The health effects of Sure Start

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Preface

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Executive summary

From lagging well behind most European countries in the early 1990s, the UK is now one of the highest spenders on the under-5s in Europe (OECD, 2014). One of the biggest programmes for this age group is Sure Start. It offers families with children under the age of 5 a 'one-stop shop' for childcare and early education, health services, parenting support, and employment advice, with the aim of improving children's school readiness, health, and social and emotional development.

Sure Start has had a turbulent history. The programme was first introduced in 1999 as Sure Start Local Programmes, targeted at highly disadvantaged neighbourhoods. Five years later, the 10-Year Strategy for Childcare called for 'a children's centre in every community', transforming the initiative into a universal service. At its peak in 2009–10, Sure Start accounted for £1.8 billion of public spending (in 2018–19 prices), about a third of overall spending on programmes for the under-5s (Belfield, Farquharson and Sibieta, 2018). But in the decade since, the context has been one of funding cuts, consolidation and centre closures, with funding falling by two-thirds to £600 million in 2017–18.

Despite its rapid expansion and the subsequent cuts, evidence on the impact Sure Start has had on the health of children and their families is still relatively scarce. The initial evaluation of the programme focused on the earliest years of Sure Start and found some health benefits for children living in neighbourhoods with access to the earliest Sure Start Local Programmes. A subsequent evaluation centred on describing and assessing different patterns of family use in the early 2010s and found little evidence of a link between more frequent use of services and children's health.

In this report, we take a step back to consider the overall impacts on health of the Sure Start programme as a whole between its inception in 1999 and its peak in the late 2000s. Our focus on health outcomes is motivated by the fact that, while Sure Start's services were multifaceted and varied between centres and over time, one of its objectives was to improve children's health and an important component of its offer was health services. These include both direct provision of new services (e.g. baby-weighing clinics) and outreach to signpost parents to existing healthcare.

Data and methodology

We use a new data set that gives the location and opening date of all Sure Start centres (both Children's Centres and their predecessor Local Programmes) combined with big data sets that cover children across England and collect outcomes (on hospitalisations and obesity) over many years. The scale of these data makes it possible for us to generate more precise estimates of the programme impacts and to investigate how these effects differ for different groups.

These big data sets also make it possible for us to look at the impacts of Sure Start for children born over the entire period from its inception in 1999 to its peak in 2009–10. We focus on their medium-run health outcomes to see whether the programme has any impacts that last after children start school (and stop visiting Sure Start centres). We explore the impact that Sure Start has on hospitalisations throughout primary school (ages 5–11). We also provide exploratory evidence about the programme's impacts on child obesity and on maternal mental health.

We use statistical techniques to robustly estimate the *causal* impact that Sure Start has on children's outcomes. We use new data on the precise location and opening date of Sure Start Local Programmes and Children's Centres to define a measure of 'access to' Sure Start, which changed over time as the programme was rolled out. Using a difference-in-differences methodology, we compare the outcomes of children in the same neighbourhood with more or less access to Sure Start, after accounting for both permanent differences between neighbourhoods and nationwide differences between years. This strategy lets us isolate the causal impact of *having access* to Sure Start during the first five years of life on children's later-life health and the health of their mothers.

The measure of 'access' that we use is the number of centres per thousand children of Sure Start age in each local authority. Sure Start centres were designed to serve local children and families, so from a policy perspective benefits should be measured across all the children they were intended to serve (and not only those who use them). For each child, we calculate their access to Sure Start each month for their first five years of life, then calculate an average level of coverage over the ages 0–4.

Key findings

- **Sure Start reduced the likelihood of hospitalisation among children of primary school age. These benefits get bigger as children get older.** We measure the impact of providing greater access to Sure Start – equivalent to boosting Sure Start coverage from nothing to the average level of coverage at the programme's peak, around one centre per thousand children – on the probability that there is at least one hospital admission among the boys or girls born in a particular month and year in a particular neighbourhood. At age 5, greater Sure Start coverage reduced this probability by 4% of its baseline level (though we cannot be statistically confident that this effect is not a result of chance). By age 11 – the last age we look at – the impact has grown to 18% and is strongly statistically significant. This effect is equivalent to averting 5,500 hospitalisations of 11-year-olds each year.

- **At younger ages, a reduction in infection-related hospitalisations plays a big role in driving these effects. At older ages, the biggest impacts are felt in admissions for injuries.** This could suggest that Sure Start is helping young children to develop their immune systems, perhaps through supporting immunisations or exposing children to other children's illnesses (e.g. in childcare). Sure Start drives a significant fall in injuries at every age we consider, with the probability of an injury-related hospitalisation falling by around 17% at the younger ages and by 30% at ages 10 and 11.

- **Sure Start benefits children living in disadvantaged areas most.** While the poorest 30% of areas saw the probability of any hospitalisation fall by 11% at age 10 and 19% at age 11, those in more affluent neighbourhoods saw smaller benefits, and those in the richest 30% of neighbourhoods saw practically no impact at all. The bigger benefits in the poorest neighbourhoods could come about because disadvantaged children are more able to benefit from Sure Start, because the types of services Sure Start offers in poorer areas are more helpful, or because children in disadvantaged areas were more likely to attend a centre.

- **We find no evidence that Sure Start has impacted child obesity at age 5, or maternal mental health.** However, given data limitations, the lack of evidence for Sure Start's impacts should not be taken as evidence that there is in reality no effect.

- **A simple cost–benefit analysis shows that the benefits from hospitalisations are able to offset approximately 6% of the programme costs.** We consider the costs of giving access to Sure Start to a cohort of children, and the corresponding benefits of reducing hospitalisations for injuries and infections, in terms of averted direct healthcare costs, averted indirect (parental and societal) costs, and the financial impacts of their consequences over the life course.

Conclusions

Sure Start is one of the biggest early years programmes in England, and one of the most controversial. Since the removal of ring-fenced funding in 2011, local authorities have had to make decisions about whether – or to what extent – to protect their Children's Centre services. Their choices have been starkly different; some authorities have closed most or all of their centres, while their neighbours have chosen not to officially close any. Instead, many local authorities have delivered cuts in other ways, such as curtailing opening hours or cutting back on services delivered through the centres (Smith et al., 2018), or merging them.

These decisions have been made in the context of limited evidence about the impact that Sure Start has had on children and their families. Our research suggests that the Sure Start programme has had significant benefits for children's health, preventing hospitalisations throughout primary school. But these benefits are only felt in the most disadvantaged areas. Of course, these findings relate only to health outcomes; Sure Start might have different effects on other outcomes, such as children's school readiness or parents' employment decisions. Nevertheless, our findings about the health impacts of Sure Start raise two questions for policymakers.

The first question is about Sure Start's total level of resources: given the substantial benefits for children's health that we find, is the current level of cuts to Sure Start's budget appropriate? Measured on a purely financial basis, the reduction in hospitalisations at ages 5–11 saves the NHS approximately £5 million, about 0.4% of average annual spending on Sure Start. But the types of hospitalisations avoided – especially those for injuries – also have big lifetime costs both for the individual and the public purse (e.g. through healthcare costs and the tax and benefit system). Including these savings as well, the financial benefits of Sure Start's effects on hospitalisations amount to 6% of its budget.

Of course, it is crucial to emphasise that we are considering the financial impacts of only a narrow set of potential outcomes and setting them against the total cost of Sure Start. Further research is needed (and some is already under way) to determine whether the programme has had other effects – for example, on academic and behavioural outcomes and on the demand for social care. As well, society might value the improvement in children's health on its own merits, particularly since these benefits are concentrated in the most disadvantaged areas.

But even if some of the cuts to Sure Start are reversed, it would be a long journey back to the funding levels the programme enjoyed in 2009–10. So in a context where spending is cut back from its peak, how should these cuts be delivered? Our results suggest that one way to deliver more value for money would be to focus on providing services to the disadvantaged areas, which are more likely to benefit from them, and to consider which types of services and models of provision could most effectively help this group.

More broadly, all interested parties – politicians, policymakers and practitioners – should continue to assess how the resources targeted at early intervention can best be used to improve outcomes for children and their families, and how they can best encourage the families who would benefit the most to use the centres.

The government has recently commissioned the Early Intervention Foundation to review the existing evidence on Children's Centres and other early years services and to develop practical materials for local authorities to use in producing a model for Children's Centres in their area.

The government has been clear that it believes that decisions about Sure Start should be taken at the local level, with local authorities free to decide how to fund their Children's Centres and which services the centres should deliver. It is crucial that both local authorities and central government consider the effectiveness of Sure Start for different groups when deciding how services should be delivered going forward. Our research clearly shows that the majority of the benefit of Sure Start has been felt in the most disadvantaged neighbourhoods – so policymakers at both the central and local levels should think about how Sure Start spending can be targeted to deliver the best value for money.

Our research cannot say why children in disadvantaged areas benefit so much more from access to Sure Start than their better-off peers. Children in disadvantaged areas may be more likely to use these services. The types of services or the longevity of centres in disadvantaged areas might differ from those in richer neighbourhoods. There might also be more scope for Sure Start to improve health in more disadvantaged areas, since on average they have more hospitalisations. Unpicking the extent to which each of these possibilities holds will be crucial for deciding how to draw lessons from the success of Sure Start in the most disadvantaged neighbourhoods. To do so, researchers must have access to data on the take-up of services. These data are already being collected by local authorities, but it is very difficult for researchers to access.

More broadly, as Sure Start enters its 20th year, the government has an opportunity to set out a vision of the purpose of Children's Centres based on the best available evidence.

1. Introduction

From lagging well behind most European countries in the early 1990s, the UK is now one of the highest spenders on the under-5s in Europe (OECD, 2014). The introduction and expansion of programmes such as free childcare and tax credits for families accounted for a big rise in spending on this age group, from about £100 million in the early 1990s to almost £6 billion in 2017–18 (Belfield, Farquharson and Sibieta, 2018). One of the biggest programmes for this age group is Sure Start, which offers families with children under the age of 5 a 'one-stop shop' for childcare and early education, health services, parenting support, information about health and child development, and employment advice. Introduced in 1999, the programme saw a meteoric rise: at its peak in 2009–10, there were more than 3,200 Sure Start Children's Centres in operation and the programme accounted for about a third of spending on the early years (Belfield, Farquharson and Sibieta, 2018). Since then, however, the context has been one of funding cuts, consolidation and centre closures, with funding falling by two-thirds to £600 million in 2017–18.[1]

Sure Start Local Programmes and, subsequently, Children's Centres were created with the goal of delivering the best start in life for every child by improving their outcomes and supporting parents both in their relationships with their children and in their aspirations towards employment (Childcare Act 2006). Although the programme 'universalised' as time passed, Sure Start was initially created to help the most disadvantaged children, thus aiming at reducing inequalities in child outcomes. Unlike many other well-known early years programmes targeted at the poorest, such as Head Start in the US, Sure Start was designed to serve all children in the community (rather than being means-tested). The Sure Start programme was also designed to promote child development by offering a range of integrated services extending much beyond the provision of early education. This holistic approach meant that Sure Start had the potential to improve many aspects of child development, including cognitive ability, social and emotional skills, and health.

Despite its rapid expansion and the subsequent cuts, evidence on the impact Sure Start has had on the health of children and their families is still relatively scarce. The initial evaluation of the programme focused on the earliest years of Sure Start, while the subsequent evaluation richly described and assessed the differences in types of services and patterns of family use. In this report, we take a step back to consider the overall impacts on health of the Sure Start programme as a whole between its inception in 1999 and its peak in the late 2000s.

Our focus on health is motivated by the fact that, while Sure Start's services were multifaceted and varied between centres and over time, one of its objectives was to improve children's health and an important component of its offer was health services. These include both direct provision of new services (e.g. baby-weighing clinics) and outreach to signpost parents to existing healthcare.[2]

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[1] Monetary amounts in this paragraph are in 2018–19 prices.

[2] The earliest guidance for Sure Start included 'primary and community healthcare and advice about child health and development and family health' as a core service to be offered by the first Sure Start centres (Department for Education and Employment, 1999). From 2004, Sure Start centres were asked to contribute to achieving the outcomes set out in Every Child Matters policy, the first of which was for every child to 'be healthy'. The Childcare Act 2006 made health services statutory partners in providing services (Lewis, 2011), while the 2013 statutory guidance for Children's Centres states that 'the core purpose of [Sure Start]

Moreover, an emerging international evidence base suggests that early intervention programmes – even those that do not target health directly – can have benefits for health both in the short run and over the life cycle. These programmes can therefore be a cost-effective way to prevent disease, with benefits for both individual welfare and the public purse (Shonkoff, Boyce and McEwen, 2009; Conti, Mason and Poupakis, 2018; García, Heckman and Ziff, 2018).

While the existing evidence base suggests that Sure Start might have benefits for health, the Sure Start programme is quite different from what has been studied before. The existing evidence base centres mostly on programmes that treat a small, highly targeted group to an intensive intervention. These have been run mainly in the US, and existing studies that suggest that some of the important outcomes of these programmes – such as take-up of private health insurance – will not be relevant in the UK context (Conti, Heckman and Pinto, 2016). Other recent research evaluating universal public health interventions in Europe tend to focus on initiatives introduced in the first half of the 20th century, when the provision of public services was quite different from what it is today (Bhalotra, Karlsson and Nilsson, 2017; Hjort, Sølvsten and Wüst, 2017; Bütikofer, Loken and Salvanes, 2018).

Because of these differences in the types of programmes evaluated and the context that they are in, the existing evidence is only of limited relevance to understanding the health impacts that big, universal, relatively light-touch interventions such as Sure Start can have over and above a status quo that includes free healthcare and subsidised early education and childcare.

Previous research on Sure Start

Sure Start itself has been evaluated twice before: once in the earliest years of the programme and again after its peak in the early 2010s. The first evaluation, the National Evaluation of Sure Start (NESS), analysed a sample of the earliest Local Programmes located in the poorest 20% of areas in England. The children in these neighbourhoods were compared with others surveyed in an earlier national survey living in disadvantaged areas not served by the earliest Sure Start programmes. This evaluation found an increase in hospitalisations at age 9 months, no change in health outcomes at age 3 years, and lower body mass index (BMI) and better health status by age 5 years for children living in the Sure Start neighbourhoods (National Evaluation of Sure Start, 2005, 2008 and 2010).

The authors also found that children in Sure Start neighbourhoods were more likely to be immunised and less likely to have had an accidental injury at age 3. However, they note that 'caution is warranted in interpreting these two effects'; because data on comparison areas were collected two years before data on children in Sure Start neighbourhoods, these results might conflate the impact of Sure Start with a general, nationwide improvement in these outcomes over time (National Evaluation of Sure Start, 2008).

Between 2011 and 2013, the Evaluation of Children's Centres in England (ECCE) collected detailed and extensive data on a subsample of centres and their users. This study estimated impacts of Sure Start by comparing the outcomes of children who use the

children's centres is to improve outcomes for young children and their families and reduce inequalities between families in greatest need and their peers in ... child and family health and life chances' (Department for Education, 2013).

services with varying frequency. The advantage of this approach is that it focuses on the outcomes of children who actually use Sure Start services, and it is able to discuss in detail the association between different patterns of use (frequency and types of services) and the outcomes of parents, families and children. However, because the use of these services is optional, with this approach we cannot be sure to what extent the effects reflect the impact of Sure Start itself or differences in the types of families that use Children's Centres frequently or not at all.[3] 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 outreach and formal childcare – on the probability of parent-reported health status worsening over time (Sammons et al., 2015).

The overall impacts of Sure Start

Our research builds upon and adds to the existing body of evidence about the impacts of Sure Start in several important ways. First, we cover a longer period of Sure Start's operation; rather than focusing on the earliest Local Programmes or the operation of a sample of Children's Centres in the early 2010s, our analysis covers the period from the inception of Sure Start in 1999 until 2010. This is the period when Sure Start was expanding and operating at its peak. While there was quite a bit of variation in the services offered both between centres and over time, during this period the funding allocated to Sure Start was relatively protected.

In addition to considering a longer period of Sure Start's operation, our analysis also considers longer-run impacts that the programme has had on children. We estimate the impact of the programme on children's hospitalisations from age 5 to age 11. This is particularly important since it helps to trace out whether the impacts of Sure Start persist after children have aged out of Sure Start eligibility. Because health at older ages builds on health in the early years, tracing out these effects in the medium term is an important step towards a comprehensive cost–benefit analysis of the programme.

Finally, an important innovation of our study is the data and methodology that we use. We use big data sets that cover children across England and collect outcomes (on hospitalisations and obesity) over many years. The scale of these data makes it possible for us to generate more precise estimates of the programme impacts and to investigate how these effects differ for different groups.

At the same time, these big data sets allow us to use statistical techniques to robustly estimate the *causal* impact that Sure Start has on children's outcomes. This means that we can disentangle the impact of providing access to Sure Start services from the impact of other characteristics, such as local levels of disadvantage or parents' attitudes, that might also affect health outcomes. Specifically, we use a quasi-experimental strategy exploiting the roll-out of the centres over time to isolate the causal impact of *having access* to Sure Start during the first five years of life. This is different from the results reported in ECCE, which measure the impact of *using* the services of centres. However, it is no less policy-

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[3] While the ECCE team takes into account some observable characteristics, such as family income, there are many potential drivers of the decision on when and how to use Sure Start services that are not observed by researchers but might also be associated with children's health. For example, if parents who are particularly eager to improve their child's health are both more likely to use Sure Start and more likely to take other measures, this study would overstate the benefit of Sure Start. On the other hand, if families with existing health problems are more likely to visit Children's Centres – perhaps because of outreach by centre staff – then this method might understate Sure Start's effects.

relevant: Sure Start centres were designed to serve local children and families, so from a policy perspective benefits should be measured across all the children they were intended to serve (and not only those who use them).

Our analysis is focused on whether Sure Start as a whole was effective in promoting children's health outcomes. Of course, Sure Start was not just one programme; different centres offered different services at different times, and with different levels of effectiveness. We therefore also discuss the lessons from the NESS and ECCE evaluations on some of the characteristics of particularly effective centres.

The report proceeds as follows. The next chapter presents the institutional background of Sure Start and a detailed analysis of the roll-out of the programme across England. Chapter 3 describes the services provided and discusses the channels through which we might expect Sure Start to improve children's health. Chapter 4 details our analytical strategy and the data we use. In Chapters 5 and 6, we present the impacts of Sure Start on hospitalisations in general and for specific causes, respectively. Chapters 7 and 8 examine Sure Start's impacts on child obesity and maternal mental health, respectively, and Chapter 9 provides a cost–benefit analysis. Finally, Chapter 10 concludes.

2.   The roll-out of Sure Start

Key findings

- **In line with policy guidance, disadvantaged areas opened Sure Start centres earlier.** Even after the 2004 pledge to universalise Sure Start, policymakers rolled out new centres to disadvantaged areas first. In each year until 2007, more than 60% of new centres opened in the 20% most disadvantaged areas. From 2007 onwards, Sure Start 'filled in' by opening centres in better-off neighbourhoods. By the end of the roll-out period in 2010, Sure Start coverage was lowest in the poorest areas, which had seen the fastest population growth.

- **Other characteristics of the local authority were related to the speed of the Sure Start roll-out, over and above the guideline characteristics.** Areas with lower employment rates were more likely to see higher Sure Start coverage the following year, possibly because policymakers were targeting Sure Start centres as part of a push to help mothers to work. A higher share of non-native-English-speaking children is associated with lower Sure Start coverage, while areas where control of the county or borough council switched between 1993 and 2010 tended to have higher coverage when controlled by the Labour party (who were in power nationally) the previous year.

- **There is variation in the level of access to Sure Start and the speed of the roll-out between similar areas.** While Sure Start centres generally opened in more disadvantaged areas first, there was wide variation in the roll-out across England. Similar local authorities often experienced quite different roll-outs, and this cannot be fully accounted for by a wide range of local authority characteristics. This means that there is enough variation in access to Sure Start between similar areas to support the methodology we use to estimate Sure Start's impacts.

- **Since 2011, over 500 Sure Start sites have been closed, and more have cut back on services.** Since 2011, local authorities have adjusted to the removal of the funding ring fence in several ways, including centre closures, consolidation of several centres into one and/or reduction in services and hours of operation. The poorest 20% of neighbourhoods have seen 19% of closures, though since they had more centres to start with they have closed a smaller fraction of their centres than better-off areas.

Sure Start is a major part of the UK's early years landscape. First introduced in November 1999, the programme has changed substantially over the last 20 years in its size, scope and service offer.

