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Effect of a telephonic alert system (*Healthy Outlook*) for patients with chronic obstructive pulmonary disease: cohort study with matched controls

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Abstract

Background: *Healthy Outlook* was a telephonic alert system for patients with Chronic Obstructive Pulmonary Disease (COPD) in the United Kingdom. It used routine meteorological and communicable disease reports to identify times of increased risk to health. We tested its effect on hospital use and mortality.

Methods: Enrolees with a history of hospital admissions were linked to hospital administrative data. They were compared with control patients from local general practices, matched for demographic characteristics, health conditions, previous hospital use and predictive risk scores. We compared unplanned hospital admissions, admissions for COPD, outpatient attendances, planned admissions and mortality, over 12 months following enrolment.

Results: Intervention and matched control groups appeared similar at baseline (n=1,413 in each group). Over the 12 months following enrolment, *Healthy Outlook* enrolees experienced more COPD admissions than matched controls (adjusted rate ratio 1.26, 95% CI, 1.05 to 1.52) and more outpatient attendances (adjusted rate ratio 1.08, 95% CI 1.03 to 1.12). Enrolees also had lower mortality rates over 12 months (adjusted odds ratio 0.61, 95% CI, 0.45 to 0.84).

Conclusion: *Healthy Outlook* did not reduce admission rates, though mortality rates were lower. Findings for hospital utilisation were unlikely to have been affected by confounding.

Introduction

Chronic obstructive pulmonary disease (COPD) is a leading cause of morbidity, with approximately 800,000 diagnosed patients in England.[1] The progression of the disease is marked by exacerbations, or acute periods of deterioration in respiratory symptoms.[2] These exacerbations are often caused by viruses such as influenza.[3] Further, cold weather has been linked to exacerbations, mortality and hospital admissions for COPD.[4–7] Therefore, it has been hypothesised that, if one can predict the advent of cold weather or detect rises in influenza levels, then an anticipatory care intervention might prevent exacerbations and hospital admissions.[4,8]

Approaches to managing long-term conditions like COPD are increasingly using technology as part of the delivery of services.[10] However, sophisticated technological approaches such as automatic monitoring of blood oxygen levels can be expensive,[11] may not be suitable for all patients,[12,13] have doubtful effectiveness,[14] and are comparatively rarely used.[15] Simpler solutions using familiar and readily available technology such as the home telephone may be more cost effective, as well as more acceptable to patients.

Healthy Outlook was developed by the UK Meteorological Office (Met Office) and combines: i) a forecasting model to predict when the outdoor environment is likely to adversely affect the health of COPD patients; and ii) an anticipatory care intervention package. Patients receive telephone calls, during which they are reminded to keep well, asked to check their medication, and advised to contact their general practice in the event of a problem. A patient survey showed that the majority of respondents found the information provided as part of the service to be useful, while 36% of respondents had been prompted by the telephone calls to seek repeat prescriptions or to check COPD medication.[16]

It is hoped that, by providing timely and effective preventive support, *Healthy Outlook* will reduce COPD exacerbations, hospital admissions and deaths. However, evidence on this point has been equivocal. A Randomised Controlled Trial (RCT) found that fewer intervention patients had an exacerbation than controls (58% *vs.* 68%), but this did not reach statistical significance with a sample size of only 79 patients.[17] Since the service was since embedded into routine care for the population with COPD in many parts of England, there is opportunity to conduct large, observational studies with high levels of generalisability. However, existing studies have either been small and uncontrolled,[18] or have relied on aggregated analysis at the general practice level.[19] Analysis for higher-level units such as general practices can miss impacts for the subset of patients that receive the intervention of interest.

The wide coverage of hospital data sets in England and the use of anonymised patient identifiers meant that it was possible to conduct a larger study exploiting these operational data sets. We tested the effect of *Healthy Outlook* on hospital admissions and deaths against a matched control group.

Methods

Intervention and eligibility

Healthy Outlook was established in November 2007 to help patients with COPD to manage their condition, keep well and avoid hospital admissions.[20] Within the populations that were provided with the service, all general practices were eligible to participate and recruit patients.

Any patient with COPD could be referred into *Healthy Outlook* by general practice staff. Patient recruitment was usually done on an opt-in basis, with a minority of practices automatically enrolling patients. Patients who signed up for the service provided their telephone number to receive automated, interactive phone calls when the alerts were made. Patients also received two thermometers and an information pack. The information pack described the forecasting system and how cold weather could affect COPD symptoms. It also contained detailed advice on self-management of COPD.

Decisions about whether to signal a telephonic alert were made weekly by the Met Office during winter, which used meteorological and influenza virus data to determine whether there was an increased risk of COPD exacerbation. Consideration was given to indicators relating to calendar week; temperature; influenza virus levels (based on communicable disease reports);[21] and a forecaster evaluation of the overall synoptic situation and air quality. Decisions about whether to signal alerts were made on a regional basis, with the regions corresponding to the ten former Strategic Health Authorities (SHAs) in England.

When alerts were made, patients were telephoned up to three times, at their preferred time of day. Once the patient with COPD had been reached, a message was given informing the patient that people with COPD might become more unwell in the following two weeks. The automated telephone call then worked through a script, based on patients' replies to a series of questions. This covered the patients' COPD symptoms and medication, and is described in more detail elsewhere.[19] If a repeat prescription was needed or symptoms had worsened, patients were advised to contact their practice or respiratory nurse.

Study endpoints

We assessed the total number of urgent and unplanned ('emergency') hospital admissions, as well as hospital admissions for COPD, identified from hospital administrative data by the principal diagnosis, using *International Classification of Diseases*, *tenth revision* (*ICD-10*) codes J43 (emphysema) and J44 (other chronic obstructive pulmonary disease). Secondary metrics were numbers of planned ('elective') admissions and outpatient attendances. Finally, we examined mortality rates.

A preliminary sample size calculation was based on detecting a 20% change in emergency admissions over the twelve months following enrolment into *Healthy Outlook*, at 90% power and two-sided p-value < 0.05. We assumed that emergency admission rates would be 0.8 per year, based on national rates (standard

deviation 1.7; correlation between intervention and matched control groups 0.15). This produced a target sample size of 2,019 intervention patients.

Data linkage

We studied the anonymised hospital care histories of a sample of *Healthy Outlook* patients recruited in England between the start of the service (November 2007) and a cut-off point of September 2011. The sample was taken from the operational system used to manage participant data for *Healthy Outlook* and was primarily based on the availability of the unique patient identifier ('NHS number') needed for data linkage.

