The Health Effects of Early Interventions: Evidence from Sure Start

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Abstract

Early intervention programmes which deliver educational, social, and health services are widely recognised as a potentially effective way of promoting better health and preventing the development of costly health problems. While existing evidence demonstrates the potential benefits of targeted and often intensive interventions, knowledge of what works at scale is much scarcer. We provide the first evidence for a fully universal, scaled-up intervention which brings together education, employment, and health services for children under five and their families - Sure Start (SS) in England. Using the staggered introduction and expansion of SS between 1999 and 2010, we trace out its impacts on health from infancy through to age 15. SS affects substantially hospitalizations through several channels. For infants and toddlers it raises hospital admissions, mainly driven by respiratory illnesses and infections. Once children have aged out of the program, there is a reduction in hospitalizations for these causes, consistent with a strengthening immune system. Among adolescents, the program reduces injury-related hospitalizations, potentially reflecting safer environments or behaviors. Rather than fading out, the overall effect on hospitalizations grows larger as children age and remains substantial during adolescence, when greater access to the program reduces the probability of a hospitalization by 8% to 14%. These effects are equivalent to about 11,500 averted hospitalizations per year. The impacts are concentrated in the poorest neighborhoods and boys.

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1 Introduction

Children's early exposure to poor, unsafe, and non-stimulating environments can have long-lasting impacts on their health and many other dimensions of their development (Almond, Currie and Duque, 2018; Shonkoff, Boyce and McEwen, 2009). Early childhood interventions aimed at improving healthy behaviours or fostering supportive and nurturing environments have potential to mitigate the impact of children's early exposure to adverse conditions and produce long-lasting benefits for health (D'Onise, McDermott and Lynch, 2010; Conti, Mason and Poupakis, 2019). In doing so, these interventions can be a cost-effective way of preventing disease, with benefits for both individual welfare and the public purse (García, Heckman and Ziff, 2018).

Most of this evidence, however, comes from randomized controlled trials of small-scale interventions in the U.S. (Campbell et al., 2014). Those are characterized by controlled delivery of intensive services and a very disadvantaged client base. Moreover, several studies have found that the benefits of these interventions are frequently mediated through increased take-up of private health insurance (Conti, Heckman and Pinto, 2016; Muennig et al., 2009). This raises important questions as to whether this type of early integrated interventions can retain any of its effectiveness when implemented at scale and in contexts with free healthcare provision.

This paper addresses these questions by evaluating the health benefits of Sure Start, a universal intervention that offered families of under-fives parenting support, childcare, healthcare and parental employment support in England, a context with free universal healthcare. Remarkably, although the program has been in place in England since 1999, its impacts on children's outcomes have never been robustly evaluated. This paper shows that the program had significant health benefits for children that lasted well beyond the years during which children and their families were allowed to access the services.

Although Sure Start was eventually rolled out to serve most of the population, the program was modelled after Head Start, one of the longest-running and largest-scale early intervention programmes in the U.S. Like Head Start, it offered a variety of services, not only including childcare and early education, but also parenting support, health services, and even parental employment support. The difference in the two initiative lies mostly in its scope: Head Start eligible children are from families with income below the poverty line, while Sure Start is universal. While the multi-faceted nature of these programmes is perhaps one reason behind their success, this aspect creates particular challenges for their evaluation and the interpretation of findings. In particular, the fact that these programs offer a bundle of services means health could be affected through a variety of channels, sometimes creating impacts of different directions in the short and the long run. For example, by bringing children more into contact with others, large-scale early years programs may increase the risk of infection in the short run while improving the immune system and so reducing more serious infections in the medium run. Another possibility is that the program could have short-term impacts on non-health outcomes of parents and children (for example, parenting or behavior) which in turn drive health impacts at older ages. To fully account for the impacts of early years programs and to understand the channels through which they come about,

it is therefore imperative to trace out the trajectory of their impacts from infancy and for as long as possible. The evidence on the medium-term impacts of early childhood programs is notoriously scarce, and our paper is one of the first to present evidence of the impact of an early childhood intervention for what Almond, Currie and Duque (2018) have referred to as the 'missing middle years'.

Exploiting variation in programme exposure induced by the 11-year rollout of the program across areas of England, we trace out the impact of the program on hospitalisations of children from infancy up to the age of 15. We find that the program led to an increase in hospitalizations of infants and toddlers (mainly related to infectious illnesses). The overall impact on hospitalizations becomes steadily more negative throughout later childhood, and at ages 11 through 15 adolescents who were more intensively exposed to Sure Start in their early years see significant and substantial decreases in their probability of being hospitalized. The impacts are concentrated in the poorest neighbourhoods and boys.

These findings suggest that different mechanisms are more prevalent at different ages. At younger ages, when children are directly exposed to Sure Start, the increased exposure to infections in group-based settings is important. Infection-related hospitalizations then fall (though not statistically significantly) during the first few years of school, consistent with stronger immune systems. From age 3, we see statistically significant falls in hospitalizations for external causes (predominantly injuries and poisonings); among young children, these causes are most closely related parental information about child safety and child health. A reduction in maltreatment may be an additional channel for these effects, but hospitalizations for causes linked to maltreatment are relatively rare, and so we do not detect significant effects here. In mid-adolescence we see impacts on several different causes, which suggests that several mechanisms may drive these longer-term effects. In particular, reductions in external admissions (now mainly due to injuries) could reflect less aggressive or risky behavior, while falls in hospitalizations for preventable causes may be related to better health due to earlier detection of problems. The multifaceted nature of the Sure Start intervention means that some of the program's impacts on health could come about indirectly as a result of parts of the intervention focused on improving parenting, the home environment, or children's behavioral and cognitive development.

The potential for health impacts in the medium run to arise from multifaceted early years interventions has been seen with Head Start, which was the initial inspiration for Sure Start. The evidence on Head Start has been mixed, especially with regard to how long its benefits persist. The Head Start Impact Study RCT finds short-term benefits for children's cognitive and socio-emotional development, health status, health service use, and health insurance coverage which all fade out by the end of third grade (DHHS, 2010, 2012). However, more recent research has shown that the fade out of impacts might be partly explained by failing to account for the substitution between different types of public services (Kline and Walters, 2016) or by substantial heterogeneity in the effectiveness of Head Start centres (Walters, 2015). As well, other studies have found medium-term benefits for mortality due to conditions that could plausibly be affected

by the programme (Ludwig and Miller, 2007); obesity (Frisvold and Lumeng, 2011; Carneiro and Ginja, 2014); and depression (Carneiro and Ginja, 2014). In the longer run, Head Start has benefits for adult earnings, education, health insurance coverage, and risky behaviors such as smoking (Thompson, 2018; Bailey, Sun and Timpe, 2020).

Our paper contributes to the international evidence base on the impacts of early years interventions. Although there is a rich evidence base for programs such as the Carolina Abecedarian (ABC) and Perry Preschool Program (PPP), as well as for Head Start, these programs are all targeted at highly disadvantaged populations in contexts where the existing social safety net is relatively weak. For example, evaluations of all three programs found increases in health insurance coverage - a channel which will not be available in countries with universal health insurance, which is common today in many high-income countries (and is increasingly debated in the U.S. as well).

So far, there has been less evidence to understand how fully universal early intervention programs affect child outcomes. A recent strand of work has analyzed long-term impacts of programmes dating back to the origins of the welfare state in Scandinavian countries. Bhalotra, Karlsson and Nilsson (2017) study the introduction of universal post-natal health care, information, and support in the 1930s in Sweden. They find that the programme led to a 23% reduction in infant mortality and a 6.5% increase in the probability of surviving to age 75. Bütikofer and Salvanes (2020) also evaluate the very long-run impacts of a 1930s programme of mother and child health centres and post-natal home visiting in Norway. They find that access to the child visits during the first year of life increased schooling and earnings by age 30, it reduced the incidence of obesity and cardiac events in men, and that the effects were stronger for centres that offered a wider variety of health services. These evaluations of universal early interventions in Scandinavian countries have focused on the impacts of universal public health interventions introduced in the first half of the 20th century; like with the literature on Head Start, ABC and PPP, the provision of public services in this context was quite different from what it is today (Bhalotra, Karlsson and Nilsson, 2017; Hjort, Sølvsten and Wüst, 2017; Bütikofer and Salvanes, 2020).

Available evidence from contemporaneous programmes in Europe suggests that universal early years interventions - particularly expanding access to pre-school education and formal childcare - can still benefit child cognitive and socio-emotional development in modern contexts with universal and free healthcare (Felfe and Lalive, 2018; Havnes and Mogstad, 2011; Black et al., 2014).[1] The evidence on the health benefits of such programs is just starting to emerge, with the handful of papers available again focusing on the Scandinavian countries. Siflinger and van den Berg (2018), for example, study a reform that provided universal childcare in one region of Sweden. Using hospital registry data for inpatient and outpatient visits by children aged 1 to 7, they find that decreasing the price for childcare at age 1 decreases the number of medical visits at ages 4-5 and 6-7. They interpret these findings as the result of an income effect, where families that

[1] By contrast, Baker, Gruber and Milligan (2008) find negative effects for children of universally subsidised childcare in Quebec, Canada.

pay less for childcare have more resources available for the consumption of other inputs into early child health, such as more nutritious foods.

Our paper contributes to the existing literature by providing the first evaluation of a fully universal, scaled-up, integrated program at a national level, in a contemporaneous context with universal health coverage. In common with much of the rest of the literature, we evaluate an early years program that incorporates health services but is not solely targeted at improving health outcomes. Our results provide evidence that a fully universal intervention that is much less intensive than 'model' programs like PPP and ABC can deliver benefits even in an institutional context with free at the point of use healthcare and pre-existing universal home-visiting services. Our focus on early health outcomes that are strongly linked to health status later in life complements and extends the existing evidence base for early intervention, and contributes to filling the gap about mid-term impacts of childhood programmes.

2 History and Expected Effects of Sure Start

Sure Start is one of the largest and most ambitious government initiatives ever implemented in England for children under the age of 5. In this section, we first discuss the history of the program and the official factors influencing its rollout, which underpins our empirical strategy. We then turn to a description of the services that Sure Start offered, before discussing the potential channels through which Sure Start could influence children's hospitalizations. We also discuss briefly the findings of two government studies of Sure Start.

2.1 The history and rollout of Sure Start

The program was first introduced in 1999 as Sure Start Local Programs (SSLPs), targeted at highly disadvantaged neighbourhoods. The programme was conceived as an area-based intervention whose services would be available to all families in the neighbourhood of the centre, with no means-testing (Melhuish et al., 2008). Catchment areas were intended to be small ("perhaps one to two miles in radius in urban areas"), to be walkable, and to "make sense to the local community" (DfEE, 1999). SSLPs were designed and encouraged to be responsive to local needs, and partnerships and programme managers had wide latitude in the services they offered. Sure Start was given a budget of £450 million over the period 1999-2002 to set up 250 projects, anticipated to reach 150,000 children over seven to ten years (Pugh and Duffy, 2010).

While Sure Start was intended to be a flexible initiative that would respond to local needs, central government had overall control of its funding, and therefore input into how it was allocated between local areas. These functions were carried out by the Sure Start Unit (SSU), a joint responsibility of the education and health departments, which was responsible for the program's operation in England. The SSU's initial role included identifying eligible local authorities, administering the application process, setting national targets, coordinating support for local programmes, and monitoring the performance of the overall Sure Start initiative.

To decide which areas would get funding to open a Sure Start centre, the SSU developed a set of guidelines for the rollout. The most important of these was that disadvantaged communities should have priority in opening Sure Start facilities, but other measures were used as well.[2] Based on these characteristics, the SSU identified 60 'trailblazer' districts that would be invited to submit project proposals. All 60 trailblazers submitted a proposal, and on 9 April 1999 the government announced the first 21 projects to go ahead, with a further 30 announced in July. By November of that year, 15 had opened their doors as Sure Start Local Programmes, across almost all regions of the country (DfEE, 1999). The public reception of SSLPs was extremely positive.[3] Despite the opposition of the senior civil servants running the program, in 2000 the government announced that it would more than double the program target, from 250 SSLPs to 530 (Eisenstadt, 2011). In total, the government opened 524 Local Programmes.

In 2003, the government announced that, rather than being phased out after 10 years, Sure Start would be universalized and supported indefinitely. Under the new strategy, the SSLPs and some other area-based early years services would transition into Sure Start Children's Centres (SSCCs), and responsibility for the programme would be transferred to local governments (Lewis (2011), p. 76). The following year, the 10-Year Strategy for Childcare pledged "a children's centre in every community" by 2010, transforming what had so far been a highly targeted initiative into a universal program. The budget for Sure Start eventually rose from about £500m a year to £1.8 billion (in 2018-19 prices) at its peak in 2009-10, about a third of overall spending on programmes for the under-5s in England.[4]

The Department for Education and Skills (DfES) had overall responsibility for the target to establish 3,500 children's centres by 2010. However, it delegated responsibility to local authorities for planning and managing the children's centre program in their area.[5] This meant that, instead of the central government funding individual programmes through the SSU, LAs received funding (based on their number of children under five and the level of deprivation) to allocate between Sure Start centres within their jurisdiction. Local authorities were responsible for delivering their share of the children's centres target as agreed with the DfES but had responsibility for choosing the specific locations of their Children's Centres (Lewis, 2011).

The period between 2005 and 2010 saw the rapid increase in the number of Sure Start centres (see Figure 1), at a pace which was described as 'cruel' and 'demanding' (House of Commons, 2010). This expansion proceeded in broadly three phases: 'Phase 1' centres (opened approxi-

[2] In particular, the initial 60 'trailblazer' districts were selected based on the 1998 Index of Local Deprivation, augmented with low birth weight and teen pregnancy indicators; the set of trailblazers was also chosen to offer a good spread of different types of areas around the country (DfEE, 1999).

[3] Director of the Sure Start Unit Naomi Eisenstadt reports that "The only complaint that [the government was] hearing from Members of Parliament about Sure Start was, "Why can't my constituency have one?" (Eisenstadt, 2011, p.48).

[4] In the decade since, the program has experienced large funding cuts of about two-thirds by 2018-19, leading some centres to consolidate and many to close.

[5] The delegation to local authorities was formalized in law by the 2006 Childcare Act, which created legal duties for local authorities to establish and run children's centres. In 2009 further legislation put all children's centres under local authority control.

mately between 2004 and 2006) were intended to offer full coverage of the 20% most disadvantaged neighborhoods, and mostly grew out of existing Local Programmes or Early Excellence Centres. Between 2006 and 2008, 'Phase 2' of the Children's Centre rollout opened centres covering the 30% most disadvantaged neighborhoods, with some centres outside these areas. The final phase of the SSCC rollout saw the universalization of the program, with full coverage of the remaining 70% of areas.

The maps in Figure 2 showing the location of Sure Start centres in 2000, 2004 (at the start of the Children's Centre rollout), 2006 (at the start of Phase 2 of the Children's Centre rollout) and 2008 (at the start of Phase 3), suggest that these deprivation-based phases were taken seriously. During the first years of the program rollout, local programmes were predominantly opened in the most disadvantaged local authorities (shaded in darker green in the figure). By the time all of the Local Programmes had been opened, in 2004, the focus on relatively poor areas was even more obvious. The maps also show a 'filling-in' pattern with facilities spreading relatively (though by no means perfectly) evenly across the country and intensifying over time, rather than starting in one region and spreading out from there. This is consistent with policymakers' aim to ensure a good spread of Sure Start around the country.

In this paper, we evaluate the impacts of the program as it expanded. This is for two reasons. First, the core set of services it offered, as well as the age group it targeted, were much more clearly defined until 2010 than in the following decade. Second, as we will discuss in section 3, the openings of centres during the program rollout, which underlie our empirical strategy, are more arguably exogenous than their closures. In what follows, we describe the services that the program offered throughout its expansion and the population it offered them to. We then discuss the potential mechanisms through which the program could have affected children's hospital admissions and the nature of the effects we expect to detect in the data.

2.2 What services did Sure Start offer and to whom?

As the name suggests, the original Sure Start Local Programmes were designed and encouraged to be responsive to local needs, and partnerships and programme managers had wide latitude in the services they offered. However, all SSLPs were required to deliver services that would meet key principles, including: integrating services; involving parents; avoiding stigma; targeting specific objectives; and evaluating progress towards those goals (DfEE, 1999). Meadows (2011) report the expenditure shares of the different activities in 2003-2005. Just under a third (29%) of SSLPs expenditure was incurred on play, learning and childcare activities (excluding early years education for three- and four-years old, which was funded separately); a fifth of expenditure (19%) went on support for parents, and the same share on community healthcare – this was for provision not available through local mainstream NHS health services (e.g. postnatal depression services). A sixth of spending (17%) went on outreach and home visiting; support for children with special needs then accounted for 7% of the spending.