The Sure Start programme scaled up very quickly, growing from 21 Sure Start Local Programmes in 1999 to almost 3,300 Children's Centres by 2010. But more recently, the context of the Sure Start programme has been one of cuts to funding and of consolidations and closures in the network of Children's Centres.

In this chapter, we focus mainly on the roll-out of Sure Start, which underpins the methodology that we will use in later chapters to evaluate the programme's impacts. Section 2.1 gives a short overview of the history of the programme, including the guidelines behind the roll-out and the services that Sure Start offers. Section 2.2 presents a more detailed analysis of the factors that drove decisions about whether, where and when to open new centres. In Section 2.3, we return to the patterns of closures and provide a preliminary description of how they are distributed around the country.[4]

2.1   History of Sure Start in England[5]

First introduced as a flagship programme of the New Labour government, Sure Start grew rapidly between 1999 and 2010. Figure 2.1 shows that this growth was not always even; the number of Children's Centres designated in each year substantially increased after 2005. The pattern of growth reflects some of the policy decisions made during the roll-out, which are summarised in Table 2.1.

The initial rapid roll-out of Sure Start reflected policymakers' conviction that early intervention could improve outcomes as well as the programme's popularity with parents around the country. When the programme was first announced, the government set aside three years of funding (1999–2000 to 2001–02) to set up 250 projects in areas with very high concentrations of children under 4 living in poverty. Each project was intended to run for seven to ten years and, in total, the programme would reach up to 150,000 children (Pugh, 2010).

In January 1999, the newly formed Sure Start Unit (a joint responsibility of the education and health departments) identified 60 'trailblazer' districts that would be invited to submit project proposals. Timelines were tight; proposals were due by 19 March 1999. 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.

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4   Although we describe patterns of closures in the report, our empirical analysis of the health benefits of Sure Start does not exploit them. We are primarily interested in the impacts of the programme before 2011.

5   In this section, we give a brief overview of the history of Sure Start. There are many other sources that cover the history of this programme in more depth, such as Eisenstadt (2011). Moreover, we focus on the programme in England. Scotland and Northern Ireland both run similar 'Sure Start' programmes, while Wales introduced the analogous 'Flying Start' programme in 2007.

In 2000, the government announced that it would more than double the programme target, from 250 Local Programmes to 530. In total, the government opened 524 Local Programmes (Meadows et al., 2011).

Figure 2.1. Number of Sure Start centres in England
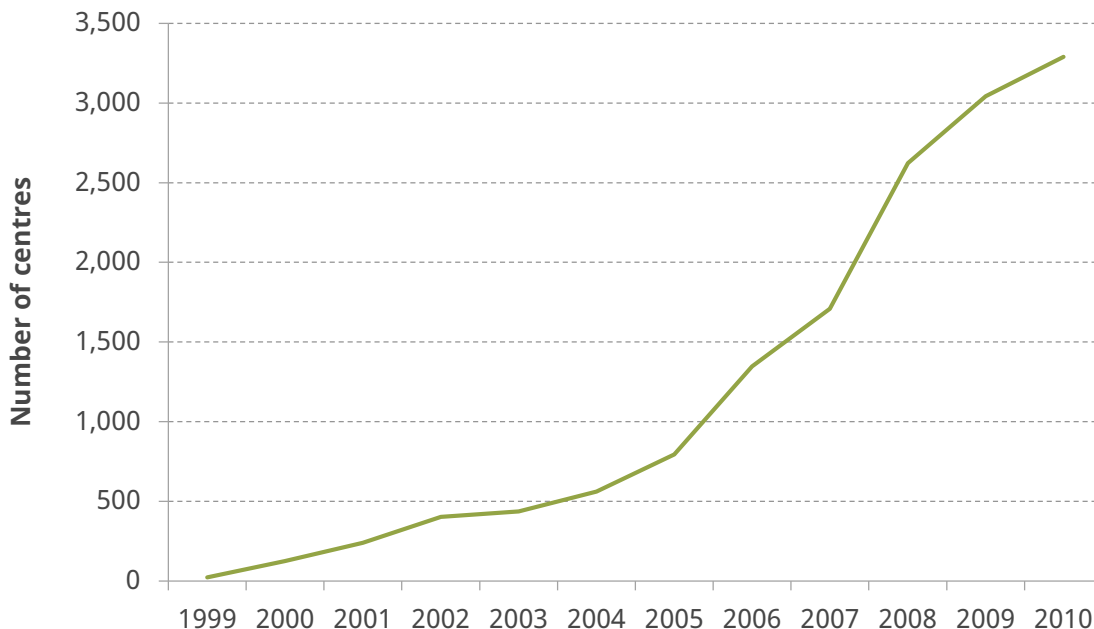
Note: The number of centres is based on centres observed in data received from the Department for Education. Since the treatment of arrangements such as satellite sites was not always consistent, these numbers might not exactly match other data sources. We assume that a Sure Start Children's Centre (SSCC) 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.

Table 2.1. Timeline of the Sure Start roll-out

	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 Sure Start Transition from Local Programmes to Children's Centres
Dec. 2004	10-Year Strategy for Childcare pledges a Sure Start Children's Centre in every community by 2010
2004–06	Phase 1 – targeting 20% most disadvantaged areas Most SSLPs transition to SSCCs
2006–08	Phase 2 – targeting 30% most disadvantaged areas
2008–10	Phase 3 – 'a Children's Centre in every community'

The launch of the Every Child Matters initiative in 2003 strengthened the government's focus on its early childhood strategy, of which Sure Start was an integral part. In 2003, Chancellor Gordon Brown announced that Sure Start programmes would not be phased out after 10 years, as had been planned. Instead, the government proposed a new long-term plan to transfer responsibility for the programme to local governments, who would keep the programme running indefinitely as Sure Start Children's Centres (Lewis, 2011).

In addition to the promise that Children's Centres would be supported indefinitely, the government also announced plans to universalise Sure Start, with the 10-Year Strategy for Childcare in 2004 pledging 'a children's centre in every community' by 2010. This led to the rapid increase in the number of Sure Start centres from 2005 onwards, though the pace of expansion was described in evidence to a Select Committee as 'cruel' and 'demanding' (House of Commons Children, Schools and Families Committee, 2010).

Guidelines for the roll-out[6]

Sure Start was intended to be a flexible initiative that would respond to local needs. However, central government had overall control of the funding for the programme as a whole, and therefore input into how it was allocated between local areas.

These functions were carried out by the Sure Start Unit (SSU), which was responsible for the programme's operation in England. The SSU's initial role included identifying eligible local authorities (LAs), administering the application process, setting national targets, coordinating support for Sure Start Local Programmes (SSLPs) 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 roll-out. The most important of these was that disadvantaged communities should have priority in opening Sure Start facilities, but other measures were used as well. For example, the initial 60 'trailblazer' districts were selected based on the 1998 Index of Local Deprivation, augmented with low birthweight and teen pregnancy indicators (and with the set of trailblazers chosen to offer a good spread of different types of areas around the country) (Department for Education and Employment, 1999).

The transition to Children's Centres brought with it a shift of administrative control, with Children's Centres operating under LAs' control. This meant that former SSLPs that had been led by an organisation other than the LA (including those that had been led by the NHS) were transitioned into LA management. Instead of central government funding individual programmes through the Sure Start Unit, the LAs received funding to allocate between Sure Start centres within their jurisdiction. LAs were given a target number of Children's Centres to develop over time and were allocated a budget based on their number of children under 5 and the level of deprivation, but had responsibility for choosing the specific locations of their Children's Centres (Lewis, 2011). Between 2004 and 2011, funding for the development and management of Children's Centres services was channelled by the Department for Education (DfE) through a series of ring-fenced grants to LAs which could only be spent on early years and childcare services. Since April 2011, however, the ring-fencing has been removed.

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6   Further details about the guidelines are available in Appendix A.

Policymakers were given less guidance on how to run the roll-out of the Sure Start Children's Centres (SSCCs). The pledge to universalise the programme meant that the question was no longer which children would be served, but rather how quickly they would get access to it. In addition, the very tight timeline for programme expansion and greater LA control meant that the roll-out of SSCCs was more strongly driven by feasibility than by scores on a standardised set of indicators.

However, the Children's Centre roll-out retained a strong focus on prioritising the most disadvantaged areas. 'Phase 1' centres (opened approximately between 2004 and 2006) were intended to offer full coverage of the 20% most disadvantaged wards,[7] and mostly grew out of existing Local Programmes or Early Excellence Centres.[8]

Between 2006 and 2008, 'Phase 2' of the Children's Centre roll-out opened centres covering the 30% most disadvantaged areas, with some centres outside these areas. The final phase of the SSCC roll-out saw the universalisation of the programme, with full coverage of the remaining 70% of areas. Figures 2.2 and 2.3 suggest that these deprivation guidelines were taken seriously. The four maps in Figure 2.2 show with red dots the location of Sure Start centres in 2000, 2004 (at the start of the Children's Centre roll-out), 2006 (at the start of Phase 2 of the Children's Centre roll-out) and 2008 (at the start of Phase 3).[9] For each year, the figure presents maps of England (on the left) and Greater London (on the right), shaded by their rank in the 1998 Index of Local Deprivation so that more disadvantaged areas are darker.

The Local Programmes opened in the first two years of the roll-out were predominantly located in more disadvantaged areas. By the time all of the Local Programmes had been opened, in 2004, the focus on relatively poor areas was even more obvious. This is particularly clear when looking at London, where the inner London boroughs that were among the 5% most disadvantaged local authorities had several centres each, while neighbouring, more affluent boroughs such as Harrow had not yet opened a single one.

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.

Figure 2.3 confirms that Sure Start was strongly targeted at the most disadvantaged areas first. Over 90% of Local Programmes opened in the first year were located in neighbourhoods that were among the 20% most disadvantaged nationally (and 70% of the first SSLPs were in the 10% most disadvantaged areas). These most disadvantaged fifth of neighbourhoods opened between 60% and 80% of new centres until 2006. In the late 2000s, centres were much more likely to open in relatively better-off neighbourhoods.

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[7] Following the release of the 2004 deprivation indices at lower-layer super output area (LSOA) level, this target was redefined as the 30% most deprived LSOAs (National Audit Office, 2006).

[8] Early Excellence Centres (EECs) were introduced in December 1997 and funded through March 2006. They were intended to integrate early education and childcare for children under 5 and to develop models of best practice in existing provision. Two-thirds of pilot EECs were located in wards in the bottom 20% of the distribution of the Index of Local Deprivation (House of Commons Children, Schools and Families Committee, 2010).

[9] Information about the location and opening date of the Sure Start centres comes from data provided by the Department for Education.

These better-off neighbourhoods therefore received Sure Start *later* (and perhaps *less intensively*), but – as promised by the drive for 'a Children's Centre in every community' – they were not shut out of the programme entirely.

Figure 2.2. Sure Start centres around England
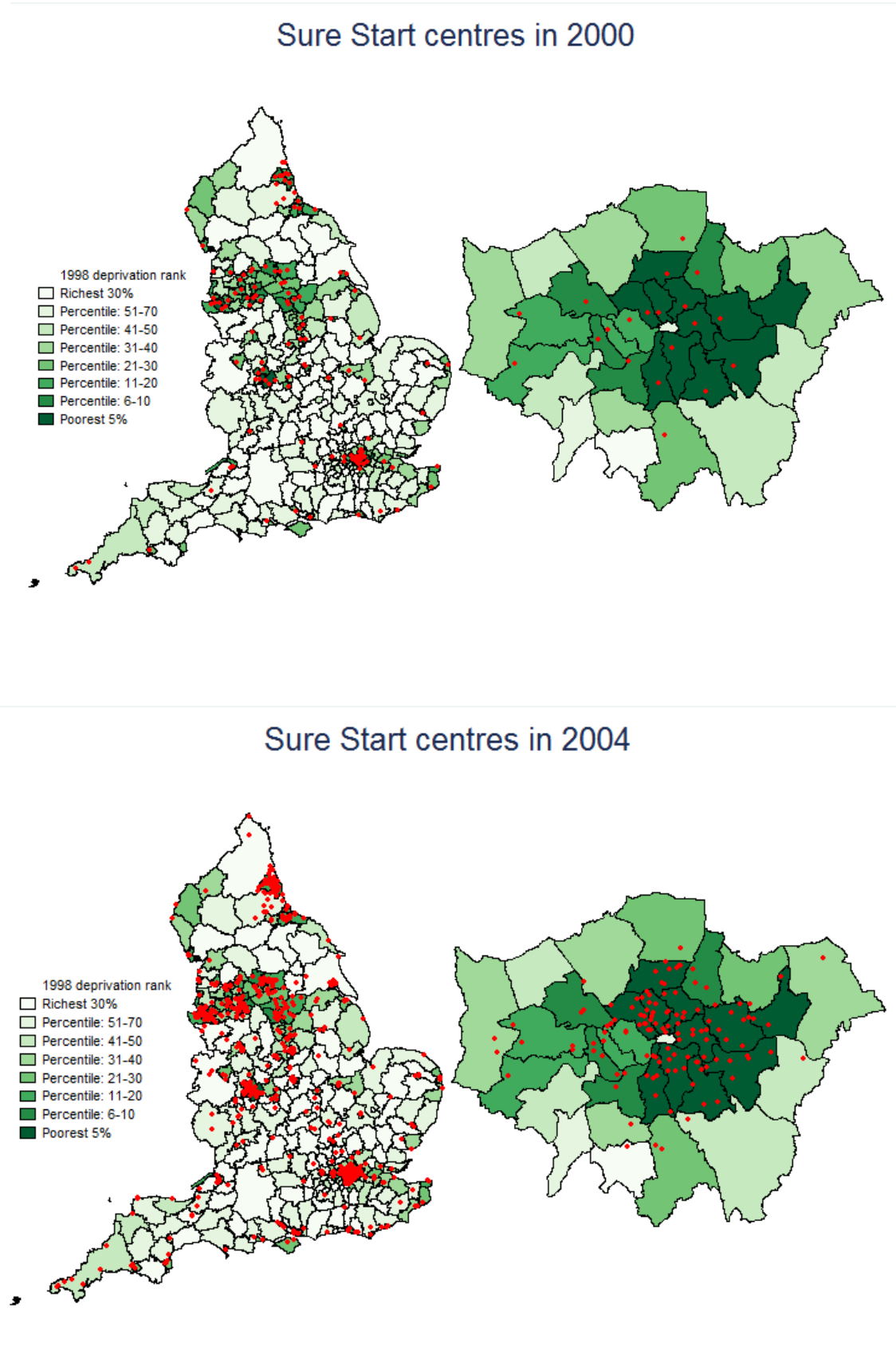
Sure Start centres in 2006
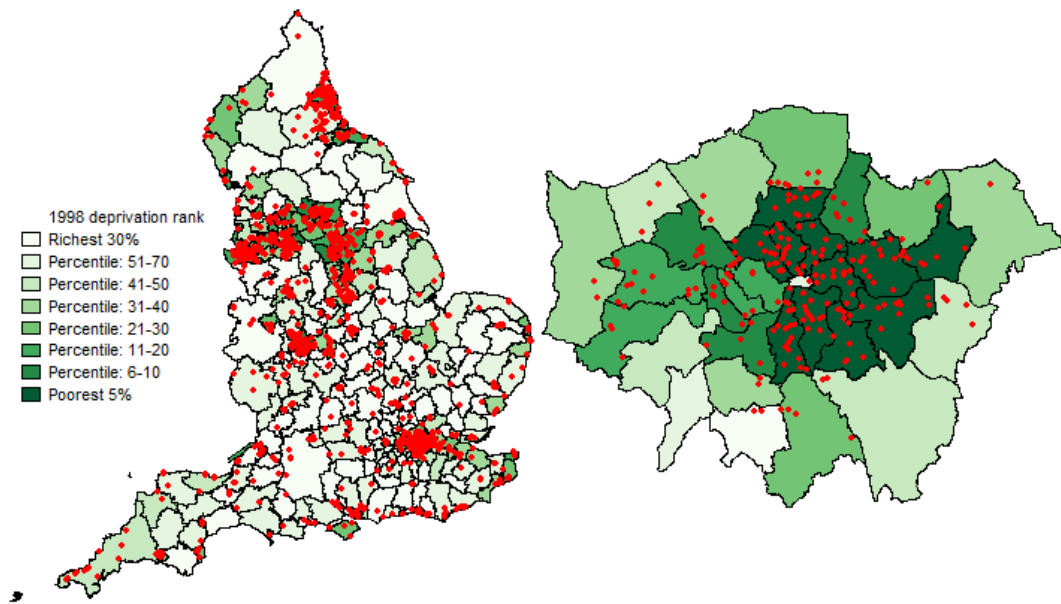
Sure Start centres in 2008
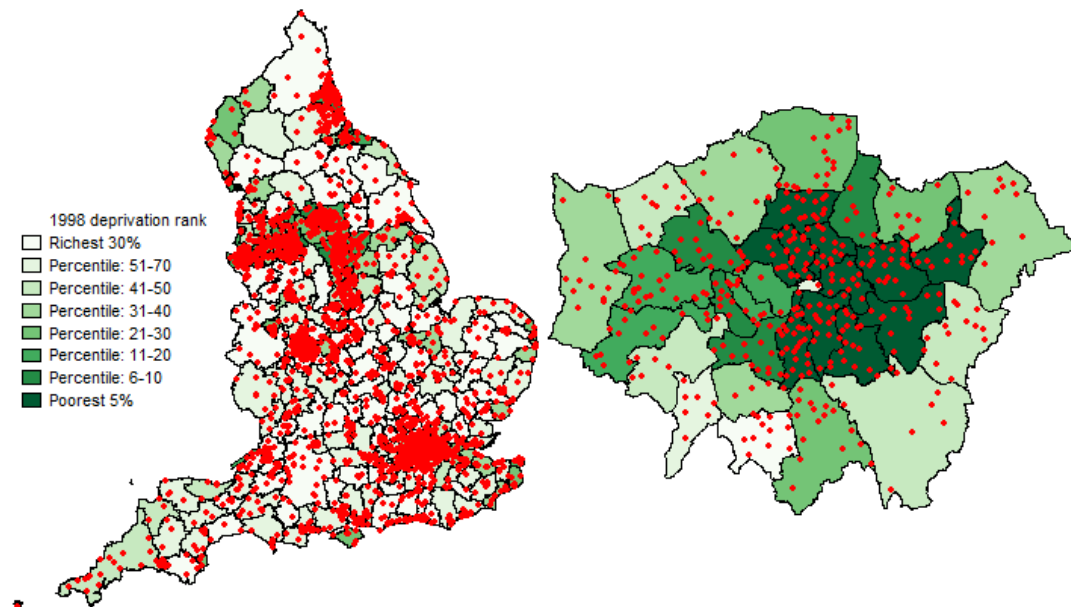
Note: Local authorities are coloured by their rank in the 1998 Index of Local Deprivation, with more disadvantaged areas shaded more darkly. Each red point indicates the location of a Sure Start centre (SSLP or SSCC).

Source: Authors' calculations using data provided by the Department for Education and the 1998 Index of Local Deprivation.

Figure 2.3. Share by deprivation of new centres opened each year
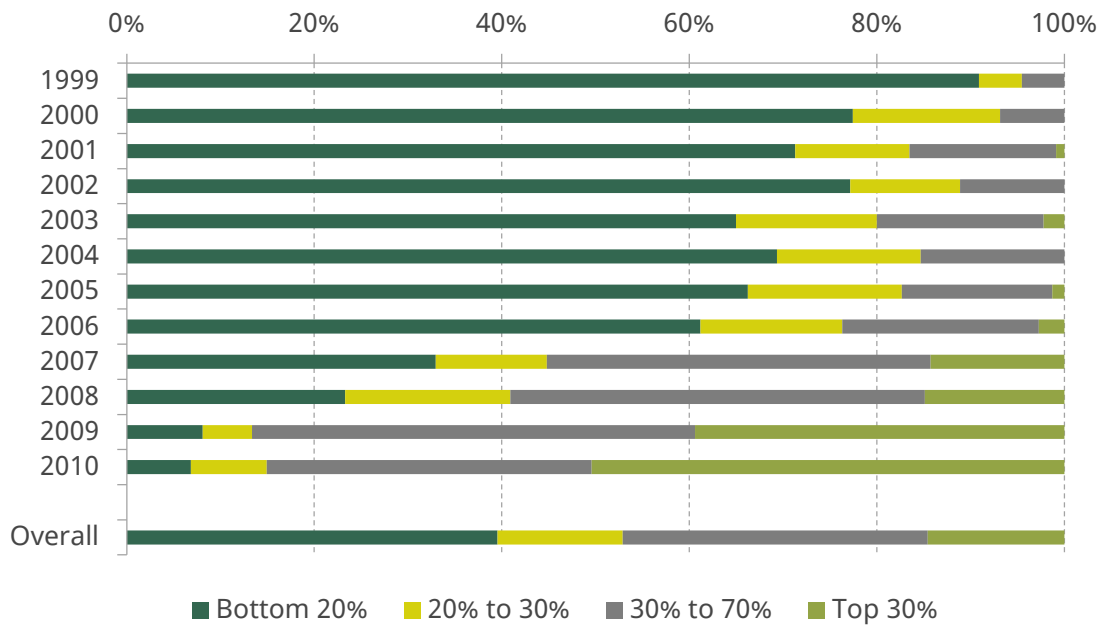
Note: 'Bottom 20%' refers to neighbourhoods (LSOAs) in the bottom 20% of the national 2004 Index of Multiple Deprivation ranking, i.e. the most disadvantaged. Other categories are similarly defined, with 'Top 30%' incorporating the least disadvantaged areas. Centres in the City of London, Isles of Scilly and West Somerset are excluded for consistency with the impact analysis in this report.

