Journal of Health Economics 68 (2019) 102242

Contents lists available at ScienceDirect

Journal of Health Economics

journal homepage: www.elsevier.com/locate/econbase

Convenient primary care and emergency hospital utilisation☆

Edward W. Pinchbeck[a,b]

[a] Department of Economics, School of Arts and Social Sciences, City, University of London, Northampton Square, London EC1V 0HB, United Kingdom
[b] Centre for Economic Performance, London School of Economics, Houghton Street, London WC2A 2AE, United Kingdom

ARTICLE INFO

Article history:
Received 11 December 2018
Received in revised form 16 July 2019
Accepted 12 September 2019

JEL classification:
I11
I12
I18
D12

Keywords:
Primary care
Emergency care
Access
Utilisation decisions

ABSTRACT

Participation and utilisation decisions lie at the heart of many public policy questions. I contribute new evidence by using hospital records to examine how access to primary care services affects utilisation of hospital Emergency Departments in England. Using a natural experiment in the roll out of services, I first show that access to primary care reduces Emergency Department visits. Additional strategies then allow me to separate descriptively four aspects of primary care access: proximity, opening hours, need to make an appointment, and eligibility. Convenience-oriented services divert three times as many patients from emergency visits, largely because patients can attend without appointments.

For some services discrepancies between social and individual benefits warrant government action on efficiency grounds. In other cases, society may intercede to ensure individuals can access some hitherto unattainable level of service. Interventions to improve the accessibility of services conceivably come in many guises, for instance improving affordability or widening eligibility; providing more, closer, or better services; shorter waiting times; or more convenient opening hours (e.g. Millman et al., 1993; Hiscock et al., 2008). The ways in which interventions are designed and structured may have consequences for utilisation, service costs, and the attainment of policy objectives.

This paper investigates how dimensions of access to primary care affect the demand for unplanned use of hospital Emergency Departments (EDs). I draw on *Equitable Access to Primary Medical Care* (*EAPMC*), a policy reform in the English National Health Service (NHS) designed to make primary care more convenient across the country, and to address geographical imbalances in access. Under *EAPMC*, around 250 new primary care services were deployed between 2008 and 2012. More than half were "walk-in clinics": practices with evening and weekend opening hours, and offering walk-in services with no need to register or make an appointment. The remainder, targeted to administrative districts with the lowest concentration of primary care physicians, were "extended hours practices": regular services requiring registration but open at least 5 hours per week more than conventional practices. The comprehensive nature of the English NHS, where all patients have access to free primary care, allows me to abstract from insurance issues, and to focus on physical proximity and other less well-understood, but potentially important, convenience dimensions of access.

To contrive a quasi-experimental research design from the *EAPMC* policy reform I use hospital records to capture the evolution of hospital utilisation in small neighbourhoods, then generate a measure of primary care access as a non-parametric function of distance to *EAPMC* services.[1] Restricting regression samples to places receiving new facilities under the policy, specifications estimate

☆ This paper was part completed while I was working at the Centre for Economic Performance which is funded by the ESRC under grant number ES/M010341/1. My thanks to the editor and two anonymous referees for valuable suggestions, and to Gwyn Bevan, Philippe Bracke, Peter Dolton, Steve Gibbons, Christian Hilber, Alex Jaax, Alistair McGuire, Henry Overman, Sefi Roth, and Olmo Silva as well as participants at the LSE/Spatial Economics Research Centre seminar, the Royal Economic Society conference, and staff at NHS Improvement for helpful comments, or other input to earlier versions of this work. I am grateful to Professor Peter Griffiths and Trevor Murrells for generously providing ancillary data. This paper was produced using Hospital Episode Statistics provided by NHS Digital under Data Sharing Agreement NIC-354497-V2J9P. This paper has been screened to ensure no confidential information is revealed.
E-mail address: edward.pinchbeck@city.ac.uk

[1] More concretely access intensity is computed by counts of open services in a series of distance buffers centred on the neighbourhood centroid, where distance

https://doi.org/10.1016/j.jhealeco.2019.102242

an average treatment effect on the treated (ATT) from changes in hospital outcomes when an *EAPMC* service opens or closes, with a control group composed of areas suitable for similar services but not experiencing access changes at that particular time. Using timing differences for identification is underpinned by evidence that: *(i)* service roll-out is unrelated to pre-reform primary care access measures; and *(ii)* trends in ED visits are broadly parallel across cohorts. This aligns with policy documents that indicate service deployment timetables were driven by administrative factors that are plausibly unrelated to the determinants of hospital utilisation.

This research design is leveraged to generate three sets of findings. The first documents policy-relevant estimates of the impact of walk-in clinics on neighbourhood wide ED use. Conditional on fixed neighbourhood factors, labour-market trends, and demographic changes, proximity to these convenience-oriented services results in strongly significant reductions in unplanned ED visits. Reductions in ED visits are in the order of 1.5–4%; implying that each facility reduces annual ED throughput by approximately 1000–2000 visits. The robustness of these estimates is bolstered by auxiliary analyses, the use of alternative sources of variation, as well as numerous robustness and falsification tests.

Parameter estimates imply that some 5–20% of walk-in clinic visits substituted for a trip to an ED. Despite the lower costs of primary relative to ED care, this implies a net increase in health care spending (in the region of £ 10–20 per walk in visitor), but says nothing about possible patient benefits or diversion from regular primary care services. The former, which include anxious patients being able to consult with primary care physicians promptly when faced with an uncertain need for care, may be considerable given that many services proved extremely popular. A full welfare analysis, which lies beyond the scope of the current paper, would need to account for these benefits. Regarding the latter, NHS primary care physicians are paid a capitation fee per registered patient so the analysis undertaken is a helpful guide to budgetary implications.

A second suite of results exploits the richness in my data to unearth further patterns. First, using an event study approach I trace out the time dynamics of ED diversion from time of first exposure. This is inconclusive: a test rejects equality of the post exposure event time indicators, yet there is no clear pattern nor a significant linear trend in effects. The spatial dimension in the data, however, reveals a much sharper relationship: diversion from EDs is subject to a strong, near-linear, decay with distance to a walk-in clinic. Turning next to characteristics of ED visits, subsequent findings indicate that diversion from EDs is largely driven by patients whose visit does not result in a hospital admission, and by patients that were neither referred to the ED nor conveyed there in an ambulance. This points towards a conclusion that the effects of walk-in clinics mainly arise from influencing care utilisation decisions of individuals with less urgent health problems, and with more discretion over the location of their treatment.

The third and final part of the analysis unpicks further channels though which primary care access determines ED utilisation. Here I rely on a descriptive approach that compares walk-in clinics and extended hours practices in under-doctored administrative districts which received both types of service under *EAPMC*. When estimated simultaneously, walk-ins divert three times as many patients from EDs. Although both types of service have meaningful effects on ED visits outside of standard practice hours, the greater bite of walk-in services predominantly occurs during these standard hours. To the extent that services are well matched on unobserved features, this suggests that being able to attend without registering or pre-booking strongly influences where patients seek treatment.

This paper's overarching contribution is to provide new evidence on the extent to which convenient primary care reduces visits to hospital EDs. Shifting care from EDs to primary care is likely to be socially beneficial because some 15–40% of ED visits are for health problems that could be safely treated in less costly settings outside hospitals (Mehrotra et al., 2009; Weinick et al., 2010; Lippi Bruni et al., 2016). Moreover, rapid growth in ED use in many OECD countries has resulted in well-documented congestion in EDs (Berchet, 2015).[2] The possible adverse effects of crowding has made reducing pressure at EDs an increasing priority (e.g Pines et al., 2011; Morley et al., 2018). In the English NHS, various initiatives have been adopted, or are proposed. Primary care access features prominently. For example, prior to the 2015 election both major political parties put forward access policies in expectation that reduced pressure at EDs would follow (Cowling et al., 2015). Since that time the government has introduced a 7-day primary care policy, and is currently rolling out Urgent Treatment Centres (UTCs) across the country.[3]

Despite this, the evidence available to inform policy decisions is far from clear-cut. A review by Ismail et al. (2013) cautions against using evidence from studies prior to 2011, and findings from more recent research do not always point in the same direction. One strand, using national data and GP access measures from surveys, shows that (self-referred) ED visits are strongly associated with timely GP access in the cross-section (Cowling et al., 2013), but associations are much weaker, or else nonexistent, when examining year-on-year practice level variation (Cowling et al., 2018). A second strand applies careful research designs to obtain plausibly causal estimates of the recent 7-day primary care policy, albeit in narrower settings. Dolton and Pathania (2016) estimate a 10% reduction in ED visits in 4 London practices. Whittaker et al. (2016) examine the policy's impact in 56 Manchester practices, finding that self-referred ED trips for minor problems fell by some 25%, although the estimate for all ED trips (−3.1%) is not significant.[4]

By exploiting a nation-wide natural experiment in primary care access, this paper provides internally valid, generaliseable, and policy-relevant estimates that complement this earlier work. A key distinction is that here access variation is generated by new services, whereas others rely on reconfiguration of existing practices or else recalled experience of access reported in surveys. Besides being directly relevant to any *ex post* evaluation of *EAPMC* and the rollout of UTCs in the NHS, the resulting estimates should generalise to other settings in which policy-makers are seeking to expand primary care in suitable locations. Despite different sources of variation, several findings align with previous work: for example, diversion is driven by patients with less severe health needs. Other results are novel. For example, the unique neighbourhood level approach permits the strong distance decay in ED diversion to be robustly identified for the first time, while to the best of my knowledge a comparison of ED diversion from different types of primary care service is also new to the literature.

A related but more general contribution is to adopt a multidimensional view of health care access. Previous research typically focuses on single dimensions — affordability (Selby et al., 1996); opening hours (Dolton and Pathania, 2016; Whittaker et al., 2016);

buffers vary across space based on the distribution of distances travelled to access emergency care locally.

[2] Between 1995 and 2010 visits to US EDs increased by 34% (National Center for Health Statistics, 2013), while visits to Accident & Emergency departments in the England rose by 40% (Appleby, 2013).

[3] UTCs are similar to the walk-in clinics studied here. See The NHS Long Term Plan for details.

[4] These papers concentrate wholly or in part on ED visits, which is my chief interest. Many other studies concentrating on other outcomes, notably admissions, are neglected for reasons of economy.

or proximity (Van Dort and Moos, 1976; Currie and Reagan, 2003; Buchmueller et al., 2006). This paper indicates a more comprehensive view of access is warranted.[5] *EAPMC* suggests that proximity and being able to attend without appointments are important factors in determining the extent to which primary care diverts patients from EDs. Unobserved, and possibly non-linear, costs may be driving these findings, but these results also chime with evidence from other settings that inconvenience and hassle can be powerful barriers to participation (e.g. Bertrand et al., 2006; Kahn and Luce, 2006).

1. Background

1.1. Institutional context

Patients desiring unplanned care from the NHS in England and Wales have traditionally had two main options: visit a hospital Accident and Emergency (A&E) Department, a Consultant-led 24 hour services with full resuscitation facilities catering for all kinds of emergency (equivalent to Emergency Departments, or EDs); or consult with a family physician – know locally as a General Practitioner (GP) – at a primary care practice. It is widely acknowledged that providing care in EDs is considerably more costly than in settings outside hospitals such as physicians' offices/surgeries (e.g Mehrotra et al., 2009). This is also true in the NHS context: for example, recent figures indicate that a visit to A&E costs £ 124 while a GP practice consultation costs £ 32.