Hospital data came from a national administrative database of inpatient and outpatient care paid for by the National Health Service at all acute hospitals in England, the Hospital Episode Statistics (HES). We also obtained a linked mortality file that contained the date of death for patients with a HES record, regardless of whether death occurred inside or outside hospital.

Study cohorts

From the set of linked intervention patients, we excluded those without a HES inpatient admission between 2001 and the date of enrolment. This was done because HES inpatient data was our richest source of data, and so we could not as accurately characterise patients who had not been admitted. We also required that the inpatient admission contained a primary or secondary diagnosis of chronic obstructive pulmonary disease (ICD-10 codes J43-J44).

A matched control group was selected retrospectively from a group of potential controls that was sourced from the same regions as the intervention patients. Like intervention patients, potential controls had a previous inpatient admission with a diagnosis of COPD. We excluded as controls all patients who had ever been registered at a general practice that offered *Healthy Outlook*. Thus, potential controls met the eligibility criteria for *Healthy Outlook* but were not registered at a general practice at which the service was offered.

For intervention patients, baseline variables were calculated at the date of enrolment into *Healthy Outlook* (see Table 1). These baseline variables have been shown to be predictive of future emergency hospital admissions.[22] Potential control patients were randomly assigned eight index dates, corresponding to months during the study period (November 2007 to September 2011); baseline variables were then created at each of these index dates. However, we removed records where the index date was after the date of death or before the date of the first diagnosis of COPD in inpatient data.

A matching algorithm was used to select, from the potential control records (up to eight per potential control patient), a subset that was similar to the intervention patients with respect to the baseline variables. To do this, we used genetic matching,[23] which is a computer-intensive search algorithm that has

been shown to produce more closely balanced groups than more traditional methods.[24] The matching was conducted separately by region (ensuring that controls were resident in the same broad geographical location as intervention patients) and on a 1:1 basis.

After matching, we assessed the similarity of intervention and matched control groups at baseline using the standardised difference (defined as the difference in sample means as a percentage of pooled standard deviation).[25] A threshold of 10% has been used to denote a meaningful imbalance.[26] As the standardised difference only measures a difference in means, we also assessed the ratio of variances in the two groups.[27]

Statistical methods

We estimated the effects of *Healthy Outlook* on hospital use and mortality over the year following enrolment. Analysis was done at the patient-level, regardless of subsequent death. Counts of hospital activity were compared using Poisson regression, adjusting for the baseline variables, with coefficients presented as rate ratios. Analogously, mortality rates were compared using logistic regression, with coefficients presented as odds ratios. Models contained random effects for the matched pair to account for the expected correlation structure of the data.[25]

Additional analyses

Two further analyses are presented in the supplementary material. First, we examined the meteorological data that were used to determine when the telephonic alerts were made, and confirmed that these were predictive of hospital admissions and mortality. Second, we examined short-term changes in hospital utilisation over the 7, 14, and 28 days following the telephone alerts (rather than over the year following enrolment).

Results

Figure 1 shows the flow of patients into the study. In total, 3,946 patient spells contained an NHS number and 3,581 were mapped to the hospital data, giving a linkage rate of 90.7%. A prior diagnosis of COPD was recorded on inpatient data for 40.1% of linked patients, leaving a sample of 1,425 patients. Our sample was registered at 102 general practices, with a median number of study patients per practice of 10 (range 1-66).

HES provided 697,095 potential controls from 8,736 general practices. Compared with the potential controls, intervention patients were younger, less likely to have co-morbidities, and had experienced fewer emergency hospital admissions (see Table 1 and Figure 1).

All except 12 intervention patients were matched to a control (n=1,413). Matched controls and intervention patients were similar, with all standardised differences less than the 10% threshold (Table 1).

Differences following enrolment

In the year following enrolment, intervention patients experienced more COPD admissions per head than matched controls (0.20 *vs.* 0.16). This difference reached statistical significance with an adjusted rate ratio of 1.26 (95% CI, 1.05 to 1.52). The intervention group also experienced more outpatient attendances than matched controls (adjusted rate ratio 1.08, 95% CI, 1.03 to 1.12). However, only 5.6% of intervention patients died during the year following enrolment, compared with 8.5% of matched controls (adjusted odds ratio 0.61, 95% CI, 0.45 to 0.84). See Table 2.

Discussion

Main findings of this study

There was no evidence of lower admission rates amongst the intervention than matched control patients. In fact, during the year following enrolment into *Healthy Outlook*, intervention patients experienced more COPD admissions per head than matched controls, along with more outpatient attendances. When we analysed utilisation in the days immediately following the alerts (see on-line only material), we found similar trends, though generally these did not reach statistical significance.

Mortality rates were significantly lower among intervention patients than matched controls during the year following enrolment. It is possible that the intervention reduced rates of COPD exacerbation and thereby prevented deaths; however, this seems unlikely given the trends towards increased admissions. Alternatively, the intervention might have prompted patients or healthcare professionals to respond differently to risks to health. For example, the intervention might have increased awareness of threats to health, thereby producing more inpatient and outpatient care but reducing deaths. The positive finding for mortality needs to be understood in relation to the limitations of the study as detailed below.

What is already known on this subject?

To our knowledge, *Healthy Outlook* is the only telephonic intervention developed to prevent hospital admissions for COPD patients on the basis of routine meteorological data. Previous evaluations of *Healthy Outlook* have not satisfactorily addressed the potential impacts of the service on hospital admissions. For example, a previous observational study found that *Healthy Outlook* patients showed reductions in admissions over time,[18] but, in the absence of a control group, it was not possible to say whether these reductions might have occurred anyway.[34] Another study relied on analysis at the general practice level.[19] This failed to find a statistically significant impact on admissions, but unobserved impacts might have existed at the individual level.

What this study adds

From this study, it appears that *Healthy Outlook* may have increased COPD admissions and outpatient attendances. One possible explanation is that the additional information provided about risks to health

prompted patients or their healthcare professionals to seek more care in hospital settings. Health benefits may have resulted, but the intended reductions in utilisation and concomitant cost seem not to have occurred. Although the observed mortality difference is promising, this is the first evaluation of *Healthy Outlook* to identify such an effect, and we consider that further studies are needed to confirm it.