Source: Authors' calculations using data provided by the Department for Education and the 2004 Index of Multiple Deprivation.

2.2   Analysis of Sure Start roll-out

There were clear guidelines as to how Sure Start ought to be rolled out: disadvantaged areas were consistently prioritised to receive Sure Start services earlier and more intensively, and the earliest phase of the Sure Start roll-out also focused on areas where teen pregnancy and low birthweight were significant local problems. In Section 2.1, we presented descriptive evidence showing that the roll-out was indeed strongly related to deprivation, which influenced the order in which areas received new centres. However, given the very fast expansion of the programme in the mid 2000s, it is possible that there are factors other than deprivation which also predict the pattern of Sure Start's roll-out. Indeed, policymakers recognised that the programme did not always adhere perfectly to these guidelines: in 2010, the House of Commons Children, Schools and Families Committee called for 'an evaluation of the rollout process, so that lessons can be learned for the future' (para. 72).

Understanding the drivers of the Sure Start roll-out is important from a policy perspective, but it is also of fundamental importance to the methodology that we use to evaluate Sure Start's impacts. By understanding the drivers of the roll-out, we can take them into account in our main analysis of Sure Start's impacts and therefore avoid conflating the effects of Sure Start itself with the impacts of local characteristics that affect the roll-out of the programme. We therefore consider both the guideline variables discussed in Section 2.1 and a wide range of other potentially important predictors, including labour market characteristics, vital statistics, existing service provision, other markers of need and the

political alignment of the local council. Appendix B lists these variables, their sources, and the years and geographic level at which they are available.[10]

As a summary statistic for the roll-out, we analyse the 'coverage' of Sure Start in each of England's local authority districts.[11] This is also the measure we use when analysing the impact of access to the programme on hospitalisations and obesity. We define coverage as the number of open Sure Start centres per thousand children aged 0–4 in each local authority. 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). We divide the number of centres by the population of children to capture the potential demand for Sure Start in each area: areas with higher numbers of children in the right age range are likely to have higher demand and so, all else equal, will need more centres to meet this need.

Sure Start coverage across England

Figures 2.4 and 2.5 illustrate the importance of normalising by the relevant population when looking at how access to Sure Start is distributed across England. Figure 2.4 shows

Figure 2.4. Number of Sure Start centres open in England, by region and year
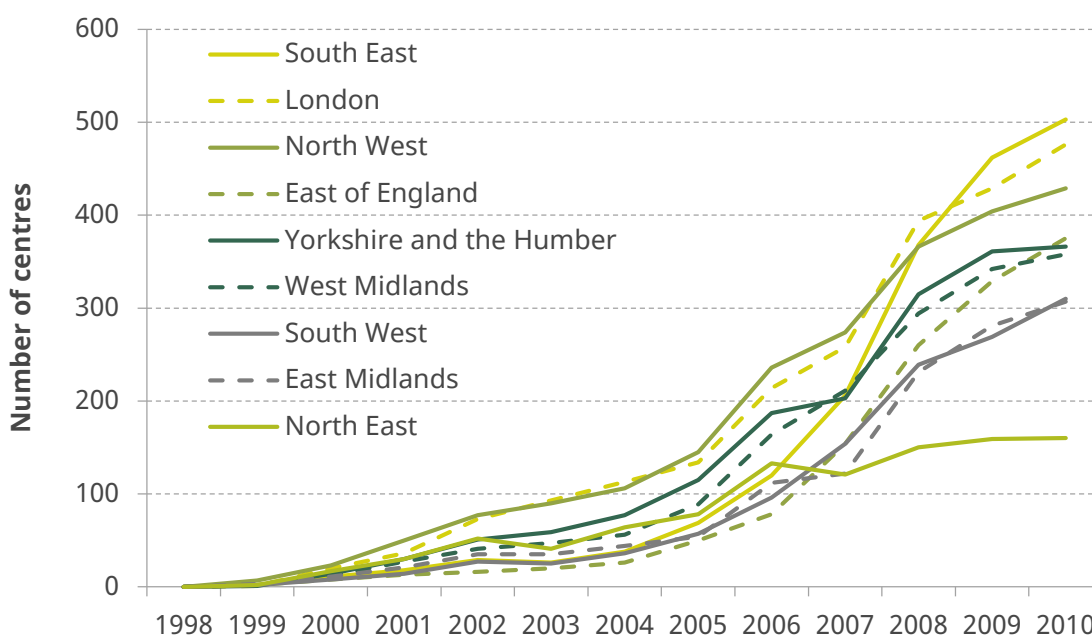
Note: Figure plots all Sure Start centres that had been opened by the end of the calendar year. Between 2003 and 2006, the number of centres includes both Local Programmes and Children's Centres, less any Children's Centre that shared a postcode with a Local Programme. Isles of Scilly, City of London and West Somerset are excluded.

Source: Authors' calculations using data provided by the Department for Education.

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[10] As Table B.1 shows, these variables were available at different levels of aggregation. In order to have a common geographical unit of analysis, we used lookups from the Office for National Statistics (ONS) and the Ordnance Survey to map between different sets of local authorities (e.g. following local government reorganisations).

[11] 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 roll-out of Sure Start. In particular, there are few children aged 0–4 residing in these areas.

Figure 2.5. Sure Start centres per thousand children aged 0–4 in England, by region and year
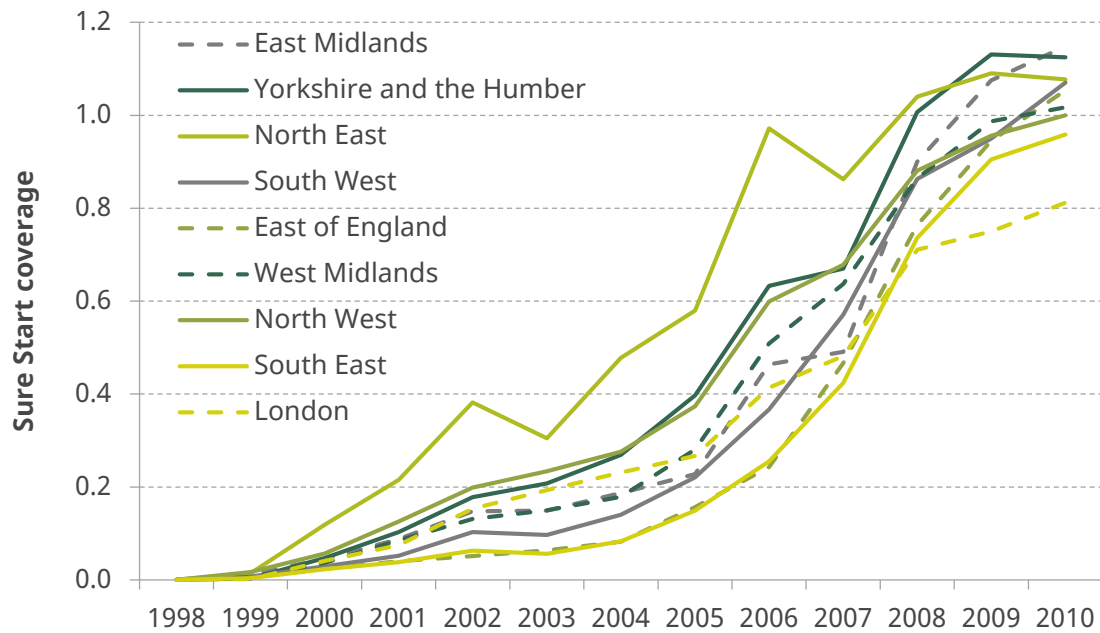
Note and source: As for Figure 2.4. Sure Start coverage is calculated as the number of open Sure Start centres per thousand children aged 0–4 in each local authority.

Figure 2.6. Sure Start centres per thousand children aged 0–4, by 1998 Index of Local Deprivation quintile and year
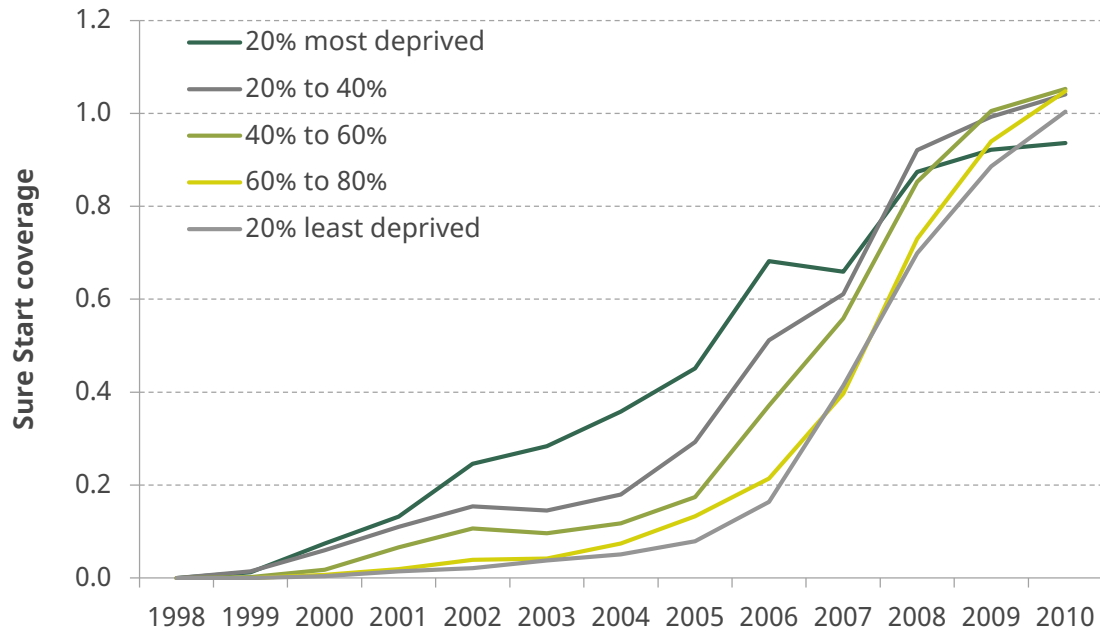
Note and source: As for Figure 2.5.

that London was consistently one of the regions with the most Sure Start facilities. But after taking into account its population (in Figure 2.5), it fell from middle of the pack in the early 2000s to the region with the lowest coverage by some distance from 2009 onwards. On the other hand, while the North East had far fewer centres than other regions in 2010, it had the third-highest coverage.

Figure 2.6 looks at how the coverage of Sure Start changed over time based on local authorities' level of disadvantage. It ranks local authority districts based on their scores in the 1998 Index of Local Deprivation and then divides them into five equal groups. Consistent with the figures in Section 2.1, 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 (primarily as a result of the rapid population growth in some of the metropolitan authorities among the most disadvantaged).

Statistical model of the Sure Start roll-out

Using linear regression, we can uncover some of the statistical relationships between the Sure Start roll-out and local characteristics, including deprivation, as listed in Appendix B. For each of 323 local authority districts in England used in our main analysis and for each of the years between 1999 (when the first Local Programmes opened) and 2010 (when virtually all centres had opened), we regress the coverage of Sure Start on a range of local characteristics.

In the first column of Table 2.2, we regress coverage only on a set of year fixed effects (which capture the general increase in coverage over time) and the 1998 Index of Local Deprivation rank (where lower ranks indicate more disadvantaged local authorities). We find that, on average, being one rank less disadvantaged in the deprivation league table is associated with the number of centres per thousand children falling by 0.003. There is a very strong statistical relationship between deprivation rank and coverage, confirming the descriptive results that we presented in Section 2.1.

Given that Phase 1 centres were supposed to open in the poorest 20% of areas and Phase 2 centres in the 30% poorest, the relationship between the roll-out and deprivation may not necessarily be linear. Thus, in column 2, rather than incorporating a local authority's rank, we create different categories of deprivation: local authorities in the 20% most disadvantaged nationally, those in the bottom 20–30%, and the middle of the league table (the 30th to 70th percentiles). Relative to the 30% least deprived local authorities, coverage in the bottom 20% of districts is on average 0.221 centres per thousand children higher. The districts in the 20th to 30th percentiles have on average 0.144 more centres per thousand than these richest areas. The effect of being in the middle of the league table is smaller but still strongly statistically significant. This confirms that, over the entire period of its roll-out, Sure Start coverage was higher in more disadvantaged areas. However, the explanatory power of these deprivation categories (given by the R^2 statistic at the end of the table) is similar to that in column 1, indicating that it may be sufficient to consider a single deprivation rank (as in column 1).

Table 2.2. Association between Sure Start coverage and local characteristics

	(1)	(2)	(3)	(4)	(5)
Deprivation rank	−0.003***		−0.000**	−0.000***	
	(0.000)		(0.000)	(0.000)	
Deprivation dummies					
Bottom 20%		0.221***			
		(0.029)			
20% to 30%		0.144***			
		(0.028)			
30% to 70%		0.047***			
		(0.017)			
Lagged Sure Start coverage					
Coverage at t−1			0.748***	0.746***	0.599***
			(0.017)	(0.017)	(0.021)
Guideline variables					
% low birthweight			0.004	0.003	0.001
			(0.002)	(0.002)	(0.003)
Teen conception rate			0.013***	0.012***	0.005
			(0.003)	(0.003)	(0.006)
Labour market					
Male earnings *(full-time weekly pay)*			0.000 (0.000)	0.000 (0.000)	0.001** (0.001)
Female earnings *(full-time weekly pay)*			−0.000 (0.000)	−0.000 (0.000)	−0.001 (0.000)
Jobseeker's allowance			−0.004	−0.006*	−0.055***
			(0.004)	(0.004)	(0.008)
Employment rate			−0.004***	−0.004***	−0.002**
			(0.001)	(0.001)	(0.001)
Vital statistics and potential demand					
Population density			−0.004*	−0.002	−0.169***
			(0.002)	(0.003)	(0.044)
Fertility rate			−0.019	−0.016	−0.058***
			(0.012)	(0.013)	(0.022)
% EAL in primary school			−0.001***	−0.001***	−0.001**
			(0.000)	(0.000)	(0.000)

	(1)	(2)	(3)	(4)	(5)
Children looked after *(rate per '000 aged 0–4)*			−0.001 (0.002)	−0.000 (0.002)	0.007* (0.004)
Service provision					
Jobcentre Plus *(centres per '000)*			1.348*** (0.514)	1.343*** (0.510)	1.150* (0.663)
GPs *(practices per '000)*			−0.069* (0.040)	−0.056 (0.040)	−0.191 (0.120)
Funded childcare *(% of 3- to 4-year-olds with a place)*			0.012 (0.026)	0.003 (0.029)	0.059 (0.041)
Political alignment					
District aligned			−0.005 (0.008)	−0.006 (0.009)	−0.026 (0.016)
County aligned			0.016** (0.007)	0.018** (0.008)	0.045*** (0.015)
Constant	0.165*** (0.015)	−0.049*** (0.010)	0.429*** (0.064)	0.443*** (0.070)	0.819*** (0.128)
No. of observations	3,876	3,876	3,876	3,876	3,876
R^2	0.748	0.732	0.887	0.887	0.900
Year fixed effects?	Yes	Yes	Yes	Yes	Yes
Area fixed effects?				Region	LA district

Note: Deprivation rank comes from the 1998 Index of Local Deprivation, where lower ranks indicate more disadvantaged local authorities. '% low birthweight' captures the share of babies born weighing less than 2,500 grams. Teen conceptions are measured per thousand women aged 15–17. Male and female earnings are median weekly earnings for full-time work. 'Jobseeker's allowance' is the JSA receipt rate. Population density is measured as the population (in thousands) per square kilometre. '% EAL' refers to the share of primary school pupils with English as an additional language. 'Children looked after' is the number of children looked after per thousand children aged 0–4. Jobcentre Plus and GP provision are measured as centres or practices per thousand residents in the local authority. 'Funded childcare' refers to the take-up rate of the 3- and 4-year-old free entitlement to part-time funded childcare. District councils refer to lower-level councils while counties are upper-level councils; in unitary authorities and London and metropolitan boroughs, the council counts as both a 'district' and a 'county'. 'Aligned' councils are controlled by the same party as the national government. 'LA district' fixed effects capture the 323 lower-level local authorities, including single-tier authorities (we exclude the Isles of Scilly, City of London and West Somerset, which are all strong outliers in Sure Start coverage). These fixed effects also capture the deprivation rank, which is constant within LAs over time. Results are estimated for the years 1999–2010 using linear regression. Standard errors are shown in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively. Years of measurement for the local characteristics are available in Appendix B. Each of the variables is measured one year prior to the outcome measure.

Source: Authors' calculations using data provided by the Department for Education and the sources listed in Appendix B.

Column 3 incorporates all of the other potential roll-out predictors into the model.[12] These include labour market variables (male and female median weekly earnings, the jobseeker's allowance receipt rate and the employment rate); vital statistics and potential demand (thousands of residents per square kilometre, fertility rate, the percentage of primary school pupils with English as an additional language, and the number of children per thousand aged 0–4 who are looked after by the local authority); service provision (Jobcentre Plus and GP practices per thousand population and the take-up rate of free childcare among 3- and 4-year-olds); and political alignment (whether the upper- and lower-level councils are politically aligned with the national government). Each of these variables is measured in the year prior to the outcome – so we consider, for example, how the employment rate in 2000 is associated with Sure Start coverage in 2001.

Over and above the role that deprivation and the guideline variables play in explaining the roll-out, several others seem to be significant predictors as well. Areas with a higher share of children with English as an additional language tend to have lower coverage (this could be partly mechanical, with areas with big inward migration serving a greater number of children year on year; it could also be a proxy for greater ethnic diversity). On the other hand, areas with a higher concentration of Jobcentre Plus (JCP) tend to have much higher coverage of Sure Start the following year (it could be the case that, for example, local authorities deliberately open JCPs prior to opening Sure Start centres because of the close links that Children's Centres were meant to have with employment support).

There is also a negative relationship between Sure Start and another measure of potential demand: population density. This is likely to be a mechanical effect. Because closures were not a very important part of the Sure Start landscape during the period we are considering, Sure Start centres tend to persist over time. But since we are measuring Sure Start coverage – centres per thousand children – more children with the same number of centres will lead coverage to fall. The finding that a growing population is associated with lower Sure Start coverage therefore suggests that changes in Sure Start centre numbers did not perfectly keep pace with population growth – either because policymakers' decisions did not keep pace with a growing number of children in every area or because they built a network of centres for the community to grow into.

We also take into account the persistence in the number of Sure Start centres from one year to the next. We include Sure Start coverage from the previous year in the model, so that we only look at the impact that the other predictors have on coverage over and above the centres that are already on the ground. This strengthens our model, which now explains 89% of the variation in coverage from year to year.

The local employment rate is negatively associated with Sure Start coverage, with areas with higher employment rates experiencing lower coverage the following year. This could suggest that policymakers are targeting Sure Start centres – which provided childcare services and job-searching help for parents – to areas with low employment as part of a push to help mothers to work. Finally, local authorities where control of the upper-level

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[12] For columns 3–5 of Table 2.2, we have computed a variance inflation factor (VIF) to assess the extent to which these time-varying characteristics are capturing the same information. Because we have imputed the employment and EAL rates for years where the data are not available (see Appendix B for details), the VIF for these two variables is greater than 10, as are the year fixed effects for the years when EAL data are missing. Other than these mechanical results, all the VIFs are comfortably below the threshold of 10 that is typically used as a cut-off.