Children's Centres had less autonomy than Local Programmes in choosing the services they

would deliver. The SSCCs had an increased focus on improving parental employment outcomes, both through integration with the new JobCentrePlus initiative and through offering free childcare under a national programme to improve parents' access to the labour market (Lewis, 2011). Centres in the 30% most deprived areas (Phase 1 and Phase 2 SSCCs) were required to provide the 'Core Offer', which consisted of integrated early education and childcare; parental outreach; family and parenting support; child and family health services; and links with JobCentrePlus (Lewis, 2011). For centres serving the 70% least disadvantaged areas, services were less intensive, but all centres had to offer activities for children and links to JobCentrePlus, and were expected to develop health and outreach services. All centres were expected to open 5 days a week, 10 hours a day, 48 weeks a year. Between 2011 and 2013, the "core offer" was reformed into a less prescriptive "core purpose". This focused more on the outcomes the centres wanted to achieve for young children and their families, and gave LAs the freedom to develop Children's Centres in a way which met local needs.

Little historical information is available to provide a detailed account of the services offered by each and every centre over their existence. In 2011, Goff and Chu (2013) surveyed 121 children's centres and report that they offered a range of services, some focusing on the child, some on the adults, and others on the community. Among a list of 50 services that the 121 CCs were asked during the fieldwork, they reported offering an average of 28, with a range between 13 and 42. Over 90% of the 121 CCs offered stay and play, evidence-based parenting programmes (especially Incredible Years, Triple P and Family Nurse Partnership), early learning and childcare, developing/supporting volunteers, breastfeeding support. Other frequently offered services included midwife and health visitor clinics; sports and exercise for babies and children; advice on accessing welfare benefits, housing, and managing debt; adult learning; parent forums; and antenatal and postnatal classes.

2.3 The expected effects of Sure Start Centers on children's hospitalization rates

From its inception, Sure Start was created as a key mechanism for improving outcomes for young children, thereby reducing inequalities and helping to bring an end to child poverty. Its approach was based on the recognition that the needs of families, particularly disadvantaged families, often span many traditional areas of support and that child well-being is multi-dimensional. Thus, the program offered a bundle of services that could have affected children's health, cognitive and socio-emotional development, through a variety of direct and indirect channels.

As mentioned in the previous section, one component of both the SSLPs and the SSCCs was the direct provision of health services. Successive sets of statutory guidance on health service availability in Children's Centres, for example, suggested that SSCCs should offer: antenatal education; appropriate maternity services (including postnatal support); breastfeeding promotion and support; immunisation promotion; advice on accident and injury prevention; advice on obesity, diet and nutrition; promotion of active play; support for mental health and for families with disabilities; speech and language development; and links to specific early intervention programmes (e.g.

DfES, 2003; DfE, 2010). The ECCE showed that 92 of the 121 centres evaluated offered the services of a health visitor, but not direct medical services. Hence, Sure Start might be expected to improve children's overall health status through an increase in health-promoting activities (because of improved information), a greater willingness to use health services (e.g. due to lower stigma or increased perceived benefits; see Currie (2006)) and/or better screening for conditions that might benefit from treatment. If this is the case, we would expect to see an either negative or positive impact of Sure Start in healthcare usage (hospitalisations), but a negative effect on hospitalisations in the medium-term.

In addition to the health-promoting activities undertaken in the Sure Start centres, there are also a number of indirect channels through which Sure Start could affect children's health. One potentially important channel is through parental employment. Sure Start provided parental job-search assistance and job-related training. A higher family income (resulting from new employment or longer hours) would allow parents to buy more and/or better food or healthcare for their children (Carneiro and Ginja, 2016). At the same time, parents who work more are likely to have less time to spend with their children, potentially affecting time available for children to make use health services during childhood.

Greater parental employment and greater access to childcare services through the Sure Start centres might also change the types of environments that children are exposed to. As parents work more, children tend to spend more time in formal or informal childcare. Depending on the quality of non-parental care the child is exposed to when the parent is at work, the child may now spend more time in an environment that is more or less safe than when she is at home (Bernal and Keane, 2011).

Childcare can have a direct health effect on children's immune systems; spending time around other children means that the child might be exposed to a greater variety of illnesses. In the short run, this would tend to be bad for health, since the child might be more likely to get sick. But early exposure to a variety of pathogens also helps to build up the immune system, which might have benefits in the longer run (Henderson et al., 1979; Siflinger and van den Berg, 2018). If this were the case, we would expect to see hospitalisations for infection to increase in the short-term, but go down in the medium-term. We would not expect these negative effects to be particularly long-lasting however, given that all children go to school around the age of 4 in England and we would expect the immunity of those who were not in collective care earlier to catch up once they start school.

In addition to changing the types of environments that children spend time in, Sure Start might also change their quality. In particular, the centres might have helped parents to provide a better home environment for their children, in turn changing children's outcomes and behaviours. For example, many Sure Start Children's Centres offered parenting programmes such as Incredible Years, which aim to advance children's social and emotional behaviour. A calmer and less fidgety child may be less prone to injure herself and may be easier for parents to care for, thus potentially reducing the chance of parental neglect and maltreatment. Sure Start might also improve the

quality of the home environment indirectly – for example, by providing parental mental health support.

We are not able to robustly test for all of these channels. However, we employ two strategies to help understand which of the mechanisms appear more likely. First, we carefully examine hospitalisations for different ages which allows to separate impacts during eligibility period (ie, up to age 5) and post-eligibility period. We expect that better screening activities may have an immediate impact in the use of health services for children resulting in increased hospital admissions, while changes in home environment may have lasting effects which will go beyond Sure Start age. Second, we study the impacts on admissions due to specific causes (see section 4). For example, by studying hospitalisations related to infections, we can analyse whether the pattern of effects that we see is consistent with the immunisation hypothesis.

2.4 Previous studies of the effects of Sure Start Centres

Although Sure Start has been in place since 1999, its causal impacts on children's outcomes have not yet been robustly evaluated. Sure Start was first evaluated by the National Evaluation of Sure Start (NESS) project, which collected data on children living in neighbourhoods served by the first phase of Sure Start. These children were compared to others surveyed in an earlier national survey who lived in areas not served by the programme. The NESS evaluation finds an increase in parent-reported hospitalisations at 9 months, an increased prevalence of immunisations and a reduced probability of accidental injuries at age 3, and lower Body Mass Index (BMI) and better parent-reported health status by age 5 for children living in the Sure Start neighbourhoods (NESS, 2005, 2008, 2010). However, the authors acknowledge that the non-random rollout of Sure Start, which was initially targeted at the most deprived areas, and the differences in timing of the outcome data between treated and control children are limitations of their approach.

Following NESS, the next phase of Sure Start was evaluated by the Evaluation of Children's Centres in England (ECCE). This study collected detailed and extensive data on a subsample of centres and their users, and estimated impacts of Sure Start by comparing the outcomes of children who use the services with varying frequency. The authors found no significant effects of visiting Sure Start centres on child health status, but some impact of using specific Sure Start services - health visitors and formal childcare - on the probability of changing health status (ECCE, 2015). Our study improves upon these two evaluations of the programme on two dimensions. First, we use for the first time data on the exact location and opening date of each of the approximately 3,500 Sure Start facilities in England, which combined with high-quality administrative data allow to probe the validity of our empirical strategy and obtain estimates of the causal impact the programme. Second, we are able to trace-out impacts since infancy and up age 15.

3 Data and Research Design: Using the rollout of Sure Start Centers to quantify their effects on hospitalizations

3.1 Data

Our main results are obtained using a large administrative dataset to examine the impacts of Sure Start on child health outcomes – more specifically, on the administrative data on all hospital admissions to public hospitals in England (which account for virtually all hospitalizations). We complement these data with a panel of data containing detailed characteristics of LAs in England and, crucially, with a data set containing the exact date of opening of all SS centres. We discuss this latter dataset first.

Data on Sure Start facilities Our main source of information about Sure Start is a unique dataset, provided to us by the Department for Education (DfE), containing the postcode and date of opening of each Sure Start Local Programme and Children's Centre since the beginning of the roll-out.[6] We map the postcodes of each centre to its local authority district (LAD/LA) to construct a monthly panel of the number of centres open in each LA.[7] We only use information up until 2010, the period in which Sure Start was expanding as most closures took place after 2014 and to avoid measurement error related to post-2010 centre closures.[8]

Based on this set of centres, we construct a measure of the coverage of Sure Start for each month as the number of open centres in the local authority per thousand children aged 0-4. We focus on this measure of 'coverage' rather than distance-based measures for two reasons. First, Sure Start centres within local authorities were encouraged to operate as part of a network, and families were encouraged to attend different centers on different days to attend the programming that was most relevant to them. Using an LA-based measure of coverage does a better job of capturing these networks than simply using the distance to the nearest center. Second, we do not observe precise postcode information in the HES data, so measures based on distance from the home will be subject to some measurement error. The final step in constructing our treatment variable is to average these monthly measures of coverage: for every month and year of birth, we then average coverage over the first 60 months of life (ages 0 through 4), or - for outcomes measured before age 5 - over the months between birth and measurement.

[6] To be precise, we define a centre's opening date as the date it was designed as children's centre by the Department of Education. At that point, the children's centre did not always already offer all the services included in its core offer. This usually happened about a year after the date of designation.

[7] We do this incorporating both Local Programmes and Children's Centres. While the opening dates of all centres are precisely known, pooling in this way requires us to make an assumption about the closure dates of SSLPs. Since over 90% of SSLPs had transitioned into Children's Centres by 2006 (NAO (2006), p.9), we assume that (a) any Local Programme that shares a postcode with a Children's Centre closed at the same time as the associated Children's Centre opened; and (b) all other Local Programmes closed in December 2006.

[8] While we observe all centre opening dates precisely, we have only incomplete information about centre closures. We have this data only for Children's Centres, and it indicates only when centres were formally closed. In practice, many local authorities chose to 'hollow out' services rather than close centres, in some cases to as little as half a day per week. Other LAs chose to close some of their centres but deliver a full set of services through others.

Hospital Episode Statistics To study the impact on hospitalizations, we use the Hospital Episode Statistics (HES), an administrative dataset tracking the universe of patients using public English hospitals. Data on inpatient admissions has been collected since 1997 and up to 2017 and tracks all hospital admissions, providing information on the admission, discharge, clinical diagnoses (up to 20 for each patient), and demographics of each patient. We include one record per hospitalization (though patients may have several 'episodes' under different physicians during a single spell of hospitalization) and exclude admissions related to the birth of a child.

Information on patient demographics includes sex, ethnicity, month and year of birth, and the Lower Super Output Area (LSOA) of residence at the time of admission.[9] This allows us to construct LSOA-level counts of all-cause and cause-specific admissions; to merge in Sure Start treatment variables;[10] and to control for local authority trends and for the time-varying area characteristics found to be important in the rollout analysis in robustness checks. To estimate the econometric model described in the next section, we collapse the patient-episode level data to cells for each LSOA-birth year-admission age-sex combination, and cells without admissions are assigned zero, so that we obtain a balanced panel.

3.2 Empirical Specification

Our empirical strategy uses the rollout of Sure Start across different LAs and over time to provide identifying variation to estimate a difference-in-difference model.

The simplest model that captures the relationship between Sure Start and health outcomes compares the outcomes of children who use Sure Start services with those who do not. To do so, one would estimate the following basic model:

$$y_{it} = \alpha + \beta SS_i + \epsilon_{it}$$

where y_{it} is an outcome for child i measured at age t, SS_i is an indicator variable that takes value 1 if the child participated in the programme when age-eligible and 0 otherwise, and ϵ_{it} is an error term. However, since participation in Sure Start was not random, naive comparisons between the outcomes of those who have and have not participated in the programme mean that β does not identify its causal impact and the it is not clear the direction of the bias in such estimates. For example, β could overestimate the impact of Sure Start if more motivated parents are more likely to enrol their children in the programme, but also take care of their health in other ways (for example, by feeding them with healthier food). On the other hand, β could underestimate the

[9] The LSOA is a very small geographic unit. There are around 32,000 LSOAs in England, and the average LSOA has a population of around 1,500 residents. LSOAs are a unit of statistical rather than practical geography, so there are no administrative or electoral responsibilities that are conducted at the LSOA level. However, LSOAs map to local authorities (of which there are 326 in the period we consider), which do have a role in administering a wide range of policies.

[10] This is subject to error in case of mobility between ages 0-4 and 5-15; however, classical measurement error in the treatment variable would lead to attenuation bias, hence we would underestimate the treatment impacts. We show in section 4 how the results change when we use the LSOA of birth rather than the LSOA of residence at time of admission to define treatment.

impact if priority in enrolment is given to children of the most disadvantaged families, who live in a less healthy home environment and could also be the most resistant to engagement with the programme goals.

As shown above, Sure Start coverage was higher in more deprived areas. This means that a naive comparison of the outcomes of children living in areas with low vs. high coverage at one point in time would confound the impacts of Sure Start with those of other factors correlated with the type of area these two groups of children live in. Hence, we use variation in exposure based on a child's month and year of birth and local authority of residence.

Our main estimating model when studying the impact of SS on hospitalizations is based on cells defined by the neighbourhood (LSOA), month and year of birth, age (in years) of admission, and sex; in other words, our outcome measure is a binary indicator for whether there is an admission for that cell. For each outcome y and measurement age a, we estimate:

$$y^a_{ldtm} = \alpha + \delta^a SS_{dtm} + \pi_{ld} + \gamma_{tm} + \epsilon^a_{ldtm}, a = 1, ..., 15 \quad (1)$$

where y^a_{ldtms} denotes the outcome y at a given age a for children born in year t and month m residing in LSOA l of local authority d. SS_{dtm} denotes the average coverage of Sure Start defined as the number of centres per thousand children aged 0-4 for every month and year of birth, averaged over the first 60 months of life (ages 0 through 4) if $a = 5, ..., 15$, or for outcomes measured before age 5 ($a = 0, ..., 4$) over the months between birth and measurement.[11] This measure captures the 'extensive' margin of Sure Start (whether there is any centre open in the local authority) as well as the 'intensive' margin (how many centres are available). π_{ld} is a set of over 32,000 LSOA l fixed effects to account for time-invariant LSOA-level unobserved heterogeneity; γ_{tm} is a set of month-year of birth fixed effects; ϵ^a_{ldtm} are idiosyncratic shocks.[12]

Relative to a comparison of the outcomes of children from areas with a lot of centres and those of children from areas with fewer centres, our approach allows us to control for all fixed differences between these areas that may drive differences in outcomes (i.e. deprivation). However, for this approach to be valid, changes in outcomes in LAs exposed to a faster expansion of coverage of Sure Start relative to LAs with a slower expansion should only be driven by the exposure to differential coverage rates, and not by differences in local characteristics which could impact both the expansion of the program and health outcomes. We return to this issue in the next section, where we present detailed analysis of the determinants of the Sure Start rollout and discuss how this relates to our identifying assumption.

[11] Panels (a) and (b) of Figure A.2 illustrate the importance of normalising by the relevant population when looking at how access to Sure Start is distributed across England. Panel (a) shows that London was consistently one of the regions with the most Sure Start facilities. But after taking into account its population, it fell from middle of the pack in the early 2000s to the region with the lowest coverage by some distance from 2009 onwards (panel (b)). On the other hand, while the North East had far fewer centres than other regions in 2010, it had the third highest coverage.

[12] We use a linear probability model primarily due to computing constraints to estimate non-linear models with about 8 million observations while including over 32,000 fixed effects (and 900 trends in the robustness checks). However, we have repeated a few main specifications using a logit model, and the average marginal effects are broadly similar to the coefficients obtained from the LPM. The results are available from the authors upon request.

We interpret the coefficients as intention-to-treat effects (ITT), since we are not estimating is the impact of a child actually visiting Sure Start. Rather, we focus on the causal impact of providing greater access to Sure Start centres (as measured by increasing the coverage of Sure Start facilities in a local area). There are two key reasons for this. The first is practical: there is no representative data source for England with information on repeated cohorts of children, their health outcomes, their place and date of birth, and their participation in Sure Start. Second, even if these data were available, there is still a strong argument for focusing on the impact of giving children greater access to Sure Start when evaluating its effectiveness. Sure Start is not a mandatory service: apart from a small number of safeguarding sessions, families are not required to make use of the services offered by the local Sure Start centre. So one of the main policy levers that the government is able to pull is increasing access to make it easier for families to take up these services.

We report results for admissions for any cause and for admissions for specific causes; additionally, we examine heterogeneity in treatment effects by gender, for urban and rural areas, and level of deprivation.

For all specifications, we present robust standard errors clustered at the level of local authority at the time of admission to account for autocorrelation in the outcomes (Bertrand, Duflo and Mullainathan, 2004).