The meteorological data used by *Healthy Outlook* successfully identified times of increased risk of COPD admissions and death (see on-line only material). Therefore, the algorithms used by *Healthy Outlook* may still be valuable, even though the intervention itself may require refinement. For example, as some *Healthy Outlook* patients reported that they were already sufficiently aware of prevailing weather conditions,[16] other activities might be needed alongside the telephone calls to reduce utilisation. These activities might include more personalised support aimed at engaging patients in their care,[35] or initiatives to improve housing conditions.[36] The absence of reductions in admissions might also be due to insufficient integration of *Healthy Outlook* with other services, including pharmacies and specialist care.

Limitations of this study

When interpreting the results from this study, it is important to be mindful that, in the absence of randomisation, there may have been systematic differences between intervention and matched control groups.[28] The study was designed to reduce this risk. For example, the focus on patients with a previous COPD admission standardised inclusion criteria across intervention and control groups, while a matching algorithm balanced observed baseline characteristics.

Despite these efforts, it is nonetheless possible that unobserved differences existed between groups. For example, there may have been differences in people's abilities to manage their own health and healthcare, factors that are not currently recorded in administrative data.[29] Intervention patients might have already been more likely than matched controls to seek care when concerned about their health, even before enrolment into *Healthy Outlook*. Also, although the intervention and matched control groups were similar in terms of socioeconomic deprivation score, these scores were defined at area level. It may be that the intervention patients tended to live in more affluent households within their area, and so tended to be better prepared to respond to cold conditions. Other unobserved variables include smoking status and disease severity.

While confounding will always be a threat in observational studies, we were able to control for an established set of prognostic variables. The Patients at Risk of Rehospitalisation model, on which our variable selection was based, has good predictive ability.[30] Many of the unobserved variables (including attitudes towards using emergency care and disease severity) will be correlated with variables that were controlled for, such as prior hospital utilisation. Furthermore, based on the variables in Table 1, *Healthy Outlook* patients were generally less severely ill at the point of enrolment than the general admitted population with COPD. Therefore, if there were unobserved differences between the groups, these might also point towards intervention patients being less severely ill than matched controls. Despite this

possibility, we still found more COPD admissions and more outpatient attendances among intervention patients than matched controls. Overall, it seems unlikely that we missed reductions in hospital utilisation.

Despite their limitations, observational studies are useful to examine interventions that are already embedded into routine health care, which often cannot be examined in RCTs. The effect of an intervention may well be different in routine settings than in an RCT, due to differences in the intervention design, implementation, selection of patients, or context.[31] Our study, which included a large number of general practices (n=102) across seven English regions, should have high generalisability. Although our findings only apply to COPD patients with a previous hospital admission, these patients are at highest individual risk of experiencing admissions in future.[32]

In this study, we focussed on patients with an NHS number (around 10% of all enrolled patients); limited information was available on patients not linked to HES, so we could not check empirically for the representativeness of the sample. As the decision to enter NHS numbers was made by general practice staff, it is reasonable to suppose that our sample was more complete in some general practices than others.

Our target number of patients was 2,019, but data for only 1,413 patients were available. However, the difference in emergency admissions observed (-5%) was notably smaller than the amount that was considered meaningful (20%) and the 95% confidence interval (-14% to +4%) excluded a meaningful effect (Table 2).

Administrative data enable retrospective analysis on large samples, without some of the problems of self-reported data such as non-response and recall bias.[33] They also meant that the hospital utilisation of patients could be tracked even if the patients moved between areas of England. However, the quality of the data was not directly under our control, and there was limited insight about the appropriateness of the care provided. This study examined impacts on hospital use and mortality, but *Healthy Outlook* may have affected numbers of emergency department visits, COPD exacerbations, patient experience, quality of life, or use of primary care. These were beyond the scope of this study, but could be examined in future.

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Data linkage was conducted by the Health & Social Care Information Centre, which acted as a trusted third party so that the research team did not receive any identifiable data. Written confirmation was obtained from the National Information Governance Board that no application was necessary under Section 251 of the NHS Act (2006) to conduct the data linkage.

Contributors: AS, MB and NM designed the study. AS oversaw the data linkage, conducted the analyses and prepared the first draft of the manuscript. All authors contributed towards the preparation of the manuscript and approved the final version. AS is guarantor.

Ethics and transparency: Ethical approval was not needed for this study as it was a retrospective analysis of pseudonymised routine data. AS affirms that the manuscript is an honest, accurate, and transparent account of the study being reported; that no important aspects of the study have been omitted; and that any discrepancies from the study as planned have been explained.

Data sharing: No additional data available.

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Table 1: Baseline Characteristics (data are percentage of group unless otherwise stated)
Note: S.D. = standard deviation

	Intervention patients (n=1,425)	Potential controls (n=697,095) (a)	Matched controls (n=1,413)	Before matching standardised difference (variance ratio)	After matching standardised difference (variance ratio) (b)
Mean age in years (SD)	71.2 (9.9)	72.6 (12.5)	71.6 (10.1)	-12.9 (0.62)	-4.0 (0.95)
Female	44.8	48.3	45.0	-7.0	-0.4
Mean socioeconomic score (SD) (c)	25.0 (13.1)	24.5 (12.6)	25.0 (13.0)	4.0 (1.09)	-0.4 (1.01)
Mean number of chronic conditions (SD) (d)	2.0 (1.5)	2.3 (1.7)	1.9 (1.5)	-16.2 (0.87)	4.3 (1.01)
Mean index date (SD) (e)	18199.4 (285.3)	18193.8 (357.3)	18200.3 (288.6)	1.8 (0.64)	-0.3 (0.99)
Health conditions recorded inpatient data over 3 years					
COPD	79.0	80.0	76.9	-2.4	5.0
Hypertension	39.4	43.8	38.8	-8.9	1.2
Ischaemic heart disease	21.3	24.5	20.7	-7.5	1.6
Asthma	18.2	20.5	16.6	-5.7	3.9
Angina	14.7	16.5	14.2	-4.9	1.2
Injury	14.3	21.9	14.5	-19.9	-0.2
Diabetes	11.7	15.4	10.9	-10.7	2.2
Atrial fibrillation	11.2	16.5	10.7	-15.4	1.6
Respiratory infection	9.8	13.7	9.2	-12.1	1.9
Peripheral vascular disease	8.8	10.4	7.5	-5.4	4.9
Cancer	8.3	13.0	7.5	-15.2	3.1
Mental health	8.1	14.2	8.0	-19.7	0.5
Congestive heart failure	7.6	12.6	7.7	-16.7	-0.3
Anaemia	6.6	9.8	6.4	-11.7	0.9
Falls	6.5	11.0	7.1	-15.9	-2.2
Cerebrovascular disease	5.8	7.7	5.0	-7.6	3.7
Iatrogenic	4.1	6.4	4.2	-10.2	-0.4
Renal failure	3.2	6.0	3.3	-13.1	-0.4
Drug abuse	0.3	0.5	0.2	-3.7	1.4
Mean numbers of secondary care contacts per head, 1-360 days before enrolment (SD) (f)					
Emergency admissions	0.58 (1.10)	0.93 (1.53)	0.56 (1.06)	-26.5 (0.52)	2.1 (1.08)
COPD admissions	0.17 (0.59)	0.19 (0.64)	0.15 (0.51)	-2.8 (0.87)	3.4 (1.34)
Elective admissions	0.56 (1.40)	0.68 (1.75)	0.47 (1.14)	-7.3 (0.64)	7.3 (1.50)
Outpatient attendances	4.19 (4.81)	4.29 (5.84)	3.79 (4.55)	-1.9 (0.68)	8.2 (1.11)
Mean numbers of secondary care contacts per head, 361-730 days before enrolment (SD)					
Emergency admissions	0.53 (1.00)	0.60 (1.25)	0.49 (0.99)	-6.1 (0.63)	4.6 (1.02)
COPD admissions	0.17 (0.53)	0.12 (0.50)	0.14 (0.51)	9.3 (1.16)	4.2 (1.11)
Elective admissions	0.45 (1.00)	0.50 (1.49)	0.39 (0.89)	-4.0 (0.45)	6.1 (1.27)
Outpatient attendances	3.83 (4.56)	3.54 (5.08)	3.40 (4.26)	6.1 (0.81)	9.7 (1.15)