(county) council switched between 1993 and 2010 also tended to increase their coverage more during the years when they were controlled by the same party as the national government.[13] That is, councils that were controlled by the Labour party (which also held power in Westminster) had around 0.02 more centres per thousand children the following year.

While we have considered a range of observable predictors of the roll-out, there are many other characteristics of different local authorities that we do not observe. For example, we do not have data on the extent to which local authorities prioritise Children's Services. In columns 4 and 5, we therefore include fixed effects at the level of the region and the local authority respectively. By including these fixed effects, we strip out the impact of any characteristics – observed or unobserved – that do not change over time.

In practice, these fixed effects make relatively little difference to the explanatory power of our model: our R^2 is identical between columns 3 and 4, and rises only a little in column 5. This suggests that most of the variation in coverage is explained by the overall national pattern of rising coverage (captured by the year fixed effects) and by the variables that we have included in our model. Notably, the jobseeker's allowance (JSA) rate becomes a significant predictor of the Sure Start roll-out after controlling for local authority district fixed effects (column 5), with a higher JSA rate associated with lower coverage the following year. At the same time, a higher employment rate is also associated with lower coverage. This is consistent with policymakers targeting areas where more parents are out of the labour force (neither in work nor looking for a job), perhaps in an effort to encourage them into work.

2.3   Sure Start closures

The policy landscape for Sure Start has changed considerably since 2009–10. As shown in Figure 2.7, spending on Sure Start has fallen sharply over this period, even as spending on other early years programmes – childcare tax credits and the free entitlement – has held up or risen.

Following the removal of the funding ring fence in April 2011, local authorities had the freedom to choose how to respond to these cuts. Some authorities have subsidised Sure Start services from other budget lines, while in other areas the Sure Start budget was used to fund other local priorities.

This affects the provision of Sure Start services across England. The data on Sure Start closures are imperfect; the Department for Education maintains a list of sites that have been 'de-designated' as Children's Centres, meaning that they are no longer considered to offer Sure Start services. But a wealth of anecdotal and survey evidence suggests that local authorities adjusted services in other ways to respond to a tighter funding environment. Some areas consolidated several centres into one. Others introduced a greater focus on targeting Sure Start services at the families deemed most likely to benefit. Still others have cut back on the hours or types of services offered while keeping at least some presence at each site. Smith et al. (2018) use surveys and case studies to

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[13]   These estimates are based only on local authorities where control of the local council switched at least once during the roll-out period. For councils that were always controlled by one party, political alignment becomes part of their 'fixed effect', so these areas do not contribute to the estimates for council control.

Figure 2.7. Spending on Sure Start and other early years programmes
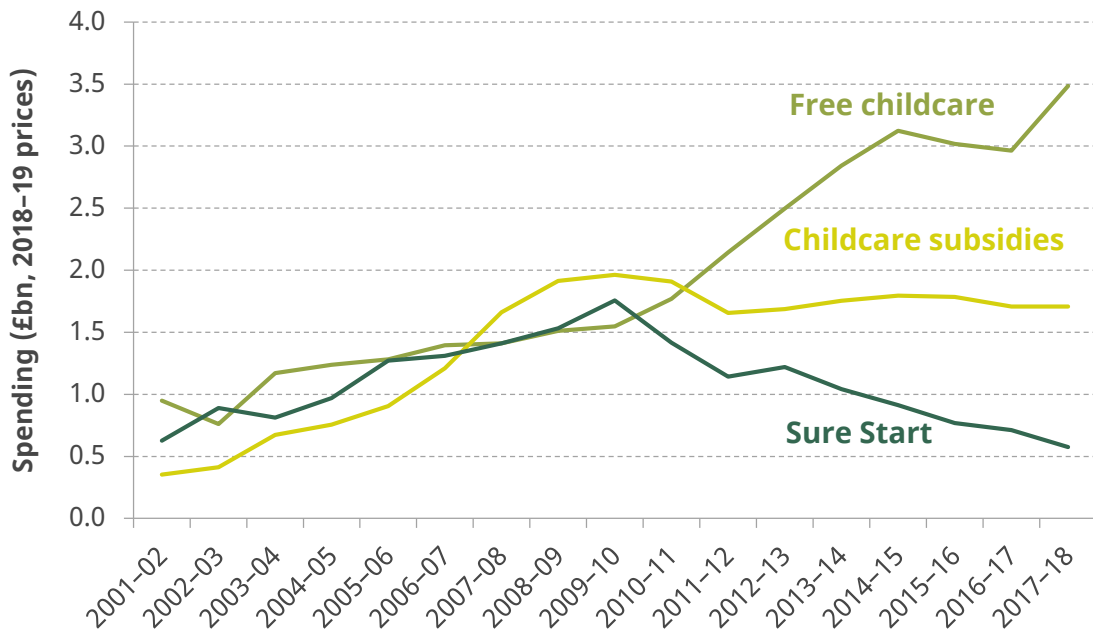
Source: Belfield, Farquharson and Sibieta, 2018.

Figure 2.8. Sure Start closures by year
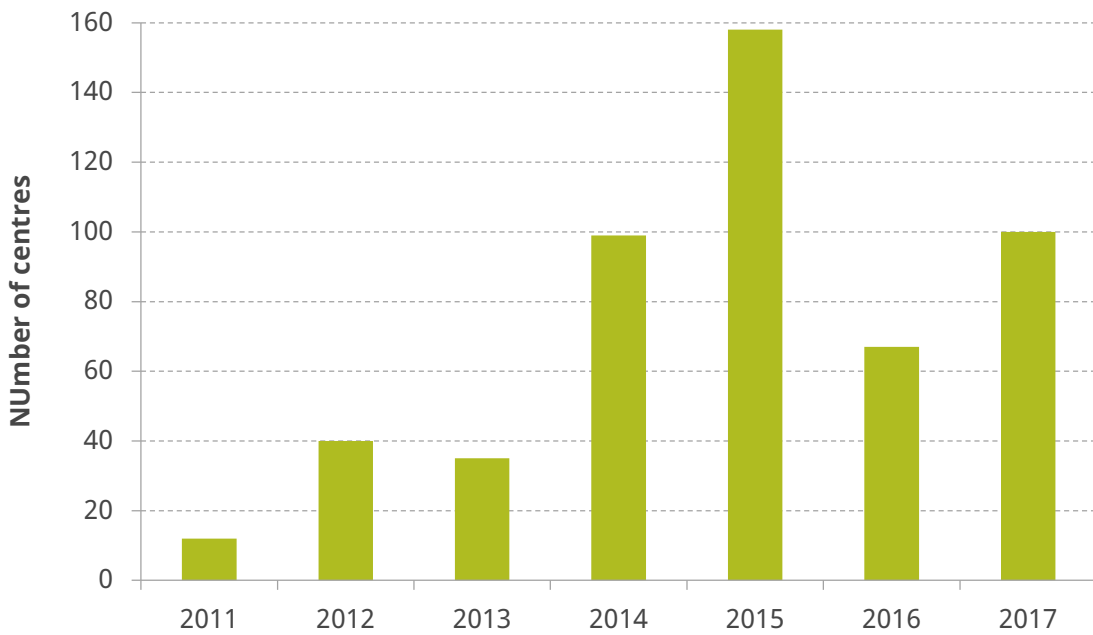
Note: Sure Start sites are included if they have been officially recorded as closed in the Department for Education's records. Isles of Scilly, City of London and West Somerset are excluded.

Source: Authors' calculations using data provided by the Department for Education.

Figure 2.9. Share by deprivation of Sure Start closures each year
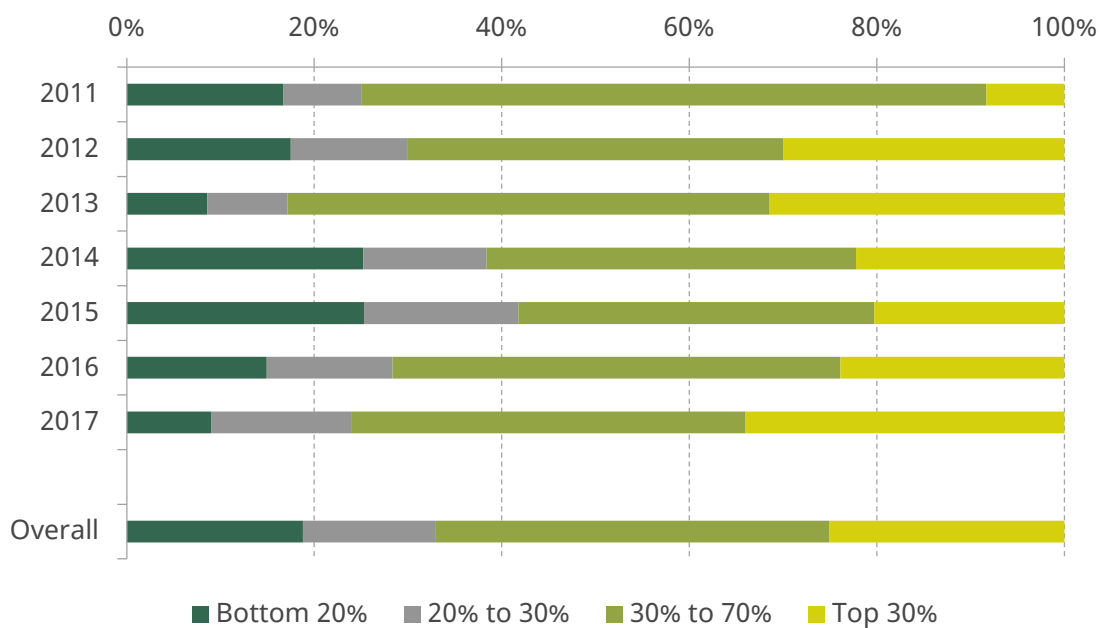
Note: 'Bottom 20%' refers to neighbourhoods (LSOAs) in the bottom 20% of the national 2010 Index of Multiple Deprivation ranking, i.e. the most disadvantaged. Other categories are similarly defined, with 'Top 30%' incorporating the least disadvantaged areas. The figure only counts Sure Start sites officially recorded as closed by December 2017 in the Department for Education's records. Isles of Scilly, City of London and West Somerset are excluded.

Source: Authors' calculations using data provided by the Department for Education and the 2010 Index of Multiple Deprivation.

provide a detailed overview of some of the responses short of official centre closures that local authorities have used.

This range of responses means that the list of Sure Start site closures will understate the changes that have occurred to Sure Start since 2010. However, looking at the closures does provide some evidence about how different types of areas and authorities have responded to the tighter funding environment.

Between 2011 and 2017, 511 Sure Start sites were closed.[14] As Figure 2.8 shows, most of these closures occurred in 2014 and later, with the biggest set of closures in 2015. Although data for the whole of 2018 are not yet available, between January and July of that year 15 centres were closed, which – if closures continued at that rate for the second half of the year – implies that the rate of closures has slowed dramatically.

Figure 2.9 mirrors Figure 2.3's analysis of the opening of Sure Start facilities, looking at the level of deprivation of the neighbourhoods that the closed sites were located in. While in the earliest years Sure Start closures were concentrated in neighbourhoods in the middle of the deprivation league table, overall the closures have fallen relatively evenly across the country. The poorest 20% of neighbourhoods have seen 19% of closures and the 40% of

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[14] Throughout this section, we discuss Sure Start 'site' closures, since the DfE's official records of closures count individual Sure Start locations (which may have been administered by another centre). This also means that attempts to calculate the closure rate as a share of Sure Start centres ever opened will be imperfect.

Figure 2.10. Share of Sure Start sites closed, by deprivation
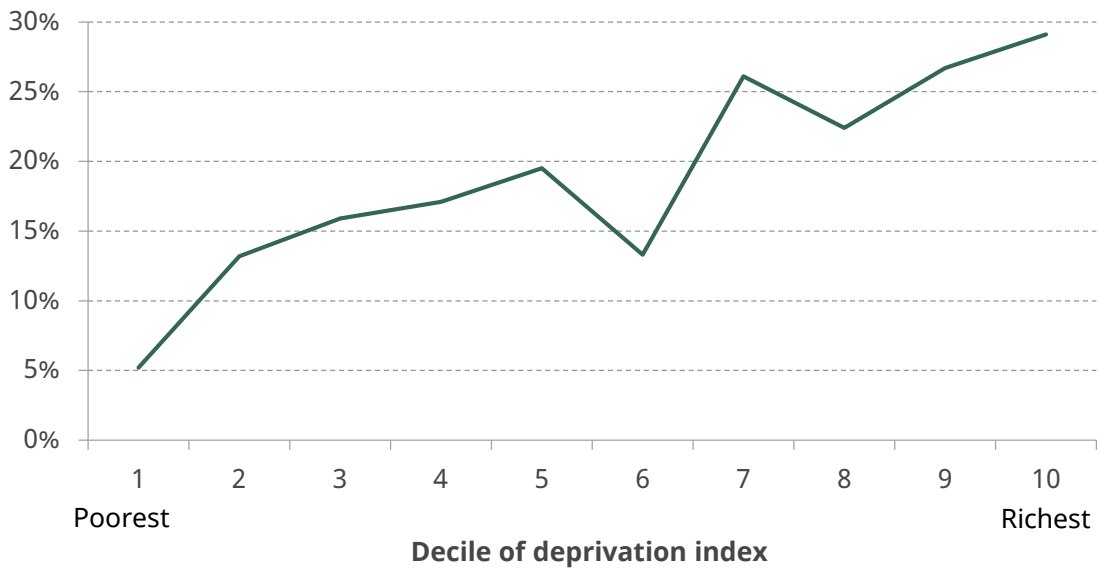
Note: This is an imperfect estimate based on dividing the number of Sure Start 'sites' officially recorded as closed in the Department for Education's records by the number of Sure Start centres observed opening in the same neighbourhoods. Isles of Scilly, City of London and West Somerset are excluded.

Source: Authors' calculations using data provided by the Department for Education and the 2010 Index of Multiple Deprivation.

Figure 2.11. Share of Sure Start sites declared closed by July 2018, by local authority
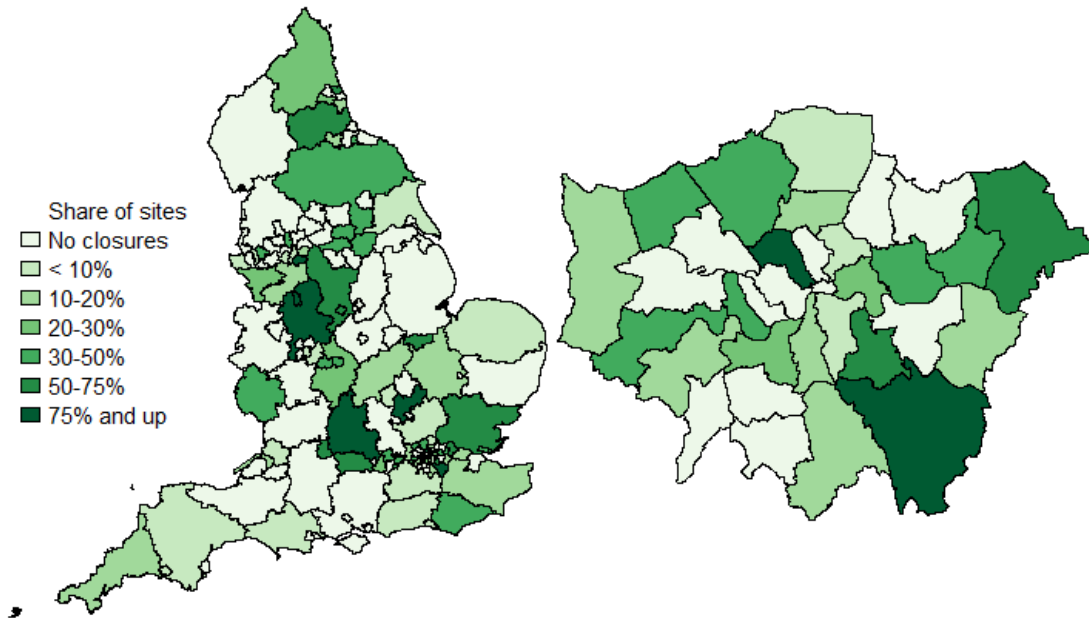
Note: Percentage of sites closed is calculated for each upper-tier authority based on the number of centres it had open in 2010 and the number of sites in that area recorded as closed in the Department for Education's official records.

Source: Authors' calculations using data provided by the Department for Education.

areas between the 30th and 70th percentiles account for 42% of closures overall. The richest 30% of neighbourhoods have been somewhat less affected, with just a quarter of centre closures in these areas.

However, it is important to remember that Sure Start centres were not equally distributed across the country. More disadvantaged areas had more centres to start with, so the same number of closures reflects a smaller share of their overall Sure Start coverage. Figure 2.10 shows that the most disadvantaged 10% of neighbourhoods have seen around 5% of their Sure Start centres closed, compared with 29% of centres in the richest 10% of areas.

Finally, Figure 2.11 looks at where around England these closures have taken place. It shows that there are stark differences even between neighbouring local authorities. While Islington and Westminster have both avoided closing any sites, Camden – sandwiched between them – has closed over 75% of its Sure Start sites. Central Bedfordshire, Bromley, Stockport and Oxfordshire also stand out for having closed many of their Sure Start facilities. On the other hand, 195 local authority districts – 60% of the total – have not closed any of their Sure Start sites (though they may of course have made use of any of the other measures of consolidation outlined above).

3.   How could Sure Start affect health?

Key findings

- **Sure Start services usually include a focus on childcare, early learning and health.** The services offered by Sure Start vary a lot around the country and over time. However, existing evidence indicates that most centres, most of the time, offered some services targeting health, child development and parenting support.

- **Sure Start may affect children's health through a variety of direct and indirect channels.** Sure Start may affect children's health directly through the provision of health services and health-related information. But children's health may also benefit from improved maternal mental health, increased parental income and safer parenting practices.

Sure Start was intended to target disadvantaged children early in life, to improve health, educational and social outcomes by bringing together a number of other programmes and services to create a one-stop shop for parents. Although the intervention prioritised disadvantaged areas, it was not means-tested. Rather, it was area-based: any family living in the Local Programme's catchment area was invited to register and use its services (National Evaluation of Sure Start, 2005).

In keeping with its focus on locally determined priorities, Sure Start was quite a varied programme, with centres delivering different types of services in different areas and at different times. In this chapter, we give an overview of the central government requirements of Sure Start Local Programmes and Children's Centres. We provide examples of the types of services that they offered and discuss the different channels through which we would expect the programme to affect children's health.

3.1   Sure Start services

Local Programme services

As the name suggests, 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 (Department for Education and Employment, 1999).

Meadows et al. (2011) report the expenditure shares of the different activities in 2003–05. Just under a third (29%) of SSLPs' expenditure was incurred on play, learning and childcare activities (excluding early years education for 3- and 4-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 enhancing what was available through local mainstream NHS health services (e.g. increased postnatal depression services). A sixth of spending (17%) went on outreach and home visiting, while support for children with special needs accounted for 7% of the spending.

Children's Centre services

Children's Centres had less autonomy than Local Programmes in choosing the services they would deliver. Centres in the 30% most deprived areas 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 Jobcentre Plus (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 Jobcentre Plus, 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 that met local needs.

Goff et al. (2013) report that the Evaluation of Children's Centres in England (ECCE) sample of 121 Children's Centres 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 Children's Centres were asked about during the fieldwork, they reported offering an average of 28, with a range between 13 and 42. The following five services were reported by over 90% of the centres: stay and play, evidence-based parenting programmes (especially Incredible Years, Triple P and Family Nurse Partnership), early learning and childcare, developing/supporting volunteers, and breastfeeding support; other frequently offered services were midwife and health visitor clinic, sports and exercise for babies and children, benefits and tax credit advice, housing advice, debt advice, adult learning, parent forum, and antenatal and postnatal classes.

3.2   Potential mechanisms

Given the variety of services offered by Sure Start, there are many direct and indirect channels through which we would expect the programme to affect children's health. 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. Department for Education and Skills, 2003; Department for Education, 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.

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. This might directly affect some health investments: for example, a mother who moves into full-time work when her child is aged 6 months might decide to stop breastfeeding earlier than she would have otherwise.

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; van den Berg and Siflinger, 2018).

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.

In this report, 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 specific causes (see Chapter 6) and at different ages. For example, by looking at hospitalisations related to infections, we can analyse whether the pattern of effects that we see is consistent with the immunisation hypothesis.