Despite universal coverage and no demand-side cost sharing, patients using NHS emergency care incur time and travel expenses. In addition, EDs and primary care are subject to access frictions. In this regard EDs are arguably more convenient than conventional primary care: patients can visit any ED whenever they wish, and due to closely monitored performance targets, can normally expect to wait less than 2 hours for treatment. Conversely, access to specific primary care services requires registration, and is usually only available to patients living within a practice's catchment boundary. Access is via an appointment, an emergency appointment, or – where available – by using a primary care Out of Hours service on evenings and weekends. Although almost all individuals are registered at a primary care practice, they may have to wait a week or longer to obtain a regular appointment with a family doctor; and, although often available, same day emergency appointments can be difficult to book. Even then, appointments may not be convenient.[6] From the late-1990s, alternative ways to access unplanned care emerged in the shape of new urgent care services designed for patients with minor medical problems. These included a telephone advice service and facilities offering easy access to face-to-face advice and treatment. NHS Walk-in Centres are one type of urgent care service that were introduced in this period.[7] These facilities provide routine and emergency primary care for minor ailments and injuries with no requirement for patients to pre-book an appointment or to register (Monitor, 2014). Most are located away from hospitals although some are co-located with hospital EDs, so that on arrival patients are directed (triaged) to the appropriate service. In total approximately 230 Walk-in Centres have opened in England since 2000. Some 150 (or 65%) of this total number were commissioned following a report in 2007 that led to the creation of the Equitable Access to Primary Medical Care (*EAPMC*) policy reform.

EAPMC was set-up with the twin objectives of delivering more personalised and responsive primary care across England, and improving access in the most under-doctored areas. To meet these objectives *EAPMC* comprised two discrete initiatives. The first funded 100 new primary care practices in the 38 Primary Care Trusts (PCTs) with the lowest provision of family doctors.[8] These practices were similar to conventional primary care services already available, but had to meet certain core criteria such as having at least 6000 patients and being accredited training practices. They were also required to facilitate access opportunities through extended opening hours, with a minimum of 5 hours per week beyond Monday to Friday 8.30 am–6.30 pm, and by setting large catchment boundaries (Department of Health, 2007) (see Appendix A for the full list of criteria). I refer to these services henceforth as "extended hours practices". The second strand of *EAPMC* compelled each of the 152 PCTs to establish a "GP-led Health Centre", a new service type designed to offer more convenient access to primary care. These facilities — which I refer to throughout as "walk-in clinics" — had to offer both a regular registered primary care service with bookable appointments, as well as a walk-in service for any member of the public from 8 am-8 pm, 365 days a year. Core criteria required the centres to be located in areas maximising convenient access and opportunities to integrate with other local services (Department of Health, 2007).

Fig. 1 shows the spatial distribution of *EAPMC* walk-in clinics (LHS) and extended hours practices (RHS). The policy brought walk-in services to a wide range of locations, including some less urban areas in England while the extended hours practices were mainly located in cities, particularly those in northern England. Fig. 2 charts counts of walk-in services (LHS) and primary care practices (RHS) between 2006 to the end of 2012. During this period walk-in clinics more than doubled in number, peaking in 2010, before falling again. This variation is driven by openings and closings of *EAPMC* services. The right-hand plot shows the *EAPMC* extended hours practices temporarily reversed a secular downward trend in primary care practice numbers. The sharp rise in practices between 2009 and 2011 was driven by *EAPMC* services, but the steep fall in 2012 was not related to the *EAPMC* policy. This fall potentially poses a threat to identification and is addressed in robustness checks in Section 3.8.

Fig. 2 demonstrates that *EAPMC* came on stream in a staggered fashion between late 2008 and the end of 2011. For example, the first walk-in clinic (the Hillside Bridge Centre in Bradford) had opened by December 2008; roughly a third of all *EAPMC* walk-in clinics had opened before May 2009, more than two thirds by the end of 2009, and all but two before 2011. What drove this pattern of deployment? Guidance issued by the Department of Health highlights that local administrators were under pressure to establish the new services quickly, with an expectation that all procurements should be finished in financial year 2008/9 (Department of Health, 2007). Although some did meet this timetable, many others did not, with sources suggesting that deployment timing was mainly

[5] This wider perspective may help to resolve puzzles and anomalies in care utilisation. For example, Miller (2012) find that the 2006 Massachusetts health insurance expansion led to a 5–8% reduction in ED visits and infer substitution from hospital visits to primary care. Taubman et al. (2014) and Finkelstein et al. (2016) find a 40% increase in ED visits with no evidence of substitution to primary under later reforms in Oregon. Chen et al. (2011) note that the *availability* of primary care physicians may be behind this heterogeneous response to changes in *affordability*.

[6] For example, surveys indicate the average wait to get a GP appointment is around 13 days *Pulse* (2016). Average waiting time for GP appointment increases 30% in a year, June 10. In the July 2017 GP Patient survey, 32% of patients did not find it easy to get through to their practice by phone; 29% were not able to see or speak to someone at the time they wanted; 31% who wanted a same day appointment could not get one; 24% say that their practice is not open at times that are convenient for them; and only two thirds of patients rate their overall experience of out-of-hours NHS services as good.

[7] Others include Urgent Care Centres and Minor Injury Units, both of which usually do not provide primary care services. See Monitor (2014) for a review.

[8] Until 2013 Primary Care Trusts were legal entities responsible for purchasing and managing NHS health care for all residents living in defined geographical areas of the country. Between 2006 and 2013 England was split into 152 such areas.

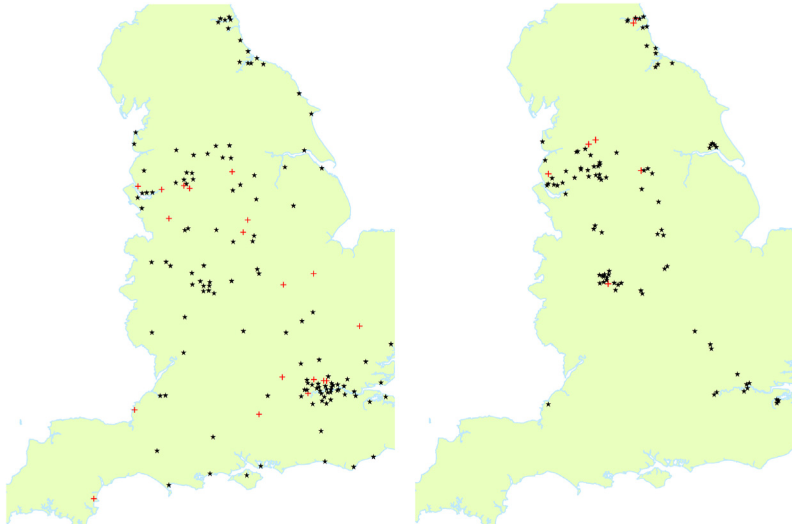

Fig. 1. *EAPMC* walk-in clinics (LHS) and extended hours practices (RHS) *Notes*: Black stars are facilities still open in September 2014; red plus symbols had closed by that date.

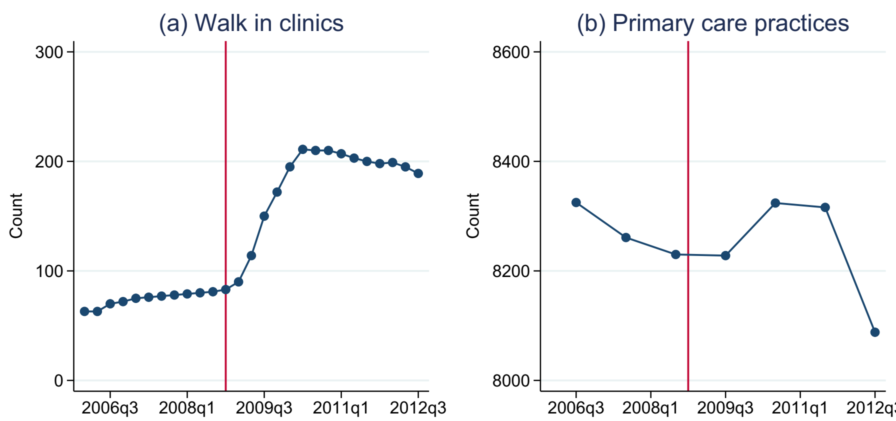

Fig. 2. Walk-in clinics and primary care practices in England. *Notes*: Walk-in clinic plot constructed by author using regulator reports. Primary care practice plot based on data contained in HSCIC (2016) and excludes walk-in clinics. Vertical line indicates the first *EAPMC* service opening (fourth quarter of 2008). Note that walk-in clinics open prior to *EAPMC* were mainly led by nurses.

driven by local administrative factors, for example readiness on the part of administrators to specify services and identify suitable premises, the speed of procurement processes, and the time needed to prepare sites.

1.2. Primary care and ED utilisation

In standard formulations, individuals seek care when the private costs of obtaining treatment p are lower than the perceived private treatment benefits $b(\sigma)$, which are increasing in illness severity σ. From a social perspective, treatment is warranted when private benefits are higher than the social costs of treatment c; such that when $p<c$ there may be some over-treatment. When EDs and primary care are substitutes, patients seek treatment in primary care when they perceive a net private benefit from primary care ($b_{PC}-p_{PC}>0$); and when primary care offers a higher perceived net benefit than an ED ($b_{PC}-p_{PC}>b_{ED}-p_{ED}$). A more general case allows for behavioural biases and is set out in Appendix B.

Primary care access interventions reduce private costs of primary care, either through lowering co-pays or, as in the English NHS, by reducing time costs and travel expenses. The perceived net benefit of primary care may also rise when convenience-enhancing interventions allow anxious patients to obtain advice and reassurance more promptly. Any such intervention can have an effect at the extensive margin by inducing marginal agents to utilise primary care instead of not seeking any kind of care. Additionally, through the second condition an intervention can divert patients from EDs to primary care. The stylised facts presented in Section 1.1 suggest the following. First, EDs can treat all patients, but those with illness severities above some point $\bar{b}(\sigma)_{PC}$ are not treatable in primary care. Second, ED care is available at any time but primary care can only be accessed during practice opening hours. Third, treatment costs in EDs are strictly higher than primary care ($c_{ED}<c_{PC}$). These stylised facts predict that following an increase in primary care access: *(i)* agents with less severe medical problems should be expected to divert to primary care; *(ii)* diversion should take place primarily during primary care opening hours; and *(iii)* diversion of primary care treatable patients from EDs to primary care represents a social gain.

Later analysis uses micro-data to estimate the extent to which *EAPMC* services divert patients from EDs. This is warranted because the aggregate utilisation data depicted in Fig. 3 is inconclusive. In the period when *EAPMC* services were being deployed (2008/9–2010/11), visits to walk in clinics and other urgent care services for minor problems (denoted “Type 3 units”) rose steadily while ED visits (denoted “Type 1 Departments”) remained flat. These trends could be consistent with effects purely at the extensive margin i.e. walk in clinics meeting previously unsatisfied

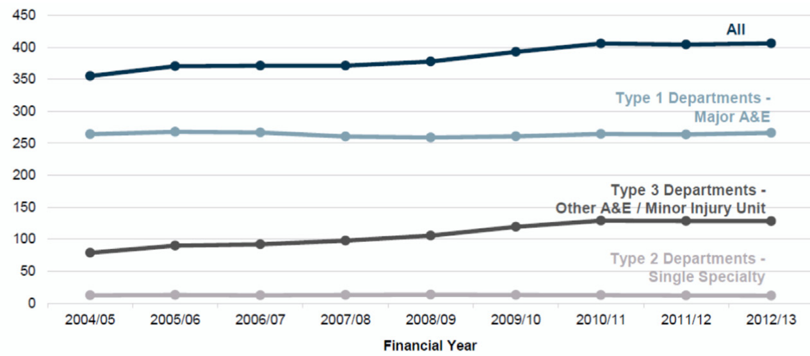

Fig. 3. Attendances per thousand population by unit type, 2004/5 to 2012/13. *Source*: Health and Social Care Information Centre.

demand.[9] However, the same outcome can also arise when walk-ins substitute for ED care. In the limit, aggregate demand for emergency care is perfectly inelastic. In this case, and all else equal, every clinic visit is offset by one less visit to an ED. Under such conditions, the aggregate trends in Fig. 3 could reflect unrelated shifts in emergency care demand, for example from, say, an aging population or increased patient expectations.