3.3 Validity of empirical strategy

Figure A.3 in the Appendix shows how the SS coverage changed over time based on local authorities' level of disadvantage. It ranks local authorities districts based on their scores in the 1998 Index of Local Deprivation and then divides them into five equal groups. Consistent with Figure 3, the most disadvantaged areas enjoyed much higher coverage through the mid-2000s; even in 2006, the coverage in the 20% most disadvantaged authorities was more than four times as high as coverage in the 20% least disadvantaged areas. After 2006, centres opened much more rapidly in better-off areas as policymakers sought to universalise the programme, and the gap in coverage quickly closed. By 2010, all areas had a similar level of coverage, with the lowest coverage in the most disadvantaged local authorities. Therefore, we start by presenting evidence from two empirical tests to support the key identification assumption of our strategy, that the change in SS coverage is uncorrelated with other determinants of changes in hospitalizations.

First, we study how socio-demographic characteristics of LAs predicted the expansion of program coverage. Table 2 presents estimates for η in the following equation:

$$\Delta SS_d = \eta \mathbf{X}_{d,t0} + \pi_r + \chi_d \tag{2}$$

where ΔSS_d is log of the difference between the coverage in 2010 (when virtually all centres had opened) and 2000 in LA d. $\mathbf{X}_{d,t0}$ is a vector of pre-SS LA-level socio-demographic characteristics and primary health care resources and π_r are region fixed effects (for the nine regions in England). We use 1998 as our baseline year for the socio-demographic characteristics and primary health care

resources. As potential determinants of the rollout, we consider not only the official guidelines but also a wide range of other potentially important predictors that capture local labour market shocks, vital statistics, existing provision of child care and health services, and different markers of family needs.[13] Table A.1 in the Appendix lists these variables, their sources, and the years and geographic level at which they are available. As the table shows, these variables were available at different levels of aggregation. In order to have a common geographical unit of analysis, we used crosswalks from ONS and the Ordnance Survey to map between different sets of local authorities (e.g. following local government reorganisations).

Column (1) of Table 2 shows that within regions, a faster expansion occur in poorer LAs, confirming the description in subsection 2.1. In particular, the expansion was greater in the poorest 30% LAs (see column 2).[14] Column 3 incorporates other potential rollout predictors related with early life disadvantage which were also used assessing priority, namely, the proportion of low birth weight deliveries (i.e., babies born weighing less than 2500g), teen conception rate, and potential demand for services (proxied by population density and the share of population between ages 0 and 4). While the proportion of low birth weight deliveries and teen conception rate do not predict a faster expansion of SS, LAs with relatively more potential users (children ages 0-4) had faster expansion, once deprivation is accounted for. Finally, in Column 4 we control for local labour market variables (proxied by the Jobseekers Allowance receipt rate) and a proxy of health care provision (GP practices per 1000 inhabitants). Again, potential demand is associated to a faster expansion of coverage, as is a higher rate of unemployment benefit claiming.

To account for these potential threats to internal validity, we control for LSOA fixed effects to account for pre-existing differences in levels across areas. We also provide additional robustness checks to our analyses by including controls for LAs linear trends in baseline characteristics of municipalities (see Acemoglu, Autor and Lyle (2004)), in particular, we include trends for socioeconomic indicators measured in 1998 (quadratic of the deprivation score, log of total population, and share of population of ages 0-4, children in care ages 0-4); labor market indicators measured in 1998 (as the rate of Job Seekers) and health care indicators (GP practices per 1000).

Second, we provide suggestive evidence that the of expansion of SS was uncorrelated with levels or trends in pre-program outcomes or other variables. This could be the case if, for instance, the program was implemented earlier in locations with declining trends in hospitalizations (or related variables). In particular, in Figure A.4 and Figure A.5 we present suggestive evidence that LADs that implemented the SS at different times and expanded the programme at different paces were on parallel trends. Figure A.4 shows that LAs that introduced SS for the first time in different years were in parallel trends in the 1990s for unemployment (proxied by JSA claiming rate), number of GPs in LA, fertility and number of children looked after between ages 1-4. Figure A.5 shows

[13] There are 326 local authority districts, or lower-tier local authorities, in England (including single-tier Unitary Authorities, London Boroughs, and Metropolitan Boroughs). We exclude three of these – the Isles of Scilly, City of London, and West Somerset – from this analysis because they are outliers in the rollout of Sure Start. In particular, there are few children aged 0-4 residing in these areas.

[14] Patterns are similar if we define ΔSS_d as the log of the difference between the coverage in 2005 and 2000 in LA d to focus on earlier expansion.

similar pre-SS trends when we split LADs into two groups, depending on the change in coverage rate between 2000 and 2010: the low(high) growth group includes LAs whose SS coverage growth rate between 2000 and 2010 is below (above) the median (median is 0.98).[15]

Sensitivity Analysis We run a battery of robustness checks for threats to the validity of our research design. We summarize here the alternative specifications we use and defer to section 4 the discussion of the results. First, as mentioned above, we control for LAD linear trends in baseline characteristics of LAD. Second, we control for an indicator of political alignment between the party in control of the LAD and in national government in a given year. Third, we use the fact that we are able to measure hospitalizations before the introduction of SS to include LA-level pre-programme linear trends in this variable and account for omitted trends in outcomes that might be correlated with the introduction and expansion of SS.[16] Fourth, we control for time-varying local characteristics to relax the assumption that there are no time-varying characteristics that influence both the roll-out of Sure Start and the trend in hospitalisations.

Lastly, we deal with an additional concern, that of selective migration of individuals residing in low SS coverage LAD to high SS coverage LAD. We investigate this possibility re-estimating model 1 using for each individual not the LSOA of residence the LSOA of birth or the LSOA of first admission. We note that this is a more stringent test than testing for cross LAD mobility due to SS. The results using LSOA of birth or the LSOA of first admission are similar to those using the LSOA of residence at admission (which is our baseline model) suggesting that there is no mobility associated to SS (see Table A.2).[17]

4 Estimates of the effects of Sure Start Centers and Hospitalizations

One of the goals of the earliest phases of the Sure Start rollout was to reduce hospitalizations among children (see Armstrong, 2017). In this section, we consider whether increasing an area's access to Sure Start reduces the chances that its children are admitted to hospital. We trace out the effects of Sure Start during eligibility ages (ie, before age 5) and post-eligibility age, between ages 5 and 15. We consider inpatient admissions to hospital, not visits to the emergency room or outpatient appointments.[18] Hospitalizations at these ages are relatively rare, but quite costly. For

[15]Since the HES data starts in 1997 we cannot provide such evidence on pre-existing parallel trends for all ages, however in future drafts we will children too old to be exposed to programme.

[16]We estimate LAD-specific trends using data before the implementation of SS, and we obtain a slope estimate λ_d for each LAD. We then extrapolate the pre-expansion time trends to the post-reform period as follows (see also Bhuller et al. (2013)):

$$y^a_{ldtm} = \alpha + \delta^a SS_{dtm} + \pi_{ld} + \gamma_{tm} + \delta\widehat{\lambda_d} t + \epsilon^a_{ldtm}, a = 1, ..., 15.$$

[17]We only present the estimates for the three years of life, when the retrieving rate of the LSOA of birth is higher. For infants, 32% it is not possible to retrieve the LSOA of birth; this proportion increases with age, reaching 40% by age 10 and 60% by age 15.

[18]In part, this reflects the period that the data are available: emergency room attendance data are only considered reliable from April 2007 onwards, so there is less scope to look at the impacts of Sure Start across the entire lifecycle of

example, children born in 1993 – the last "baseline" cohort that was entirely unexposed to Sure Start – experienced just over 65,000 hospitalizations at age 5, falling to around 45,000 admissions at age 11.

One of the challenges with using hospitalization records is that we only observe children who are admitted to hospital, and we do not have information on anyone who isn't hospitalized in this period. We therefore construct a dataset that counts the number of admissions within "cell" defined by the neighborhood (Lower Super Output Area), sex, and month and year of birth. So for example, our dataset contains the number of hospitalizations of boys born in June 2000 in a specific neighborhood at age 1, 2, and so on up to age 15. Because we know all of the combinations of neighborhoods, sex, and month and year of birth, we can then infer zero admissions in cells where no hospitalization was observed. While some cells have several admissions, in the vast majority of cases there is only one. We therefore consider as our outcome a binary indicator of whether there is any hospital admission within the neighbourhood-sex-birth month-birth year cell (rather than the number of admissions).

Overall, the probability of a cell having any hospitalization is relatively low, ranging from about a 6% at age 5 to 4% at age 11. As mentioned, our measure of access to Sure Start for post-eligibility ages is the coverage, defined as the number of centres per thousand children aged 0-4 in a local authority, averaged over the first 60 months of life of the child. An increase of one centre per thousand children is equivalent to the average increase in coverage across the whole rollout period (although individual areas will have seen higher or lower increases). This means that our results can be interpreted as the impact that increasing access to Sure Start by one centre per thousand children has on the probability of whether a neighbourhood-sex-birth month-birth year cell experiences at least one hospitalization.

We first consider the impact that Sure Start had on hospitalizations for any reason up to age 15. These results are presented in Table 3, which reports the estimates from 15 separate regressions – one for each age of admission. The table shows that, during eligibility age, there is an increase in admissions; thereafter, hospitalizations are reduced from age 5 onward, with significant impacts from age 11. In particular, an increase of one Sure Start centre per thousand children in the local authority raises the probability of any hospitalization in a cell by 1.4 percentage points among infants, or 0.56ppt for toddlers. This is consistent with effective health-promoting activities (because of improved information), a greater willingness to use health services (e.g., due to lower stigma or increased perceived benefits) and potential identification of health problems among children while at the centres being dominant during the first two years of a child's life. Table 3 shows a reduction in hospitalizations between ages 11 and 15 that starts at 0.030pp, at age 11, and reaches 0.074pp by age 15.

Because the baseline incidence of hospitalizations differs substantially by age group, these magnitude of these coefficients is not easily comparable across ages. To aid comparison across ages, Figure 5 rescales the estimates in Table 3, dividing each by the relevant baseline mean for

the programme.

each age group (measured in 1996, the earliest year of data available for all age groups we consider). Figure 5 shows clearly that Sure Start coverage initially increases hospitalizations, for example increasing hospitalizations by 15% at age 1 when a local authority increases Sure Start coverage by one centre per thousand children in the year prior to the child turning one. As children get older, and particularly once they turn 5 and stop being age-eligible to use Sure Start services, the overall impact on hospitalizations becomes more or less steadily more negative. Increasing SS coverage in the local authority one centre per thousand children when children were 0-4 averts around 8% of hospital admissions at age 11, rising to nearly 15% among 15 year olds. This is remarkable – it suggests that the benefits of Sure Start are not only persistent, but grow stronger as children age.

We note that, even with the re-scaling done in Figure 5, the set of children considered at each age is slightly different (for each age we include in Table 3 the first and last cohort of birth). For example, for outcomes at age 1, we can only include children born in 1995 or later (as the HES data starts in April 1997).[19] For outcomes at age 5, we include children born between 1993 and 2006. This pattern continues so that, by age 15, we are considering children born between 1993 and 2003.[20] Since the types of services offered by Sure Start changed over time, changing the cohorts included in the estimation might confound cohort with age effects. In practice, this does not seem to be a major concern; Figure 5 shows both our main estimates (in light green) and a set of estimates that are all based on children born between 1993 and 2003 (except for ages 1 and 2): the results based on a common set of cohorts tell a similar story to our main results, and if anything they suggest that including later cohorts of children tends to reduce our estimates of Sure Start's benefits. This might be due, at least in part, to the fact that older cohorts were more likely to be in more disadvantaged areas.

4.1 Heterogeneity by gender and deprivation

We next assess heterogeneity in the impacts, by gender and levels of disadvantage, since past research has shown significant differences in the health effects of early interventions along these two dimensions (see, e.g., Conti, Heckman and Pinto, 2016). Table 4 and the marginal effects on Figure 6 show how the effects of Sure Start on all-cause hospital admissions vary between girls and boys. The impacts in Table 4 are in line with those in Table 3; however, the increase in hospitalizations among infants is driven by both boys and girls, with the impact being larger for boys than for girls (see $p-value$ for $H_a : \beta_1 \neq \beta_2$ in the bottom of Table 4). Among post-eligibility ages, the reduction in hospital admissions from age 11 in Table 3 is also driven by boys; the $p-value$ for $H_a : \beta_1 \neq \beta_2$ shows that between ages 11 and 14 the impacts for boys and girls are statistically different.

We then compare the effect of Sure Start in the most disadvantaged 30% of neighborhoods, the

[19] We only ever use children born in 2006 or earlier, since they will have been exposed to Sure Start exclusively before 2011, when some Sure Start centres started to close. See Section 2 for further detail.

[20] We limit our data to children born in 1993 and later so that we are not considering pre-treatment cohorts born far before the Sure Start program was introduced.

top 30% best-off neighborhoods, and those in the middle of the distribution of disadvantage. As Figure 7 and Table 5 illustrate, the increase in admissions among infants and toddlers detected for the whole sample in Table 3 is driven by those residing in neighborhoods falling into the poorest 70% of the deprivation distribution.[21] From ages 10 to 15, the drop in hospital admissions attributed to SS is even more concentrated in the areas with the highest levels of disadvantage.

There are many reasons why the benefits of Sure Start might be bigger in the most disadvantaged areas. The children in these areas might be more able to benefit from Sure Start, or more likely to take it up. The centres in disadvantaged areas are on average older and so might be better-established. And the centres themselves might offer different types of services; for example, policy guidance suggests that Local Programmes (and Children's Centres delivering the full Core Offer in disadvantaged areas) were more likely to deliver additional services directly, rather than relying on signposting families to existing programs. Without data on take-up or service provision in different areas over time, we cannot say to what extent each of these factors drives our finding that the biggest benefits of Sure Start are felt in the poorest neighborhoods. However, this finding nevertheless sheds light on the question of whether universal early years programs can support children's health; while we do find substantial overall impacts of Sure Start, this analysis suggests that they are driven by services for disadvantaged areas.

Finally, in Table A.3 in the Appendix we allow the impacts to vary by urban and rural local authorities. The p-value of the difference in impacts across areas shows that, for all but 3 ages, the impacts are similar in urban and rural areas. The exceptions are age 2 (which sees larger increases in hospitalizations in urban areas) and ages 4 and 5 (when there are larger reductions in admissions in rural areas).

4.2 Validity of the strategy and Sensitivity Analysis

We have showed that Sure Start has a strong and statistically significant effect on hospitalisations, especially among infants, toddlers and young adolescents. We now examines potential threats to the internal validity of the study, which include local shocks that both change hospitalizations and occur concurrently with the expansion of Sure Start.

We start by subjecting our methodology to two placebo tests. The first falsification test involves changing the outcome that we consider. We here consider any admissions due to congenital chromosomal defects before a child turns 1; since these are genetic conditions, they are not plausibly affected by Sure Start. We therefore expect our estimates to show no impact of Sure Start on admissions for these conditions. Indeed, Table A.4 shows that increasing SS coverage is unrelated to changes in the likelihood of hospitalizations due to congenital chromosomal defects.

As a second type of falsification, we conduct a randomization inference test for all-cause admissions by manipulating the definition of treatment to check whether our results are picking up on an underlying relationship between Sure Start coverage and outcomes that is not related to the

[21] We use these percentiles rather than equally-sized quantiles because they map on to the different phases used in the rollout of Children's Centres.

actual impacts of Sure Start (Duflo, Glennerster and Kremer, 2006). We randomly reshuffle the rollout profile across all 323 local authorities (for example, Bristol might be assigned the actual rollout that took place in Cornwall, and Cornwall might be assigned the rollout that happened in Cambridgeshire). In this way, we preserve the overall shape of the rollout across the country – the national average level of treatment in each month will be the same. But we change where the increases in treatment happen. Since our methodology relies on comparing local authorities who opened Sure Start centres earlier or later, shuffling the rollout profiles across local authorities essentially means that we will be looking for effects at the wrong time. Because of this, we should expect to see that the estimates are on average close to zero and not statistically significant. We repeat this exercise 500 times, randomly reshuffling the rollout profiles each time. This allows us to build up a distribution of placebo coefficients. Figure A.6 plots these placebo coefficients (in beige) for a subset[22] of the ages at which we find significant impacts (ie, ages 11 to 15 in Table 3), along with the true coefficient in green. The distribution of placebo coefficients is centred close to zero and it is clear that our actual estimates are quite far outside the distribution of placebo estimates. These results indicate that our model is picking up the true differences in hospitalization outcomes caused by the rollout of Sure Start, increasing our confidence in our results.