(a) For the purposes of this table, one index date was selected randomly for the potential control patients
(b) Compared to the 1,413 intervention patients matched to controls.
(c) Based on index of multiple deprivation score (2004) attributed to general practice.[39] n=1,327 (intervention group and matched controls); 692,520 (potential controls).
(d) Based on inpatient diagnoses and including: angina; asthma; cerebrovascular disease; congestive heart failure; COPD; diabetes; hypertension; ischaemic heart disease; and renal failure.
(e) Expressed as number of days since an arbitrary index date (1 January 1960).
(f) Inpatient activity was restricted to ordinary admissions, and excluded transfers, regular ward attendances, and maternity events. Admissions were classified by defined admission methods into emergency (unplanned) and elective (planned) activity. Outpatient activity was restricted to appointments that were attended.

Table 2: Estimated effect of *Healthy Outlook* over one year before and after enrolment (n=1,413 in each group)

	Year before enrolment: Mean (SD)			Year after enrolment: Mean (SD)			Rate ratio (95% confidence interval)
	Intervention patients	Matched controls	Difference	Intervention patients	Matched controls	Difference	Adjusted rate ratio following enrolment
Emergency admissions per head	0.58 (1.10)	0.56 (1.06)	0.02	0.59 (1.22)	0.61 (1.46)	-0.02	0.95 (0.86, 1.04) p=0.271
COPD admissions per head	0.17 (0.59)	0.15 (0.51)	0.02	0.20 (0.66)	0.16 (0.56)	0.05	1.26 (1.05, 1.52) p=0.012
Elective admissions per head	0.56 (1.40)	0.47 (1.14)	0.09	0.47 (1.51)	0.41 (1.06)	0.06	0.97 (0.87, 1.09) p=0.656
Outpatient attendances per head	4.18 (4.80)	3.79 (4.55)	0.39	4.13 (5.03)	3.56 (4.74)	0.57	1.08 (1.03, 1.12) p=0.003