2. Data and empirical approach

2.1. Data

Subsequent analysis rests on two separate quarterly panel data sets for 2009 to 2012 that combine measures of access to primary care services with data on hospital activity throughout England. The panels are constructed for two different spatial scales. In the main neighbourhood level panel, the units of analysis are 32,844 Lower Super Output Areas (LSOA). LSOAs are a census geographical unit that house 1,630 residents on average, making them comparable to but somewhat smaller than US Census tracts. The second is a provider level panel in which the units of analysis are 144 NHS Trusts that contain at least one ED. Population demographic data for LSOAs from the Office for National Statistics and primary care access measures are then appended to the activity data. The latter are generated using reports issued by the hospital regulator and the Department of Health by first compiling a list of *EAPMC* services, geocoding each site using the full postcode, then adding facility opening and closing dates using information provided by the Health and Social Care Information Centre (HSCIC), and manually checking each data against other sources. Shapefiles released by NHS England identify patient registration boundaries for a sub-set of practices.

Hospital activity data is drawn from main sources: the Quarterly Monitoring of Accident and Emergency (QMAE) dataset published by NHS England, and Hospital Episode Statistics (HES) records provided by HSCIC. QMAE was the official source of information on ED activity in the period 2009 to 2012, and is generally considered to be the most comprehensive and reliable source of aggregate information on emergency care activity. It captures aggregate ED visit counts at the NHS Trust, rather than the site, level. For most NHS Trusts this is inconsequential as there is only one ED, but some NHS Trusts have multiple emergency care sites, in which case the split of attendances across sites cannot be observed. To account for mergers, I group together earlier data for NHS Trusts which will eventually merge in order to create a consistent panel.

For the neighbourhood level analysis, three hospital utilisation variables are derived from two distinct HES data resources. Both contain anonymised patient records, and include the patient's residential location (LSOA) as well as details of care received. The first and principal utilisation measure records *unplanned visits* to hospitals: *(1)* the total number of visits to hospital EDs. Two further measures relate to *admissions* to hospital: *(2)* the total number of admissions, and *(3)* the proportion of unplanned admissions that could potentially have been avoided with appropriate primary care.[10] The source for the second and third variables is the HES Admitted Patient Care dataset while the first is by necessity drawn from the (separate) HES A&E dataset. This distinction is important because Admitted Patient Care data contain complete and consistent diagnostic information but A&E data do not. This omission precludes analysis of ED visits by the categories used in Taubman et al. (2014) i.e. "Non-urgent," "Urgent, primary-care treatable," etc.

The HES A&E dataset is a rich source of data on ED activity, albeit was published as experimental statistics until 2012/13. The use of these data to compute ED visits is challenging because in early iterations of the data collection health care service providers were not strictly required to record the type of emergency unit that a patient attended (for example an ED or another type of emergency care facility, such as an eye hospital or Minor Injury Unit). Completing this field in the data then subsequently became mandatory. As a result emergency unit type codes are missing for close to 30% of patient records for NHS Trusts in 2009/10. The share of missing codes then falls to around 11% in 2010/11, 3.5% in 2011/12, and 1.5% of records by 2012/13, a trend depicted in the series of bars labeled 1 in Fig. 4.

An implication of this is that changes in ED visits observed in the raw data between 2009 and later years will in part reflect better coding practices rather than genuine ED activity changes. This is problematic as better coding coincides strongly with the introduction of *EAPMC* services. I circumvent this problem in two steps, which are visually illustrated in Fig. 4. First, I exploit that the QMAE data described above indicates that some hospital-quarter cells only contain ED attends whereas others contain only non-ED attends. Cross-referencing to QMAE thus allows me to impute true type codes for more than half of the uncoded NHS Trust attendances in the HES A&E dataset in my sample window. Nevertheless, as depicted in the second series of bars in Fig. 4, a substantial number of missing codes remain.

[9] Note that this setting is unlikely to induce supplier-induced demand (in the sense of doctors encouraging patients to consume more health care) as emergency care is unplanned and not influenced by doctor behaviour.

[10] Avoidable admission are admissions for conditions that could potentially have been avoided with appropriate primary care, for example by preventing the onset of disease preventable by vaccination, managing an acute illness such as dehydration, or a chronic condition such as diabetes. I follow earlier literature in defining these admissions using ICD-10 codes for a set of 19 presenting conditions – see Appendix Table A3 for the ICD-10 codes used.

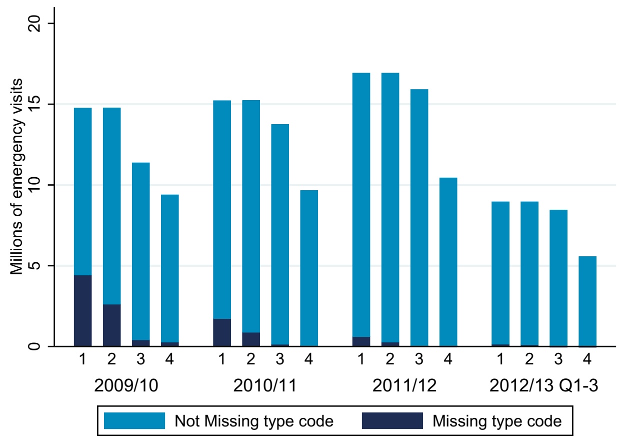

Fig. 4. HES Data operations and ED visits missing type code. *Notes*: Figure shows total visits to emergency care units not missing type code (light blue bars) in NHS Trusts and missing unit type codes (dark blue bars), 2009/10 to 2012/13 Q3. Four sets of bar pairs are shown (1) the raw data; (2) after imputing missing codes using QMAE; (3) after dropping quarter-neighbourhood cells that contain fewer than 50 ED visits; (4) as (3) but retaining a balanced panel of neighbourhoods with non-missing data in each quarter.

Second, after removing duplicate records and collapsing the data to quarter-neighbourhood cells, I then exclude any quarter-neighbourhood cells that contain fewer than 50 ED visits (which represents the 17th percentile in the distribution) from the final estimation sample. A 50-visit threshold is used because this sample selection criterion is effective in eliminating missing codes from the data i.e. it reduces the number of uncoded emergency care visits to inconsequential levels. This is shown in the third (the resulting unbalanced neighbourhood panel) and fourth (the associated balanced neighbourhood panel) series of bars in Fig. 4. However, and while this strategy is unlikely to be a source of bias, it potentially raises generalisability concerns as results may be specific to places with high ED use. Later findings that indicate a close correspondence between the neighbourhood and NHS Trust level results, as well as an alternative strategy detailed in full in Section 3.8, give reassurance that this is not the case.

2.2. General empirical framework

My data constitutes two quarterly panels of hospital utilisation measures at the NHS Trust and the LSOA administrative geography and a database of *EAPMC* services including opening and closing dates. The general estimation framework I use is common across both panels:

$$y_{it} = EAPMC\prime_{itb}\beta + x\prime_{it}\gamma \quad + f(i, t) + \epsilon_{it} \tag{1}$$

where observation units are indexed by subscript $i \in$ (LSOAs, NHS Trusts). The dependent variable is a hospital utilisation outcome in quarter t. *EAPMC* is a primary care access intensity measure that captures *EAPMC* services within distance buffer b from unit i at time t. Time varying controls variables are contained in the vector x, and $f(i, t)$ are fixed effects which allow for unobserved time and place variation.

The majority of estimates that follow are generated from neighbourhood-level (i =LSOA) regressions that take the form:

$$y_{it} = EAPMC\prime_{itb}\beta + x\prime_{it}\gamma \quad + \phi_i + \phi_{tm(i)} + \phi_{T(t)b} + \epsilon_{it} \tag{2}$$

Here x captures time varying counts of population in five age bands (aged less than 10, aged 10–19, aged 20–49, aged 50–69, aged 70+) and their squared values to control flexibly for changes in neighbourhood population and demographics. To account for unobserved variation, specifications include LSOA fixed effects (ϕ_i), quarter indicators interacted with labour-market area (indexed by m) dummies (ϕ_{tm}), and separate year (indexed by T) indicators for all neighbourhoods that obtain exposure to services in distance buffer b at any time in the panel ($\phi_{T(t)b}$). These fixed effects are intended to eliminate factors that could bias results, including any time invariant neighbourhood characteristics such as access to a walk-in clinic that existed prior to the *EAPMC* policy, as well as general labour-market wide changes, for example in the supply of hospital or community care.

Ancillary specifications at the hospital level (i = NHS Trust) are useful as they require no sample restrictions to deal with data coding issues. Regressions take the form:

$$y_{it} = EAPMC\prime_{itb}\beta + \phi_i + \phi_{tg(i)} + \phi_{T(t)b} + \epsilon_{it} \tag{3}$$

Besides the different unit of observation, in contrast to Eq. (2) these regressions omit demographics given there is no simple way to assign population to NHS Trusts, and account for area trends at the level of 9 regions (London, South East, South West, West Midlands, North West, North East, Yorkshire and the Humber, East Midlands, and East of England, indexed by g), reflecting that in many cases a labour-market area contains only a single ED.

In both panels, the principal object of interest is *EAMPC*, a vector that captures time-varying primary care access intensity as a non-parametric function of proximity to services. These measures are generated from counts of the number of open walk-in clinics (or extended hours practices) within concentric distance buffers surrounding the centroid of each neighbourhood or NHS Trust. As shown in Appendix Fig. A5, the median travel distance to access emergency care in England differs considerably over space, so I allow distance buffers to vary according to the distribution of observed distances in the data. In practice, this means buffers are computed for each of the 149 labour-markets in my data then assigned to all neighbourhoods/NHS Trusts with centroids falling in that area.[11]

Buffers are constructed in a discrete way such that each service falls into only one buffer for each neighbourhood or NHS Trust. In most cases effects in three distance buffers are estimated: the lower quartile distance travelled (p25), the median (p50), and the upper quartile (p75). Around 15 of the walk-in clinics in my data are co-located at hospital EDs. To allow for different effects for these services I create a separate treatment for all such services within the median travel distance i.e. within the first two buffers. This yields four buffers in total, and the following estimated equation for walk-in clinics:

$$y_{it} = \beta_1\, WiC^{p25}_{it} + \beta_2\, WiC^{p50}_{it} + \beta_3\, WiC^{p75}_{it} + \beta_4\, WiC^{ED}_{it} \\ + x\prime_{it}\gamma \; + f(i, t) + \epsilon_{it} \tag{4}$$

[11] The Office for National Statistics calculates labour-market areas, known locally as Travel to Work Areas (TTWAs), using commuting data. They each contain one or more cities and they nest LSOAs. The labour-market distance buffers are computed using distances travelled to attend EDs in the HES data between 2008/9 to 2012/13. I approximate patient starting location as registered primary care practice and ED visit location as the closest ED (relevant if an NHS Trust has more than one ED). Using patient trips to EDs is driven by practical considerations (walk-in clinic attendances are not well recorded in HES) but also has the benefit of ameliorating concerns about the endogeneity of resulting buffers. Results in an earlier working paper (Pinchbeck, 2014) show that computing buffers across alternative administrative geographies other than TTWAs leaves results materially unchanged, but that setting buffer distances universally based on national averages introduces substantial noise. Later results are unaffected when the sample is restricted to places with median buffers that lie between the 25th (3 km) and 75th (5 km) percentiles of the buffer distribution, in which case buffers distances are very similar.

2.3. Identification

Obtaining causal estimates from policy-induced variation is challenging because policy choices are unlikely to be blind to local circumstances. When places (or people) unaffected by a policy are systematically different to places (or people) that are, the untreated group may not provide a valid control group, i.e. the average outcomes of the groups may have evolved in a different way in the absence of the policy.