Sensitivity Analysis We have tested the robustness of the hospitalization results to a battery of robustness checks. In this version, we include estimates for a version of model 1 which includes controls for LA-level pre-program linear trends; these estimates are presented in Figure A.7 in the Appendix, and are similar to our baseline estimates (see subsection 3.3 for additional checks being conducted). We also estimate a model that includes all of the time-varying local area characteristics which we use in the rollout analysis (over 30 characteristics including labour market, politics, public health, public services, and demographics). We again find that our main estimates are very similar to this robustness check, giving us confidence that our main results are not driven by an unobserved factor affecting both hospitalizations and the Sure Start rollout.[23]

5 Mechanisms for the Effects of Sure Start Centers on Hospitalizations

In section 4 we showed that Sure Start increase the hospitalisations of infants and toddlers and it reduces the among children 11 or older, particularly for those in disadvantaged neighbourhoods. In order to better understand the source of these impacts, it's helpful to consider the potential mechanisms through which Sure Start might have had an effect. In subsection 2.3, we have outlined some of the mechanisms through which Sure Start might impact children's health. In particular, we have identified several potential channels: direct provision of health services, such as

[22] These results are computationally intensive, and the results for the youngest ages have not yet finished running; they will be included in a subsequent draft.

[23] The complete set of results are not yet available for this updated draft. A previous version of our work included these results for hospitalizations between ages 5 and 11; see Appendix D in https://www.ifs.org.uk/uploads/R155-The-health-effects-of-Sure-Start.pdf.

extra midwives or immunisation support; direct provision of information, for example advice on preventing accidents and injuries; exposure to illnesses and infections, for example from meeting other children in childcare; identification of potential health and home environment problems; support for parents, including promoting parental mental health; support for children's intellectual and social development, such as programmes to improve children's behaviour. One way to start to understand which of these channels might be particularly important in driving the overall benefits of Sure Start is to study the types of hospitalisations that it prevents. We consider several types of cause-specific admissions, summarised in Table A.5. These different conditions will give us some information about which mechanisms appear to be particularly important. For example, a fall in hospitalisations for respiratory illness, infections or preventable conditions might suggest that children's immune system is stronger, for example, because children were timely vaccinated or have been exposed to more illnesses earlier in life through childcare.[24] "External" conditions include injuries and poisonings, and a decline in admissions for these causes might suggest that children in areas with better access to Sure Start have safer environments in their homes and neighbourhoods, or that parents engage in better parenting practices, or that the child is better behaved and hence less prone to injuries.

Table 6 shows the impact of Sure Start coverage on the probability of hospitalisations for three of these causes: respiratory illnesses, infections or parasites and external events (like broken bones or poisoning); depending on the age, these conditions account for 38-58% of all admissions. Finally, panel D includes preventable (ACS) conditions. The lines in Figure 8 show the percentage change in the probability of a neighbourhood having at least one child admitted for the relevant cause, rescaling in each case by the probability of an admission in 1996. Panels A and B show that higher Sure Start coverage increases the probability of admissions due to respiratory infections and intestinal and parasitic infections among infants and toddlers and reduces them among adolescents 13 to 15 years old. There are a few channels through which Sure Start might affect infections. First, Sure Start centres provided information and support for vaccinations, which could reduce the likelihood of getting a serious infection that requires hospitalisation. Second, Sure Start brings together groups of children. Exposure to the illnesses and infections of others is one way to strengthen the immune system, and previous research suggests that this is an important mechanism underlying the health benefits of other childcare interventions (Siflinger and van den Berg, 2018). Third, the centres may detect potential health problems of the child and refer parents to use medical services.

Additionally, Panel C of Table 6 shows that Sure Start significantly reduces hospitalisations for external causes across nearly all ages we study. There is a fall in the probability that a neighbourhood-sex-birthday cell experiences a fall in admissions between ages 2 to 4 of 10% and 23%, respectively. At ages 5 and 6 the decline is about 20% and from age 7 remains relatively stable at 10%. This re-

[24] 'Ambulatory Care Sensitive' (ACS) or 'preventable' conditions are conditions that should not result in a hospital admission, either because they can be managed with good primary care (e.g. asthma) or because they should not be occurring at all (for example, gangrene). See, for example, Blunt (2013) for a discussion of these conditions. https://www.nuffieldtrust.org.uk/research/focus-on-preventable-admissions

duction consistent with several of the potential mechanisms that we discussed in subsection 2.3. First, Sure Start might have provided information to parents about how to make their home safe for children. However, for this channel to account for the benefits at older ages, the information that parents received when their child was young would also need to be relevant at older ages – for example, keeping hazardous chemicals out of reach. Sure Start also might have improved parents' behaviour, for example helping them to provide a more nurturing home environment. A third potential channel is that Sure Start might change children's behaviour, for example by making them less likely to display "externalising" behaviours like hyperactivity or conduct problems. By changing children's behaviour in the medium run, this would plausibly have bigger effects at older ages, when children generally have more freedom for potentially dangerous behaviour (such as falling or getting into fights). Previous evidence from the ECCE study finds that children from families who used services at baseline tended to have better behaviour (fewer externalising problems), though other characteristics of centres and families receiving extended outreach tended to have more behavioural issues (Sammons, Goff and Smith, 2015). Using data from the Millennium Cohort Study (a cohort of children born in 2000), we find strong correlational evidence on this last channel, i.e. that behavioural difficulties (measured using the Strengths and Difficulties Questionnaire) matter. In particular, for any-cause hospitalisations, both internalising and externalising behaviours matter (but internalising is more important; when looking specifically at accidents and injuries, externalising behaviours is the only one which matters, whereby children with greater problems are more likely to suffer from injuries.[25]

Finally, Panel D of Table 6 shows that the increase Table 3 in includes the impacts on the admissions due to preventable conditions. While the conditions explain part of the increase in admissions among infants and toddles, they are do not contribute for the reduction on admissions among those ages 11-15.

As mentioned, Figure 8 shows that one of the major drivers of the reduction on admissions during post-eligibility ages are external causes, accounting for around a third of the total effect we estimate at age 11. To further disentangle the possible channels that drive these effects, Table A.6 breaks out admissions related to injuries (such as broken bones) and those related to poisonings (including from household substances and medications). Although it's impossible to clearly assign a single mechanism to each type of admission, in general falls in injury-related admission might more plausibly be driven by better behaviour while reductions in poisonings speak more directly to a safer home environment. Table A.6 shows that the falls in external admissions are driven by Sure Start's impacts on hospitalisations related to different types of injuries. This is not surprising – injuries make up the bulk of admissions for external causes, accounting for between 60% and 70% of external-cause admissions depending on the age group.

The conclusion that Sure Start has such a big impact on injury-related hospitalisations is notable for the relationship that these conditions have to child maltreatment. Of course, not all

[25]The coefficient is 30% of the outcome mean at age 5, 41% at age 7, and 47% at age 11. All this supportive evidence from the Millennium Cohort Study is available from the authors upon request.

injuries are due to child maltreatment; however, previous research has used conditions like these as proxies for potential maltreatment scenarios (see, e.g., González-Izquierdo et al., 2010). Because a relatively small share of childhood injuries are due to maltreatment, this is unlikely to be the sole driver of the impacts we see, but it is an important potential channel because of the major costs that maltreatment imposes.

5.1 Other Results

We now turn to further results. These should be regarded as preliminary.

Maternal Health Although the Sure Start does not include any direct component of maternal health services, it can affect maternal health in several ways. On one hand, it can affect maternal health through parental support and by improving maternal employment possibilities, and hence increase family income which may reduce hospitalizations. On the other hand, by linking mothers to the welfare system, it can lead to the identification of potential disruptive family situations, namely, mental health problems.

To understand how mothers more exposed to SS are affected by the programme, Table A.7 in the Appendix includes estimates for model 1, but where the dependent variable is a dummy that takes value one if there is at least one hospitalisation by mothers in the LSOA. The sample used includes mothers whose youngest child was born before 2006, and treatment is defined as the SS coverage in the first x years of life before the eldest child turns age x, interacted with a dummy that takes value 1 if any other child is below 5 years old (and 0 otherwise). So at age 15 the treatment is SS coverage from birth to age 14 and 11 months (interacted with the aforementioned dummy), and at age 4 it is defined as the coverage between birth and age 3 and 11 months. Controls include mother's age at birth of the eldest child, number of children in various age bands at admission, ethnicity, birthmonth/year of the eldest child, LSOA fe and LAD clustering. We exclude maternal hospitalisations related to deliveries.

The estimates on Panel A of Table A.7 show that SS reduces maternal hospitalizations after the youngest child turns five years old. In future drafts we will include a detailed analyses of the impacts on specific causes of hospitalisations, but here we include only in Panel B the impacts on mental health related admissions. The estimates in Panel B reveal that while the youngest child is age-eligible to SS, higher access to the programme increases hospital admissions by moms due to mental health conditions; this can be motivated by the identification of problematic situations by centres, lowered stigma to use health services and/or more information about mental health provided with greater access to SS. After the youngest child turns 12, greater access to SS is associated with a reduction on admissions related to mental health problems.

Child Obesity Although not included in this draft, in the companion report to the draft, Cattan et al. (2019), we also analysed the impacts on BMI, probability of being overweight or obese during

reception year (ie, ages 4-5). We do not find any impact of the higher exposure to SS on any of these outcomes.[26]

6 Conclusion

In this paper we have presented the first causal evidence that a fully universal, scaled-up, area-based intervention which brings together education, employment, and health services for children under five and their families - Sure Start in England - has significant health benefits from infancy and up to age 15. Using the staggered introduction and expansion of SS between 1999 and 2010, we trace out the program's impacts on health outcomes from infancy through to age 15.

We find that access to SS drives substantial effects on hospitalizations through several channels. At younger ages, when children are age-eligible to attend Sure Start, greater access to the program increases hospitalizations (especially due to respiratory illness and infections). Once children have aged out of the program, the overall effect on hospitalizations turns negative and the size of the reduction in hospitalizations grows, more or less steadily, through later childhood and early adolescence. By adolescence, greater access to the program reduces the probability of a hospitalization by 8% to 14%. These effects are equivalent to about 11,500 averted hospitalizations per year.

While we use strong quasi-experimental variation to identify these effects, there are several limitations to our approach. First, we rely on measures of very local geography to define our treatment and incorporate fixed effects. However, our data sources only offer contemporaneous information about residence (HES) or school address (NCMP). This means that we must assume that any mobility in the child's first years of life is unrelated to outcomes (conditional on our covariates); this rules out responses such as spatial sorting. A second limitation of our approach is that we are not able to define the impact of actually using Sure Start services (rather than simply having greater access to them). However, our Intent to Treat estimates are still of substantial relevance for policymakers choosing whether and how to introduce similar area-based interventions without the ability to ensure take-up of the services themselves.

These results build on the evidence base for early interventions; while there is good evidence for the benefits of small-scale, highly targeted, very intensive programs in contexts with relatively few other services, our findings suggest that universal, area-based, holistic interventions operating at scale can be an effective complement to universal health care and other social services for improving child health. Our results suggest that government policy should explicitly consider early interventions that span a range of health and non-health services as part of an overall strategy of preventing illness and disease. Our findings also highlight the importance of assessing the impacts of these programs over time; while the short-term effect of Sure Start is to increase hospitalizations, we find that there are persistent and growing medium-term reductions in hospital admissions that more than compensate for these earlier increases.

[26] See https://www.ifs.org.uk/uploads/R155-The-health-effects-of-Sure-Start.pdf.

However, our results also suggest that this overall effectiveness of the Sure Start intervention might have come despite, rather than because of, its universality. We find that our results are mainly driven by impacts in the 30% most disadvantaged neighborhoods: these neighborhoods see a larger increase in hospitalizations at younger ages, and larger falls during later childhood and adolescence. While the lack of detailed data on Sure Start's operation and take-up in different areas over time prevents us from drawing conclusions about why the program is so much more effective in poorer neighborhoods, this finding nevertheless suggests that some form of targeting could be efficient in this intervention. However, this does not necessarily need to be targeting based on individual measures of disadvantage; rather, targeting centers at highly disadvantaged neighborhoods could also help the program to be targeted more efficiently.

While this paper has focused on the Sure Start program's impacts on children's hospitalizations, the multifaceted nature of the program means that it could also have impacts on a wide range of other outcomes. Indeed, we find that access to Sure Start also had impacts on maternal hospital admissions, which - like its impacts on children - start out by increasing admissions before reducing admissions among the mothers whose eldest child has aged out of Sure Start eligibility. These effects are particularly pronounced for hospitalizations related to maternal mental health. In ongoing work, we are also considering the program's impacts on children's cognitive development (measured by test scores); their socio-emotional development and behavior (measured by youth crime and absences from school); parenting and the home environment (measured by access to social services); and maternal employment rates. Of course, these are not only outcomes in their own right, but will also help to shed light on the mechanisms underlying the impacts we have shown on children's hospitalizations.

Our results confirm the important role that early interventions can play in preventing poor health. Based on the strengthening evidence base, policymakers should consider holistic early years programmes as part of an overall strategy to promote health, with benefits both for families and finance ministers.

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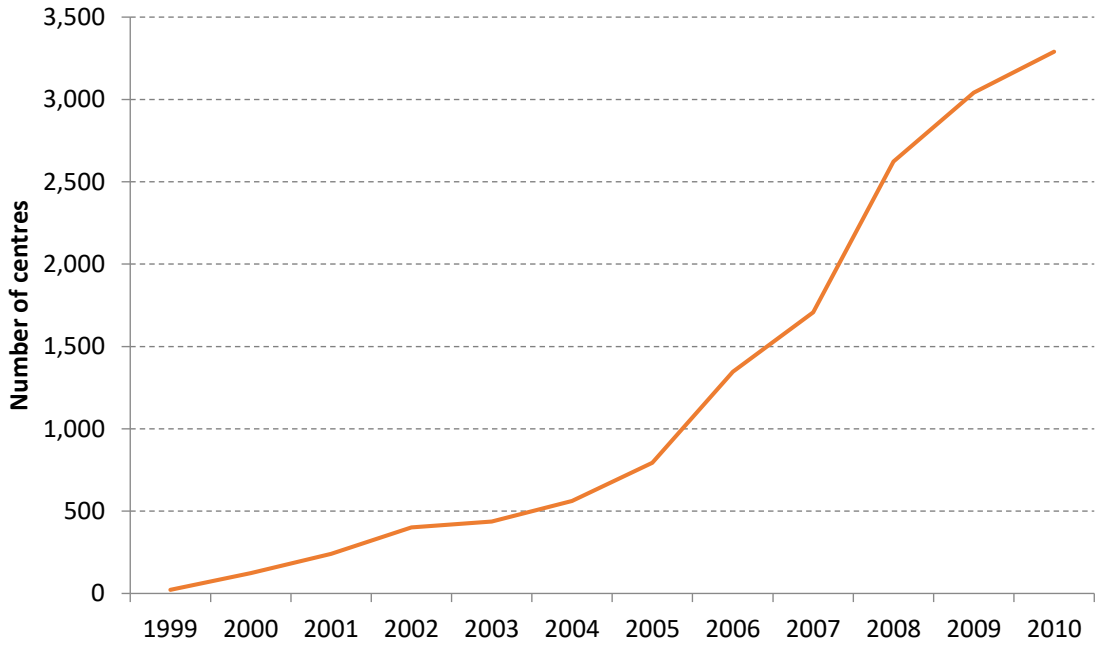
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7 Tables and Figures

Figure 1: Number of Sure Start centres in England

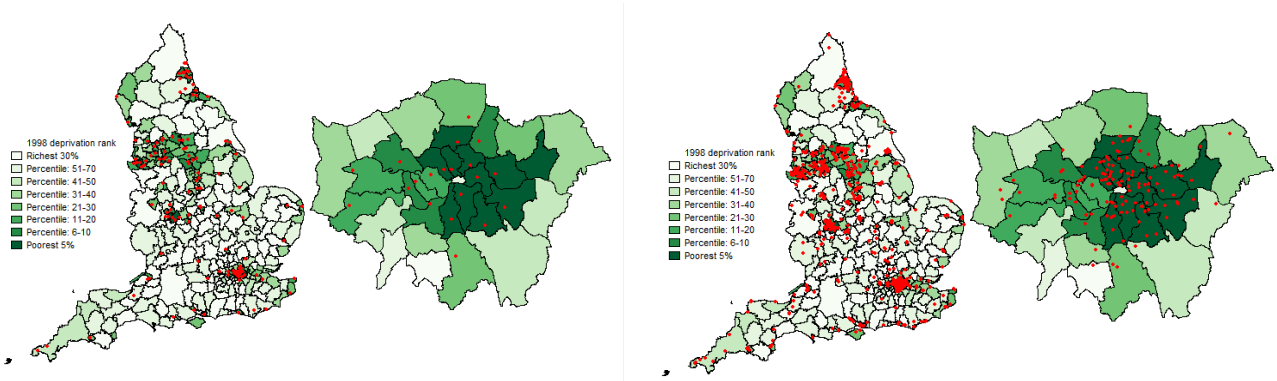


Notes: The number of centres is based on centres observed in data received from the Department for Education. Since the treatment of arrangements like satellite sites was not always consistent, these numbers might not exactly match other data sources. We assume that a Sure Start Children's Centre (SSCC) opening at the same postcode as a Sure Start Local Programme (SSLP) replaces the SSLP; otherwise, we count both SSLPs and SSCCs between 2003 and 2006, and assume all SSLPs have closed from 2007 onwards. Source: Authors' calculations using data provided by the Department for Education.