Our second strategy considers maternal mental health directly (see Chapter 8). In addition to being a potential mechanism affecting children's health, mental health is an important outcome in its own right. Unfortunately, we must use a much smaller data set to explore this outcome, with the result that we lack the statistical power to robustly determine whether Sure Start coverage has an effect.

4. Methodology and data

Key findings

- **Causal estimates of Sure Start's impacts must distinguish between the programme itself and the characteristics of places that have it and people who use it.** Our research focuses on understanding the impact that Sure Start access itself has on children and families. However, the families who have access to – or visit – Children's Centres may be different in many ways from those who do not. To understand the causal impact of Sure Start, we need to account for these differences; we cannot simply compare the groups.

- **We can use variation from the roll-out of Sure Start to overcome this evaluation problem.** We use a statistical method called 'difference-in-differences' to account for differences between groups with more or less access to Sure Start. Under some conditions – which we explore in the appendices – this allows us to estimate the impact of access to Sure Start on children's health.

- **We focus on *access* to Sure Start, not *use* of Children's Centres.** Our research looks at the effect of opening new Children's Centres on all children in the local authority, not just those who use the Children's Centre. This corresponds to one important policy lever that government can pull: deciding whether and where to have Children's Centres. However, the effects we estimate may understate the impact Sure Start has on those who use Children's Centres.

4.1 The evaluation problem

Our goal is to estimate the causal impact of Sure Start on a variety of health outcomes.

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

$$Y_{it} = \alpha + \beta SS_i + \varepsilon_{it},$$

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 and 0 otherwise, and ε_{it} is an error term. The coefficient β then gives the overall difference in outcome Y between children who do and do not use Sure Start services.

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 will not recover its causal impact. This is because there could be other factors that affect both

whether a child uses Sure Start services and his or her health outcomes. By not including these characteristics in the model, we would conflate the impact of Sure Start itself with the impact that these characteristics have on outcomes.

It is not clear which direction this bias would go in. 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 (e.g. by feeding them with healthier food). On the other hand, β could underestimate the 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.

Uncovering the causal impact of Sure Start itself means that we need to break the link between these characteristics that drive Sure Start use, and the health outcomes that we measure. One rigorous way to do this is to randomly assign Sure Start treatment to participants. In this case, the 'treatment' group (who use Sure Start) would be on average identical to the 'comparison' group (who are randomised out of using these services), and the only difference between the groups would be Sure Start itself. This gives researchers confidence that any difference in outcomes between the groups is the result of Sure Start itself.

Unfortunately, Sure Start was not subject to a randomised, experimental evaluation. However, researchers have developed a wide range of quasi-experimental methods that use features of the policy environment or other data to model access to treatment as being 'as good as random'. One of the most widely used of these is the difference-in-differences (DiD) method. At its simplest, the DiD method defines 'treatment' and 'comparison' groups whose outcomes before the start of the treatment are on similar trends (though not necessarily at the same level). It then uses the trend of the comparison group to estimate what the treatment group's outcomes would have looked like, had they not been exposed to the treatment. This allows researchers to uncover the effect of the treatment by comparing the outcomes of treated children with an estimate of what their outcomes would have been in the absence of the programme.

For the DiD method to be valid, we need to assume that the trends in the outcomes of the comparison group reflect what would have happened to the treatment group in the absence of the treatment. In practice, the more similar the two groups are to each other, the more plausible this argument becomes.

4.2   Methodology

To estimate the causal impact of Sure Start on health outcomes, we apply this difference-in-differences methodology to the roll-out of Sure Start in England. We use the staggered introduction of Sure Start Local Programmes and Children's Centres between 1999 and 2012 to generate variation in the level of access to Sure Start based on a child's month and year of birth and local authority of residence. As a measure of access to Sure Start, we use the average coverage (number of centres / number of children aged 0–4 years in the local authority district) a child has experienced in the first 60 months of life.

Our DiD methodology thus looks 'within' a neighbourhood (LSOA) at changes in its local authority's access to Sure Start over time. By looking 'within' neighbourhoods, we account for any permanent characteristics of the area that are associated both with its level of hospitalisations and its level of access to Sure Start – for example, this controls for the fact that disadvantaged areas are more likely to have both higher Sure Start coverage and more hospitalisations. At the same time, our methodology also uses information from other local authorities (LAs) to account for changes in outcomes over time at the national level – for example, an increase in hospitalisations during a particularly bad winter flu season.

This is a more robust approach than one involving a static comparison of the outcomes of children from areas with a lot of centres and those of children from areas with fewer centres because it 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 programme and health outcomes. Indeed, a major challenge to our empirical approach is that the roll-out of Sure Start was not random even within local authorities, as seen in Chapter 2.

We deal with this issue in two ways. First, we include neighbourhood fixed effects at the level of the lower-layer super output area (LSOA) to control for permanent unobserved differences across very low-level areas. There are 32,482 of these neighbourhoods in England, each containing around 1,500 residents – so the unobservable differences that we account for are measured at a very local level. However, the LSOA fixed effects will not account for any local characteristics that change over time. Hence, we also check the robustness of our results to the inclusion of time-varying characteristics and LA-specific linear trends.[15]

Interpreting the estimates

It is important to emphasise that the effect that we are estimating is *not* 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.

However, 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

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[15] Thus, we can write the simplest version of model we estimate as

$$Y_{ilnt} = \alpha + \beta CovSS_l + \varphi_t + \pi_n + \varepsilon_{ilnt},$$

where Y_{ilnt} denotes the outcome Y at a given age for child i who lives in LSOA n of LA l and was born in year and month t. $CovSS_l$ denotes the number of centres per thousand children aged 0–4 that were open in the LA when the child was aged between 0 and 4, φ_t denotes fixed effects for the year–month of birth, π_n denotes the LSOA fixed effects and ε_{ilnt} is an error term.

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.

4.3   Data sources

We use two large administrative data sources to examine the impacts of Sure Start on child health outcomes – more specifically, on hospitalisations and obesity. We complement them with a large household panel survey to study potential mechanisms. In this section, we describe each of them, as well as the data on Sure Start that we use.

Sure Start data

We have access to a unique data source, provided to us by the Department for Education, which includes the exact location (at the postcode level) and opening date of all Sure Start centres since the beginning of the roll-out.[16] We use these data to construct our measure of access to Sure Start (centres per thousand children aged 0–4 in the local authority district).

Our measure of coverage includes both Local Programmes and Children's Centres. Unfortunately, while we have precise information on the opening dates of both types of centres, we have no information on when Local Programmes closed. Instead, we assume that Local Programmes at the same postcode as a Children's Centre close when the Children's Centre opens (since most SSLPs became SSCCs). For SSLPs that do not match on postcode, we assume a closure date of December 2006.[17] While we do observe closure data for Children's Centres, they are incomplete: they indicate when centres were formally closed, but nothing about the changes short of closures that many Children's Centres have undergone. Rather than risk biasing the estimate of Sure Start coverage in different local authorities based on incomplete information, we disregard closures in our data and – as discussed in the introduction – focus primarily on Sure Start up until 2010.

Based on this set of centres, we construct a measure of the coverage of Sure Start for each month from the opening of the first Local Programmes in January 1999 through the end of 2014 (with zero coverage before the Sure Start programme began). For every month and year of birth, we then average coverage over the first 60 months of life (ages 0 through 4), or over the months between birth and measurement when a child is observed before age 5.[18]

Figure 4.1 plots the coverage experienced by children with different months and years of birth in the 323 local authority districts we consider as well as the overall average for England (shown in blue). Coverage generally increases over time, but at different rates in different districts. On average, over the course of the roll-out, Sure Start coverage in

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

[17]   According to the National Audit Office (2006), by September 2006 around 500 SSLPs (of the original 524) had transitioned into Children's Centres, so this is a fairly conservative cut-off.

[18]   When we look at children's outcomes directly, we only measure outcomes at age 5 or later, so all children are assigned the full treatment over the first five years of life. When we look at maternal mental health, we include mothers whose children are younger than 5, and we calculate the treatment for those children based on the months of treatment they have actually experienced.

Figure 4.1. Average coverage over the first 60 months of life, by local authority and month and year of birth
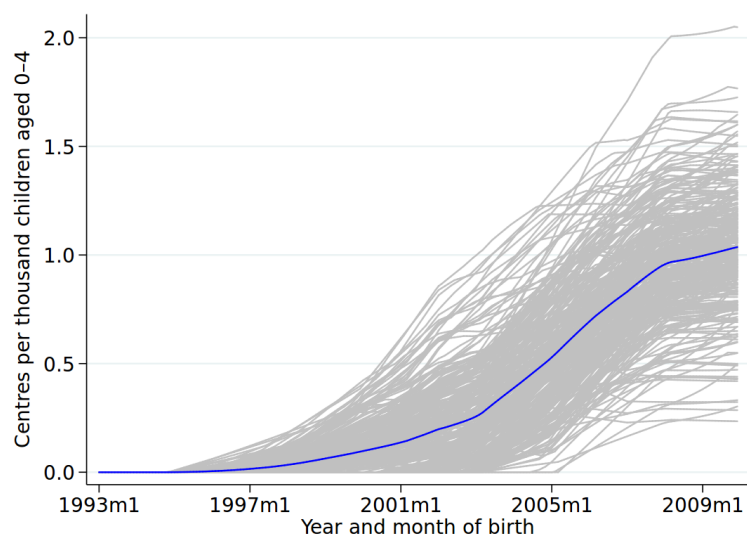
Note: Each grey line represents one of 323 local authority districts in England (excluding the Isles of Scilly, City of London and West Somerset). The blue line shows the average for all of England. The lines plot the average Sure Start coverage (centres per thousand children aged 0–4 in the district) 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.

England went from zero (prior to the start of the programme) to about one centre per thousand children aged 0–4 by the time Sure Start was operating at its peak.

Hospital Episode Statistics

We use the Hospital Episode Statistics (HES) to study the impact of Sure Start on children's hospitalisations, overall and for specific causes. The HES is an administrative data set tracking all patients using public hospitals in England. Data on inpatient admissions have been collected since 1997–98 and provide information on the admission, discharge, clinical diagnoses (up to 20 for each patient) and demographics of each patient. We include one record per hospitalisation, independently of the length of stay, and exclude admissions related to the birth of a child. Information on patient demographics is limited to sex, ethnicity, and month and year of birth; crucially, however, we also know the LSOA of residence at the time of admission, so we can merge the HES with data from the Sure Start roll-out.[19] The HES analysis is presented in Chapters 5 and 6.

National Child Measurement Programme

The National Child Measurement Programme (NCMP) was launched in 2006 and is an annual programme that measures the height and weight of children in the Reception Year and in Year 6 within state-maintained schools.[20] We use the NCMP to estimate the impact

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[19] Ideally, we would know the child's LSOA of residence throughout their first five years. Instead, we proxy this with their LSOA at the time of hospitalisation. We use the LSOA to estimate the fixed effects but the treatment is defined at the (much larger) local authority level, meaning that the treatment we assign will be correct as long as children remain within the same local authority.

[20] This accounts for about 95% of pupils at primary level.

of Sure Start on children's body mass index (BMI) and their likelihoods of being overweight and obese in the Reception Year.[21]

Participation in the NCMP is not compulsory, but non-participation is on an opt-out basis only. Before the programme starts in each school year, local authorities write to the parents and carers of all children eligible for measurement to inform them of the programme and to give them the opportunity to opt their children out. Due to the combination of this opt-out possibility and the fact that the programme was launched in 2006, there is a high and non-random rate of missing information in the data for the academic year 2006–07; for this reason, we were advised by analysts at Public Health England, which is the entity responsible for the data management, to exclude this year from our analysis.[22]

UK Household Longitudinal Study

The UK Household Longitudinal Study (UKHLS, also known as Understanding Society) is a longitudinal survey of the members of approximately 40,000 households in the UK. Households recruited at the first round of data collection are visited each year to collect information on changes to their household and individual circumstances. Interviews are carried out face-to-face in respondents' homes by trained interviewers or through a self-completion online survey. Young people aged 10–15 complete a youth questionnaire, whilst respondents aged 16 and over complete the adult survey. The overall purpose of UKHLS is to provide high-quality longitudinal data about subjects such as health, work, education, income, family, and social life to help understand the long-term effects of social and economic change, as well as policy interventions designed to impact upon the general well-being of the UK population. We use the UKHLS (combined with its predecessor the British Household Panel Survey, BHPS) in Chapter 8 to study one possible mechanism through which Sure Start might have achieved the health impacts we detect: maternal mental health.

Cohorts included in the analysis

Since our analysis spans a range of data sets and outcome ages, data availability means that we are not always able to include the same set of birth cohorts across the different sets of results.

In the **Hospital Episode Statistics** data, we are limited to children who are hospitalised between 1997 and 2014. For outcomes at age 5, we include children born between 1993[23] and 2009 (with the last cohort being 5 years old in 2014, the last year for which we have hospitalisations data). But for outcomes at age 6, we can only include children born in 2008 and earlier – we do not observe children born in 2009 when they are 6 years old in

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[21] While the NCMP, like the HES, also contains information on the year and month of birth of the child, it does not contain information on the residential address, but only on the school postcode. Hence, as we explain in greater detail in Chapter 7, we limit our analysis to children in the Reception Year, who are closer to the years of potential exposure, to limit the bias arising from potential measurement error in the treatment variable due to using the incorrect address.

[22] For further information about the data, see https://digital.nhs.uk/services/national-child-measurement-programme/.

[23] We restrict the sample to children born in 1993 or later for all outcome ages so that we analyse children born at most six years before the first Sure Start Local Programme opened. By restricting the timeframe that we consider, we help to reduce any potential differences – observed or unobserved – between children born before or after the Sure Start programme started.

our data. This pattern continues so that, by age 11, we are considering children born between 1993 and 2003.

In the **National Child Measurement Programme** analysis, we consider children in the Reception Year (aged 4 and 5). We use data from children in the school years 2008–09 to 2013–14, which translate into birth cohorts 2003 through 2010. Importantly, this means that there is very little overlap between these cohorts and the cohorts used in our HES analysis of hospitalisations at older ages.

In our analysis of maternal mental health using the **UK Household Longitudinal Study**, we consider parents rather than children. However, to improve consistency between this work and our primary analysis in the HES, we restrict the sample to families whose eldest child was born between 1993 and 2014.

As Chapter 3 suggests, the types of services offered by Sure Start changed over time, so its effects might be different for different cohorts of children. We have the most scope to examine this in the HES data, where using different groups of children to estimate outcomes at different ages might confound the changes in effects for different cohorts with the changes in effects at different ages. We therefore present robustness checks showing how our results change when we instead follow the same group of children over time.

5.   Impact on hospitalisations

Key findings

- **Greater access to Sure Start reduces the likelihood of hospitalisations among children aged 5–11, with benefits growing with age.** An extra Sure Start centre per thousand children aged 0–4 reduces the probability of an LSOA having any hospitalisation at age 5 by 4% of its baseline (and we cannot be confident that this result is genuinely different from zero). By age 11 – the last age we look at – the probability falls by 18% of its baseline level. This is equivalent to around 5,500 hospitalisations averted each year among 11-year-olds and is strongly statistically significant.

- **Poorer areas benefit a lot from Sure Start, while there are few effects in the richest neighbourhoods.** We cannot say whether the bigger benefits in the poorest neighbourhoods come about because disadvantaged children are more able to benefit from Sure Start or because the types of services Sure Start offers in poorer areas are more helpful. Either way, these results suggest that one way to deliver value for money is to focus on providing services to the disadvantaged areas most likely to benefit from them, and to consider which types of services are most effective for this group.

One of the goals of the earliest phases of the Sure Start roll-out was to reduce hospitalisations among children (Armstrong, 2017). In this chapter, we consider whether increasing an area's access to Sure Start reduces the chances that its children are admitted to hospital. In order to trace out the effects of Sure Start in the medium run, we look at hospitalisation outcomes between ages 5 and 11. This means we are looking at outcomes in the period after children stop being eligible for Sure Start and investigating whether and to what extent any benefits are sustained in the medium term.

We consider inpatient admissions to hospital, not visits to A&E or outpatient appointments.[24] Hospitalisations at these ages are relatively rare, but quite costly. Children born in 1993 – the last 'baseline' cohort that was entirely unexposed to Sure Start – experienced just over 65,000 hospitalisations at age 5, falling to around 45,000 admissions at age 11.

Hospitalisations are not distributed evenly around the country. Figure 5.1 shows the all-cause hospitalisation rates of children born in 1993 at ages 5 and 11. It ranks neighbourhoods by their level of disadvantage (based on the 2004 Index of Multiple Deprivation).[25] Children in the most disadvantaged neighbourhoods were hospitalised at a

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[24] In part, this reflects the period that the data are available: A&E attendance data are only considered reliable from April 2007 onwards, so there is less scope to look at medium-run impacts of Sure Start across the entire life cycle of the programme.

[25] We use the 2004 Index of Multiple Deprivation rather than the 1998 Index of Local Deprivation because the former was calculated at the much more granular LSOA level (rather than the local authority district level).

Figure 5.1. Hospitalisation rate by neighbourhood disadvantage (1993 birth cohort)
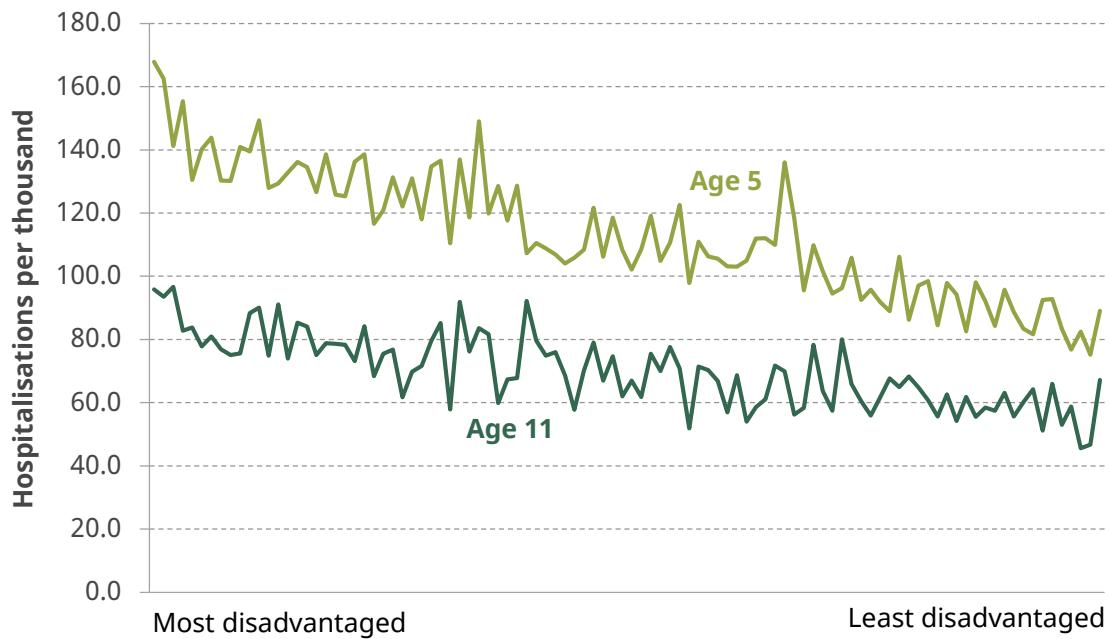
Note: Hospitalisation rates are calculated for each percentile of the distribution of the 2004 Index of Multiple Deprivation based on admissions for the 1993 birth cohort at ages 5 and 11. Each percentile contains approximately 330 LSOA 'neighbourhoods'. Rates are the number of hospital admissions divided by the number of children of that age living in the relevant neighbourhoods.

Source: Authors' calculations based on Hospital Episode Statistics and ONS population data.

much higher rate than their better-off peers. For example, at age 5, there were 144 hospitalisations per thousand children in the 10% most disadvantaged neighbourhoods; this falls to 85 in the 10% richest areas.