Access to *EAPMC* primary care services reflects a series of decisions by health administrators, such as where and when to open a new facility. The suspicion must be that location decisions could be related to hospital outcomes or the (possibly unobservable) underlying drivers of these outcomes. For example, national policy guidance required *EAPMC* services to be in easily accessible locations, and easy access may coincide with high health need e.g. in deprived city centres. Moreover, local administrators are likely to have positioned services to address (excess) demand pressures, so it might be reasonable to expect that *EAPMC* services were targeted to places with poor primary care access or to places experiencing increasing ED attendances (or expected future increases). If true, any associations between primary care access and ED attendances that ignore policy targeting could be biased towards finding that access to primary care leads to more ED visits i.e. results would be underestimated.[12]

Fortunately, *EAPMC* offers another source of variation: timing differences in service availability. Using this variation, which can be leveraged by discarding units unaffected by the policy to focus on units that are, is viable in this setting because of the staggered roll out of services and because some services close in my sample window. Strategies can be designed to use this variation in different ways. In a baseline difference-in-difference (DD) approach, I retain places gaining an *EAPMC* service prior to the start of my sample (2009q2) as a control group and use closures as well as openings. Estimation results below evaluate robustness to alternative choices. Irrespective, in all cases, an assumption required for identification is that groups of units experiencing access changes at different times provide a valid counterfactual for one another. Although this assumption cannot be tested directly, balancing tests and visual checks can probe it indirectly.

Even if sample selections yield parallel trends, recent work warrants caution. This is because β from specification (2) represents a weighted average of underlying DD estimates from multiple simultaneous experiments, where each experiment corresponds to a group treated in one period being compared to another group, and least squares weights are proportional to group size and the variance of treatment (Goodman-Bacon, 2018). As weights are largest for units treated in the middle of the panel (because treatment variance is highest), the DD ATT may not correspond closely to the sample share weighted ATT. In recognition of these issues, and following Goodman-Bacon (2018), a range of regressions are used to test the sensitivity of the baseline DD estimates to alternative specifications that alter how the underlying experiments are weighted together, for example weighting estimates by population, and adding MSOA- or LSOA-specific linear trends.

3. Results

Table 1 provides descriptive statistics for hospital utilisation and control variables. The "NHS Trust sample" refers to the 118 NHS Trusts that were exposed to walk-in clinics under the *EAPMC* policy. The "Walk-in clinic sample" refers to the sample of neighbourhoods that were exposed to walk-in clinics under the *EAPMC* policy. The "Under-doctored sample" refers to neighbourhoods in areas of the country eligible to receive new extended hours practices under *EAPMC*. The latter two samples overlap as *EAPMC* introduced new walk-in clinics in all areas of the country.

Table 1 refers to information underpinning regression samples, with the neighbourhood level descriptives excluding duplicated or incomplete records in the underlying patient-level data, including around 2% of records missing patient's residential neighbourhood, and after dropping LSOA-quarter cells with low counts of ED visits. The mean number of LSOA ED visits per quarter is 140 (national average 95), which implies around 35 annual visits to the ED per 100 residents.

By consequence of the timing-driven research design all neighbourhoods in the LSOA regression samples are exposed to at least one walk-in clinic or new *EAPMC* extended hours GP practice in the sample window. To illustrate how access to walk-in clinics varies by neighbourhood, I create a variable capturing "maximum exposure" to walk-in clinics – i.e. the highest number of ED and other walk-in clinics that each neighbourhood becomes exposed to at any point in the period April 2009 to September 2012, and cross-tabulate results in Appendix Fig. A6. Neighbourhoods in the main sample were exposed to between 0 and 9 non-ED walk-in clinics and either 0 or 1 ED-based clinics. However, the vast majority gained access to only one or two clinics at any time: around 60% were exposed to one in the panel period, whereas some 80% were exposed to no more than two.

3.1. Walk-in clinics and hospital utilisation

Table 2 reports the effect of walk-in clinics on ED utilisation in difference-in-difference regressions corresponding to Eqs. (2) and (3). The first column is the baseline specification that uses the unbalanced panel of LSOAs that comprise the main estimation sample. To allow for arbitrary spatial correlation standard errors are clustered on 7,201 Middle Super Output Areas.[13] The uppermost parameter estimate indicates that neighbourhoods in close proximity to walk-in clinics co-located at EDs experience reductions in ED attendances of approximately 3.75%. For other walk-in facilities coefficients are smaller and decay with distance – the strongest impacts are evident in the closest neighbourhoods, roughly halve in the next buffer, and are insignificant and close to zero in locations that gain a clinic beyond the median distance travelled to attend an ED. The baseline estimates are robust to restricting attention to a balanced panel of LSOAs (column 2), and estimating Eq. (3) with NHS Trust data (column 3) yields highly similar, albeit more imprecisely estimated, coefficients. These results serve to demonstrate that the sample restriction noted in Section 2.1 is not critical to findings.

The remaining columns in Table 2 report further effects of walk-in clinics in the LSOA panel using the specification described in Eq. (2). The fourth and fifth columns split ED attendances into visits during walk-in opening times — Monday to Sunday 8am to 8pm — and at other times. Coefficients imply the overall effects estimated in the first column are almost wholly driven by the former. For example, applying the first coefficient (−0.0376) to the sample mean ED visits (140) implies 5.2 fewer ED visits whereas the corresponding calculation for the fourth column implies 4.8 fewer visits. These results are a meaningful cross-check on internal validity because they rule out omitted factors which commonly drive

[12] Systematic differences are indeed evident in the data. For example, neighbourhoods exposed to *EAPMC* services in the main sample were more deprived (mean Index of Multiple deprivation score of 30 against national average 22) and had lower average house prices (£ 170,000 vs. national average £ 196,000) in 2010.

[13] MSOAs nest LSOAs and each house between 5,000 and 15,000 inhabitants.

Table 1
Descriptive statistics.

	NHS Trust sample mean	NHS Trust sample sd	Walk-in sample mean	Walk-in sample sd	Under-doctored sample mean	Under-doctored sample sd
Emergency Department utilisation						
Emergency Department (ED) visits	24,692	10,573	140	47	141	45
ED visits - walk-in open times			97	32	98	30
ED visits - walk-in closed times			43	18	43	17
ED visits - regular primary care times			60	21	61	20
Other hospital utilisation						
Hospital admissions			110	37		
Potentially avoidable admissions (%)			23	8.6		
Neighbourhood demographic controls						
Population aged <10			222	78	222	80
Population aged 10–19			210	79	215	77
Population aged 20–49			763	235	717	204
Population aged 50–69			326	91	330	80
Population aged 70+			160	73	163	67

Table 2
Effects of walk-in clinics on hospital utilisation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample:	LSOA (unbalanced)	LSOA (balanced)	NHS Trust	——— LSOA (unbalanced) ———			
Outcome:		——— ED visits ———				All Admit.	Avoid Admit.
	All hrs	All hrs	All hrs	WiC hrs	Other hrs		
ED WICs	−0.0376***	−0.0379***	−0.0302	−0.0492***	−0.0149	0.0239	−0.0873
	(0.0113)	(0.0118)	(0.0255)	(0.0123)	(0.0169)	(0.0154)	(0.5058)
p0-p25 WICs	−0.0278***	−0.0326***	−0.0259	−0.0433***	−0.0011	0.0026	0.1211
	(0.0050)	(0.0078)	(0.0194)	(0.0060)	(0.0053)	(0.0074)	(0.1911)
p25-p50 WICs	−0.0149***	−0.0194***	−0.0189	−0.0215***	−0.0048	−0.0027	−0.2168
	(0.0040)	(0.0059)	(0.0201)	(0.0046)	(0.0044)	(0.0061)	(0.1594)
p50-p75 WICs	−0.0003	0.0020	0.0020	−0.0041	0.0041	−0.0025	0.1005
	(0.0029)	(0.0042)	(0.0088)	(0.0033)	(0.0030)	(0.0072)	(0.1166)
Quarter-labour market FX	√	√		√	√	√	√
Quarter-region FX			√				
Year-distance buffer FX	√	√	√	√	√	√	√
Population controls	√	√		√	√	√	√
Observations	125945	52220	1643	125945	125945	125929	125945
R-squared	0.866	0.837	0.954	0.810	0.791	0.816	0.283

Notes: LSOA panels in columns (1), (2), and (4)-(7) contain quarter-LSOA cells with 50 or more ED visits. LSOA panels are unbalanced except for column (2). Column (3) is estimated on an NHS Trust panel. Dependent variables are in logs except final column which is a rate. WIC hrs are between 8am-8pm Monday to Sunday. The 9 regions in Column (3) are London, South East, South West, West Midlands, North West, North East, Yorkshire and the Humber, East Midlands, and East of England. Standard errors in parentheses clustered at the MSOA level except in column (2) clustered at the NHS Trust level. ***p < 0.01, **p < 0.05, *p < 0.1.

ED use during and outside of clinic opening times. For example, a significant role for confounders such as socio-economic changes in the composition of neighbourhoods or changes in the local supply of 24 hour hospital care is improbable because these would likely show up in ED visits outside of walk-in opening times.

The last two specifications in Table 2 present regressions on outcomes referring to the volume and mix of admitted patients. Coefficients in the fourth column indicate small and insignificant impacts of access to walk-in clinics on the log count of hospital admissions. Similarly, the last column signals no evidence of effects on the proportion of admissions that may have been prevented with appropriate primary care.[14]

3.2. Balancing tests and trends

Difference-in-difference applications assume that trends in treatment and control groups would be parallel absent treatment. The research design outlined in Section 2.3 means that places that host *EAPMC* services act as both a treatment and control group, with identification coming off the timing of service deployment. An assumption necessary for identification is that service deployment time should be unrelated to the determinants of ED visits, conditional on general labour-market trends. If in fact new services are deployed to places at times when ED visits are rising or falling more quickly than the general trend, then the control group of past and future locations for services will not provide a valid counterfactual.

The discussion in Section 1.1 suggests the actual timetable for the new centres was driven by administrative factors (e.g. availability of suitable premises and speed of procurements etc.) which are plausibly unrelated to ED visits. To test this premise, pre-reform primary care access variables (measured in both levels and changes) are regressed on the number of quarters between the policy announcement and the neighbourhood's first exposure to a service, as well as the time-invariant analogues of the control

[14] Earlier versions of this work used mean ED waiting times as another outcome. However, identification is complicated by possible endogenous responses in hospital resourcing and operating decisions, as well as the likely violation of Stable Unit Treatment Value Assumption (SUTVA) i.e. because to the extent that walk-in service affect waiting times, they will do so for all patients using an ED regardless of whether they themselves gain better primary care access. I therefore leave this analysis for future work. Note that results for ED visits are unchanged when controlling for ED waiting times, which should rule out SUTVA-type spillover concerns on my main results. Another potentially interesting outcome to consider would be referrals to EDs by GPs, which may plausibly increase with better primary care access. Whittaker et al. (2016) obtain a point estimate of 4.43%, but this is imprecisely estimated (95% C.I -4.11% to 12.74%). The quarterly panel methods I adopt in this paper are not well suited to examining this outcome, and in any case the volume of ED visits of this type is relatively small, so I also leave this to future work.