Figure 2: Sure Start centres around England

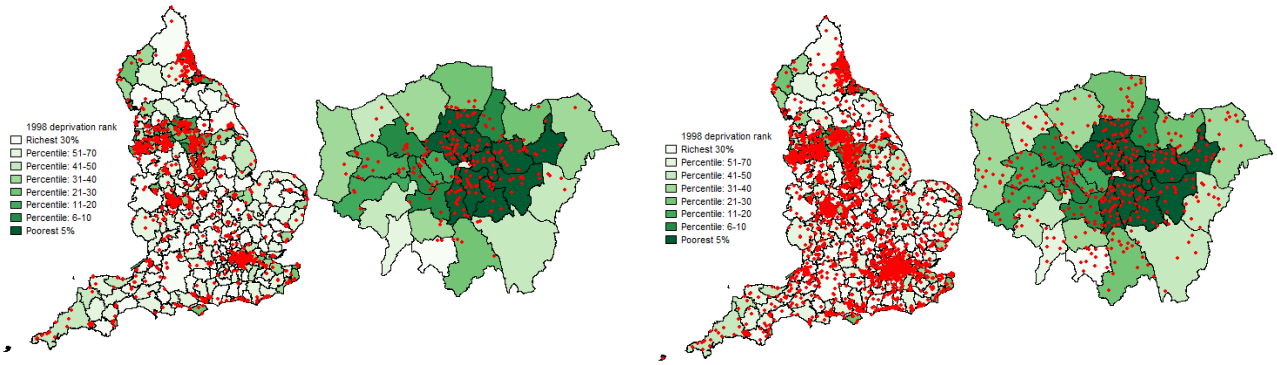
(a) SS Centres in 2000

(b) SS Centres in 2004



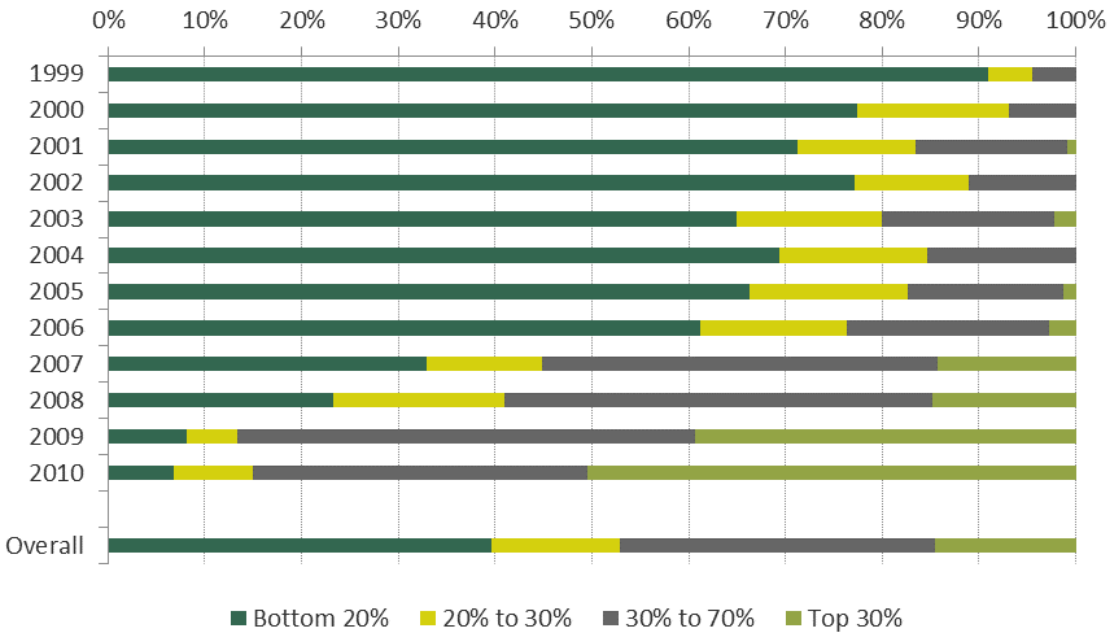
(c) SS Centres in 2006

(d) SS Centres in 2008



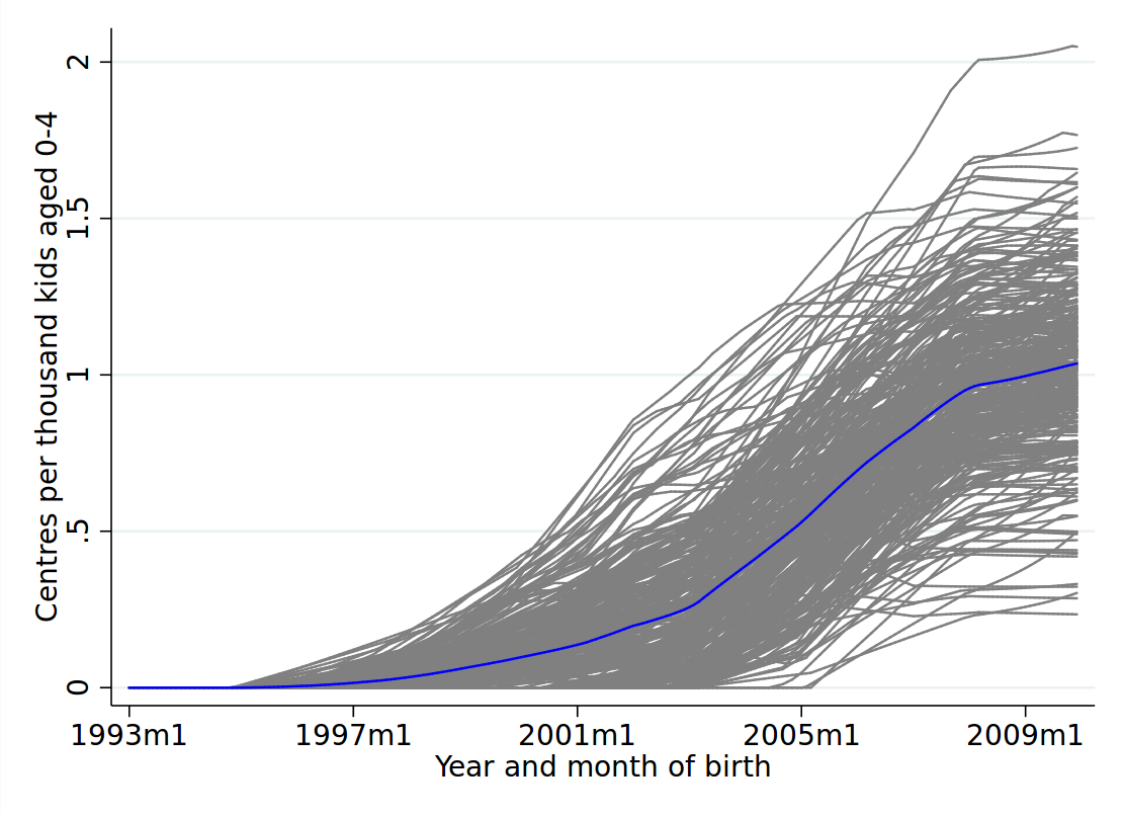
Notes: Local authorities are coloured by their rank in the 1998 Index of Local Deprivation, with more disadvantaged areas shaded more darkly. Each red point indicates the location of a Sure Start centre (SSLP or SSCC). Source: Authors' calculations using data provided by the Department for Education.

Figure 3: Share by deprivation of new centres opened each year



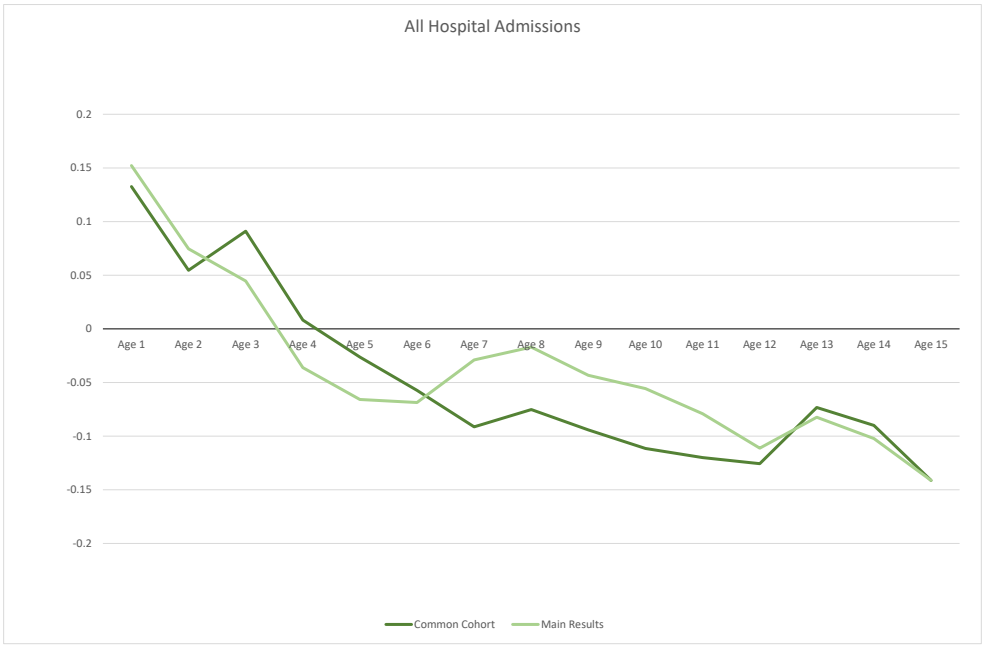
Notes: Bottom 20% refers to neighbourhoods (LSOAs) in the bottom 20% of the national 2004 Index of Multiple Deprivation ranking, i.e. the most disadvantaged. Other categories are similarly defined, with Top 30% incorporating the less disadvantaged areas. Centres in the City of London, Isles of Scilly, and West Somerset are excluded. Source: Authors' calculations using data provided by the Department for Education and the 2004 Index of Multiple Deprivation.

Figure 4: Average coverage over the first 60 months of life, by local authority and month and year of birth



Notes: Each grey line represents one of 323 local authority districts in England (excluding the Isles of Scilly, City of London, and West Somerset). The blue line shows the average for all of England. The lines plot the average Sure Start coverage (centres per thousand children aged 0-4 in the district) over the first five years of life for children based on their month and year of birth. Source: Authors' calculations using data from the Department for Education and ONS population estimates.

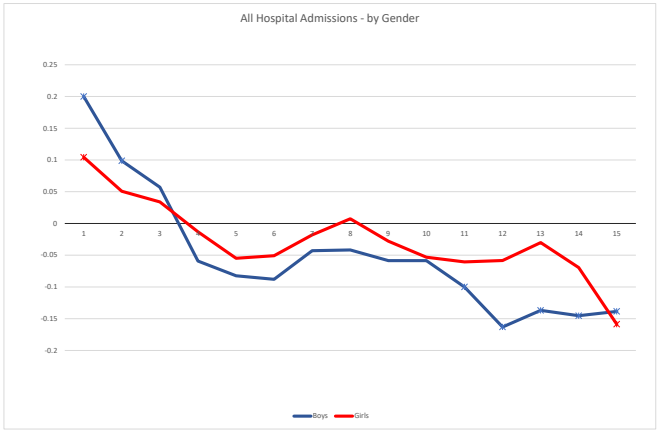
Figure 5: Effect of an increase in Sure Start coverage on probability of any hospitalisation in the neighbourhood, rescaled by baseline probability: All cohorts and with common sample



	Age 1	Age 2	Age 3	Age 4	Age 5	Age 6	Age 7	Age 8	Age 9	Age 10	Age 11	Age 12	Age 13	Age 14	Age 15
Common	**		*						*	**	**	**		**	***
Main Results	***	*									*	***	*	***	***

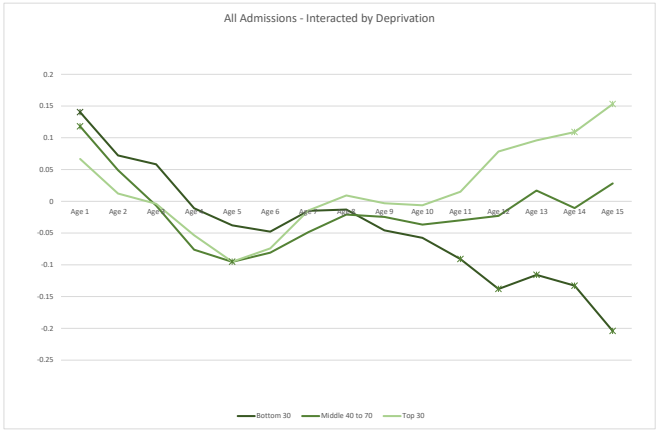
Notes: Effect sizes are constructed by rescaling the estimates by the pre-Sure Start baseline probability of a hospitalisation at each age. "Common sample" results use the same cohorts (birth years 1993 to 2003 inclusive) to estimate the impact at each age. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

Figure 6: Sure Start's effect on probability of any hospitalisation, rescaled by baseline probability: Differences by gender



Notes: Figure shows coefficients from separate regressions for each outcome age, with Sure Start treatment interacted with gender. Coefficients are rescaled by the gender-specific baseline (1996) mean for each age. Asterisks on the lines indicate whether individual coefficients are statistically significant at the 5% level. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

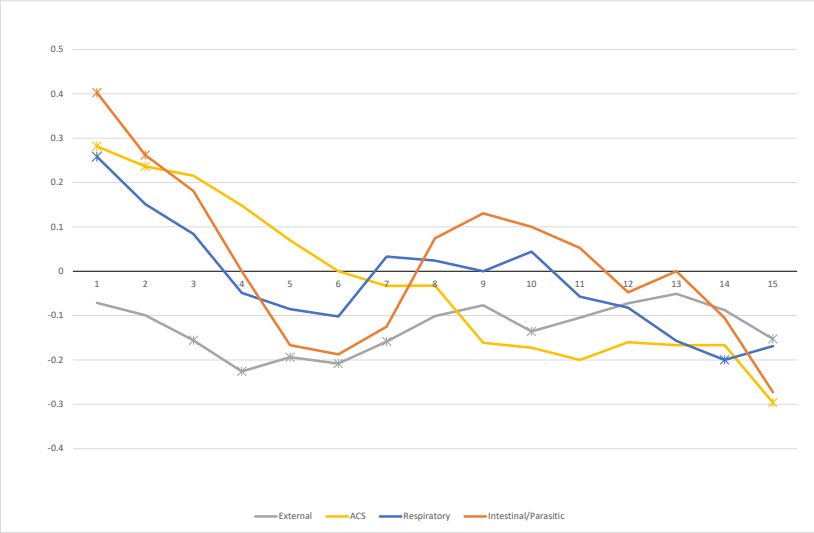
Figure 7: Sure Start's effect on probability of any hospitalisation, rescaled by baseline probability: Differences by disadvantage



Notes: Figure shows coefficients from separate regressions for each outcome age, with Sure Start treatment interacted with the three disadvantage categories. Coefficients are rescaled by the gender-specific baseline (1998) mean for each age. Asterisks on the lines indicate whether individual coefficients are statistically significant at the 5% level. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

Figure 8: Sure Start's effect on probability of hospitalisation for specific causes, rescaled by baseline probability

(a) By all causes



Notes: Figure shows coefficients from separate regressions for each outcome age and admission cause. Definitions for admission causes are given in Table A.5. Points with a star marker are statistically significant at the 5% level. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

Table 1: The timeline of Sure Start

	Sure Start Milestones
Jan. 1999	First 60 'trailblazer' districts identified, invited to submit applications
Nov. 1999	Full approval of the first 15 Local Programmes
2000	Government target rises from 250 to 530 Local Programmes - Funding more than doubles
2003	Government pledges to universalise and expand SS Transition from Local Programmes to SSCCs
Dec. 2004	10-Year Strategy for Childcare pledges a SSCC in every community by 2010
2004-06	Phase One - targeting 20% most disadvantaged areas Most SSLPs transition to SSCCs
2006-08	Phase Two - targeting 30% most disadvantaged areas
2008-10	Phase Three - 'a Children's Centre in every community'

Table 2: Association between Sure Start coverage growth between 2000 and 2010 and local characteristics

	(1)	(2)	(3)	(4)
Deprivation Rank	-0.008***		-0.005**	-0.002
	(0.001)		(0.002)	(0.002)
Deprivation Dummies				
Bottom 30%		0.443***		
		(0.094)		
30-70%		0.047		
		(0.069)		
% Low Birth Weight			0.026	0.024
			(0.031)	(0.030)
Teen Conception Rate			0.002	0.000
			(0.003)	(0.003)
Density			-0.000	-0.000
			(0.000)	(0.000)
Proportion population ages 0-4			20.488***	22.024***
			(6.121)	(6.242)
Job Seekers Allowance				0.078**
				(0.039)
GP Practices per 1000 inhabitants				0.039
				(0.580)
N	323	323	323	323

Note: Deprivation rank comes from the 1998 Index of Local Deprivation, where lower ranks indicate more disadvantaged local authorities. Low birth weight is the share of babies born weighing less than 2,500 grams. Teen conceptions are measured per thousand women aged 15-17. Jobseeker Allowance is the JSA receipt rate. Population density is measured as the population (in thousands) per square kilometre. All characteristics are measured in 1998. All estimates include fixed effects for the nine regions in England. Source: Authors' calculations using data provided by the Department for Education and the sources listed in Table A.1 in the Appendix.

