (1.25)	-0.09 (1.43)	0.75 (1.10)	0.52 (1.11)	-0.23** (1.38)	0.14 (1.43)
Emergency length of stay	7.12 (15.62)	6.68 (17.45)	-0.44 (21.15)	6.05 (12.82)	5.68 (17.16)	-0.37 (19.80)	-0.07 (23.11)
Elective admissions per head	0.53 (1.03)	0.41 (0.81)	-0.13* (1.16)	0.47 (0.85)	0.40 (0.90)	-0.07 (0.95)	-0.05 (1.21)
Outpatient attendances/head	5.72 (6.19)	5.29 (5.81)	-0.43 (5.26)	5.20 (5.22)	5.11 (6.64)	-0.09 (5.65)	-0.33 (7.70)

Figures are based on the 12 months before/after intervention * Statistically significant at the 5% level ** at 1% level



Appendix J: Evaluation results for intervention G

Short-term assessment and signposting service, which involved staff visiting clients in their own environment. The initiative used the single assessment process to signpost and commission from a pre-agreed menu of community services, or referred clients to specialist services.

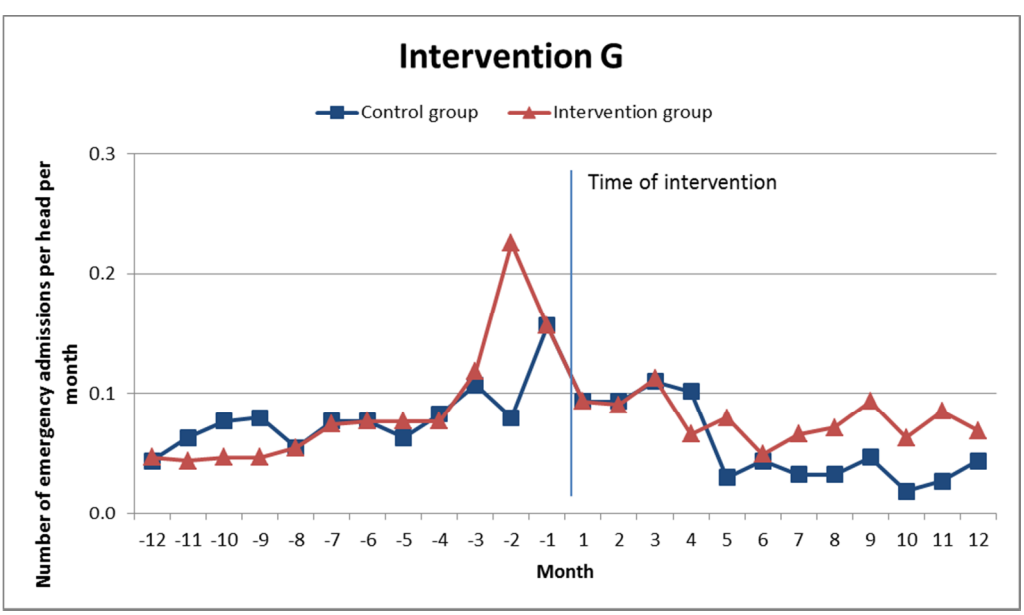
Data matching

	Intervention (N=364)	Control (N=364)	Standardised difference
Proportion aged 85+	37%	37%	0.0%
Proportion female	67%	67%	0.0%
Mean area-level deprivation score	19.2	18.1	10.8%
Mean number of emergency admissions in previous year	1.0	1.0	6.2%
Mean number of emergency admissions in previous 30 days	0.2	0.2	0.0%
Mean emergency length of stay in previous year	10.3	9.7	3.3%
Mean number of chronic conditions	1.3	1.2	2.9%
Mean predictive risk score	0.27	0.27	0.4%

Intervention effect on secondary care utilisation **Intervention effect on secondary care utilisation**

	Intervention (N=364)			Control (N=364)			Intervention effect
	Before	After	Change	Before	After	Change	
Emergency admissions per head	1.04 (1.39)	0.94 (1.34)	-0.10 (1.58)	0.96 (1.26)	0.68 (1.19)	-0.29** (1.50)	0.18* (1.57)
Emergency length of stay	10.35 (20.31)	10.21 (19.45)	-0.13 (26.31)	9.71 (18.87)	8.39 (19.04)	-1.32 (24.70)	1.19 (26.22)
Elective admissions per head	0.87 (1.61)	0.71 (1.44)	-0.15 (1.49)	0.72 (1.38)	0.62 (1.62)	-0.10 (1.76)	-0.05 (2.04)
Outpatient attendances/head	3.95 (4.68)	3.60 (3.89)	-0.35 (4.65)	3.59 (3.99)	3.04 (3.86)	-0.55** (3.50)	0.20 (5.00)