Table 3
Balancing on pre-policy primary care access conditions

	(1)	(2)	(3)	(4)	(5)
	get appoint--ment %	satisfied hrs %	satisfied phone %	GP head--count	QOF overall %
Panel A: Regressions using pre-panel levels					
Quarters until *EAPMC*	−0.0020	0.0517	0.0900	−0.0263	0.0500
	(0.2054)	(0.0927)	(0.1579)	(0.0369)	(0.0617)
R-squared	0.120	0.274	0.124	0.210	0.058
Panel B: Regressions using pre-panel changes					
Quarters until *EAPMC*	−0.0387	−0.0083	−0.0877	−0.0142	−0.0582
	(0.1032)	(0.0494)	(0.0669)	(0.0098)	(0.0746)
R-squared	0.049	0.049	0.039	0.056	0.049
Labour-market fixed effects	√	√	√	√	√
Distance buffer fixed effects	√	√	√	√	√
Population age bands	√	√	√	√	√
Observations	10910	10910	10910	10910	10910

Notes: Cross-sectional regressions using neighbourhoods that first gain access to walk-in clinics between April 2009 and September 2012. Explanatory variable is number of quarters between April 2008 and LSOA's first access to an *EAPMC* walk-in service in p75 or lower distance buffer. Dependent variable values assigned from nearest primary care practice. Columns (1)-(3) from GP Patient Survey, either levels in June 2008 (Panel A) or changes between June 2007 and June 2008 (Panel B): % of patients able to book an appointment 2 days ahead; % satisfied with practice opening hours; % satisfied with phone access. Column (4) is GP headcount in May 2008 (Panel A) and change in headcount between May 2007 and May 2008 (Panel B). Column (5) is overall QOF score in % in 2007/8 (Panel A) or change between 2006/7 and 2007/8 (Panel B). Standard errors in parentheses clustered at MSOA. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

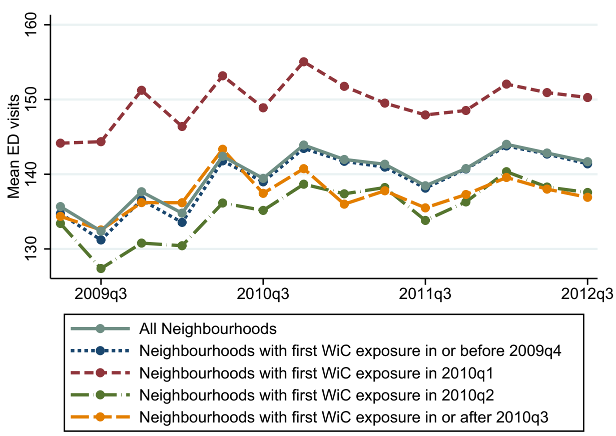

Fig. 5. Trends in ED visits by time of first exposure to walk-in clinics. *Notes*: Figure assesses whether neighbourhoods are on parallel trends by plotting quarterly ED visits for neighbourhoods (LSOAs) grouped by date of first exposure to an *EAPMC* walk-in clinic.

variables listed above (see Appendix C for details). As data for pre-reform access is not available at the neighbourhood level, values are assigned to neighbourhoods from the nearest primary care practice. The upper panel of Table 3 shows no significant correlation between *EAPMC* treatment timing and pre-reform primary care access as measured by the percentage of patients able to obtain an appointment, satisfied with phone access, and satisfied with practice opening hours in June 2008; GPs per patient in May 2008, and the overall Quality and Outcomes Framework (QOF) score for the practice in 2007/8.[15] The bottom panel of the Table similarly yields no correlation between treatment timing and local trends in primary care access between May/June 2007 and May/June 2008 in columns 1-4, or between 2006/7 and 2007/8 in column 5.

A second strategy visually assesses the extent to which groups of neighbourhoods first exposed to new services at different times follow similar trends in ED visits. Reassuringly Fig. 5, which is displayed in actual time rather than event time due to the seasonal pattern, reveals the unconditional trends in ED visits in the full sample are broadly similar across all groups throughout the sample window.

Appendix C describes further balancing tests and figures. The tests in Table 3 were conducted using time to first access to a walk-in clinics in any of the four buffers described in Section 2.2. Appendix Table A1 repeats the exercise but estimating the association between pre-reform outcomes and time to first treatment in each of the four distance buffers. Of the 40 coefficients reported in Table A1, only 3 are significantly different from zero at the 10% level (1 of which is significant at the 5% level), and none of these relate to the p25 or p50 distance buffers. That said, 2 significant coefficients relate to timing of first exposure to walk-in clinics co-located at EDs (the other is for access to walk-in services in the third distance buffer), signaling that findings for these services should be interpreted with caution.

Second, in Appendix Table A2, the approach in Table 3 is used to examine whether treatment timing is correlated with pre-*EAPMC* ED conditions. Data limitations preclude valid tests on ED visits at the neighbourhood level, so these regressions either rely on neighbourhood-level log counts of admissions to hospital via an ED (which is strongly correlated with ED vists, $\rho = 0.5$, p value < 0.0001), or else on data matched in from the nearest NHS Trust. These further balancing tests yield insignificant coefficients in all cases. Finally, Appendix Fig. A2 shows that timing groups trends in ED visits are similar for neighbourhoods exposed to services close by (the p25 distance buffer) and further away (not exposed to a clinic in the p25 distance buffer or a colocated service).

3.3. Alternative variation, re-weighting, and robustness

The estimates in Section 3.1 were generated using 12,753 neighbourhoods that were within the local 75th percentile travel distance of an *EAPMC* walk-in service at some point before or during the sample window (2009q2-2012q3). Close to three quarters of these neighbourhoods began the period without access to a service but gained at least one during it, and slightly more than one in ten experienced at least one closure during the window. Some 1750 of the neighbourhoods were exposed to the same number of centres throughout the period. In the baseline DD set-up, these neighbourhoods act purely as a control group, while those experiencing openings and closings act as both treatments and control units.

[15] QOF is an incentive scheme intended to deliver high quality primary care. It was introduced in 2004 and is part of the General Medical Services (GMS) contract for general practices. Payments are linked to scores across several dimensions of primary care quality such as clinical, organisational, and patient experience. In unreported results, I also find balance with respect to QOF clinical and organisational domain scores.

Table 4
Sensitivity to alternative sources of variation and re-weighting

	(1)	(2)	(3)	(4)	(5)	(6)
	Sources of variation			Pop.	Additonal trends	
	EAPMC +	Timing	Openings	weighted	MSOA	LSOA
ED WICs	−0.0262***	−0.0387***	−0.0407***	−0.0400***	−0.0482***	−0.0501***
	(0.0094)	(0.0118)	(0.0106)	(0.0113)	(0.0114)	(0.0120)
p0-p25 WICs	−0.0292***	−0.0299***	−0.0252***	−0.0279***	−0.0266***	−0.0280***
	(0.0048)	(0.0052)	(0.0040)	(0.0052)	(0.0048)	(0.0053)
p25-p50 WICs	−0.0157***	−0.0154***	−0.0133***	−0.0155***	−0.0123***	−0.0134***
	(0.0038)	(0.0041)	(0.0035)	(0.0042)	(0.0036)	(0.0040)
p50-p75 WICs	−0.0007	−0.0001	−0.0010	−0.0000	−0.0028	−0.0038
	(0.0027)	(0.0029)	(0.0027)	(0.0030)	(0.0026)	(0.0028)
Observations	201400	107852	56981	125945	125945	125945
R-squared	0.870	0.863	0.896	0.868	0.882	0.896

Notes: All samples contain quarter-LSOA cells with 50 or more ED visits. LSOA panels are unbalanced. Standard errors in parentheses clustered at the MSOA level. ***p < 0.01, **p < 0.05, *p < 0.1. Controls and fixed effects as for baseline specification.

Table 4 explores the underlying sources of variation. In column 1, I abandon the *EAPMC* only strategy and extend the control group to include neighbourhoods never exposed to *EAPMC* walk-in services. Results do not dramatically diverge from results in column 1 of Table 2, bar the coefficient for services located at hospital EDs which is attenuated. Column 2 proceeds in an alternative conceptual direction by removing neighbourhoods that retain the same number of services throughout the sample period from the sample. These estimates, which are identified purely from within sample access *EAPMC* changes, are highly similar to the baseline findings. Column 3 returns to the baseline specification but retains quarters prior to the end of calendar year 2010, thereby eliminating variation arising from service closures. The results remain very close to the baseline. Besides removing closures, these results suggest that short and long term effects of the services are likely to be similar, and are also reassuring as they exclude the material drop in GP practices in 2012 evident in Fig. 2.

Goodman-Bacon (2018) demonstrates that specification changes that re-weight the underlying difference-in-difference estimates in timing-driven research designs can lead to large changes in the single coefficient estimate. Following this example, columns 4–6 of Table 4 report variations on the baseline specification that may be expected to change how the underlying DD estimates are weighted together. The specification in column 4 repeats the baseline specification but now weights by neighbourhood population in 2009q2. This makes little difference. The final two columns allow for additional linear trends: in column 5 I interact a linear trend with indicators for each MSOA, which allows for common trends for spatially proximate neighbourhoods, and in column 6 I allow for a differential trend for each individual LSOA (i.e. each neighbourhood). These specifications yield moderately large changes on the estimate for co-located service, but leave other findings substantively unchanged.

Overall, Table 4 presents a mixed picture regarding sensitivity to different sources of variation and re-weighting of underlying DD estimates. For services located at EDs the gap between the lowest estimate (−0.026) and highest estimate (−0.05) is reasonably large, suggesting some caution in interpreting this result. Conversely, the estimates for other non-ED services appear to be highly robust.

Specification tests in Appendix Tables A4 and A5 provide further robustness checks. The first two columns of Table A4 signal that results are insensitive to controlling for *EAPMC* extended hours practices, and a small number of closures of walk-in clinics that pre-date *EAPMC*. The baseline estimates are also robust to using a binary 1/0 exposure variables instead of the count-based treatment intensity variables, specifying the dependent variable in levels, and removing the population and buffer control variables. This Table also highlights that effects on ED visits for children and elderly people are slightly smaller than the baseline effects, and that impacts are significantly larger in the most deprived neighbourhoods. Regarding inference, my baseline approach is to cluster at the MSOA level, which allows for serial correlation and heteroscedasticty, as well as some degree of spatial correlation in unobservables (since groups of LSOAs within each MSOA are in close spatial proximty), which seems a good way to address plausible forms of bias. Appendix Table A5, indicates that clustering at the MSOA level yields standard errors that are larger than standard errors that follow Conley (1999) to explicitly allow for continuous forms of spatial autocorrelation up to a distance cut-off of 2 km.[16]

3.4. Substitution and health care spending

Previous results show that walk-in services reduce visits to EDs but additional steps are required to understand the degree of substitution for ED activity. The mean number of ED visits for neighbourhood-quarter cells in my main sample is 140 (Table 1), and the average walk-in clinic in my data has slightly under 50 neighbourhoods in the first distance buffer, 50 more in the (thinner) second buffer, and a further 100 in the third. The point estimates in column one of Table 2 thus imply that an average ED walk-in clinic reduces annual ED visits by 2106 (=0.0376*140*100*4) whereas the average walk-in clinic located elsewhere reduces visits by 1195 (=0.0278*140*50*4 + 0.0149*140*50*4). Based on auxiliary information I assume each walk-in clinic is visited 18,000 times annually, suggesting that around 12% of patients visiting an ED walk-in clinic and around 7% of those visiting a clinic elsewhere were diverted from an ED.[17] The (unreported) 95% confidence intervals indicate that between 5 to 20% of patients attending ED based walk-in clinics and 5 to 10% of patients attending other clinics were diverted from an ED.

These rough calculations imply the lion's share of walk-in visits do not substitute for a visit to an ED, and are informative about the cost implications of the services. With a diversion rate of r, the average net savings s per walk-in patient can be calculated as $r \times c_{ED} - c_{PC}$, where r is the diversion rate, c_{ED} is the cost of treat-

[16] Allowing for spatial autocorrelation is computationally demanding so here I specify the dependent variable as log ED visits per 1000 residents and drop population controls. Coefficients are robust to this specification change.

[17] This should be considered to be an indicative estimate. Monitor (2014) reports that 70% of clinics surveyed in 2014 provide between 20,000 and 45,000 walk-in appointments per year but that attendances anticipated in commissioning contracts were typically in the range of 12,000 to 24,000 attendances. I use the mid-point of the latter range for these calculations because it provides a better match to the sample window underpinning the analysis.