Table 3: Effect of an increase in Sure Start coverage on probability of any hospitalisation in the neighbourhood

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
Av. Cov. SS	0.0140***	0.0056*	0.0029	-0.0022	-0.0040	-0.0036	-0.0013	-0.0007	-0.0017	-0.0021	-0.0031*	-0.0046***	-0.0036*	-0.0050***	-0.0074***
	(0.0034)	(0.0026)	(0.0022)	(0.0027)	(0.0024)	(0.0022)	(0.0020)	(0.0014)	(0.0012)	(0.0011)	(0.0013)	(0.0013)	(0.0016)	(0.0014)	(0.0016)
N	9057216	9844800	10632384	11026176	11026176	11026176	11026176	11026176	11026176	11026176	11026176	10632384	9844800	9057216	8269632
Cohorts															
Earliest	1995	1994	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993
Latest	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2005	2004	2003
Baseline Mean	0.0920	0.0750	0.0648	0.0608	0.0608	0.0524	0.0449	0.0412	0.0393	0.0377	0.0392	0.0414	0.0437	0.0489	0.0524
Baseline SD	0.2891	0.2531	0.2416	0.2399	0.2389	0.2228	0.2071	0.1987	0.1942	0.1906	0.1940	0.1992	0.2045	0.2156	0.2228

Note: The table shows coefficients from regression analysis at each outcome age. Observations are cells defined by the LSOA, month and year of birth, and sex. The model regresses an indicator for any hospitalisation in a cell on Sure Start coverage (number of centres per thousand children aged 0–4 in the local authority for outcomes post age 4; when studying outcomes for younger ages, $a = 1, ..., 4$, SS_{dtm} is defined the number of centres per thousand children aged 0-4 that were open in the LA d when the child was aged $a - 1$.), the population at the relevant age in the LSOA and an indicator for female and includes fixed effects for month–year of birth and the LSOA of residence. The baseline mean and SD are measured in 1996. Standard errors are shown in parentheses clustered by LAD. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Table 4: Effect of an increase in Sure Start coverage on probability of any hospitalisation in the neighbourhood: Differences by gender

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
$SSCov\times$	0.0184***	0.0074**	0.0037	-0.0036	-0.0048	-0.0045	-0.0019	-0.0017	-0.0023	-0.0022	-0.0038**	-0.0067***	-0.0059***	-0.0067***	-0.0069***
Boy(β_1)	(0.0037)	(0.0028)	(0.0023)	(0.0028)	(0.0025)	(0.0023)	(0.0020)	(0.0014)	(0.0012)	(0.0011)	(0.0014)	(0.0014)	(0.0017)	(0.0016)	(0.0019)
$SSCov\times$	0.0096**	0.0038	0.0022	-0.0008	-0.0032	-0.0026	-0.0008	0.0003	-0.0011	-0.0020	-0.0023	-0.0024	-0.0013	-0.0032*	-0.0079***
Girl(β_2)	(0.0032)	(0.0025)	(0.0022)	(0.0026)	(0.0024)	(0.0022)	(0.0020)	(0.0015)	(0.0012)	(0.0011)	(0.0012)	(0.0013)	(0.0015)	(0.0014)	(0.0017)
Marginal Eff.															
Male	0.200	0.099	0.057	-0.059	-0.082	-0.088	-0.043	-0.042	-0.059	-0.059	-0.100	-0.163	-0.137	-0.145	-0.139
Female	0.104	0.051	0.034	-0.013	-0.055	-0.051	-0.018	0.007	-0.028	-0.053	-0.061	-0.058	-0.030	-0.069	-0.159
$p-value:$															
$H_a:\beta_1\neq\beta_2$	0.000	0.001	0.111	0.000	0.003	0.000	0.032	0.000	0.007	0.653	0.001	0.000	0.000	0.000	0.459
N	9057216	9844800	10632384	11026176	11026176	11026176	11026176	11026176	11026176	11026176	11026176	10632384	9844800	9057216	8269632

Note: The table shows coefficients from regression analysis at each outcome age. Observations are cells defined by the LSOA, month and year of birth, and sex. The model regresses an indicator for any hospitalisation in a cell on Sure Start coverage (number of centres per thousand children aged 0–4 in the local authority for outcomes post age 4; when studying outcomes for younger ages, $a = 1, ..., 4$, SS_{dtm} is defined the number of centres per thousand children aged 0-4 that were open in the LA d when the child was aged $a-1$.), the population at the relevant age in the LSOA and an indicator for female and includes fixed effects for month–year of birth and the LSOA of residence. Standard errors are shown in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Table 5: Sure Start's effect on probability of any hospitalisation: Differences by disadvantage

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
$SSCov\times$	0.0176***	0.0074*	0.0051	-0.0009	-0.0031	-0.0034	-0.0009	-0.0007	-0.0023	-0.0027*	-0.0044**	-0.0069***	-0.0060***	-0.0076***	-0.0128***
Bottom 30%	(0.0043)	(0.0033)	(0.0028)	(0.0032)	(0.0028)	(0.0025)	(0.0022)	(0.0016)	(0.0014)	(0.0013)	(0.0014)	(0.0015)	(0.0018)	(0.0016)	(0.0021)
$SSCov\times$	0.0098	0.0033	-0.0004	-0.0042	-0.0053**	-0.0039*	-0.0020	-0.0008	-0.0009	-0.0013	-0.0011	-0.0009	0.0007	-0.0005	0.0014
Between 40 & 70%	(0.0029)	(0.0021)	(0.0018)	(0.0022)	(0.0020)	(0.0020)	(0.0018)	(0.0014)	(0.0011)	(0.0010)	(0.0012)	(0.0012)	(0.0014)	(0.0015)	(0.0017)
$SSCov\times$	0.0047	0.0007	-0.0002	-0.0025	-0.0044*	-0.0029	-0.0005	0.0003	-0.0001	-0.0002	0.0005	0.0028*	0.0037*	0.0047**	0.0070***
Top 30%	(0.0032)	(0.0023)	(0.0021)	(0.0024)	(0.0022)	(0.0022)	(0.0021)	(0.0016)	(0.0013)	(0.0012)	(0.0013)	(0.0013)	(0.0015)	(0.0017)	(0.0020)

Note: The table shows coefficients from regression analysis at each outcome age. Observations are cells defined by the LSOA, month and year of birth, and sex. The model regresses an indicator for any hospitalisation in a cell on Sure Start coverage (number of centres per thousand children aged 0–4 in the local authority for outcomes post age 4; when studying outcomes for younger ages, $a = 1, ..., 4$, SS_{dtm} is defined the number of centres per thousand children aged 0-4 that were open in the LA d when the child was aged $a - 1$.), the population at the relevant age in the LSOA and an indicator for female and includes fixed effects for month–year of birth and the LSOA of residence. Standard errors are shown in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

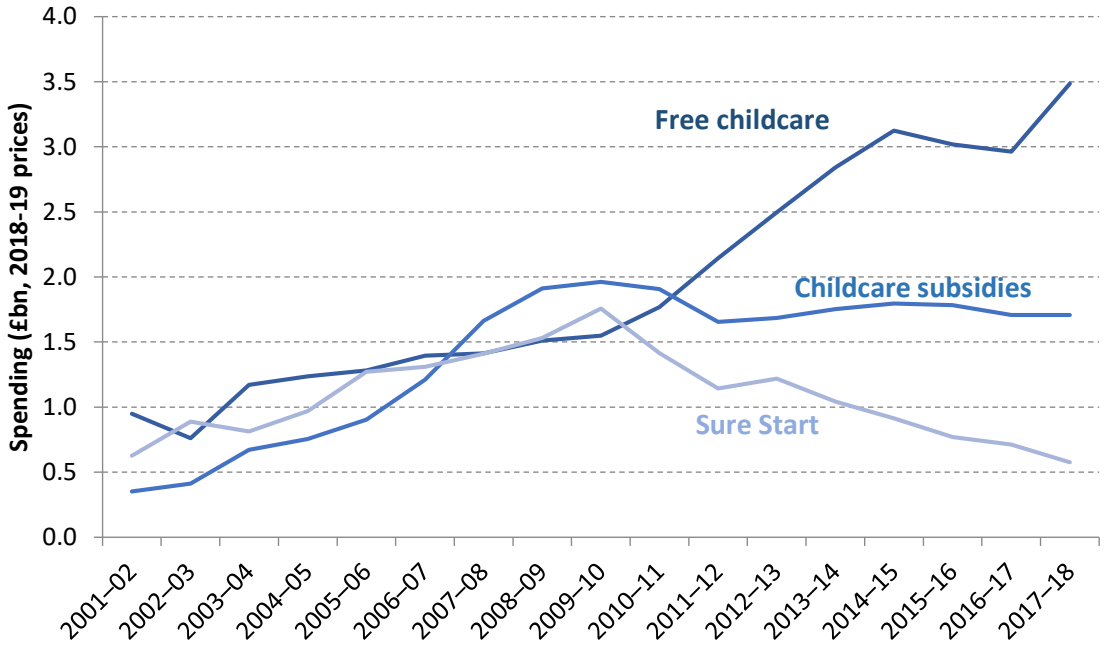
Table 6: Effect of an increase in Sure Start coverage on probability of hospitalisation for specific causes

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
							Panel A: Respiratory Admissions								
SSCov	0.0087***	0.0037*	0.0017	-0.0008	-0.0016	-0.0014	0.0003	0.0002	0.0000	0.0003	-0.0004	-0.0006	-0.0011*	-0.0015**	-0.0014*
	(0.0022)	(0.0014)	(0.0011)	(0.0012)	(0.0009)	(0.0007)	(0.0006)	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0005)	(0.0005)	(0.0006)
Marginal Eff.	0.258	0.151	0.084	-0.049	-0.086	-0.102	0.033	0.024	0.000	0.044	-0.057	-0.082	-0.157	-0.200	-0.169
Baseline Mean	0.0337	0.0245	0.0203	0.0164	0.0187	0.0137	0.0091	0.0084	0.0075	0.0068	0.0070	0.0073	0.0070	0.0075	0.0083
							Panel B: Intestinal Parasite Admissions								
SSCov	0.0068***	0.0028**	0.0013	0.0000	-0.0006	-0.0006	-0.0004	0.0002	0.0003	0.0002	0.0001	-0.0001	-0.0000	-0.0002	-0.0006*
	(0.0019)	(0.0011)	(0.0008)	(0.0006)	(0.0004)	(0.0004)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0003)
Marginal Eff.	0.402	0.262	0.181	0.000	-0.167	-0.188	-0.125	0.074	0.130	0.100	0.053	-0.048	0.000	-0.105	-0.273
Baseline Mean	0.0169	0.0107	0.0072	0.0058	0.0036	0.0032	0.0032	0.0027	0.0023	0.0020	0.0019	0.0021	0.0019	0.0019	0.0022
							Panel C: External Admissions								
SSCov	-0.0010	-0.0014*	-0.0017**	-0.0021***	-0.0019***	-0.0020***	-0.0013***	-0.0008*	-0.0006	-0.0011**	-0.0009*	-0.0007	-0.0006	-0.0012*	-0.0022***
	(0.0007)	(0.0007)	(0.0006)	(0.0005)	(0.0005)	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0006)	(0.0007)
Marginal Eff.	-0.071	-0.099	-0.156	-0.226	-0.194	-0.208	-0.159	-0.101	-0.077	-0.136	-0.105	-0.072	-0.051	-0.088	-0.153
Baseline Mean	0.0140	0.0141	0.0109	0.0093	0.0098	0.0096	0.0082	0.0079	0.0078	0.0081	0.0086	0.0097	0.0118	0.0137	0.0144
							Panel D: Preventable Condition (ACS) as a primary diagnosis								
SSCov	0.0056**	0.003**	0.0017*	0.0008	0.0003	0	-0.0001	-0.0001	-0.0005	-0.0005	-0.0005	-0.0004	-0.0004	-0.0004	-0.0008**
	(0.0019)	(0.0011)	(0.0008)	(0.0006)	(0.0004)	(0.0003)	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0003)
Marginal Eff.	0.281	0.236	0.215	0.148	0.070	0.000	-0.033	-0.032	-0.161	-0.172	-0.200	-0.160	-0.167	-0.167	-0.296
Baseline Mean	0.0199	0.0127	0.0079	0.0054	0.0043	0.0034	0.003	0.0031	0.0031	0.0029	0.0025	0.0025	0.0024	0.0024	0.0027
N	9057216	9844800	10632384	11026176	11026176	11026176	11026176	11026176	11026176	11026176	11026176	10632384	9844800	9057216	8269632
Cohorts															
Earliest	1995	1994	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993
Latest	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2005	2004	2003

Note: The table shows coefficients from regression analysis at each outcome age. Observations are cells defined by the LSOA, month and year of birth, and sex. The model regresses an indicator for any hospitalisation in a cell on Sure Start coverage (number of centres per thousand children aged 0–4 in the local authority for outcomes post age 4; when studying outcomes for younger ages, $a = 1, ..., 4$, SS_{dtm} is defined the number of centres per thousand children aged 0-4 that were open in the LA d when the child was aged $a - 1$.), the population at the relevant age in the LSOA and an indicator for female and includes fixed effects for month–year of birth and the LSOA of residence. The baseline mean and SD are measured in 1996. Standard errors are shown in parentheses clustered by LAD. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

8 APPENDIX

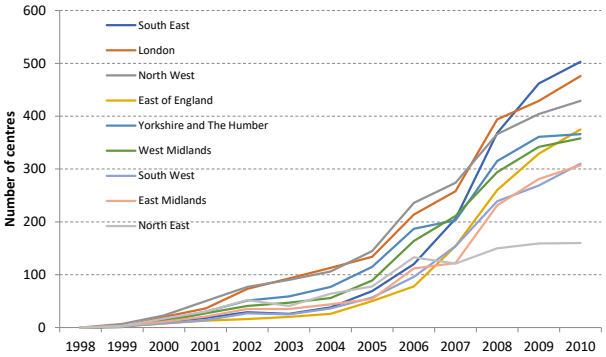
Figure A.1: Spending in Sure Start and other early years programmes



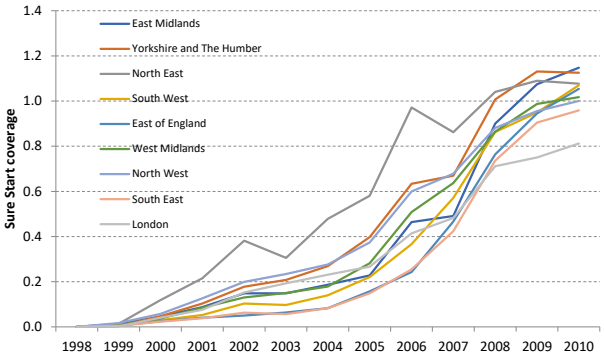
Source: Belfield, Farquharson, and Sibieta (2018).