Figures are based on the 6 months before/after intervention * Statistically significant at the 5% level ** at 1% level



Appendix K: Evaluation results for intervention H

Short-term assessment and signposting service, which aimed to improve access to low-level preventive services by establishing a single point of access. Joint prevention teams consisted of health advisers, health trainers, social care workers, link workers, a team coordinator and volunteers.

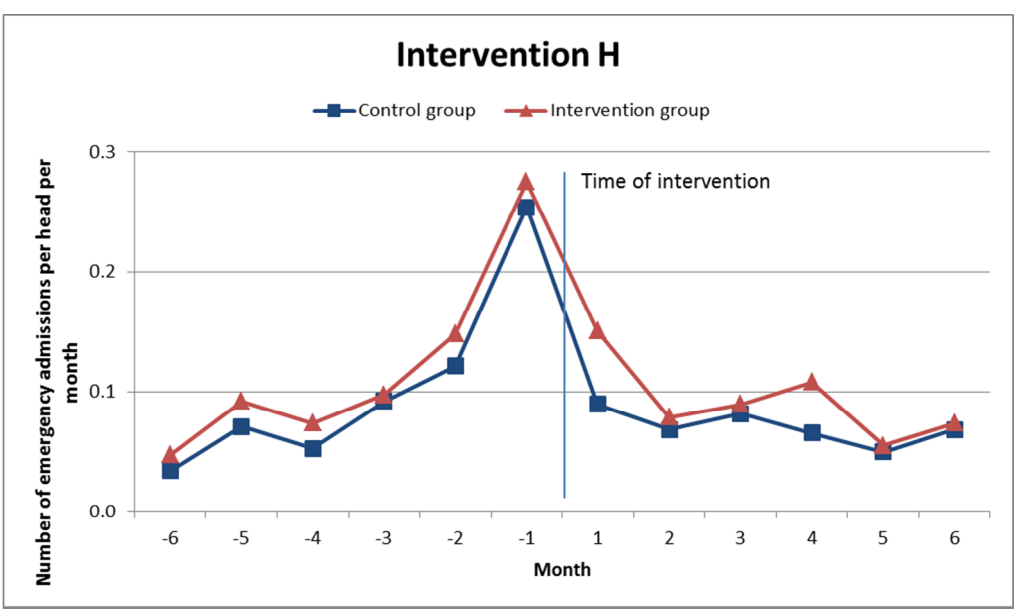
Data matching

	Intervention (N=378)	Control (N=378)	Standardised difference
Proportion aged 85+	47%	47%	0.0%
Proportion female	68%	68%	0.0%
Mean area-level deprivation score	16.6	16.2	4.8%
Mean number of emergency admissions in previous year	1.0	0.9	3.0%
Mean number of emergency admissions in previous 30 days	0.3	0.3	4.0%
Mean emergency length of stay in previous year	8.6	8.7	0.7%
Mean number of chronic conditions	1.6	1.5	4.3%
Mean predictive risk score	0.25	0.25	0.2%

Intervention effect on secondary care utilisation

	Intervention (N=378)			Control (N=378)			Intervention effect
	Before	After	Change	Before	After	Change	
Emergency admissions per head	0.74 (1.09)	0.56 (1.03)	-0.18** (1.17)	0.63 (0.92)	0.43 (1.20)	-0.20** (1.23)	0.02 (1.53)
Emergency length of stay	6.93 (13.91)	6.53 (15.60)	-0.40 (18.83)	5.33 (11.58)	5.54 (15.83)	0.22 (16.76)	-0.62 (23.80)
Elective admissions per head	0.38 (0.84)	0.33 (0.94)	-0.04 (1.01)	0.36 (0.95)	0.21 (0.54)	-0.15** (0.91)	0.10 (1.16)
Outpatient attendances/head	1.63 (1.97)	1.70 (2.17)	0.07 (2.15)	1.50 (2.09)	1.35 (1.91)	-0.15 (2.35)	0.22 (2.98)

Figures are based on the 6 months before/after intervention * Statistically significant at the 5% level ** at 1% level



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