Figure legends

Figure 1: Flow diagram showing recruitment into the study

Figure 2: Trends in hospital activity before and after enrolment

Figure 1: Flow diagram showing recruitment into the study

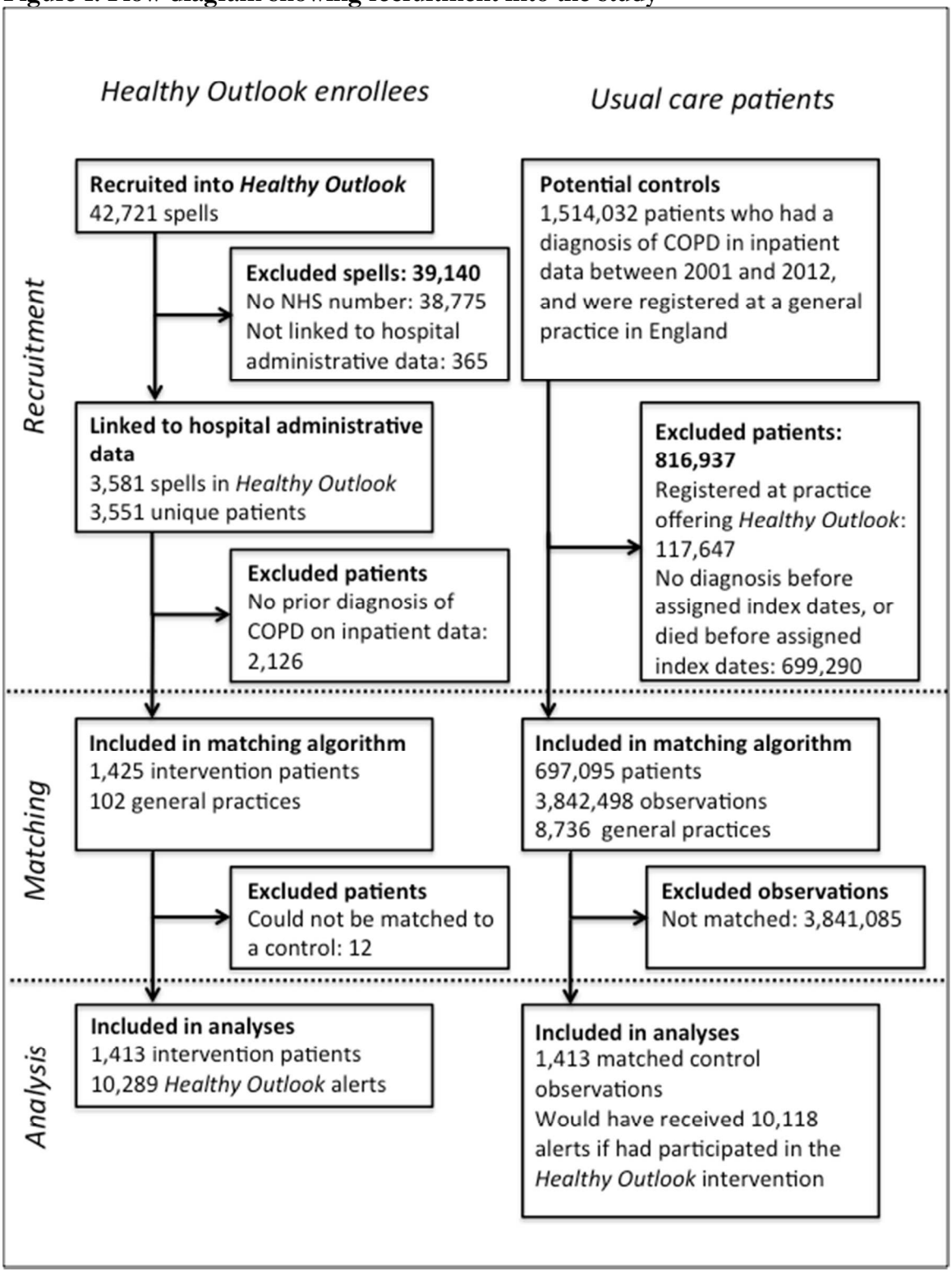
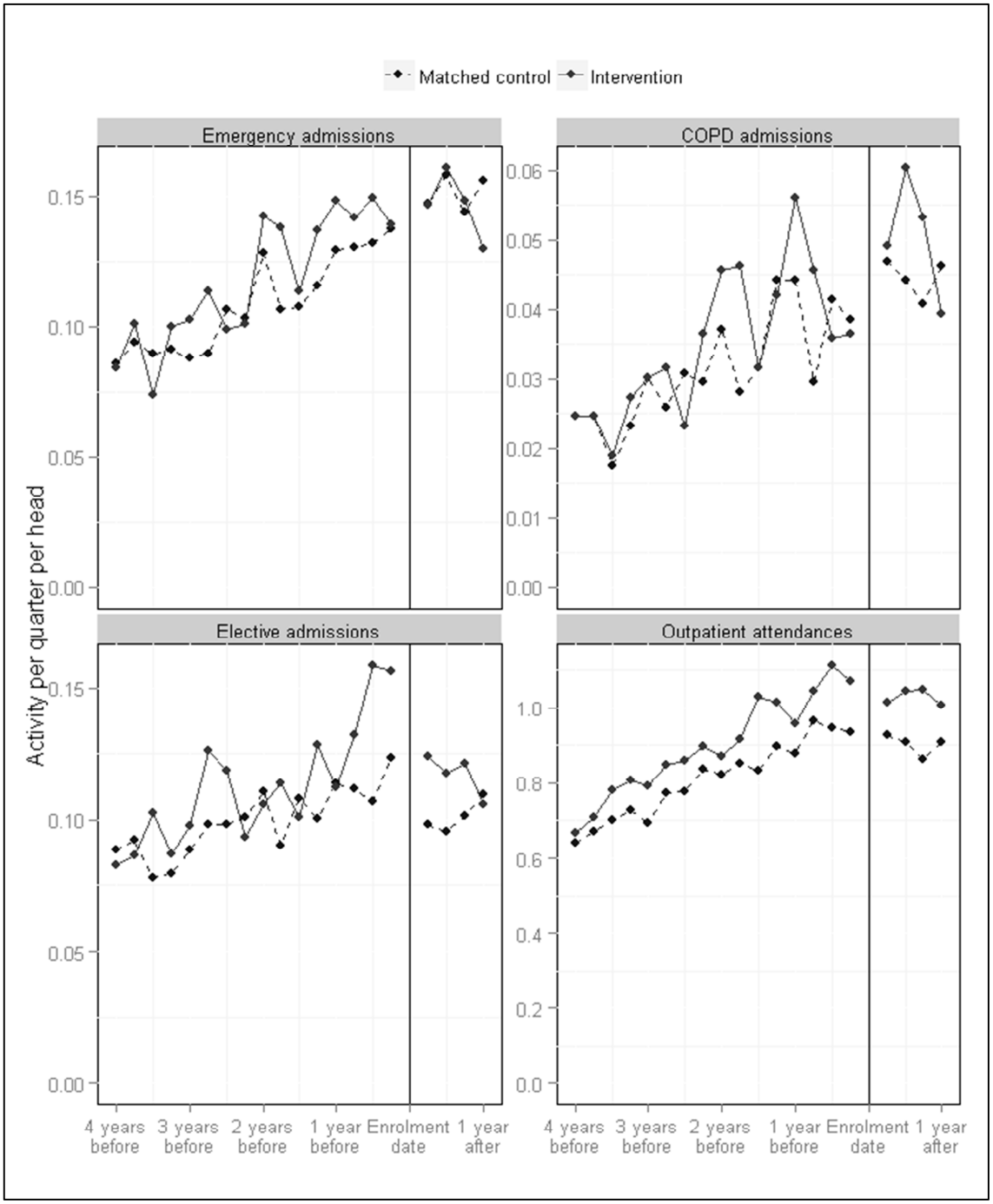


Figure 2: Trends in hospital activity before and after enrolment



Solid line shows activity for the *Healthy Outlook* patients; dashed line shows matched controls.

Supplementary, online-only material for "Effect of a telephonic alert system (*Healthy Outlook*) for patients with chronic obstructive pulmonary disease: cohort study with matched controls"

This supplementary material includes:

1. Analysis to test whether the meteorological data used by *Healthy Outlook* to decide whether to issue telephonic alerts were predictive of future hospital utilisation;
2. Analysis of the length of stay in *Healthy Outlook*; and
3. Analysis of hospital utilisation following the telephonic alerts.

1. 1. Analysis to test whether the meteorological data used by Healthy Outlook to decide whether to issue telephonic alerts were predictive of future hospital utilisation

Methods

When deciding when to issue telephonic alerts, a team at the Met Office gave consideration to indicators relating to calendar week; temperature; influenza virus levels (based on communicable disease reports from community settings);[1] and a forecaster evaluation of the overall synoptic situation and air quality. Further, in the decision-making process, particular weight was given to a 'composite algorithm score', which is proprietary to the Met Office and was calculated based on recent and forecast humidity and daily maximum temperature data. Decisions about whether to make alerts were made on a regional basis, with the regions corresponding to the ten former Strategic Health Authorities (SHAs) in England.

Our analysis of meteorological data focussed on the composite algorithm score, which we calculated using a rule set provided in confidence by the Met Office. The scores we calculated were based only on historical temperature and humidity data, and did not include the element that related to forecast values, which were not available.