Interpreting the results

One of the challenges with using hospitalisation records is that we only observe children who are admitted to hospital – we do not have information on anyone who is not hospitalised in this period. Obviously, we cannot answer questions about how Sure Start affects the probability of being hospitalised if we are only looking at people who are admitted to hospital. In order to get around this challenge, we construct a data set that counts the number of admissions within a 'cell', which is defined by the neighbourhood (LSOA), sex, and month and year of birth. So, for example, our data set would contain the number of hospitalisations of boys in a specific neighbourhood of Oldham at ages 5, 6, 7 and so on up to age 11. Since we logically know all of the combinations of neighbourhoods, sex, and month and year of birth, we can add even the cells with no hospitalisations to our data set. While some cells have several admissions, in most cases there is only one, if any.[26] We therefore consider as our outcome whether there is *any* hospital admission within the neighbourhood–sex–birth-month–birth-year cell (rather

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[26] For example, at age 5, 94.2% of cells had no admissions, 4.6% had one admission and 1.1% had more than one admission. At age 11, the figures are 96.3%, 3.0% and 0.7% respectively. When analysing admissions for specific causes (see Chapter 6), the share of cells with more than one admission is much smaller.

than the number of admissions).[27] Overall, the probability of a cell having any hospitalisation at a given age is relatively low; around 6% of cells at age 5 have at least one hospitalisation, falling to 4% at age 11.

As mentioned in Chapter 4, as a measure of access to Sure Start we use the coverage of centres – the number of centres per thousand children aged 0–4 in a local authority, averaged over the first 60 months of the child's life. An increase of one centre per thousand children is equivalent to the average increase in coverage across the whole roll-out 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 hospitalisation.

5.1   Overall impact on hospitalisations

We first consider the impact that Sure Start had on hospitalisations for any reason between ages 5 and 11. These results are shown in Table 5.1, which reports the estimates from seven separate regressions – one for each age of admission.

The effects shown in Table 5.1 are small. An increase of one Sure Start centre per thousand children in the local authority – equivalent to the average total increase over the entire roll-out of Sure Start – means that the probability of any hospitalisation in a cell falls

Table 5.1. Effect of an increase in Sure Start coverage on probability of any hospitalisation in the neighbourhood

	Hospitalisations at:						
	Age 5	**Age 6**	**Age 7**	**Age 8**	**Age 9**	**Age 10**	**Age 11**
Sure Start coverage	−0.0026 (0.0020)	−0.0024 (0.0020)	−0.0015 (0.0018)	−0.0015 (0.0016)	−0.0030* (0.0014)	−0.0043** (0.0014)	−0.0072*** (0.0019)
Observations	12,995,136	12,207,552	11,419,968	10,632,384	9,844,800	9,057,216	8,269,632
Last cohort	2009	2008	2007	2006	2005	2004	2003
Baseline mean	0.0608	0.0524	0.0449	0.0412	0.0393	0.0377	0.0392

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), 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.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

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[27] Even though the outcome of the regressions is binary, we estimate linear probability (ordinary least squares, OLS) models. We experimented with alternative modelling strategies accounting for the binary nature of our outcomes. The results were qualitatively similar, but the computing time was exponentially larger so we only report linear probability model results.

by just 0.7 percentage points at age 11, and by less than this at earlier ages. However, even small changes can have meaningful effects when added up across the population. If each neighbourhood–sex–birth-month–birth-year cell has at most one hospitalisation,[28] a 0.7 percentage point fall in the probability of there being a hospital admission in a cell translates to around 5,500 averted hospitalisations at age 11 each year.

At the same time, it is difficult to directly compare the estimated impacts at different ages set out in Table 5.1, since the likelihood of having a hospitalisation differs by age. A 1 percentage point fall in the probability of a hospitalisation means more at age 11, where just 4% of cells experience any admission, than it does at age 5, when 6% of cells see at least one child admitted.

Figure 5.2 therefore rescales the estimates in Table 5.1, dividing each by the baseline mean (measured in 1998, the year before the beginning of the Sure Start roll-out) for that age group. The points in Figure 5.2 therefore represent the share of hospitalisations in a cell that are avoided with a one-unit increase in Sure Start coverage. The vertical bars indicate our degree of confidence in the findings; where the vertical bars do not cross the horizontal axis (at ages 10 and 11 in Figure 5.2), we can be confident that the observed impact is not just due to chance. The figure shows that the impacts of Sure Start coverage

Figure 5.2. Effect of an increase in Sure Start coverage on probability of any hospitalisation in the neighbourhood, rescaled by baseline probability
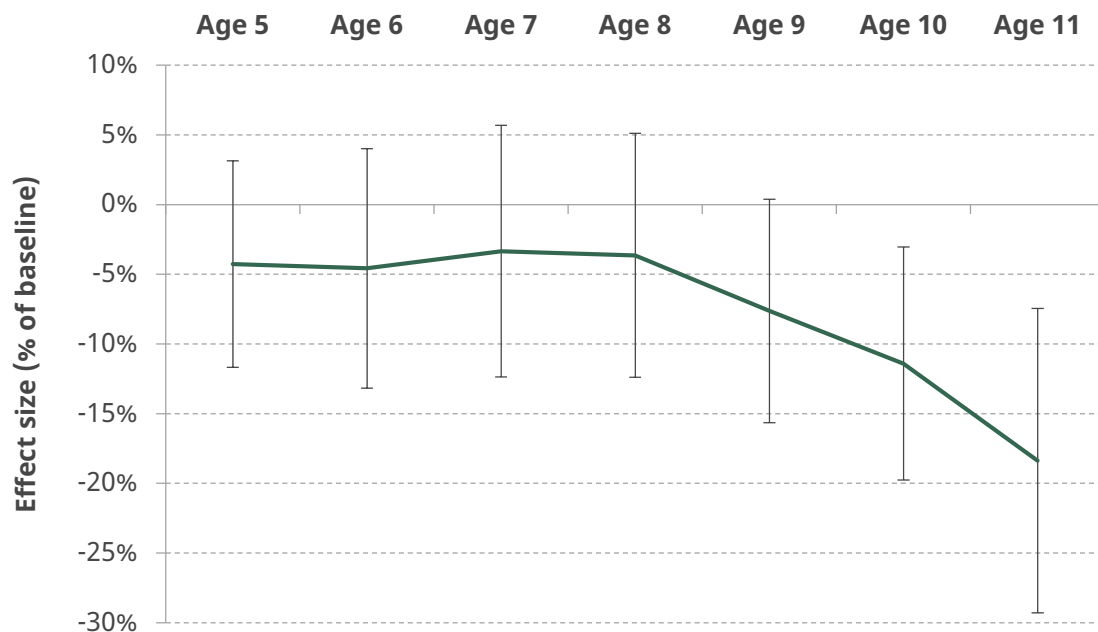
Note: Effect sizes are constructed by rescaling the estimates in Table 5.1 by the pre-Sure-Start baseline probability of a hospitalisation at each age. Vertical bars indicate 95% confidence intervals; where they do not cross the horizontal axis, Sure Start has a statistically significant impact on the probability of hospitalisation.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

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[28] For calculating the number of averted hospitalisations, this is a conservative assumption since it assumes that a cell going from having 'some' hospital admissions to having none avoids just one hospitalisation. In practice, if Sure Start averts several hospitalisations in some cells, the total number of admissions averted could be bigger.

increase with age. While around 4% of admissions for 5-year-olds are averted when a local authority increases Sure Start coverage by one centre per thousand children, by age 11 18% of admissions are avoided. So the benefits of Sure Start *grow* over time, even though all of the children in these models are no longer young enough to benefit from its services directly. This is remarkable – it suggests that the benefits of Sure Start are not only persistent, but even getting stronger over time.

As we discussed in Chapter 4, one note of caution is that, even with the rescaling done in Figure 5.2, the set of children considered at each age is slightly different. The results at age 5 include children born between 1993 and 2009, but at age 6 the last cohort is born in 2008, at age 7 it is born in 2007, and so on through to being born in 2003 for age 11. 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 5.3 shows both our main estimates from Figure 5.2 (in dark green) and a set of estimates that are all based on children born between 1993 and 2003 (in grey). Where we can be statistically confident that the results are not a result of random chance, we have marked the point with a star. The results based on a common set of cohorts tell a similar story to Figure 5.2, with bigger benefits among older children. However, the profile of the effect size is more gradual when using the common sample. This could suggest that Sure Start was more effective for

Figure 5.3. Effect of an increase in Sure Start coverage on probability of any hospitalisation in the neighbourhood, rescaled by baseline probability: with common sample results
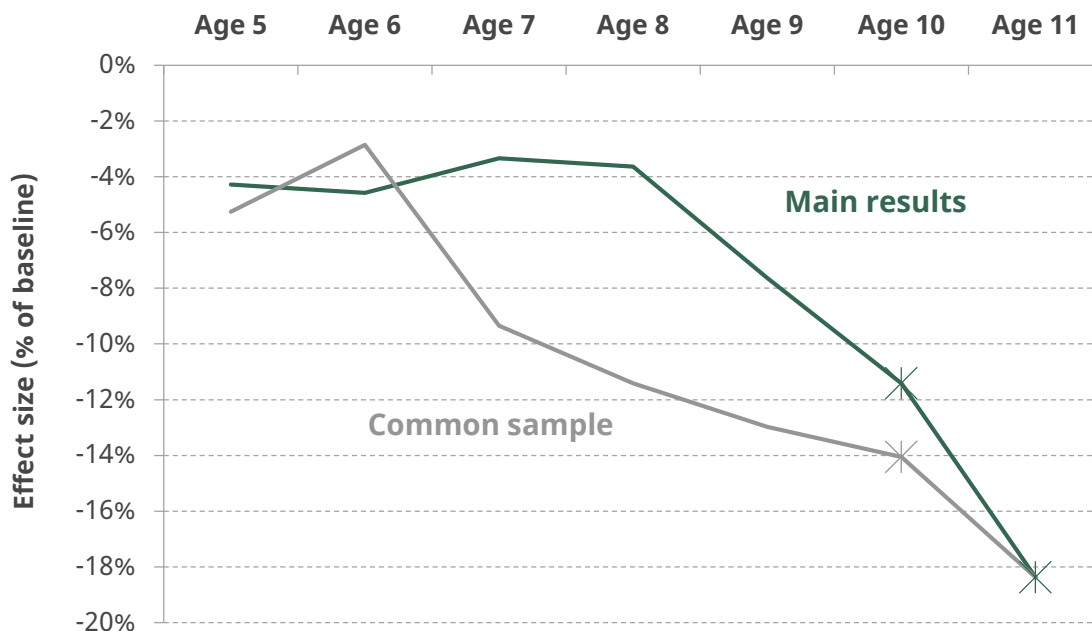
Note: Effect sizes are constructed by rescaling the estimated regression coefficients from the model described in Chapter 4 by the pre-Sure-Start baseline probability of a hospitalisation at each age. 'Main results' restate the results in Figure 5.1. 'Common sample' results use the same cohorts (birth years 1993 to 2003 inclusive) to estimate the impact at each age. Points with a star marker are statistically significant at the 5% level.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

children born in earlier cohorts, who were exposed to a more targeted version of the programme.[29] We investigate Sure Start's effectiveness in different neighbourhoods further in Section 5.2.

5.2   Variation in impacts

Sure Start is designed as a universal programme, accessible to all families. But previous evidence suggests that the benefits of such programmes can vary between different groups (e.g. Havnes and Mogstad, 2011; Carneiro and Ginja, 2014). In this section, we explore whether Sure Start's effects differ for boys and girls or for neighbourhoods with different levels of disadvantage.[30]

Figure 5.4 shows how the effects of Sure Start on all-cause hospital admissions vary between girls and boys. We find bigger impacts for boys up to age 8: the stars at the top

Figure 5.4. Sure Start's effect on probability of any hospitalisation, rescaled by baseline probability: differences by gender
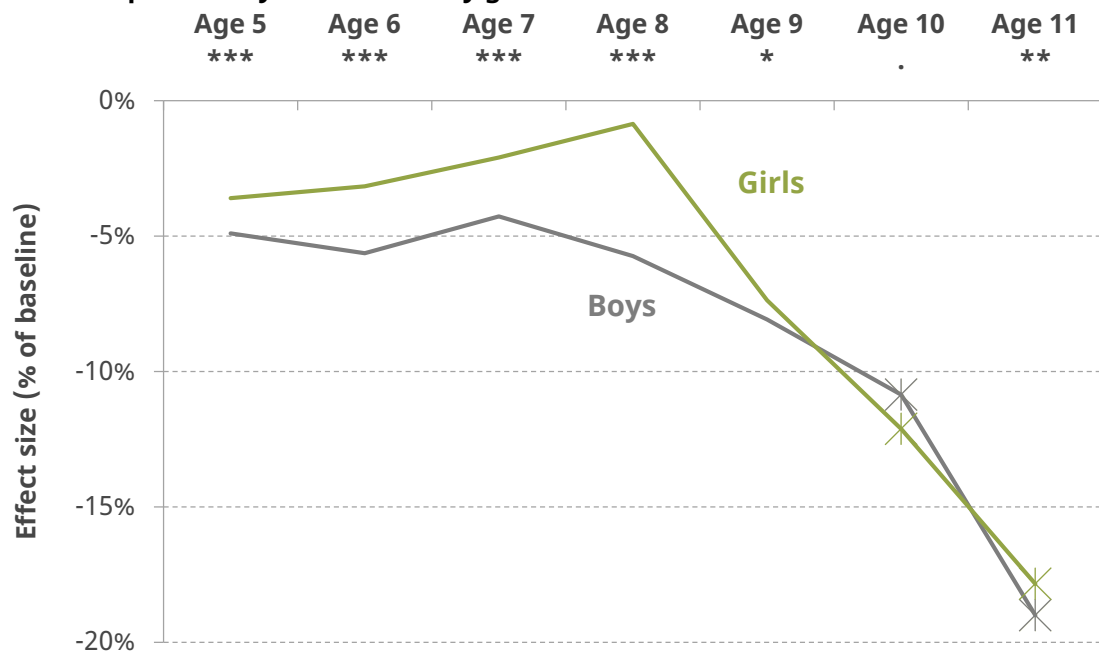
Note: The figure shows coefficients from separate regressions for each outcome age, with Sure Start treatment interacted with gender. Coefficients are rescaled by the gender-specific baseline (1998) mean for each age. The model is otherwise as described in the note to Table 5.1. Asterisks on the lines indicate whether individual coefficients are statistically significant at the 5% level. The asterisks under the age labels at the top of the figure indicate whether the impacts on boys and girls are statistically significantly different: . = not significantly different; * = significant at the 10% level; ** = significant at the 5% level; *** = significant at the 1% level.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

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[29] Another factor in the smaller estimates from the full sample is likely to be a greater degree of treatment misclassification in Sure Start coverage for later cohorts (who would have been more affected by the Sure Start closures that we cannot fully account for in our measure).

[30] We do this by extending the model described in Chapter 4 to interact the treatment variable – coverage of Sure Start – with the indicators for different subgroups. We also add the subgroup indicators to the regression directly, allowing for a different overall level of hospitalisations for each group.

of the figure indicate that, at every age except ages 9 and 10, we can be statistically confident at the 5% level that the impacts are bigger for boys than for girls. However, as with the overall results, the impacts themselves are only significant at ages 10 and 11.

In line with the previous research on the health impacts of early years programmes, it is also interesting to assess whether the impacts differ for children with different levels of disadvantage. To do this, we compare the effect of Sure Start in the most disadvantaged 30% of neighbourhoods, the top 30% best-off neighbourhoods and those in the middle of the distribution of disadvantage.

As Figure 5.5 illustrates, at younger ages we find no evidence of different impacts in these types of neighbourhoods. But at age 9 and increasingly at ages 10 and 11, we see that the benefits of Sure Start are much stronger in the middle and, especially, the poorest parts of the disadvantage distribution. At ages 10 and 11, we can be statistically confident that children in the poorest neighbourhoods benefit more than those in the richest areas from having more access to Sure Start. This is equivalent to closing a third of the baseline gap in the probability of hospitalisation between rich and poor cells at age 10 and 56% of the gap at age 11.

Figure 5.5. Sure Start's effect on probability of any hospitalisation, rescaled by baseline probability: differences by disadvantage
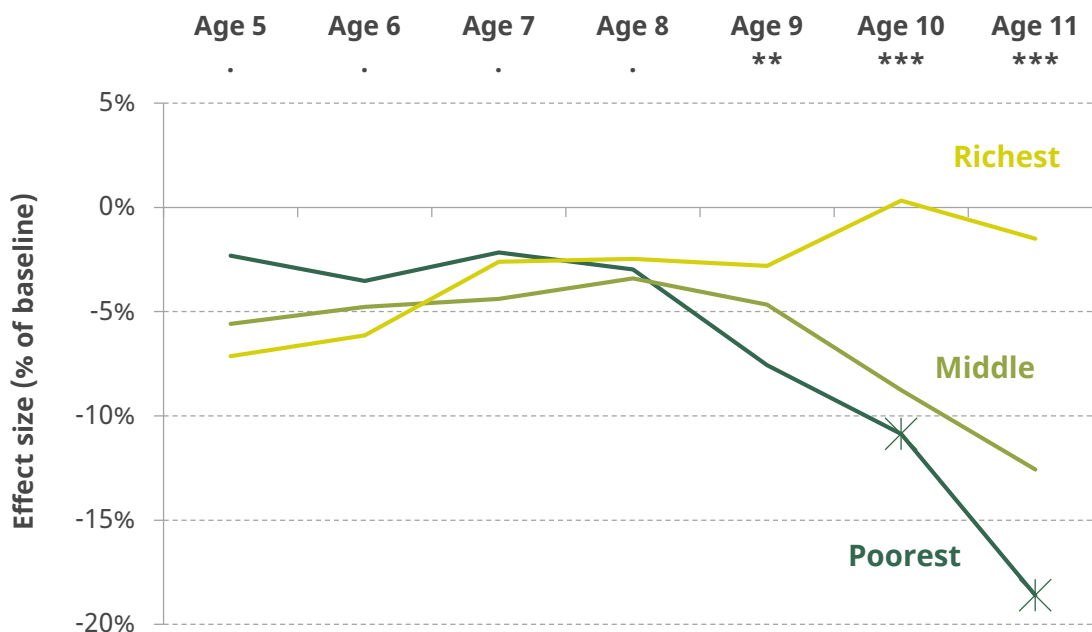
Note: The figure shows coefficients from separate regressions for each outcome age, with Sure Start coverage interacted with the three disadvantage categories. Coefficients are rescaled by the disadvantage-specific baseline (1998) mean. The model is otherwise as described in the note to Table 5.1. Asterisks on the lines indicate whether individual coefficients are statistically significant at the 5% level. The asterisks under the age labels at the top of the figure indicate whether the impacts on the poorest and the richest are statistically significantly different: . = not significantly different; * = significant at the 10% level; ** = significant at the 5% level; *** = significant at the 1% level. 'Richest' are children living in the 30% least deprived neighbourhoods, ranked by the 2004 Index of Multiple Deprivation. 'Poorest' children are in the 30% most deprived neighbourhoods and 'Middle' children are in the middle of the rankings.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

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) are more likely to deliver additional services directly, rather than relying on signposting families to existing programmes.

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 neighbourhoods. However, this is still an important finding, because it suggests that the benefits of Sure Start are not equally felt across the country. At a time when the budgets for Children's Centres are being cut, these results suggest that one way to deliver more value for money would be to focus on providing services to the disadvantaged areas that are more likely to benefit from them, and to consider which types of services and models of provision could most effectively help this group.

The effects that Sure Start has on hospitalisations are meaningful both in a statistical and an economic sense. Given their importance, we have tested that they are not just an artefact of the model we have chosen. We have performed a battery of falsification and robustness tests, which are reported in Appendices C and D. We successfully pass all of them, reassuring us that we have uncovered a robust causal effect of Sure Start on hospitalisations.

6. Hospitalisations for specific causes

Key findings

- **While Sure Start had few effects on hospitalisations for respiratory illness, there were big falls in hospitalisations for infections (at young ages) and injuries.** These different causes of admission point to potential channels through which the benefits of Sure Start are felt, such as better immune systems, safer home environments or better behaviour of children.

- **Sure Start impacts hospitalisations for several types of injuries, some of which have long-run consequences.** At almost every age we consider, Sure Start reduces hospitalisations for fractures. There is also a significant reduction in head injuries at age 5. This is particularly important because head injuries have long-lasting impacts both on children themselves and on healthcare costs.

Chapter 5 showed that Sure Start has large benefits overall in reducing children's hospitalisations, particularly at older ages and in disadvantaged neighbourhoods. In order to better understand the source of these impacts, it is helpful to consider the potential mechanisms through which Sure Start might have had an effect.

In Chapter 3, we outlined some of the mechanisms through which Sure Start might impact children's health. In particular, we can identify several potential channels:

- direct provision of health services, such as 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;
- 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 Sure Start prevents. We consider several types of cause-specific admissions, summarised in Table 6.1.[31]

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[31] 'Ambulatory care sensitive' 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 (e.g. gangrene). See, for example, Blunt (2013) for a discussion of these conditions.