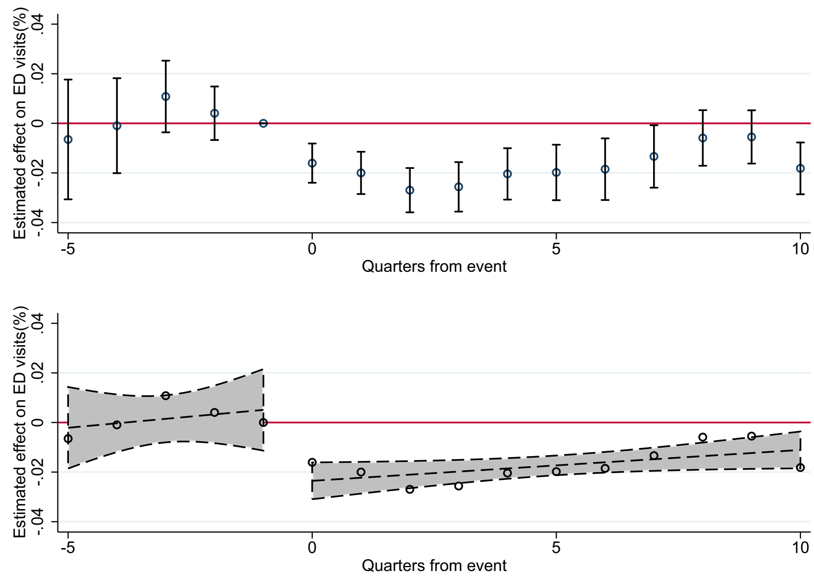

Fig. 6. Event study (p25 and p50 buffers). *Notes*: Figure shows event time estimates and 95% confidence intervals; omitted event time period is prior to neighbourhood's first exposure to a walk-in clinic in the p25 or p50 buffers. Regression includes neighbourhood fixed effects, quarter×labour-market fixed effects, buffer×year fixed effects, and population controls as well as additional controls for walk-in clinics in other buffers, *EAPMC* extended hours practices, and closures of pre-*EAPMC* walk in clinics.

ment in an ED and c_{PC} is the cost of the same treatment in a primary care walk-in. Given that the cost of treatment is higher in an ED, $c_{PC} = \lambda c_{ED}$ where $0 < \lambda < 1$, and the average per patient savings of walk-in clinics are $s = c_{ED}(r - \lambda)$. Walk-in clinics break even when $r = \lambda$, and create budgetary headway only when the diversion rate is higher than the ratio of walk-in unit costs to ED unit costs. Available estimates place the average unit cost of a visit to a walk in clinic at a third of the unit cost of an ED, so that $\lambda = 0.33$.[18] Based on these direct costs, diversion rates of 10 to 20% (0.1–0.2) imply that *EAPMC* walk-in service led to a net increase in health care spending of around £ 10–20 per walk in visit.

Clearly this is an incomplete analysis of the full possible effects of walk-in services, not least because the clinics may also substitute for care in regular primary care practices, and because access to convenient care may have additional benefits, for example in reassuring anxious patients, or from reducing congestion in EDs.[19]

3.5. Dynamic effects

In this section, a neighbourhood level event study is used to examine the dynamic effects of access to walk in services. Applying an event study design to *EAPMC* is not straightforward because service effects are allowed to vary with distance, because a given location could be exposed to multiple services simultaneously, and because the number of services accessible from a location may both increase and decrease over time. Notwithstanding, besides tracing out time variation, the event study approach provides a way to benchmark the earlier DD estimates, and facilitates tests of pre-treatment trends which could signal endogeneity concerns.

Event study plots, estimated using Eq. (5), are reported below and in Appendix D. In all cases, the event is taken to be the time of first exposure to a service in one or more buffers and take the form:

$$y_{it} = \sum_{r=-A}^{B} \beta_r \mathbf{1}\{R_{it} = r\} + x\prime_{it}\gamma \quad + \phi_i + \phi_{tm(i)} + \phi_{T(t)b} + \epsilon_{it} \tag{5}$$

Where r is the relative time (in quarters) to the quarter in which the neighbourhood was first exposed to a new *EAPMC* service (r = 0). Event time effects are estimated over 5 pre and 10 post treatment quarters i.e. relative time $r \in (-5, 10)$ and, following convention, are normalised on the quarter prior to first exposure to a service ($r = -1$). Besides the baseline controls, I include three sets of additional sets of variables: counts of walk-in clinics in other buffers, buffer-specific controls for closures of pre-*EAPMC* walk-in clinics, and buffer-specific counts of open *EAPMC* extended hours practices. These controls help to reduce noise in the event study, and as noted above, the baseline specification is insensitive to their inclusion (see Appendix Table A4 column 2).

The first event study focuses on neighbourhoods that are exposed to a clinic in either the first (p25) or second (p50) buffer. The top half of the Fig. 6 plots event time point estimates and 95% confidence intervals, revealing a clear drop between $r = -1$ and $r = 0$. The difference between the pre- and post-period event time effects is close to 2%, which is comparable to my main DD estimates. The left side of the Figure is consistent with no pre-trends: none of the estimates is individually statistically significant, and neither are they jointly significant (p value 0.16). The bottom half of the Figure fits lines through the estimates. The pre-event slope is not statistically distinguishable from zero (p value 0.46). The right side of the Figure is less conclusive. There is sufficient evidence to reject the null that the coefficients are all equal (F test p value < 0.001), yet the coefficients for periods 0 and 10 are extremely similar and statistically indistinguishable (p value 0.73), and the event time effects are approximately stable other than periods 8 and 9, which are close to zero. The line of best fit through all point estimates slopes upwards, but the slope is modest and marginally insignificant (p value 0.051). When excluding either or both of periods 8 or 9 the slope is more clearly insignificant (p values > 0.1).

Appendix D contains two further event study plots. The first examines neighbourhoods first exposed to walk-in services in the first distance buffer, and the second examines neighbourhoods first exposed to a service in the third buffer i.e. between the median and the 75% percentile distance. These yield estimates that are broadly

[18] Based on the cost of an ED visit in England of £ 100 and a cost of a walk-in clinic visit of £ 36 as reported by the BBC: Wheeler, B. (2012). Are NHS walk-in centres on the way out? BBC. June 28.

[19] Although note that regular primary care services are funded through capitated budgets in the NHS, such that payments are not linked to activity.

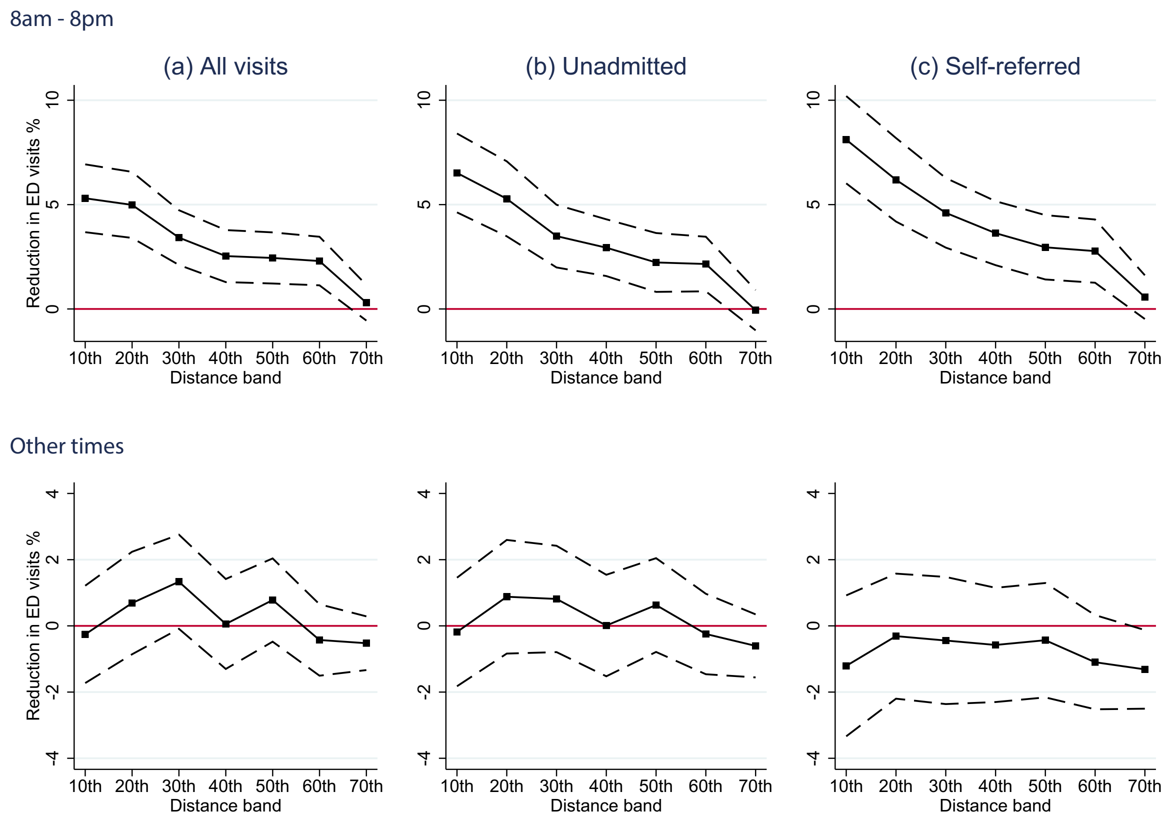

Fig. 7. Distance decay by ED visit type. *Notes*: Figure plots point estimates reported in Table A6 (black lines) and the bounds of the associated 95% confidence intervals.

comparable to earlier DD estimates, and conclusions generally mirror those for the event study depicted here.

3.6. Distance decay

Section 3.1 reported spatial patterns in the impacts of walk-in clinics on ED attendances. Distance decay is more precisely teased out in Appendix Table A6 which drops neighbourhoods close to walk-in facilities at EDs and expands the number of distance buffers to seven. Column (a) of Fig. 7 summarises the first two columns of this Table: solid black lines connect point estimates on the buffers (with sign reversed so that values above the horizontal line can be interpreted as an approximate percentage reduction in ED visits) and dashed black lines bound the 95% confidence intervals. The upper plot confirms the strong distance decay during clinic opening hours: neighbourhoods in the closest proximity experience falls in ED visits of 5.3%, declining to above 2% between the 40th and 60th percentile, and are zero at the 70th distance percentile. The lower plot charts much weaker changes outside of clinic opening hours. There are signs of small (albeit generally insignificant) effects of around 1% for close locations, but for less proximate places coefficients are close to zero.[20]

The conceptual framework in Section 1.2 suggests walk-in clinics should divert patients with less serious medical problems from EDs. Most patients that are not admitted during an ED attendance likely fall into this category. These outpatient visits make up around three quarters of all ED visits in this setting. Column (b) of Fig. 7 plots the effect of walk-in access on these patients, corresponding to the third and fourth columns of Table A6. The pattern of effects is highly similar to the first column. During walk in hours effects in the first buffers are slightly larger and the distance decay is a little steeper, although these differences are not statistically distinguishable. The corollary is that around three quarters of the overall effect of services arises through diverting patients who would not be admitted through an ED, with the remainder of the effect coming through patients who would be admitted.

Hospital records also indicate how patients came to be at the ED. Column (c) of Fig. 7, corresponding to columns five and six of Table A6, tracks impacts on patients recorded as self-referring to the ED. This group represents around 60% all visits to EDs. The remainder are patients that ostensibly had less discretion in the location of their treatment. This is because they were referred to the ED from another source (most commonly a family doctor), or conveyed to the ED in an ambulance. As with the non-admitted group of patients the effects are qualitatively similar to the overall patterns shown in column (a), but here coefficients during clinic open times are roughly one to one and a half times as large. One possible explanation is that self-referred patients have less severe health needs which can be treated in lower acuity facilities like walk-ins more readily. This finds support in the data: only 12% of the self-referred group are admitted following their attendance compared to more than 40% of the other group.

3.7. Dimensions of access

What further dimensions of access drive diversion from EDs? This section aims to shed light on this question through a descriptive comparison of the impacts of walk-in services and extended hours practices (denoted PCPs in this section) opened under the *EAPMC* policy. As noted previously, PCPs are conventional primary care services that require patients to be registered to receive services. They offer extended opening relative to core primary care hours but operating hours fall short of the 7 day services at walk-in clinics. Making comparisons across these service-types can help to

[20] In Appendix Fig. A7 I show that effects are slightly larger at very short and very long distances for neighbourhoods closer to walk-ins than EDs.