We calculated scores for the first day of every month during the study period, and then tested the association between these scores and the proportion of patients with admissions in the following 7, 14 and 28 days. This was done separately for the intervention and matched control groups, attributing composite algorithm scores to individuals based on the small geographic area[1] of their residence. Inputs into a logistic regression model were scaled to produce odds ratios associated with

[1] Lower Super Output Areas

a 0.05 change in the composite algorithm score. Such a change corresponded to a variation in humidity of around 1.5% or to a variation in temperature of around 11 degrees centigrade.

Results

Composite algorithm scores were calculated for 1,368 intervention patients (96.8%) and 1,392 matched control patients (98.5%). These groups provided a total of 42,681 and 41,746 monthly scores, respectively.

Among the matched control group, the composite algorithm score was predictive of future COPD admissions over subsequent 7, 14, and 28 day periods, with odds ratios of 1.07, 1.05, and 1.04, respectively (Figure A1). The composite algorithm score was also predictive of future mortality among this group (odds ratios: 1.07, 1.07, and 1.06, respectively) and was, marginally, negatively associated with outpatient visits over 14 and 28 days (e.g. odds ratio over 14 days, 0.99, 95% CI, 0.98 to 1.00). There was no evidence that the composite algorithm score was predictive of future emergency hospital admissions. The scores were generally less predictive among intervention patients than controls, though they more predictive of emergency admissions among intervention patients.

2. Analysis of the length of patient stay in *Healthy Outlook*

We estimated length of patient stay in the *Healthy Outlook* programme, using data from the service's operational system and Kaplan-Meier curves.[2] According to these data, patients typically remained in *Healthy Outlook* for several years. For example, 75% of stays lasted at least 2.8 years (1,017 days, 95% confidence interval, 907 to 1,216 days).

3. Analysis of hospital utilisation following telephone alerts

Methods

The analysis in the accompanying paper assessed the overall effect of *Healthy Outlook* over the year following enrolment. This might include increased awareness arising as part of the recruitment and enrolment process (for example, as a result of the information pack), as well as the specific impact of the alerts. In line with previous work in this area,[3] we also compared hospital utilisation and mortality rates between intervention and matched controls over shorter periods following the telephonic alerts. This was done in order to assess the specific effect of the telephone calls during periods of anticipated poor outdoor conditions.

Opinions differ about the lag time between the onset of a cold spell and respiratory mortality,[4] while some categories of hospital visit might take longer to arrange than others. Therefore, we

investigated periods of several durations (*i.e.*, 7, 14 and 28 days following the telephone calls). As multiple admissions are very rare in such short time periods, we tested for differences in the proportion of patients with one or more admission, using logistic regression and adjusting for baseline variables.

Results

Over three successive winters (2009/10, 2010/11 and 2011/12), *Healthy Outlook* alerts were made in England on a total of 12 dates. During this period, there were 10,289 instances of an alert being made to an intervention patient. Had matched control patients received alerts, they would have received a similar number (n=10,118).

Rates of emergency admission were not significantly different between groups in the 7, 14 and 28 days following alerts (Table A1). Odds ratios for outpatient attendance increased with the follow up time (odds ratios after 7, 14 and 28 days: 1.03, 1.07 and 1.12) and reached statistical significance in the 28-day analysis. There were no differences in elective admissions, but mortality rates were lower in the intervention than matched control group (odds ratio over 28 days 0.63, 95% CI, 0.46 to 0.87).

References

1. Health Protection Agency: **Health Protection Agency and Nottingham University Division of Primary Care Collaborative National Surveillance System weekly bulletins**. 2013.

2. Kaplan E, Meier P: **Nonparametric estimation from incomplete observations**. *J Am Stat Assoc* 1958, **53**:457–81.

3. Maheswaran R, Pearson T, Hoysal N, Campbell MJ: **Evaluation of the impact of a health forecast alert service on admissions for chronic obstructive pulmonary disease in Bradford and Airedale.** *J Public Health (Oxf)* 2010, **32**:97–102.

4. Monteiro A, Carvalho V, Góis J, Sousa C: **Use of "Cold Spell" indices to quantify excess chronic obstructive pulmonary disease (COPD) morbidity during winter (November to March 2000-2007): case study in Porto.** *Int J Biometeorol* 2012.

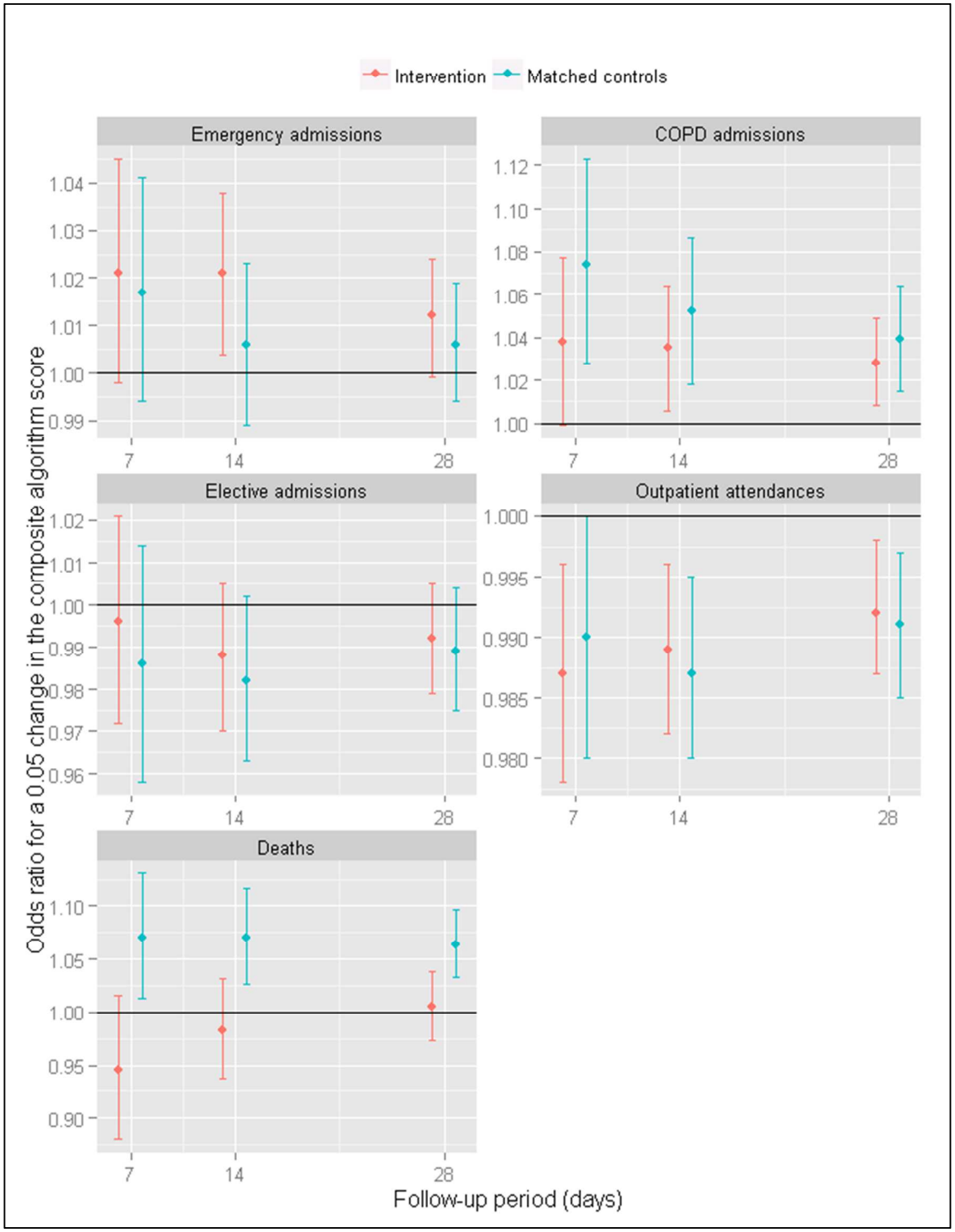
5. Billings J, Dixon J, Mijanovich T, Wennberg D: **Case finding for patients at risk of readmission to hospital: development of algorithm to identify high risk patients.** *BMJ* 2006, **333**:327.