Table 6.1. 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–T98, 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

Note: For all outcomes except ACS conditions, we consider all diagnoses – not just the primary diagnosis – when identifying cause-specific admissions. For ACS conditions, we consider only the primary diagnosis.

Figure 6.1. Hospital admissions per thousand children, 1993 birth cohort
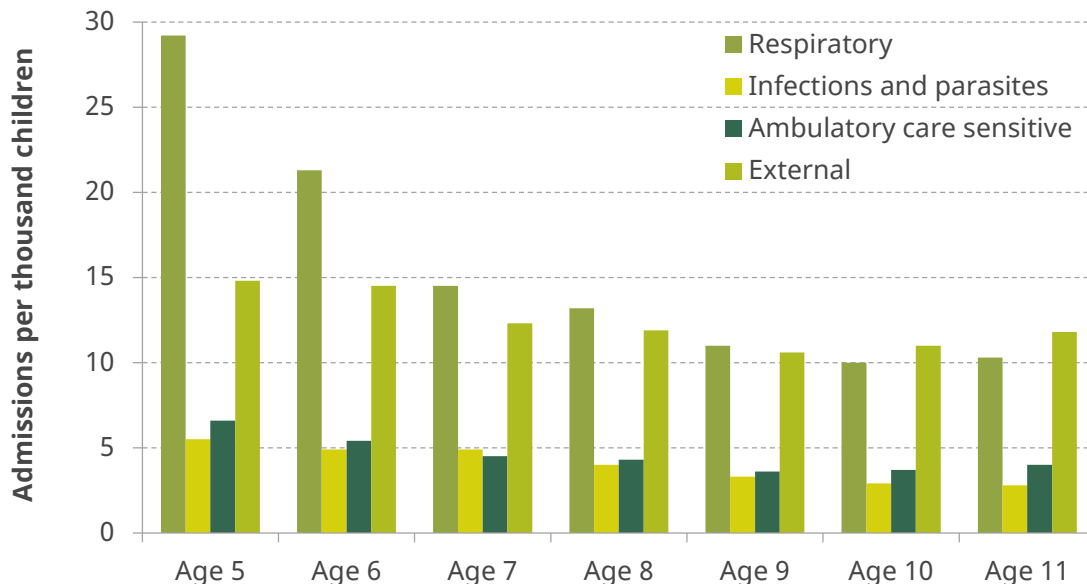
Note: Hospitalisation rates are calculated as the number of hospitalisations per thousand children of the same age in England. Cause-specific hospitalisations are defined based on ICD-10 diagnosis codes as set out in Table 6.1.

Source: Authors' calculations based on Hospital Episode Statistics inpatient data (1997–2014) and ONS population data.

Figure 6.1 presents the prevalence of these conditions at each of the outcome ages that we consider for the 'baseline' cohort of children born in 1993 (which was the last cohort entirely unaffected by Sure Start). Of the causes that we consider (outlined in Table 6.1), respiratory illnesses and externally inflicted conditions are the two most common. Externally inflicted conditions are predominantly injuries, which account for around 75% of these hospitalisations. There are relatively few admissions for infections or preventable ambulatory care sensitive (ACS) conditions.

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 ACS conditions might suggest that primary care services such as GPs are more effective in areas with more Sure Start coverage (either because the services themselves benefit from extra resources or because families are more likely to use the services), or that children's immune systems are stronger (e.g. because children have been exposed to more illnesses earlier in life through childcare). A decline in admissions for injuries and poisonings 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.

6.1   Cause-specific admissions

Figure 6.2 shows the impact of Sure Start coverage on the probability of hospitalisation for four of these causes: respiratory illnesses, infections or parasites, external events (such as broken bones or poisoning) and preventable (ACS) conditions. The lines in Figure 6.2 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 1998 (the 'baseline' year prior to the opening of the first Sure Start Local Programmes).

Higher Sure Start coverage reduces the probability of infection admissions at age 5. While the results at other ages are not different from zero in a statistical sense, it is striking that the profile across ages is upward-sloping. This indicates that the reductions in infection admissions fade out as children get older. There are a few channels through which Sure Start might affect infections. First, Sure Start centres provide 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 (van den Berg and Siflinger, 2018).

In both of these cases, it is likely that schools could deliver many of the same benefits (through in-school vaccination programmes and through contact with large groups of children). So while children who have access to Sure Start might benefit from that earlier in life, programmes delivered through schools could allow their peers to catch up to them, reducing the observed benefits of the Sure Start programme at later ages.

Another striking finding from Figure 6.2 is the importance of Sure Start in driving reductions in hospitalisations for external causes across all the outcome ages we study. We find a 12% fall in the probability that a neighbourhood–sex–birth-month–birth-year cell

Figure 6.2. Sure Start's effect on probability of hospitalisation for specific causes, rescaled by baseline probability
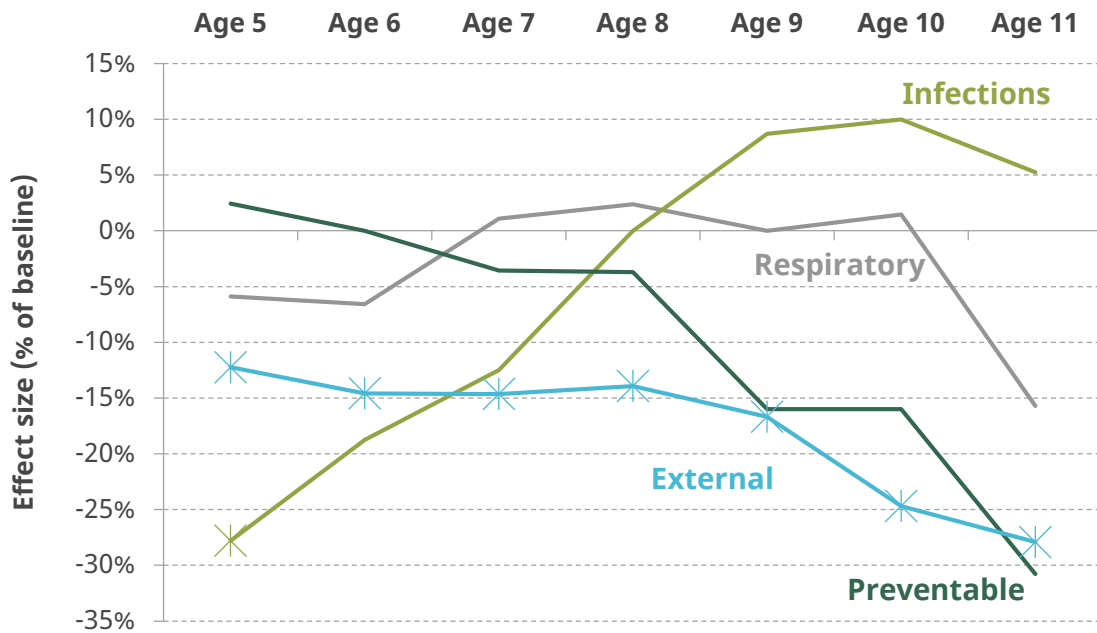
Note: The figure shows coefficients from separate regressions for each outcome age and admission cause. The model is as described in the note to Table 5.1. Definitions for admission causes are given in Table 6.1. Points with a star marker are statistically significant at the 5% level.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

experiences a fall in admissions at age 5, which grows to 25% and 28% declines at ages 10 and 11 respectively (equivalent to about 1,600 averted hospitalisations per year at age 10 and 1,900 at age 11).

This is consistent with several of the potential mechanisms that we discuss. 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, by 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 such as 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 children whose families received extended outreach tended to have more behavioural issues (Sammons et al., 2015). Evidence from the Millennium Cohort Study, presented in Appendix F, shows that there is a strong relationship between behaviour and parent-reported hospitalisations and injuries; behaviour is a more statistically significant

predictor of injuries than characteristics such as household earnings, maternal education or maternal work status.

Figure 6.2 makes it clear that one of the major drivers of our overall results is admissions for 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, Figure 6.3 breaks out admissions related to injuries (such as broken bones) and those related to poisonings (including from household substances and medications). Although it is impossible to clearly assign a single mechanism to each type of admission, in general reductions in injury-related admissions might more plausibly be driven by better behaviour while falls in poisonings speak more directly to a safer home environment.

Figure 6.3 clearly shows that the falls in external admissions are driven by Sure Start's impacts on hospitalisations related to injuries. In one sense, this is not surprising – injuries make up the bulk of admissions for external causes, accounting for between 70% and 80% of external-cause admissions depending on the age group. (Poisonings account for almost all of the rest.)

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 injuries are due to child maltreatment; however, previous research has used such conditions as proxies for potential maltreatment scenarios (e.g. Gonzalez-Izquierdo et al., 2010). Because a relatively small share of childhood injuries are due to maltreatment, this

Figure 6.3. Sure Start's effect on probability of hospitalisation for specific external causes, rescaled by baseline probability
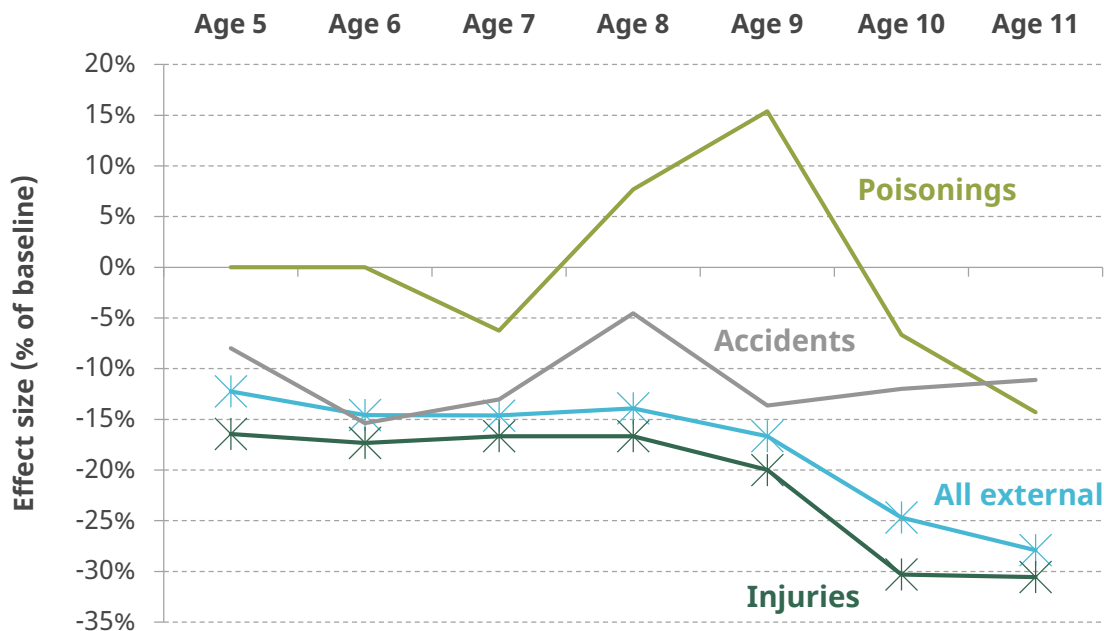
Note: The figure shows coefficients from separate regressions for each outcome age and admission cause. The model is as described in the note to Table 5.1. Definitions for admission causes are given in Table 6.1. Points with a star marker are statistically significant at the 5% level.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

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.

Looking at how these results differ by the characteristics of neighbourhoods and children, we find similar patterns to the results on overall admissions presented in Section 5.2. There is clear evidence that hospitalisations due to injuries fall most in the poorest neighbourhoods, with a statistically significant effect at every age we consider. There are some benefits for children in the middle and richest neighbourhoods at younger ages, but these are always smaller than the effects on the poorest and they disappear by age 8. The full results of this analysis are presented in Appendix E.

6.2   Different types of injuries

There is a wide range of potential injuries in childhood, with very different implications for long-run health and for cost. While a broken leg or a minor burn might heal straightforwardly with few impacts on a child's health in the long run, a serious head injury at a young age can have severe and lifelong consequences.

We therefore explore which types of injuries Sure Start is particularly effective at reducing. Figure 6.4 shows Sure Start's impacts on two specific types of injury: fractures (in grey) and head injuries (in light green). For context, we also repeat the results for overall injuries. We also attempted to analyse hospitalisations for conditions related to self-harm

Figure 6.4. Sure Start's effect on probability of hospitalisation for specific injuries, rescaled by baseline probability
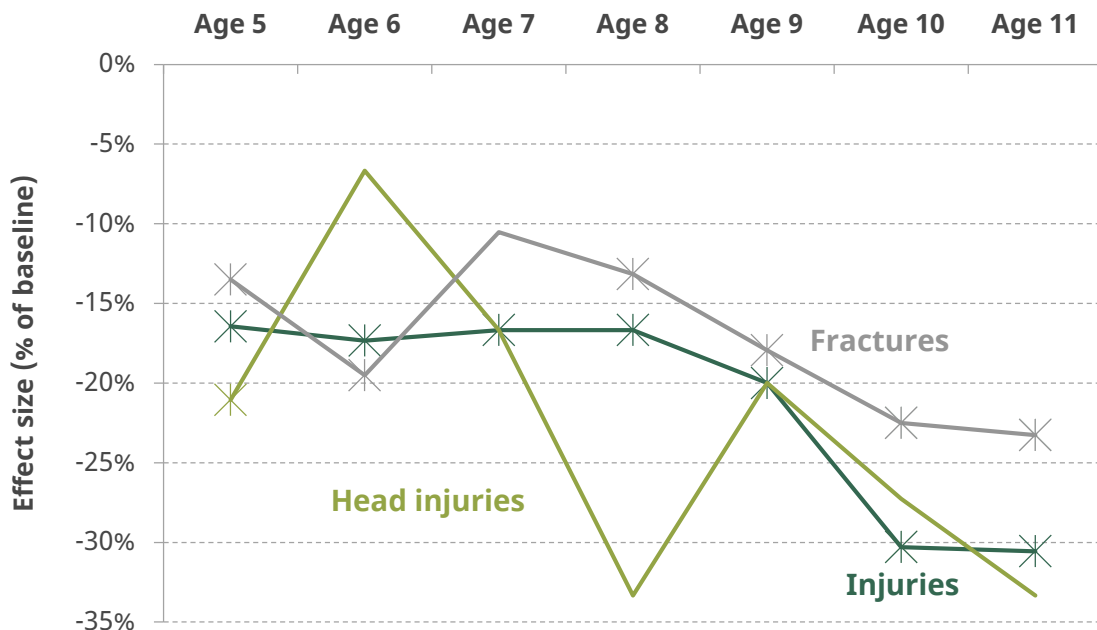
Note: The figure shows coefficients from separate regressions for each outcome age and admission cause. The model is as described in the note to Table 5.1. Definitions for admission causes are given in Table 6.1. Points with a star marker are statistically significant at the 5% level.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

and to child maltreatment (based on the coding framework proposed in Gonzalez-Izquierdo et al. (2010)). However, both of these events were so rare in the population that we are considering that we were not able to draw any robust conclusions from this work.

We find that Sure Start's impacts on fractures mirror its effects on injuries as a whole, though the decline in fractures is typically smaller than the fall in injuries as a whole. Greater access to Sure Start decreases the probability of at least one fracture-related hospitalisation among the neighbourhood–sex cohort for every age except age 7.

Head injuries are much rarer than injuries as a whole; on average, a neighbourhood–sex cohort had a 0.7% chance of having any injury-related admission at a given age, while the probability for head injuries was between 0.1% and 0.2%. Because these events are relatively rare, the impacts that we estimate are somewhat noisy and often not statistically significant. However, they broadly mirror the pattern that we have found for other outcomes, with the probability of any admission falling by at least 7% at each age and a statistically significant fall of 21% at age 5.

Taken together, these results suggest that there is unlikely to be just one type of injury driving our results. However, Sure Start clearly has a meaningful impact on reducing fractures, with a fall of between 11% and 23% in the chance of at least one member of a neighbourhood–sex cohort being admitted to hospital for a broken bone. Like the overall effects on injuries, these effects if anything get stronger as children get older, with the biggest falls in hospitalisations at ages 10 and 11.

Sure Start also appears to have some impact in reducing head injuries, though here the only statistically significant result is on hospitalisations at age 5. These injuries might be particularly concerning for policymakers, since they represent a serious risk to a child's long-term physical health and mental development as well as a significant cost to the public purse.

7. Impact on obesity

Key findings

- **We do not find evidence that Sure Start impacts obesity at age 5.** Sure Start provided services related to nutrition, including information on healthy eating during pregnancy and parenting sessions to promote healthy eating and physical activity in childhood. Nevertheless, we are unable to detect any impact of the programme on the BMI or obesity of children enrolled in Reception Year.

Both the SSLPs and the SSCCs offered services aimed at promoting a healthy weight with the goal of halting the rise in obesity among children (Armstrong, 2017). These services included the provision of information on healthy eating in pregnancy, as well as parenting sessions to promote healthy eating in infancy and toddlerhood and physical activity. In this chapter, we report the results of our analysis of the National Child Measurement Programme (NCMP) on the effects of Sure Start on weight-related outcomes amongst children aged 4–5.

7.1 Outcomes and methodology

As mentioned in Chapter 4, the NCMP collects information about the height and weight of children in maintained state schools at two points in time: during Reception Year, when children are 4–5 years old, and during Year 6, when children are 10–11 years old. Using information on height and weight, we construct five outcomes of interest. The first is a continuous measure of body mass index (BMI; weight in kilograms divided by the square of height in metres). The second and third outcomes are indicators for being (i) overweight or obese and (ii) obese, i.e. BMI-for-age at or above percentiles 85 and 95, respectively, using the British 1990 growth reference (Public Health England, 2016). The fourth and fifth ones are indicators for being (i) clinically overweight or obese and (ii) clinically obese, i.e. BMI-for-age at or above percentiles 91 and 98, respectively, based on the British 1990 growth reference.

To identify the impacts of Sure Start on BMI and obesity measures, we strive to use a similar difference-in-difference strategy to the one we use for hospitalisations. In the Hospital Episode Statistics (HES), we defined our treatment variable as the average coverage of Sure Start when the child was potentially eligible (i.e. before age 5) in the child's local authority of residence at the time of hospitalisation.[32] Two pieces of information were crucial to the construction of this variable: the child's month and year of birth and the child's home address. There are several differences between the HES and the NCMP, however, which prevent us from replicating this variable exactly and have important consequences for the definition of our sample.

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[32] This is a proxy for the child's residence during the first five years of life.

First, the NCMP does not include information on the child's month and year of birth. Rather, it includes information on the child's age in months at the time of measurement, which we use to recover the child's month and year of birth. However, although the NCMP started in 2006, the information on the month of survey is only available for the years 2008 to 2014. As a result, we restrict our analytical sample to these years.

The second difference is that the NCMP does not contain information about the child's home address. Rather, it only includes information about the location of the child's school, which we use as a proxy for the child's home residence. This approximation creates measurement error in the treatment variable, which is likely to be worse at older ages as the likelihood that families have moved out of the local authority where their child was born increases. These important considerations lead us to focus our analysis on the impacts of Sure Start on weight-related outcomes measured in Reception Year only. Our sample therefore includes children born between 2003 and 2010, who could only have been affected by SSCCs.

Before turning to our results, it is important to mention that, throughout the course of the project, we never directly accessed the NCMP. This is because the NCMP is collected under different legal arrangements from other administrative data sets, which prevent it being shared directly with researchers. Staff at Public Health England were able to use our code to run several pieces of analysis with the data and send us the output. However, constraints on their time put a cap on the number of iterations we were able to have with the data, which in turn limits our ability to explore different aspects of the relationship between Sure Start and obesity.

In the rest of this chapter, we present the results from the overall analysis that we were able to carry out, with the caveat that – because our ability to probe these results more deeply has been limited – these should be considered as more exploratory than the hospitalisation results presented in Chapters 5 and 6.

7.2   Results on obesity

Table 7.1 shows the means for the outcomes we study, by deprivation level of the school's LSOA (for the poorest 10% and the richest 10% of LSOAs in England) and by gender.[33] Panel A of the table shows that the mean BMI is higher, and the prevalence of overweight or obese children is 36% higher, in the poorest than in the wealthiest LSOAs in England: 25.4% of children in the 10% poorest LSOAs are obese, compared with 18.7% in the top decile. The last two columns show a similar trend for obesity and for the share of children clinically overweight or obese across the poorest and richest LSOAs in England. Panel B shows that the mean BMI and the prevalence of obesity and overweight are similar for boys and girls.