Figure A.2: Number of Sure Start centres open in England, by region and year

(a) Overall

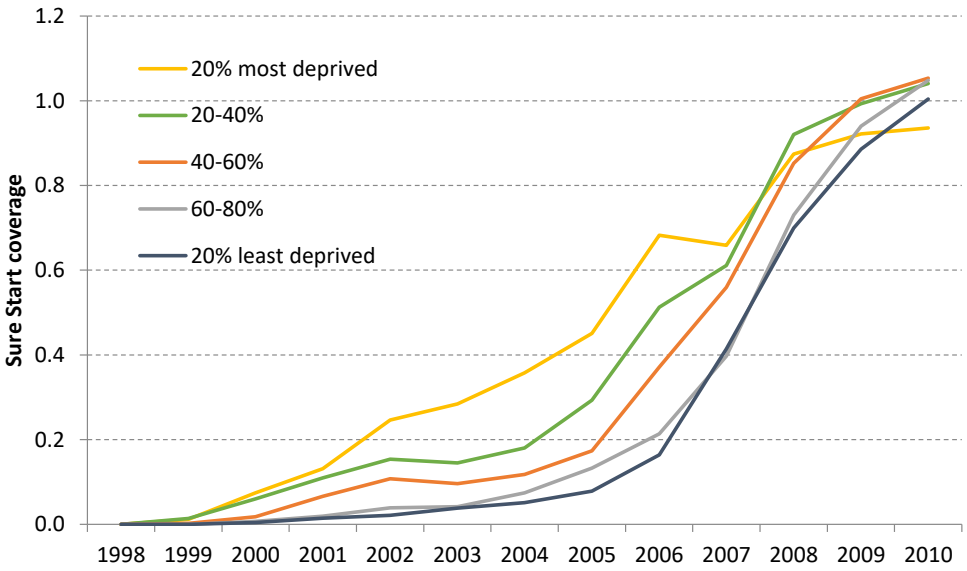


(b) Per thousand children aged 0-4



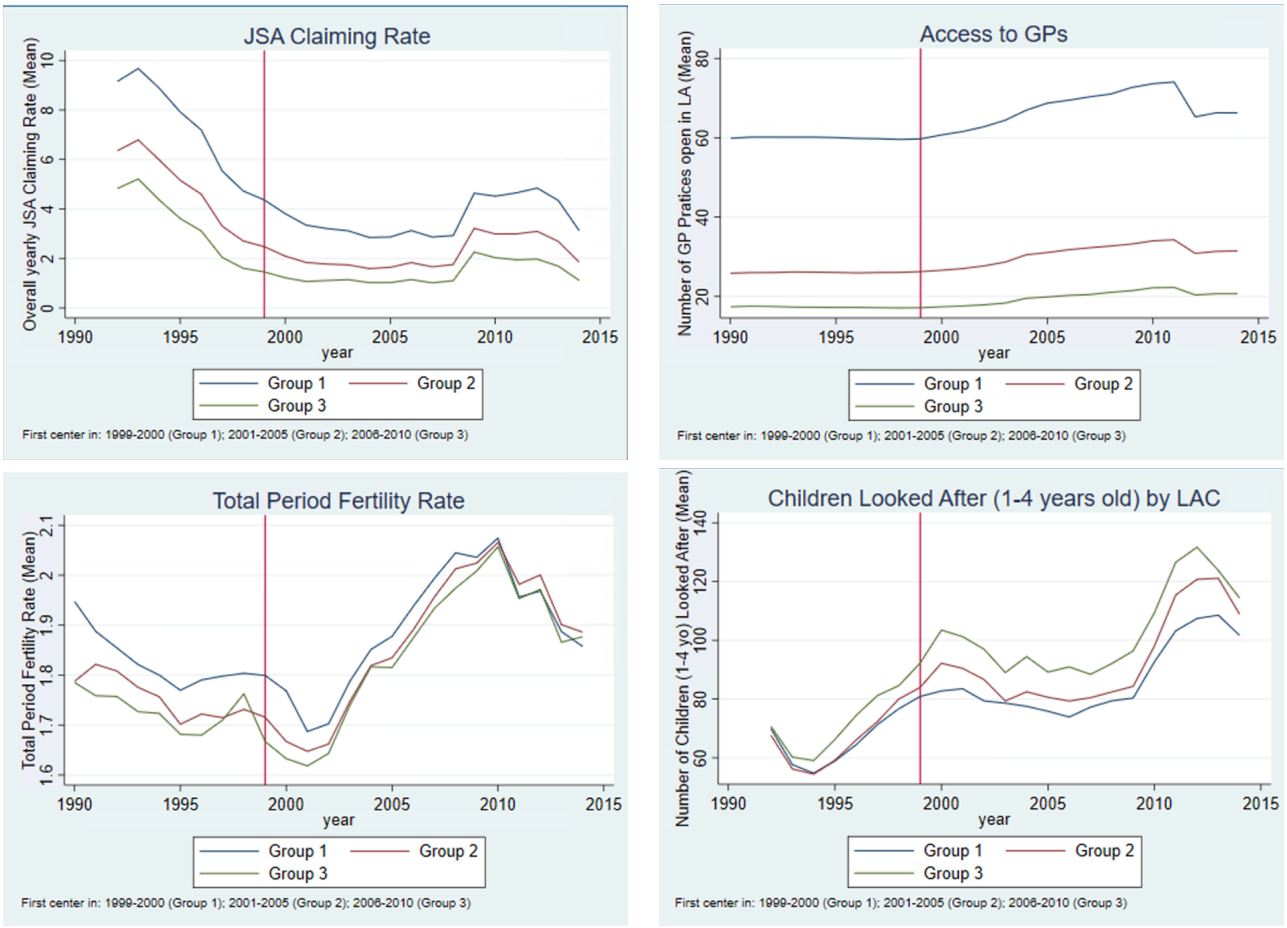
Notes: The figure plots all Sure Start centres that had been opened by the end of the calendar year. Between 2003 and 2006 the number of centres includes both Local Programmes and Children's Centres, less any Children's Centre that shared a postcode with a Local Programme. Sure Start coverage is calculated as the number of open Sure Start centres per thousand children aged 0-4 in each local authority. Isles of Scilly, City of London, and West Somerset are excluded. Source: Authors' calculations using data provided by the Department for Education.

Figure A.3: Sure Start centres per thousand children aged 0-4, by 1998 Index of Local Deprivation quintile and year



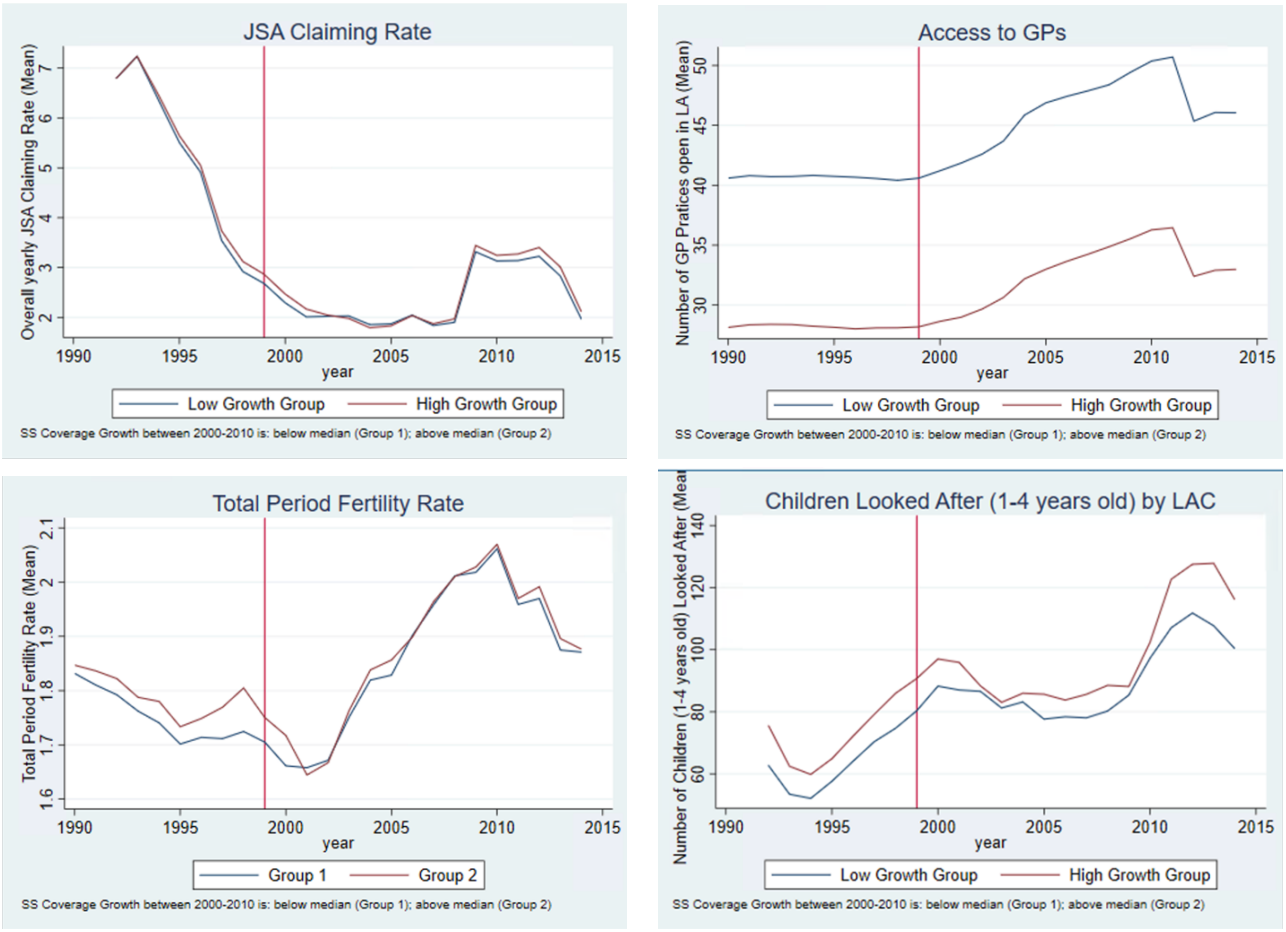
Notes: Sure Start coverage is calculated as the number of open Sure Start centres per thousand children aged 0-4 in each local authority. Isles of Scilly, City of London, and West Somerset are excluded. Source: Authors' calculations using data provided by the Department for Education.

Figure A.4: Relationship between SS introduction and local characteristics



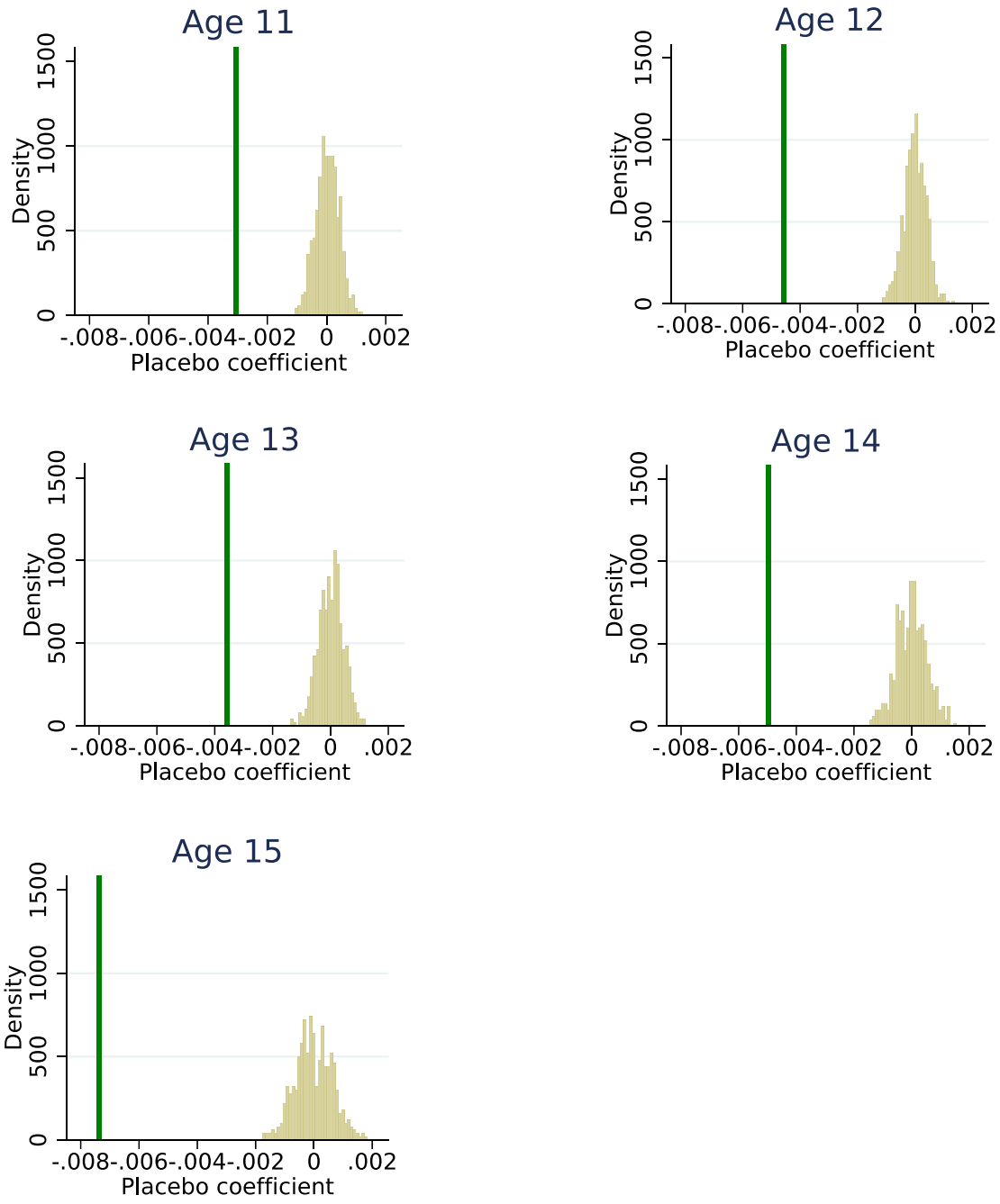
Notes: "Group 1" includes LADs that opened the first centre between 1999-2003 (107 LADs), "Group 2" includes LADs that opened the first centre between 2004-2005 (116 LADs), "Group 3" includes LADs that opened the first centre between 2006-2010 (103 LADs).

Figure A.5: Relationship between SS expansion in coverage and local characteristics



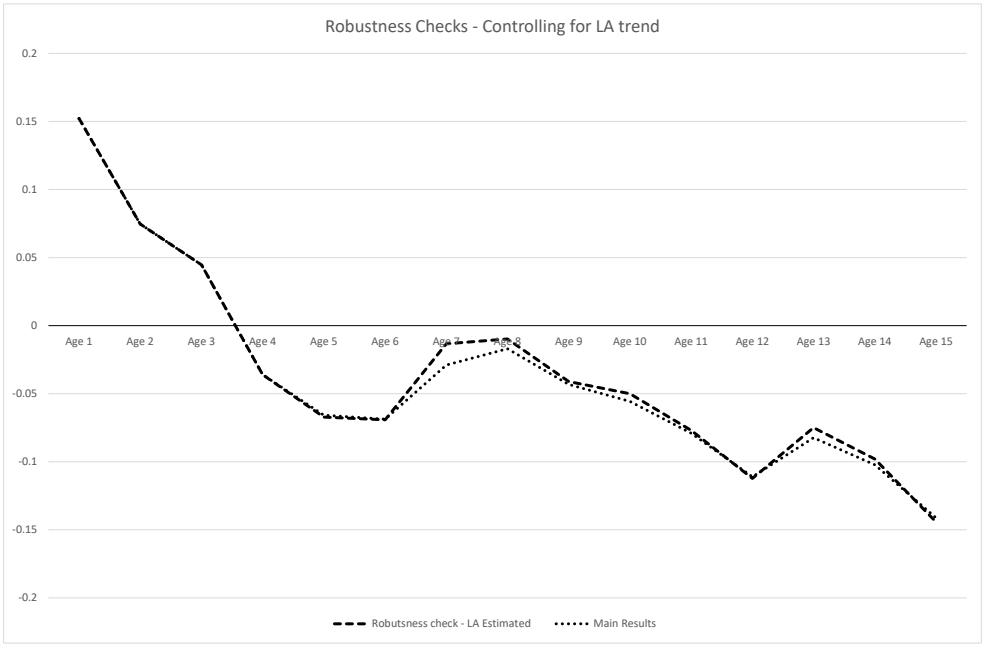
Notes: Sure Start coverage is calculated as the number of open Sure Start centres per thousand children aged 0-4 in each local authority. Low (High) Growth Group: LAs whose SS coverage growth rate between 2000 and 2010 is below (above) the median (median is 0.98).

Figure A.6: Distribution of placebo coefficients at each outcome age



Notes: Graphs show histograms of the distribution of placebo coefficients (in beige), with a bin width of 0.0001. The placebo rollout is simulated 500 times by randomly reshuffling the rollout profiles of the 326 local authorities. The true estimate is shown in green at each age. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2014) and the Department for Education's data on the rollout of Sure Start.