Table 5
Dimensions of primary care access

	(1)	(2)	(3)	(4)
	———————— ED visits at any time ————————			Core hrs
p0-p25 WICs	−0.0242***		−0.0252***	−0.0431***
	(0.0066)		(0.0067)	(0.0082)
p25-p50 WICs	−0.0127***		−0.0117**	−0.0196***
	(0.0047)		(0.0047)	(0.0060)
p50-p75 WICs	0.0019		0.0022	0.0019
	(0.0031)		(0.0030)	(0.0040)
p0-p25 EAPCs		−0.0112**	−0.0133***	−0.0153**
		(0.0046)	(0.0046)	(0.0060)
p25-p50 EAPCs		−0.0003	0.0003	−0.0008
		(0.0038)	(0.0038)	(0.0044)
p50-p75 EAPCs		−0.0055**	−0.0038	−0.0015
		(0.0028)	(0.0028)	(0.0034)
Quarter-region FX	√	√	√	√
Year-distance buffer FX	√	√	√	√
Population controls	√	√	√	√
Observations	63864	63864	63864	63864
R-squared	0.868	0.868	0.868	0.769

Notes: Sample contains quarter-LSOA cells with 50 or more ED visits. Dependent variables are in logs. Columns 1–3 include ED visits taking place at any time, Core hrs in column 4 counts ED visits between 8.30am–6.30pm Monday through Friday. Standard errors in parentheses clustered at the MSOA level. ***p < 0.01, **p < 0.05, *p < 0.1.

ascertain how opening hours and the need to make an appointment condition the extent to which patients are diverted from EDs.

The PCPs opened under the *EAPMC* policy were located in areas of the country with the lowest concentration of family doctors. To ensure a like-for-like comparison samples underpinning all regressions in this section are restricted to neighbourhoods in administrative areas eligible for both types of *EAPMC* service. Regressions, reported in Table 5, first estimate the impacts of walk-in services and extended hours practices separately, then simultaneously in the third and fourth columns. Because of the narrower geographical sample these regressions differ in two ways to earlier specifications. First, they include region rather than labour-market trends here as there is insufficient variation to separately identify the latter from changes in primary care access driven by the policy reform. Second, because there are very few walk-in facilities co-located at EDs in this sample, all neighbourhoods in close proximity to such services are dropped throughout this section.

Before comparing service types I first use this sample to assess the robustness of prior results for walk-in services. The first column of Table 2 estimated the impact of walk-in clinics across the country as a whole. In Table 5 walk-in impacts are estimated in under-doctored areas of the country on their own (in column 1), and conditional on changes in regular primary care access (in column 3). The coefficients on the walk-in service variables across these three specifications are highly similar, giving reassurance that the omission of variables capturing access to regular primary care in earlier regressions is unlikely to be a major source of bias.

The third and fourth columns of Table 5 estimate the effect of both types of primary care service concurrently. The third column uses a dependent variable constructed from ED visits taking place at any time (including evenings and weekends), which facilitates a comparison of the *overall* effects of the two service-types in my data. Relative to the extended hours practices, walk-in services divert more patients and have effects over greater distances. Because the first two buffers contain a similar number of neighbourhoods, a comparison can be obtained by summing the coefficients across the first two buffers. This implies that walk-in clinics divert roughly three times as many patients from EDs as the extended hours practices. Annual diversion can be estimated by applying the coefficients to the mean number of ED and grossing up by the number of neighbourhoods (as in Section 3.4). Walk-in clinics divert 1033 patients per year from EDs whereas PCPs divert 372, or 661 fewer visits.

The final column of Table 5 compares effects on ED visits in core primary care hours: 8.30am and 6.30pm on Monday–Friday. During these hours, both types of service are open so any differences in diversion cannot be driven by opening hours. The mean number of ED visits taking place during these times is 60 (see Table 1), so that results imply that walk-in clinics divert roughly 752 ED trips whereas PCPs divert 184, or 562 fewer visits. Both service types thus appear to divert a significant proportion of patients outside core primary care opening hours, but more than 80% of the difference in the overall effects arise when both types of service are open. If *EAPMC* walk-in clinics and extended hours practices are similar on unobserved dimensions, these findings signal that the ability for patients to attend without registering or making an appointment may have a large bearing on the ED diversion.

Appendix Table A7 finally reports the impacts of access to primary care services inside and outside practice catchment boundaries during core primary care practice hours. In theory a patient living outside a practice's boundary cannot register for regular primary care services but can attend a walk-in clinics (where these exist) as a non-registered patient. In line with this prior, extended hours practices have zero impacts in neighbourhoods outside catchment boundaries. For walk-in clinics ED diversion occurs inside and outside boundaries but is systematically larger in neighbourhoods falling inside boundaries. Although I provide no direct tests, I speculate these patterns could reflect benefits from continuity of care or competition from walk-in clinics driving improvements in practices outside my sample.[21]

3.8. Further robustness checks and placebos

In all preceding estimations fixed effects partial out time-invariant unobservables at the neighbourhood level and region- or labour market-wide trends, while population counts control for demographic changes. Besides these controls, previous sections reported some natural robustness checks, for example by examining the impacts of services during service open or closed hours and inside or outside practice catchment boundaries. A number of further placebo and robustness checks lend further support to these results. In all cases I report graphical evidence, relegating associated regression outputs to Appendix Tables A9 and A10.

[21] The difference in overall effects between the service types could also be driven by differences in boundary sizes. In Table A8 I show that access boundaries are indeed much larger for walk-in services.

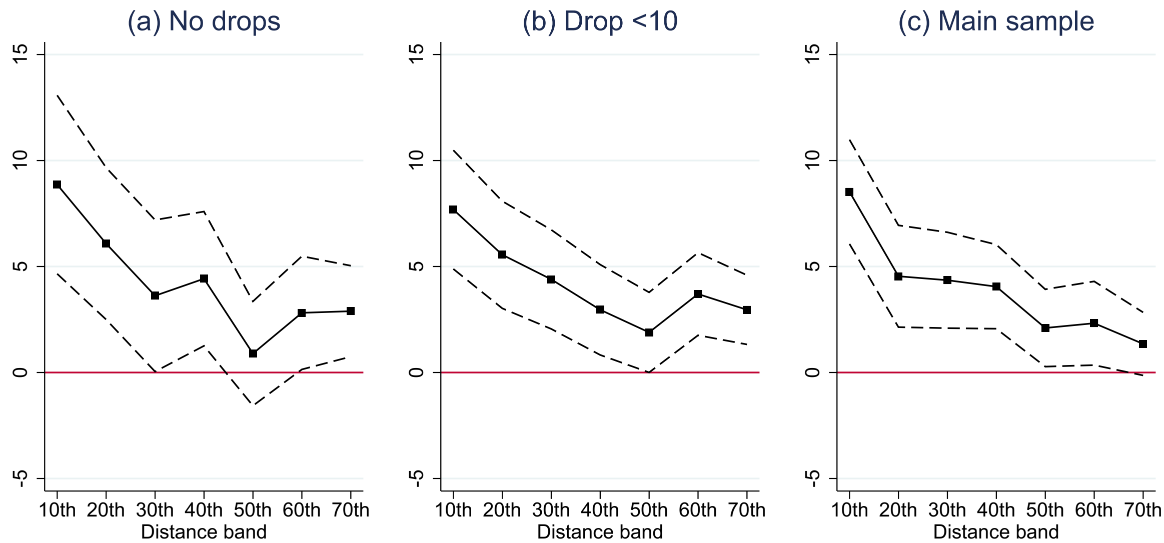

Fig. 8. Sample restriction robustness. *Notes*: All plots generated from regressions of log(self-referred ED visits) - log(other ED visits) on walk-in access changes. Left-most plot uses the full sample; middle plot drops neighbourhood-quarter cells with less than 10 visits in each group; right-most plot as main sample.

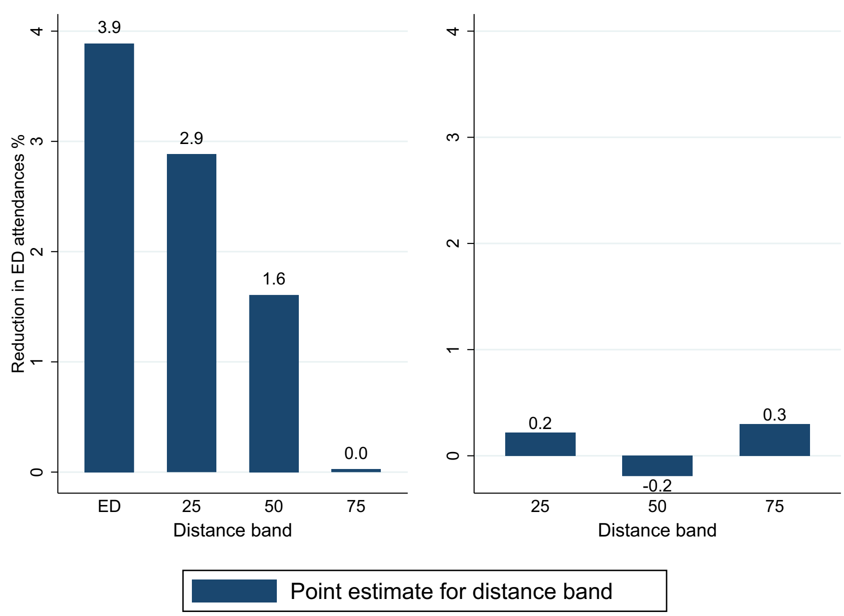

Fig. 9. Further walk-in clinic robustness. *Notes*: Figure shows robustness checks on results in Table 2. LHS plot refers to the same regression in column 1 of Table 2 but retains neighbourhoods for which the median distance buffer is between 3 and 5 km. RHS plot refers to placebo check using bogus changes in walk-in access in approximately 1,000 neighbourhoods that fall outside my main sample.

A first robustness check evaluates the sample restriction under which neighbourhood quarter cells with few ED visits were dropped. This restriction was adopted to avoid conflating changes in data reporting practices with genuine changes in ED volumes and to circumvent problems inherent in using count data. To test this strategy I re-estimate walk-in impacts under different samples but now using the difference between the logarithm of self-referred ED visits and the logarithm of ambulance or referred ED visits as the dependent variable. Changes in reporting should affect both of these patient groups symmetrically. The three plots in Fig. 8 demonstrate that distance decay of walk-in clinics on this measure are qualitatively similar when no cells are dropped (left-most plot), when cells with less than 10 ED visits are dropped (middle plot), and with the full sample restriction (right-most plot). Given earlier findings these estimates are driven largely by the self-referred patient group so it is reassuring that the patterns in all plots are broadly consistent with those in Fig. 7. More generally, differencing between these types of attendances partials out any unobserved time varying neighbourhood factors that affect both groups so provides a powerful check on earlier results.

Fig. 9 presents two more general checks on walk-in access effects. In the first, I re-run the first regression in Table 2 but now restricting the sample to places with median buffers that lie between the 25th (3 km) and 75th (5 km) percentiles of the buffer distribution, in which case buffers distances are very similar. Findings are robust to this change. The second check is a falsification test that exploits that some neighbourhoods outside my main sample host walk-in clinics established prior to 1 Apr 2008 (and as such do not figure in my earlier estimations). I generate pseudo changes in primary care access in these places during my sample frame by assigning the older clinics opening and closing dates matching a random *EAPMC* walk-in clinic from my main sample.

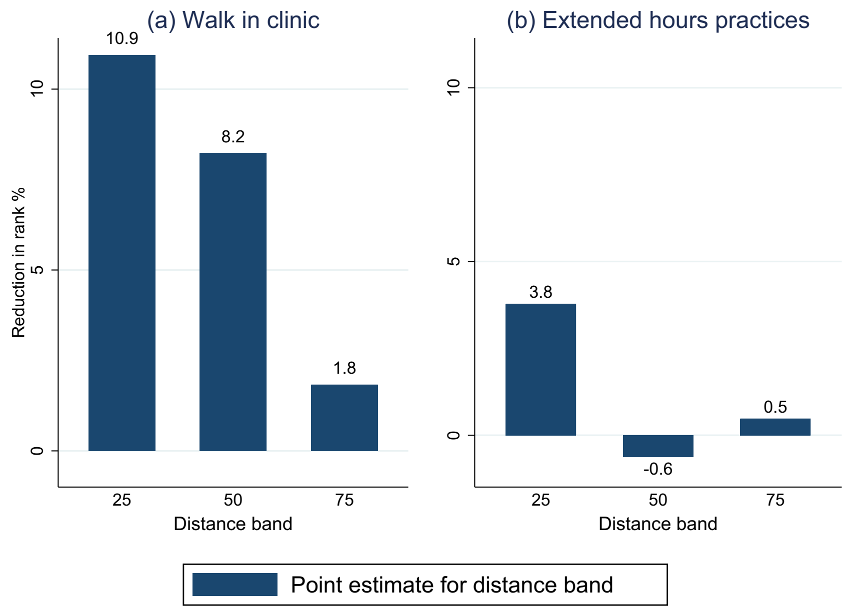

Fig. 10. Robustness to common labour market shocks. *Notes*: Dependant variable is log(rank) where rank is the neighbourhood rank in the count of ED visits within the labour-market distribution in a given quarter.