6. Communities and Local Government: *Index of Multiple Deprivation 2004*. 2013.

Table A1: Estimated effect of *Healthy Outlook* following alerts

	Alerts for intervention patients (%) n=10,289	Notional alerts for matched controls (%) n=10,118	Difference (%)	Adjusted odds ratio		
				Point estimate	95% confidence interval	p value
Proportion with emergency admission						
7 days	1.22	1.33	-0.11	0.91	(0.71, 1.17)	0.468
14 days	2.32	2.51	-0.19	0.91	(0.75, 1.09)	0.290
28 days	4.48	4.74	-0.26	0.91	(0.80, 1.04)	0.173
Proportion with COPD admission						
7 days	0.50	0.36	0.14	1.43	(0.91, 2.23)	0.118
14 days	0.88	0.76	0.12	1.18	(0.86, 1.62)	0.293
28 days	1.80	1.55	0.25	1.15	(0.92, 1.44)	0.217
Proportion with elective admission						
7 days	0.86	0.90	-0.04	0.91	(0.67, 1.23)	0.528
14 days	1.56	1.62	-0.06	0.90	(0.72, 1.13)	0.358
28 days	2.85	3.00	-0.16	0.90	(0.76, 1.06)	0.196
Proportion with outpatient attendances						
7 days	6.58	6.06	0.52	1.03	(0.92, 1.16)	0.610
14 days	12.24	10.92	1.32	1.07	(0.98, 1.17)	0.114
28 days	21.82	19.13	2.69	1.12	(1.04, 1.20)	0.002
Proportion died						
7 days	0.16	0.24	-0.08	0.64	(0.33, 1.23)	0.183
14 days	0.31	0.51	-0.20	0.61	(0.39, 0.96)	0.032
28 days	0.65	1.02	-0.37	0.63	(0.46, 0.87)	0.004

Figure A1: Association between composite algorithm score and subsequent hospital use and mortality



STROBE Statement—Checklist of items that should be included in reports of ***cohort studies***

	Item No	Recommendation
Title and abstract	1	(*a*) Indicate the study's design with a commonly used term in the title or the abstract. 'cohort study with matched controls'
		(*b*) Provide in the abstract an informative and balanced summary of what was done and what was found. The abstract states that intervention patients with a history of hospital admissions were linked to hospital administrative data and compared with matched controls. We give adjusted rate and odds ratios.
Introduction		
Background/rationale	2	Explain the scientific background and rationale for the investigation being reported. Exacerbations for COPD have been linked to circulating influenza levels and cold weather. Therefore, it has been hypothesised that, if one can predict the onset of cold weather or detect rises in influenza levels, then the correct anticipatory care intervention could reduce exacerbations and hospital admissions. *Healthy Outlook* is one of the few examples of this type of intervention, but prior studies have either been very small or of limited quality.
Objectives	3	State specific objectives, including any prespecified hypotheses. We aimed to 'test the effect of *Healthy Outlook* on hospital admissions and deaths against a matched control group'. We did not specify the direction of the effect as some previous efforts to improve community services have led to increased admissions.
Methods		
Study design	4	Present key elements of study design early in the paper. The introduction refers to a 'matched control group' and an evaluation of a service 'that is already embedded into routine care' – two important elements of the study design. We preferred to begin the methods section with a detailed description of the intervention.
Setting	5	Describe the setting, locations, and relevant dates, including periods of recruitment, exposure, follow-up, and data collection. These are described, mainly in the methods section: Setting: primary care general practices. Locations: seven regions of England (described in first section of results). Periods of recruitment: November 2007 to September 2011. Exposure: 75% of intervention patients enrolled for 2.8 years or more (see results). Follow-up: 1 year, 7 days, 14 days or 28 days, depending on the analysis. Data collection: Retrospective analysis of routine data.
Participants	6	(*a*) Give the eligibility criteria, and the sources and methods of selection of participants. Describe methods of follow-up These are described in the methods: Eligibility criteria: COPD, linked to hospital administrative data, prior hospital admission for COPD between 2001 and date of enrolment. Sources of methods of selection of patients: Intervention patients were referred by general practices, generally on an 'opt in' basis. Control patients were selected by the evaluation team based on a retrospective analysis of routine hospital administrative data. Follow-up: based on hospital administrative data.
		(*b*) For matched studies, give matching criteria and number of exposed and unexposed. Matching was done using the genetic matching algorithm, with post-matching criteria relating to standardised differences of no more than 10%. Number of exposed and unexposed is in Figure 1.