Figure A.7: Sure Start's effect on probability of any hospitalisation, rescaled by baseline probability: Baseline estimates and controlling by a LAD trend



	Age 1	Age 2	Age 3	Age 4	Age 5	Age 6	Age 7	Age 8	Age 9	Age 10	Age 11	Age 12	Age 13	Age 14	Age 15
LA Trend	***	*									*	***	*	**	***
Main Results	***	*									*	***	*	***	***

Notes: Figure shows coefficients from separate regressions for each outcome age, with Sure Start treatment interacted with the three disadvantage categories. Coefficients are rescaled by the gender-specific baseline (1998) mean for each age. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

Table A.1: Covariates used in the rollout analysis

Category	Variable	Source	Years	Geography
Deprivation	Percentile of rank distribution of Index of Local Deprivation	Department of Environment, Trade, and the Regions[1]	1998	LAD
Health Indicators	Under-18 conception rate (conceptions/1,000 women aged 15-17)	Child and Maternal Health Intelligence Network[2]	1998-2014	LAD
	Proportion of births below 2.5kg	ONS Vital Statistics[3]	1991-2014 (interpolated in 2008 and 2009)	LAD
Potential Demand for services	Total period fertility rate	ONS Vital Statistics[4]	1990-2014	LAD
	Density	ONS Population Density[5]	1990-2014	LAD
	% of primary school pupils with English as an Additional Language	National Association for Language Development in the Curriculum (NALDIC)	1999; 2004-2013	County
	Children Looked After per thousand under 1	Department for Education	1992-2014	County
	Children Looked After per thousand 1 to 4	Department for Education	1992-2014	County
Labour Market	Total employment rate	Labour Force Survey[6]	1995-2004	LAD
	Rate of Jobseekers Allowance receipt	Jobseekers Allowance[7]	1992-2014	LAD
	Median weekly pay, full-time women/men[8]	Annual Survey of Hours and Earnings, workplace series[9]	1997-2014	LAD
Political alignment	Lower-level (LAD) council politically aligned to national government	The Elections Centre	1990-2014	LAD
	Upper-level (county) council politically aligned to national government	The Elections Centre	1990-2014	County
Pre-Existing Services	Number of GPs per 1,000 population	Constructed with HSCIC data[10]	1990-2014	LAD
	Number of JobcentrePlus per 1,000 population	Department for Work and Pensions	2001-2014	LAD
	Free entitlement take-up rate among 3-year-olds	Department for Education Statistical Returns	1997-2010	County
	Free entitlement take-up rate among 4-year-olds	Department for Education Statistical Returns	1997-2010	County

Notes: [1] Downloaded 20 Nov. 2015, http://www.legco.gov.hk/yr99-00/english/bc/bc09/papers/1471e01.pdf. [2] Downloaded 02 Nov. 2015, http://atlas.chimat.org.uk/IAS/dataviews/view?viewId=96. [3] Obtained 24 Nov. 2015 from the ONS Vital Statistics Outputs Branch, with help from Laura Todd. [4] Obtained 24 Nov. 2015 from the ONS Vital Statistics Outputs Branch, with help from Laura Todd. [5] Downloaded 18 January 2016 from ONS. [6] Downloaded 02 Dec. 2015 from NOMIS. [7] Downloaded 16 Dec. 2015 from NOMIS. [8] Deflated to constant 2015 pounds using the Consumer Price Index, downloaded from ONS Consumer Price Indices – Tables, table 1.1, series CPI All Items Index (estimated pre-97, 2005=100) on 27 January 2016. http://www.ons.gov.uk/ons/datasets-and-tables/data-selector.html?dataset=mm23. [9] Downloaded 15 December 2015 from NOMIS. [10] HSCIC, 'GPs, GP Practices, Nurses, and Pharmacies', downloaded 26 November 2015.

Table A.2: Effect of an increase in Sure Start coverage on probability of any hospitalisation in the neighbourhood: LSOA of admission at birth vs. current residence

	(1) Age 1	(2) Age 2	(3) Age 3
	Panel A: LSOA of birth/first admission		
Av. Cov. SS	0.0179***	0.0092***	0.0063*
	(0.0033)	(0.0027)	(0.0025)
Marginal Effects	0.20135	0.12725	0.1008
Baseline Mean	0.0889	0.0723	0.0625
	Panel B: LSOA of residence at admission		
Av. Cov. SS	0.0140***	0.0056*	0.0029
	(0.0034)	(0.0026)	(0.0022)
Marginal Effects	0.15217	0.07467	0.04475
Baseline Mean	0.0920	0.0750	0.0648
N	9057216	9844800	10632384
Cohorts			
Earliest	1995	1994	1993
Latest	2006	2006	2006

Note: The table shows coefficients from regression analysis at each outcome age. Observations are cells defined by the LSOA, month and year of birth, and sex. In Panel A we use the LSOA of birth or LSOA of first admission if LSOA of birth is unavailable; in Panel B we include our baseline results, that control for the LSOA at the time of admission. The model regresses an indicator for any hospitalisation in a cell on Sure Start coverage (number of centres per thousand children aged 0–4 in the local authority for outcomes post age 4; when studying outcomes for younger ages, $a = 1, ..., 4$, SS_{dtm} is defined the number of centres per thousand children aged 0-4 that were open in the LA d when the child was aged $a - 1$.), the population at the relevant age in the LSOA and an indicator for female and includes fixed effects for month–year of birth and the LSOA of residence. The baseline mean and SD are measured in 1996. Standard errors are shown in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Table A.3: Sure Start's effect on probability of any hospitalisation: Impacts for urban and rural areas

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
$SSCov\times$	0.0147***	0.0061*	0.0032	-0.0015	-0.0031	-0.0031	-0.0008	-0.0006	-0.0015	-0.0022	-0.0033*	-0.0049***	-0.0039*	-0.0051***	-0.0075***
Urban(β_1)	(0.0035)	(0.0027)	(0.0023)	(0.0028)	(0.0025)	(0.0023)	(0.0020)	(0.0015)	(0.0013)	(0.0012)	(0.0014)	(0.0014)	(0.0016)	(0.0015)	(0.0017)
$SSCov\times$	0.0068	0.0002	0.0011	-0.0058	-0.0069**	-0.0048*	-0.0025	-0.0007	-0.0020	-0.0017	-0.0024	-0.0035**	-0.0021	-0.0046**	-0.0072**
Rural(β_2)	(0.0059)	(0.0030)	(0.0022)	(0.0024)	(0.0021)	(0.0020)	(0.0018)	(0.0014)	(0.0011)	(0.0011)	(0.0013)	(0.0012)	(0.0016)	(0.0017)	(0.0022)
Marginal Eff.															
Urban	0.152	0.078	0.047	-0.024	-0.051	-0.058	-0.018	-0.014	-0.037	-0.057	-0.086	-0.118	-0.090	-0.111	-0.151
Rural	0.085	0.003	0.019	-0.108	-0.132	-0.103	-0.060	-0.019	-0.054	-0.047	-0.065	-0.088608	-0.048837	-0.099783	-0.144
Baseline Mean															
Urban	0.0970	0.0787	0.0678	0.0637	0.0608	0.0532	0.0454	0.0419	0.0402	0.0384	0.0384	0.0417	0.0431	0.0461	0.0497
Rural	0.0802	0.0661	0.0576	0.0539	0.0524	0.0465	0.0418	0.0378	0.0373	0.0359	0.0371	0.0395	0.0430	0.0461	0.0500
$p-value:$															
$H_a:\beta_1\neq\beta_2$	0.180	0.050	0.333	0.025	0.014	0.210	0.183	0.946	0.583	0.587	0.384	0.122	0.145	0.745	0.898
N	9057216	9844800	10632384	11026176	11026176	11026176	11026176	11026176	11026176	11026176	11026176	10632384	9844800	9057216	8269632

Note: The table shows coefficients from regression analysis at each outcome age. Observations are cells defined by the LSOA, month and year of birth, and sex. The model regresses an indicator for any hospitalisation in a cell on Sure Start coverage (number of centres per thousand children aged 0–4 in the local authority for outcomes post age 4; when studying outcomes for younger ages, $a = 1, ..., 4$, SS_{dtm} is defined the number of centres per thousand children aged 0-4 that were open in the LA d when the child was aged $a-1$.), the population at the relevant age in the LSOA and an indicator for female and includes fixed effects for month–year of birth and the LSOA of residence. Standard errors are shown in parentheses clustered by LAD. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Table A.4: Congenital chromosomal defects with treatment at ages 0-4

	(1) Main Cohort	(2) Common Cohort
SS coverage	0.00039	0.000602
	(0.00038)	(0.000606)
N	7875840	5513088
Cohorts		
Earliest	1997	1997
Latest	2006	2003
BL mean	0.008	0.008

Note: The table shows coefficients from regression analysis at each outcome age. Observations are cells defined by the LSOA, month and year of birth, and sex. The model regresses an indicator for any hospitalisation in a cell on Sure Start coverage (number of centres per thousand children aged 0–4 in the local authority for outcomes post age 4), the population at the relevant age in the LSOA and an indicator for female and includes fixed effects for month–year of birth and the LSOA of residence. Standard errors are shown in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Table A.5: Description of cause-specific admissions categories considered

Cause	**ICD-10 codes**	**Description**
Respiratory	J00-J99	Respiratory illnesses; e.g. Pneumonia, influenza, sinusitis
Infections and parasites	A00-B99	Infections (bacterial, viral, and others) and parasites
Ambulatory Care Sensitive (ACS)	See Blunt (2013)	Conditions that are preventable or manageable with primary care; e.g. Gangrene, asthma, tuberculosis
External	S00-T88, V01-Y98	Injuries, poisonings, and other conditions caused by something external to the body
Injuries	S00-T14	Subset of 'external' admissions; e.g. Broken bones, head trauma
Poisonings	T15-T98	Subset of 'external' admissions; e.g. Poisoning by household cleaners, alcohol, medication
Accidents and assaults	V01-Y98	Subset of 'external' admissions; e.g. traffic accidents, falls, burns, assaults

Table A.6: Effect of an increase in Sure Start coverage on probability of hospitalisation for specific causes: External Conditions

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
							Panel A: Poisoning								
	**		*												**
Av SS Cov	-0.0010	-0.0008	-0.0007	-0.0001	-0.0001	-0.0001	-0.0002	0.0002	0.0002	-0.0000	0.0001	0.0001	-0.0002	-0.0005	-0.0013
	(0.0004)	(0.0004)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0003)	(0.0004)
Marginal Effect	-0.14706	-0.11765	-0.16667	-0.03571	-0.03846	-0.04762	-0.125	0.153846	0.153846	0	0.071429	0.052632	-0.06897	-0.1087	-0.23214
Baseline Mean	0.0068	0.0068	0.0042	0.0028	0.0026	0.0021	0.0016	0.0013	0.0013	0.0015	0.0014	0.0019	0.0029	0.0046	0.0056
							Panel B: Fracture Admissions								
					**	***		*	**	*	**				
Av SS Cov	-0.0002	-0.0003	-0.0003	-0.0003	-0.0005	-0.0010	-0.0003	-0.0005	-0.0005	-0.0004	-0.0005	-0.0004	0.0001	0.0003	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)
Marginal Eff.	-0.10526	-0.13043	-0.125	-0.11111	-0.13514	-0.2439	-0.07895	-0.13158	-0.12821	-0.1	-0.11628	-0.08333	0.018182	0.055556	0.021277
Baseline Mean	0.002	0.002	0.002	0.003	0.004	0.004	0.004	0.004	0.004	0.004	0.004	0.005	0.006	0.005	0.005
							Panel C: Head Injury Admissions								
				***	***			*						**	
Av SS Cov	-0.0003	-0.0004	-0.0004	-0.0010	-0.0007	-0.0002	-0.0002	-0.0003	-0.0002	-0.0002	-0.0000	-0.0001	-0.0001	-0.0006	-0.0004
	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)
Marginal Eff.	-0.100	-0.138	-0.174	-0.556	-0.368	-0.133	-0.167	-0.250	-0.200	-0.181	0.000	-0.071	-0.059	-0.286	-0.160
Baseline Mean	0.0030	0.0029	0.0023	0.0018	0.0019	0.0015	0.0012	0.0012	0.0010	0.0011	0.0012	0.0014	0.0017	0.0021	0.0025
							Panel D: Other Injury Admissions								
			**	***	***	***	***	**	*	**	*	*			
Av SS Cov	-0.0001	-0.0007	-0.0010	-0.0019	-0.0018	-0.0019	-0.0011	-0.0009	-0.0007	-0.0011	-0.0008	-0.0008	-0.0004	-0.0008	-0.0010
	(0.0004)	(0.0004)	(0.0003)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0005)
Marginal Eff.	-0.014	-0.095	-0.149	-0.297	-0.250	-0.253	-0.167	-0.136	-0.109	-0.167	-0.111	-0.101	-0.044	-0.086	-0.109
Baseline Mean	0.0073	0.0074	0.0067	0.0064	0.0072	0.0075	0.0066	0.0066	0.0064	0.0066	0.0072	0.0079	0.0090	0.0093	0.0092
N	9057216	9844800	10632384	11026176	11026176	11026176	11026176	11026176	11026176	11026176	11026176	10632384	9844800	9057216	8269632
Cohorts															
Earliest	1995	1994	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993	1993
Latest	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2005	2004	2003

Note: The table shows coefficients from regression analysis at each outcome age. Observations are cells defined by the LSOA, month and year of birth, and sex. The model regresses an indicator for any hospitalisation in a cell on Sure Start coverage (number of centres per thousand children aged 0–4 in the local authority for outcomes post age 4; when studying outcomes for younger ages, $a = 1, ..., 4$, SS_{dtm} is defined the number of centres per thousand children aged 0-4 that were open in the LA d when the child was aged $a - 1$.), the population at the relevant age in the LSOA and an indicator for female and includes fixed effects for month–year of birth and the LSOA of residence. The baseline mean and SD are measured in 1996. Standard errors are shown in parentheses clustered by LAD. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Table A.7: Effect of an increase in Sure Start coverage on probability of maternal hospitalisations in the neighbourhood

Age oldest Child	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
							Panel A: All Admissions								
Av. Cov. SS	-0.0037	-0.0050	-0.0056	0.0008	0.0025	-0.0384***	-0.0492***	-0.0482***	-0.0453***	-0.0236***	-0.0332***	-0.0432***	-0.0628***	-0.0686***	-0.0750***
	(0.0050)	(0.0034)	(0.0037)	(0.0036)	(0.0039)	(0.0046)	(0.0047)	(0.0038)	(0.0037)	(0.0035)	(0.0036)	(0.0040)	(0.0047)	(0.0052)	(0.0054)
Marginal Eff.	-0.030	-0.043	-0.053	0.008	0.027	-0.430	-0.575	-0.572	-0.571	-0.298	-0.396	-0.497	-0.720	-0.771	-0.830
Basel. mean	0.1216	0.1166	0.1049	0.0955	0.0922	0.0892	0.0855	0.0842	0.0793	0.0793	0.0838	0.0869	0.0872	0.0890	0.0904
							Panel B: Mental Health Admissions								
Av. Cov. SS	0.0012*	0.0019**	0.0020**	0.0039***	0.0054***	0.0063***	0.0025	-0.0004	-0.0016	0.0014	-0.0013	-0.0106***	-0.0238***	-0.0307***	-0.0360***
	(0.0005)	(0.0006)	(0.0007)	(0.0009)	(0.0014)	(0.0018)	(0.0020)	(0.0018)	(0.0019)	(0.0020)	(0.0019)	(0.0022)	(0.0025)	(0.0027)	(0.0028)
Marginal Eff.	0.500	0.731	0.769	1.345	1.742	1.750	0.610	-0.089	-0.327	0.264	-0.203	-1.293	-1.653	-1.408	-1.565
Basel. mean	0.0024	0.0026	0.0026	0.0029	0.0031	0.0036	0.0041	0.0045	0.0049	0.0053	0.0064	0.0082	0.0144	0.0218	0.0230
N	2863198	2863198	2863198	2863198	2863198	2863198	2863198	2863198	2863198	2863198	2834022	2705099	2556584	2373998	2158060
Cohorts															
Earliest	1997	1997	1997	1997	1997	1997	1997	1997	1997	1997	1997	1997	1997	1997	1997
Latest	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2006	2005	2004	2003

Note: The table shows coefficients from regression analysis at each outcome age. he model regresses an indicator for any hospitalisation in a cell on Sure Start coverage (number of centres per thousand children aged 0–4 in the local authority for outcomes post age 4; when studying outcomes for younger ages, $a = 1, ..., 4$, SS_{dtm} is defined the number of centres per thousand children aged 0-4 that were open in the LA d when the child was aged $a - 1$.), the population at the relevant age in the LSOA and an indicator for female and includes controls for mother's age at birth of the eldest child, number of children in various age bands at admission, ethnicity, birthmonth/year of the eldest child, fixed effects for month–year of birth and the LSOA of residence. The baseline mean and SD are measured in 1996. Standard errors are shown in parentheses clustered by LAD. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.