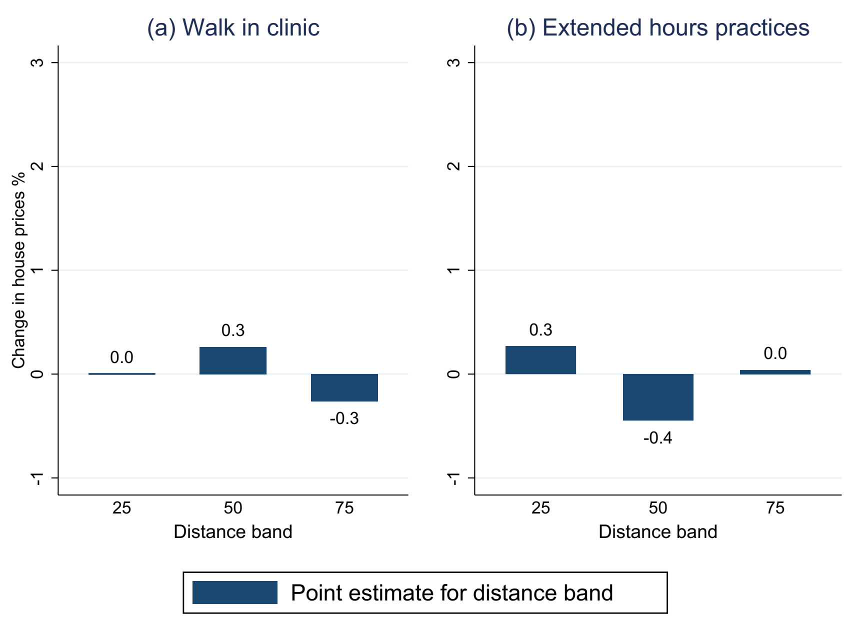

Fig. 11. Placebo: house prices. *Notes*: Dependant variable is log average house prices in the neighbourhood, computed from the Land Registry Price Paid dataset.

The right-side of Fig. 9 shows these bogus access changes have no effects on ED visits.

The walk-in clinic regressions in Section 3.1 control for shocks to labour markets through the inclusion of labour market-by-quarter interactions whereas regressions in Section 3.7 control only for less granular regional trends. The coefficients for walk-in services are similar across these specifications yet it remains possible that the latter estimates are partially driven by common shocks within labour markets. Fig. 10 follows the approach in Busso et al. (2013) by estimating the effect of primary care access on the neighbourhoods (log) rank position on ED visits within the labour-market distribution, where rank 1 is assigned to the neighbourhood with the lowest count of ED visits in the labour-market that quarter. The estimated pattern of effects is qualitatively similar to those in Table 5 albeit stronger for walk-in services relative to the extended hours primary care practices.

A final robustness check reflects the possibility that the *EAPMC* policy may be part of a wider set of interventions targeted to specific neighbourhoods such as localised employment schemes or neighbourhood regeneration. It is possible, albeit unlikely, that a combination of such policies have spatially decaying effects that are strongest at times when primary care facilities are open. Given that they are unobserved in my data such policies could confound estimates should they correlate with factors driving hospital utilisation and directly coincide with *EAPMC* service changes. Fig. 11 indicates that changes in access are uncorrelated with average house prices which goes some way to alleviating this concern.[22]

[22] In the last Column of Table A10, I also show that *EAPMC* services have no significant impact on ED visitors arriving by ambulance.

4. Conclusion

This paper examines a policy reform that introduced a substantial change in primary care access across England within a short time-frame. The reform is helpful because its implementation provides a source of plausibly exogenous variation, and is of particular interest because it created new primary care services which differ along several organisational dimensions. The first part of the analysis finds that access to convenient primary care services significantly reduces visits to hospital Emergency Departments, and documents a range of further findings that support the robustness of this result.

Parameter estimates imply that somewhere between 5 and 20% of patient visits to a walk-in facility substitute for a visit to an ED. The lower unit costs of care in the clinics relative to EDs is insufficient to offset the costs of the new utilisation, so that walk-in clinics imply a net increase in health care spending. A full assessment of the welfare implications of walk-in services lies outside the scope of this work. Shifting care outside of EDs is likely to be socially beneficial because of the lower costs of care in primary care settings. Further work would be needed to evaluate whether the social benefits of the substantial new utilisation of walk-in clinics, including any reassurance benefits to patients, outweigh the social costs of providing the services.

Subsequent sections of this article then distinguish empirically between four aspects of primary care access: proximity to services, convenience of opening hours, the need to make an appointment, and eligibility to receive care. Estimates indicate that two convenience dimensions of access — proximity and the ability to attend without appointment — are paramount in determining the extent to which primary care services divert patients from hospitals. Given that the private costs of distance and making appointments are likely to be small, these results could suggest that psychological factors influence how individuals choose to obtain treatment, which would tally with recent evidence showing that hassle factors can prove to be an important barrier to participation decisions. However, at this stage the role played by unobserved and possibly non-linear costs is unknown. Future work that provides tighter evidence on the importance of hassle and other behavioural hazards in the demand for health care services, and that characterises the effects of hassle-induced behaviours on subsequent health outcomes, would be valuable.

Appendix A. Supplementary Data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jhealeco.2019.102242.

References

Appleby, J., 2013. (2013, April). Are Accident and Emergency Attendances Increasing? The King's Fund blog, 29 April.

Berchet, C., 2015. Emergency Care Services: Trends, Drivers and Interventions to Manage the Demand. OECD Health Working Papers, No. 83.

Bertrand, M., Mullainathan, S., Shafir, E., 2006. Behavioral economics and marketing in aid of decision making among the poor. J. Public Policy Market. 25 (1), 8–23.

Buchmueller, T.C., Jacobson, M., Wold, C., 2006. How far to the hospital?: The effect of hospital closures on access to care. J. Health Econ. 25 (4), 740–761.

Busso, M., Gregory, J., Kline, P., 2013. Assessing the incidence and efficiency of a prominent place based policy. Am. Econ. Rev. 103 (2), 897–947.

Chen, C., Scheffler, G., Chandra, A., 2011. Massachusetts' Health Care Reform and Emergency Department Utilization. N. Engl. J. Med. 365 (12), e25.

Conley, T.G., 1999. Gmm estimation with cross sectional dependence. J. Economet. 92 (1), 1–45.

Cowling, T., Majeed, F., Harris, M., 2018. Patient experience of general practice and use of emergency hospital services in England: regression analysis of national cross-sectional time series data. BMJ Qual. Safety 27, 643–654.

Cowling, T.E., Cecil, E.V., Soljak, M.A., Lee, J.T., Millett, C., Majeed, A., Wachter, R.M., Harris, M.J., 2013. Access to Primary Care and Visits to Emergency Departments in England: A Cross-Sectional, Population-Based Study. PLoS ONE 8 (6).

Cowling, T.E., Harris, M.J., Majeed, A., 2015. Evidence and Rhetoric about Access to UK Primary Care. Br. Med. J., 350.

Currie, J., Reagan, P.B., 2003. Distance to Hospital and Children's Use of Preventive Care: Is Being Closer Better, and for Whom? Econ. Inq. 41 (3), 378–391.

Department of Health, 2007. Delivering Equitable Access to Primary Medial Care: Local procurements for GP practices and Health Centres. Letter to SHA Directors of Commissioning, December 21.

Dolton, P., Pathania, V., 2016. Can Increased Primary Care Access Reduce Demand for Emergency Care? Evidence from England's 7-day GP Opening. J. Health Econ. 49, 193–208.

Finkelstein, A.N., Taubman, S.L., Allen, H.L., Wright, B.J., Baicker, K., 2016. Effect of Medicaid Coverage on ED use-Further Evidence from Oregon's Experiment. N. Engl. J. Med. 375 (16), 1505–1507.

Goodman-Bacon, A., 2018. Difference-in-differences with variation in treatment timing. Technical report, National Bureau of Economic Research.

Hiscock, R., Pearce, J., Blakely, T., Witten, K., 2008. Is Neighborhood Access to Health Care Provision Associated with Individual-Level Utilization and Satisfaction? Health Serv. Res. 43 (6), 2183–2200.

HSCIC, 2016. General Practice Trends in the UK to 2015. Health and Social Care Information Centre.

Ismail, S.A., Gibbons, D.C., Gnani, S., 2013. Reducing Inappropriate Accident and Emergency Department Attendances. Br. J. Gen. Pract. 63 (617), e813–e820.

Kahn, B.E., Luce, M.F., 2006. Repeated-adherence protection model:"i'm ok, and it's a hassle". J. Public Policy Market. 25 (1), 79–89.

Lippi Bruni, M., Mammi, I., Ugolini, C., 2016. Does the extension of primary care practice opening hours reduce the use of emergency services? J. Health Econ. 50, 144–155.

Mehrotra, A., Liu, H., Adams, J.L., Wang, M.C., Lave, J.R., Thygeson, N.M., Solberg, L.I., McGlynn, E.A., 2009. Comparing Costs and Quality of Care at Retail Clinics with that of other Medical Settings for 3 Common Illnesses. Ann. Intern. Med. 151 (5), 321–328.

Miller, S., 2012. The Effect of Insurance on Emergency Room Visits: An Analysis of the 2006 Massachusetts Health Reform. J. Public Econ. 96 (11), 893–908.

Millman, M., et al., 1993. Access to Health Care in America. National Academies Press.

Monitor, 2014. Walk-in Centre Review. Final report and recommendations.

Morley, C., Unwin, M., Peterson, G.M., Stankovich, J., Kinsman, L., 2018. Emergency department crowding: A systematic review of causes, consequences and solutions. PloS One 13 (8), e0203316.

National Center for Health Statistics, 2013. Health, United States, 2012: With a Special Feature on Emergency Care. Report.

Pinchbeck, T., 2014. Walk this way: Estimating impacts of Walk in Centres at Hospital Emergency Departments in the English National Health Service. Working paper, Spatial Economics Research Centre, LSE.

Pines, J.M., Hilton, J.A., Weber, E.J., Alkemade, A.J., Al Shabanah, H., Anderson, P.D., Bernhard, M., Bertini, A., Gries, A., Ferrandiz, S., et al., 2011. International Perspectives on Emergency Department Crowding. Acad. Emerg. Med. 18 (12), 1358–1370.

Selby, J.V., Fireman, B.H., Swain, B.E., 1996. Effect of a copayment on use of the emergency department in a health maintenance organization. N. Engl. J. Med. 334 (10), 635–642.

Taubman, S.L., Allen, H.L., Wright, B.J., Baicker, K., Finkelstein, A.N., 2014. Medicaid Increases Emergency Department Use: Evidence from Oregon's Health Insurance Experiment. Science 343 (6168), 263–268.

Van Dort, B.E., Moos, R.H., 1976. Distance and the Utilization of a Student Health Center. J. Am. Coll. Health Assoc. 24 (3), 159–162.

Weinick, R.M., Burns, R.M., Mehrotra, A., 2010. Many Emergency Department Visits Could be Managed at Urgent Care Centers and Retail clinics. Health Aff. (Millwood) 29 (9), 1630–1636.

Whittaker, W., Anselmi, L., Kristensen, S.R., Lau, Y.-S., Bailey, S., Bower, P., Checkland, K., Elvey, R., Rothwell, K., Stokes, J., et al., 2016. Associations between Extending Access to Primary Care and Emergency Department Visits: A Difference-in-Differences Analysis. PLoS Med. 13 (9), e1002113.