Variables	7	Clearly define all outcomes, exposures, predictors, potential confounders, and effect modifiers. Give diagnostic criteria, if applicable. See the sections on endpoints and baseline variables. No effect modifiers were investigated.
Data sources/ measurement	8*	For each variable of interest, give sources of data and details of methods of assessment (measurement). Describe comparability of assessment methods if there is more than one group. All analysis used routine hospital administrative data. In the discussion, we refer to the possibility for the 'meaning' of the diagnosis flags to vary nationally, based on the intensity of utilisation (observation intensity bias). We also describe how we attempted to control for this by selecting groups with similar utilisation rates.
Bias	9	Describe any efforts to address potential sources of bias. This is summarised in the discussion; see the 'strengths and weaknesses section' beginning at 'This study used a variety of approaches at the design stage to reduce the risk of confounding.' In short our approaches were: 1. Consistent eligibility criteria between intervention and matched control groups; 2. Selecting controls 'locally' from within the same areas, to help standardise measurement and area-level variables (such as weather); and 3. Matching algorithm.
Study size	10	Explain how the study size was arrived at: As this was a retrospective study, we were limited by the size of the cohort that had been recruited and the available of patient identifiers to link records to the Hospital Episode Statistics. We performed a sample size calculation at the outset of the study to check that we were likely to have sufficient data to draw meaningful conclusions (see the section on 'study endpoints'). Although we ultimately had linked data for fewer patients than we expected, it is unlikely that this influenced our findings (see the sections in the discussion about the target sample size and data linkage, both under the 'strengths and weaknesses' heading).
Quantitative variables	11	Explain how quantitative variables were handled in the analyses. If applicable, describe which groupings were chosen and why. See the section on baseline variables. No groupings were used in the analysis as we checked balance using the full distribution (using the standardised difference plus variance ratio).
Statistical methods	12	(*a*) Describe all statistical methods, including those used to control for confounding. The matching algorithm and regressions are described in the methods section (see 'matching algorithm' and 'statistical methods').
		(*b*) Describe any methods used to examine subgroups and interactions. We did not examine subgroups.
		(*c*) Explain how missing data were addressed
		(*d*) If applicable, explain how loss to follow-up was addressed.
		(*e*) Describe any sensitivity analyses. These have not been presented in the article as we considered them uninformative.
Results		
Participants	13*	(a) Report numbers of individuals at each stage of study—eg numbers potentially eligible, examined for eligibility, confirmed eligible, included in the study, completing follow-up, and analysed. See Figure 1.
		(b) Give reasons for non-participation at each stage See Figure 1.
		(c) Consider use of a flow diagram See Figure 1.
Descriptive data	14*	(a) Give characteristics of study participants (eg demographic, clinical, social) and information on exposures and potential confounders. See Table 1.

		(b) Indicate number of participants with missing data for each variable of interest. Missing data are addressed throughout the report. As the baseline variables were defined based on the presence of events and flags reported in the administrative data, there were no 'missing' data as such, although it is possible that the administrative data did not tell us everything about the patients – see the discussions about unobserved confounding and observation intensity bias. All patients were followed up for the stated period of time (1 year, 7 days, 14 days or 28 days, depending on the analysis) regardless of withdrawal or death. We give data on withdrawal from the service in the supplementary material, and mortality rates are in the main paper. As the administrative data relate to the whole of England, hospital admissions and deaths were tracked even if patients moved areas, as long as they remained within England – a point mentioned in the 'strengths' and 'weaknesses' section of the discussion.
		(c) Summarise follow-up time (eg, average and total amount). This was 1 year, 7 days, 14 days or 28 days, depending on the analysis.
Outcome data	15*	Report numbers of outcome events or summary measures over time. See Table 2 and supplementary material.
Main results	16	(*a*) Give unadjusted estimates and, if applicable, confounder-adjusted estimates and their precision (eg, 95% confidence interval). Make clear which confounders were adjusted for and why they were included. We give only adjusted estimates, as the role of the regression adjustment, on top of the matching, is marginal. Precision is given using 95% confidence intervals. See Tables 2 and 3. We adjusted for baseline variables, which are described in the section called 'definition of baseline variables' and were selected because they are known to be predictive of future emergency hospital admissions.
		(*b*) Report category boundaries when continuous variables were categorized. There were no categories.
		(*c*) If relevant, consider translating estimates of relative risk into absolute risk for a meaningful time period. The regressions give estimates of relative risk, which are not easy to convert to absolute risk in a meaningful way. We have instead shown crude absolute rates in both intervention and control groups.
Other analyses	17	Report other analyses done—eg analyses of subgroups and interactions, and sensitivity analyses. We performed some sensitivity analyses: i) using difference-in-difference estimators for hospital utilisation, ii) based on examining differences between the groups in hospital utilisation and mortality during winter, adjusting for differences between the groups in the rest of the year. We have not reported these as the article is already quite long.
Discussion		
Key results	18	Summarise key results with reference to study objectives. See 'statement of findings'.
Limitations	19	Discuss limitations of the study, taking into account sources of potential bias or imprecision. Discuss both direction and magnitude of any potential bias. See the 'strengths and weaknesses' section. The main threat to validity is unobserved confounding which is difficult to estimate. The implication in the discussion of the findings is that the intervention group may have been at lower risk than the matched control group in ways that could not be observed. However, this would make our conclusions about emergency admissions stronger.
Interpretation	20	Give a cautious overall interpretation of results considering objectives, limitations, multiplicity of analyses, results from similar studies, and other relevant evidence. See the discussion. Our findings are broadly in line with previous studies of this intervention, though the mortality difference has not previously been reported. We

		consider than more studies are needed to understand the mortality difference seen.
Generalisability	21	Discuss the generalisability (external validity) of the study results. See the discussion. The main threat to external validity is sample selection, which we suggest is unlikely to have influenced the direction of the effects. The study design has the advantage of assessing impacts in routine practice rather than in efficacy in a trial setting.
Other information		
Funding	22	Give the source of funding and the role of the funders for the present study and, if applicable, for the original study on which the present article is based. See the statement of funding at the back of the manuscript. The current study is the original study.

*Give information separately for exposed and unexposed groups.

Note: An Explanation and Elaboration article discusses each checklist item and gives methodological background and published examples of transparent reporting. The STROBE checklist is best used in conjunction with this article (freely available on the Web sites of PLoS Medicine at http://www.plosmedicine.org/, Annals of Internal Medicine at http://www.annals.org/, and Epidemiology at http://www.epidem.com/). Information on the STROBE Initiative is available at http://www.strobe-statement.org.