Next, we turn to the estimates reported in Table 7.2 of the effects of Sure Start on our outcomes of interest. The coefficients reported in the table should be interpreted as the effect of having an additional centre per thousand children aged 0–4 open in the local authority of the school.

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[33] To construct the measure of deprivation, we use the 'most recent' deprivation index (Index of Multiple Deprivation, IMD) decile for each year, which is constructed using IMD2007 for 2008–09 and IMD2010 from 2010 onwards.

Table 7.1. Mean of weight-related outcomes among children in Reception Year (aged 4–5 in 2008–14)

	Mean BMI	% overweight or obese (BMI > 85th percentile)	% clinically overweight or obese (BMI > 91st percentile)	% obese (BMI > 95th percentile)
Entire sample	16.180	22.8	15.0	9.6
Panel A				
Poorest 10% LSOAs	16.280	25.4	17.6	12.1
Richest 10% LSOAs	16.000	18.7	11.4	6.7
Panel B				
Girls	16.174	21.5	13.9	8.9
Boys	16.181	23.5	15.5	9.9

Note: The table shows the means for the main outcomes used in the analysis of weight-related outcomes: BMI, percentage of overweight or obese children (both population and clinical definition) and percentage of obese children. The means are presented by the 'most recent' deprivation index (Index of Multiple Deprivation, IMD) decile for each year, which is constructed using IMD2007 for 2008–09 and IMD2010 from 2010 onwards. We also present the means separately by gender of the child.

Source: Authors' calculations using data from the National Child Measurement Programme data set (2008–14) and the Index of Multiple Deprivation.

Table 7.2. Effect of an increase in Sure Start coverage on the probability of being overweight or obese among children in Reception Year (aged 4–5)

	(1) BMI	(2) Overweight or obese	(3) Obese	(4) Overweight or obese (clinical)	(5) Obese (clinical)
Sure Start coverage	−0.007 (0.026)	−0.004 (0.005)	0.000 (0.002)	−0.002 (0.003)	0.000 (0.002)
No. of observations	3,826,111	3,826,111	3,826,111	3,826,111	3,826,111
Mean	16.180	0.228	0.096	0.150	0.052
R^2	0.001	0.001	0.001	0.001	0.001

Note: The table shows coefficients from a regression of BMI or an indicator for being overweight or obese on Sure Start coverage (number of centres per thousand children aged 0–4 in the local authority), indicators for female, white ethnicity and free school meal eligibility, indicators for the child's academic cohort, indicators for the child's month and year of birth, and indicators for the school's LSOA. Standard errors are shown in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Source: Authors' calculations using data from the National Child Measurement Programme data set (2008–14) and the Department for Education's data on the roll-out of Sure Start.

The estimates of the effects of Sure Start on weight-related outcomes are all negative, but none of them is statistically significant. This means that Sure Start could have decreased children's BMI and their likelihood of being obese or overweight, but we cannot be confident that these effects are not the result of chance.

For example, we estimate that the effect of Sure Start on BMI is −0.007. Relative to the mean BMI in the sample of 16.180, this would correspond to a 0.04% decrease in BMI due to Sure Start. Similarly, depending on which population we use as reference, our estimates suggest that Sure Start decreased the probability of being overweight or obese by between 0.2 and 0.4 percentage points (column 4 and column 2, respectively), which correspond to falls of 1.3% and 1.8% respectively. However, none of these coefficients is statistically significant.

Previous research has suggested that early interventions may have greater impacts on weight-related outcomes for particular groups of the population. For example, both Campbell et al. (2014) and Carneiro and Ginja (2014) find that early interventions (Abecedarian and Head Start, respectively) decreased the likelihood of being obese only for boys. As a result, we also explored whether the results in the entire sample mask stronger effects in particular subgroups of the population. Specifically, we compared effects between boys and girls, urban and rural areas, whites and non-whites, and across areas with different levels of deprivation. We did not find a single group for which the results were statistically significant. Overall, this analysis suggests that SSCCs did not have an impact on children's weight, at least when measured in Reception Year, for the cohorts considered (those born between 2003 and 2010).

8.   Impact on mothers' mental health

Key findings

- **We find no evidence that Sure Start affects mothers' mental health.** When we analyse how mothers' mental health was affected by the Sure Start exposure of their eldest child, we find no evidence of any impacts. The direction of any potential effect is also unclear.

- **The findings in this chapter are based on relatively limited data and should be treated with caution.** This analysis is based on a survey, rather than on large-scale administrative data. Because the sample size is much smaller, there is less scope to detect any impacts, so the absence of evidence that Sure Start had an effect on maternal mental health should not be treated as evidence that there was indeed no impact.

In Chapters 5, 6 and 7, we have outlined the overall impact that Sure Start had on children's hospitalisation rates and obesity. In this chapter, we look at the impact that Sure Start had on maternal mental health. We focus on mothers because related survey data sets, such as the Evaluation of Children's Centres in England (ECCE) survey of Sure Start users, suggests that mothers are the primary caregivers in most families and might therefore be expected to be the primary adults using Sure Start services. Sure Start might have benefited mothers' mental health by helping them to feel more confident and supported in their parenting or through providing direct mental health services, such as referrals to a specialist.

In order to conduct this analysis, we use the UK Household Longitudinal Survey (UKHLS), a longitudinal survey of households with rich information on family characteristics and mental health. The predecessor of this survey was the British Household Panel Survey (BHPS).

Unfortunately, while this data source has very detailed information on the circumstances of families and children, it is collected on a much smaller scale than the administrative data used in our analysis of hospitalisations and obesity. As discussed in Chapter 4, controlling for time-invariant differences across very small neighbourhoods is important to our methodology. This approach is very data-intensive, since controlling for these low-level fixed effects requires a large sample. The UKHLS sample is not large enough to allow us to apply the same methodology. This means that, for many of the results in this chapter, we lack sufficient statistical 'power' to estimate effects that we are confident are not due to random chance. As a result, the results presented in this chapter should be interpreted with caution as suggestive evidence.

8.1   Methodology and model

When looking at mothers' outcomes, we need to modify our main model to take into account the smaller sample size for this data and the potential for mothers to be exposed to Sure Start through several children at different times. Our sample for these results consists of mothers whose eldest child was born between 1993 and 2014 and who currently reside in a local authority in England. In some specifications, we further restrict the sample based on the child's age at the time of measurement (e.g. to the mothers of children born between 1993 and 2014 who are between 0 and 5 years old at the time of interview). This is to analyse whether the effects differ at different ages of the child.

Our main model regresses the mother's outcomes on the Sure Start coverage experienced by her eldest child, which is defined based on the mother's local authority of residence prior to the birth of her eldest child.[34] Unlike our analysis on children's outcomes (which focuses on children who have aged out of Sure Start eligibility), here we are considering children who are not yet 5 years old. We therefore take a simple average of the Sure Start coverage they have experienced in each month up to the time of interview (or up until their fifth birthday, whichever is sooner).

Of course, mothers could also be affected by any Sure Start coverage experienced by younger children in the household; however, in practice, we find that the coverage for the eldest and the youngest child are typically very similar, and so it is difficult to disentangle the separate effects of coverage for each child.

In our primary specification, we control for fixed effects at the individual level (rather than at the neighbourhood level, as we did when looking at children's outcomes).[35] We made this decision because we observe relatively few neighbourhoods with more than one mother in our data. This means that trying to control for both neighbourhood fixed effects and relevant individual characteristics (such as ethnicity) risks adding instability to the model by estimating the effect of the background characteristics based only on a small group of people. We therefore chose to focus on results that control for all fixed characteristics of individual mothers, both those that we observe in the data and those that are unobservable to researchers.

However, this approach means that we need to look at outcomes for each mother at several points in time, so we cannot estimate separate models for each outcome age as we do in Chapters 5 and 6. Within each regression, there is therefore a risk that we conflate changes due to the child getting older (e.g. mothers returning to work) with higher coverage (since coverage grows over time). To break this link, our model flexibly controls for the age in months (at the time of interview) of both the eldest and the youngest child in each wave.[36] In addition, our model includes indicators for the eldest child's month and year of birth.

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[34] Or, where this is not observed in the data, her local authority of residence at the earliest time she is observed.

[35] We also run the analysis instead using fixed effects at the neighbourhood level as a sensitivity test. These results are available in Appendix G.

[36] Specifically, we include a cubic polynomial in the age in months of both the eldest and the youngest child.

8.2   Outcomes

We measure maternal mental health using the General Health Questionnaire 12 (GHQ-12) scale, a widely used set of questions that screen for general mental ill health (rather than for specific conditions such as depression or bipolar disorder). We look at two mental health outcomes: a yes–no measure of whether the GHQ score is above the clinical threshold for having poor mental health, and a continuous measure of the GHQ score (where a higher score indicates worse mental health).[37]

The results for the yes–no outcome can be interpreted as the change in the probability of having a mental health problem that results from increasing the eldest child's exposure to Sure Start by one centre per thousand children. The continuous measure can be interpreted as the increase in a mother's GHQ score (out of 12 points, with higher scores indicating worse mental health) that results from a similar change in Sure Start coverage.

8.3   Results

Table 8.1 shows the estimates from our preferred specification for both outcomes. Neither result is statistically different from zero, which means that we cannot be confident that these effects are not the result of chance. It is important to underline that this absence of evidence of any impact on maternal mental health does not provide evidence of the absence of an impact. As discussed earlier, it is possible that Sure Start does indeed affect mothers' mental health but that our data set is not big enough for our model to detect these impacts.

Table 8.1. Estimated impact of Sure Start coverage on maternal mental health

	GHQ score	Mental health problem
Sure Start coverage (eldest)	0.630	−0.050
	(0.668)	(0.096)
No. of observations	13,683	13,683
R^2	0.503	0.472
Baseline mean	2.031	0.495

Note: The model controls for individual fixed effects and a cubic polynomial in the age in months of the eldest and the youngest child. 'GHQ score' is a score on the GHQ-12 scale, which is scored between 0 and 12, where higher scores indicate worse mental health. 'Mental health problem' is an indicator for whether the mother's GHQ score exceeds the clinical cut-off for poor mental health. The model is estimated on mothers whose eldest child was born between 1993 and 2014 and is 11 years old or younger at the time of the interview. Standard errors are shown in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Source: Authors' calculations using the pooled BHPS–UKHLS data, 1993 to 2014, and the Department for Education's data on the roll-out of Sure Start.

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[37]   There are several options available for scoring the GHQ. We have used the standard GHQ scoring, which is preferred by the author of the scale (https://www.gl-assessment.co.uk/products/general-health-questionnaire-ghq/). Under this scoring method, the clinical threshold is having a GHQ score of 1 or above.

The suggestive evidence in Table 8.1 paints a mixed picture of the effect that Sure Start had on maternal mental health. In the first column (GHQ score), the coefficient is positive, indicating worse mental health as Sure Start coverage increases. However, the second column suggests that mothers whose eldest child experienced higher Sure Start coverage are around 5 percentage points less likely to have a mental health problem.

Since neither result is statistically significant, the different directions of these effects might simply mean that, by chance, the coefficients on the two effects have different signs. It is also possible that Sure Start is effective in helping mothers who are at risk of having a mental health problem, and using a linear model for GHQ does not properly account for this.

Table 8.2 investigates whether we can detect any impact of Sure Start on maternal mental health in particularly disadvantaged or advantaged areas. We find that the effect of Sure Start is not statistically significant in any of these neighbourhood groups. Further, there are no statistically significant differences between the effects in the poorest neighbourhoods and the effects in less disadvantaged neighbourhoods. However, our estimates in the poorest neighbourhoods indicate that mothers may have worse mental health, while those in richer areas are less likely to have a mental health problem. This might suggest that the types of services offered in different areas – or the types of mothers who choose to access the centres – affect the impact that Sure Start itself has on

Table 8.2. Estimated impact of Sure Start coverage on maternal mental health, by neighbourhood disadvantage

	GHQ score	Mental health problem
Sure Start coverage (poorest)	0.953	0.033
	(1.340)	(0.168)
Sure Start coverage (middle)	1.072	−0.079
	(0.788)	(0.118)
Sure Start coverage (richest)	−0.991	−0.101
	(1.357)	(0.187)
No. of observations	13,652	13,652
R^2	0.503	0.472
Baseline mean	2.031	0.495
p-value: poorest = middle	0.934	0.546
p-value: poorest = richest	0.308	0.577

Note: As for Table 8.1. The Sure Start coverage experienced by the eldest child is interacted with indicators for living in the poorest 30% of LSOAs (based on the 2004 IMD), the richest 30% or the middle of the distribution. The bottom two rows present the p-values from testing whether the effect in the poorest neighbourhoods is equal to that in the middle and the richest neighbourhoods. Standard errors are shown in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Source: Authors' calculations using the pooled BHPS–UKHLS data, 1993 to 2014, and the Department for Education's data on the roll-out of Sure Start.

Table 8.3. Estimated impact of Sure Start coverage on maternal mental health, by age of eldest child

	GHQ score	Mental health problem
SS coverage (eldest)	0.270	−0.143
	(1.014)	(0.172)
× 1 year old	0.287	−0.008
	(0.249)	(0.046)
× 2 years old	0.276	0.020
	(0.309)	(0.051)
× 3 years old	0.016	0.049
	(0.337)	(0.055)
× 4 years old	0.300	0.009
	(0.441)	(0.074)
No. of observations	4,755	4,755
R^2	0.525	0.482

Note: The model controls for individual fixed effects and a cubic polynomial in the age in months of the eldest and the youngest child. 'GHQ score' is a score on the GHQ-12 scale, which is scored between 0 and 12, where higher scores indicate worse mental health. 'Mental health problem' is an indicator for whether the mother's GHQ score exceeds the clinical cut-off for poor mental health. The model is estimated on mothers whose eldest child was born between 1993 and 2014 and is 4 years old or younger at the time of the interview. Standard errors are shown in parentheses. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Source: Authors' calculations using the pooled BHPS–UKHLS data, 1993 to 2014, and the Department for Education's data on the roll-out of Sure Start.

mothers' mental health. However, since these results are not statistically significant, they are not a robust guide for policy.

The lack of statistical significance is not because the effects that we estimate are small. Rather, the estimates are fairly big but even more imprecise. The absence of statistically significant estimates for the impact of Sure Start on maternal mental health is not evidence that there is genuinely no such effect.

It is also possible that the effect that Sure Start has on maternal mental health differs by the age of the child. Table 8.3 looks just at mothers whose eldest child is aged 4 or younger (and so is still eligible for Sure Start).[38]

However, we find no compelling evidence of differences. For a child under 1 year old (the baseline), increasing coverage by one centre per thousand children reduces the probability that the mother has a mental health problem by 14.3 percentage points. When the eldest child is 1 year old, the total impact of Sure Start coverage reduces the probability of a mental health problem by 15.1 percentage points (adding together the baseline coefficient of −0.143 and the specific effect at age 1 of −0.008). However, the baseline coefficient is not itself statistically significant – suggesting that we cannot be

[38]   Results for a similar analysis on families where the eldest child is 5–11 years old are reported in Appendix G.

confident that Sure Start has an effect on maternal mental health in this group. The estimates for different ages are also not statistically significant, so we have no evidence that Sure Start has particularly strong impacts at any one age in this range.

Overall, therefore, our results on maternal mental health are too imprecise to draw firm conclusions. Previous research that considers maternal mental health has also found mixed results. The ECCE study found that mothers who use Children's Centres are more likely to have mental health problems, though among users those who have a consistent pattern of use are less likely to suffer from mental ill health (Sammons et al., 2015). Long-term users of childcare were a particular exception to this, and there is strong statistical evidence that they tended to have worse mental health.

9. Cost–benefit analysis

Key findings

- **The reduction in hospitalisations at ages 5–11 caused by Sure Start saves the NHS around £5 million per cohort.** This is equivalent to 0.4% of average annual spending on Sure Start.

- **Including a broader set of savings, the financial benefits of Sure Start's effect on hospitalisations amount to around 6% of its budget.** We consider the costs of giving access to Sure Start to a cohort of children, and the corresponding benefits of reducing hospitalisations for injuries and infections, in terms of averted direct healthcare costs, averted indirect (parental and societal) costs, and the financial impacts of their consequences over the life course.

- **A full cost–benefit analysis requires more information about Sure Start's impacts on other outcomes.** We only consider the financial impacts that come from the impacts on hospitalisations that we find in this report. As more evidence becomes available on the wider impacts of Sure Start, it will be important to update this cost–benefit analysis to capture a wider set of outcomes.

In Chapters 5 and 6, we have described the benefits that Sure Start has had for children's health, as measured by a fall in the number of hospitalisations. So far, we have focused on these outcomes as an end in themselves: an improvement in health (and a reduction in health inequality) is a benefit to society.

In this chapter, we look at the *financial* benefits of the reduction in hospitalisations and compare these with the cost of Sure Start. We perform a simple cost–benefit calculation, where we compute the averted costs brought about by the reduction in hospitalisations in terms of their direct costs to the NHS, indirect costs to children's parents, and long-term costs associated with the conditions for which these hospitalisations occur. Of course, it is vital to stress that this calculation is incomplete: we take into account the full cost of Sure Start to taxpayers, but weighed against that we consider only the reduction in costs brought about by fewer hospitalisations. If further research reveals that Sure Start has had impacts on other outcomes (e.g. educational attainment or safeguarding needs), these should be incorporated into the cost–benefit analysis as well.

Our simple cost–benefit comparison uses official data on government expenditures on Sure Start as a measure of the programme's cost. We combine these with the estimates of Sure Start's effects from Chapters 5 and 6, and attach a financial 'benefit' to the fall in hospitalisations based on both NHS reference cost data and the best-available results from published literature. We compute these averted costs based on the number of hospitalisations avoided as a result of providing an additional Sure Start centre per thousand children.

9.1   Previous cost–benefit analyses

We are not the first to try to quantify the monetary benefits of Sure Start. Meadows et al. (2011) calculated that the SSLPs cost around £1,300 per eligible child per year at 2009–10 prices (or £4,860 per eligible child over the period from birth to age 4).[39,40] They found that, by the time children had reached the age of 5, SSLPs had already delivered benefits to the public purse valued between £279 and £557 per eligible child (coming from a reduction in workless households), which is 6–12% of the total cost of the programme. The authors concluded that this is a large impact, given the early stage at which it is measured, but that there is insufficient information to reliably predict longer-term economic impacts.

Gaheer and Paull (2016) collected very detailed cost data on different types of services delivered in 24 of the SSCCs that participated in the ECCE: baby health, child play, parent support, specialist child support, specialist family/parent services, childcare, finance and work support, and training and education. The average hourly cost per user (the value of resources used to deliver one hour of a service to a child or parent) ranged from £6 for childcare to £55 for finance and work support (in 2014 prices). The mean cost per family using the service (which includes both the hourly cost and the hours of usage) ranges from £958 for parent support to £8,454 for childcare.[41] The authors then combined estimates on the associations between the use of different types of Children's Centre services and improved family outcomes (from the ECCE impact study) with existing evidence from the literature on long-term effects. They found that some services (such as general and specialist parent support) provide benefits to the taxpayer that exceed the cost of delivery.

9.2   Our cost calculation

We face a number of challenges in computing a cost of Sure Start that corresponds to the measure of Sure Start exposure that we use in our impact analysis. First, unlike the NESS and ECCE evaluations, we did not collect detailed cost data ourselves. Second, since we evaluate the effects of Sure Start between 1999 and 2010, our measure of cost must be valid for both SSLPs and SSCCs. Finally, our measure of cost needs to be consistent with the methods we use in our impact evaluation, which studies the effects of access to, rather than usage of, Sure Start.

For these reasons, we compute the cost of Sure Start per *eligible* child, by dividing overall government spending on Sure Start by the number of eligible children (i.e. the number of children aged 0–4 in the local authorities that had at least one Sure Start centre during that year), averaged over the years 2000–15. This is consistent with the aim of the government (especially at programme maturity) to provide access to Sure Start for every child, and with Sure Start's design as an area-based, rather than means-tested, programme. The cost per child computed in this way, plotted in Figure 9.1, follows the same overall pattern as the government expenditure, and ranges from a minimum £267 in 2014–15 to a maximum £550 in 2009–10.

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[39]   These figures are £1,510 and £5,645 in 2018–19 prices, respectively.

[40]   The authors report substantial variation, with prices ranging from around £450 to £2,500 per eligible child.

[41]   These figures are £1,021 and £9,011 in 2018 prices, respectively.

Figure 9.1. Government expenditure on Sure Start and cost per eligible child
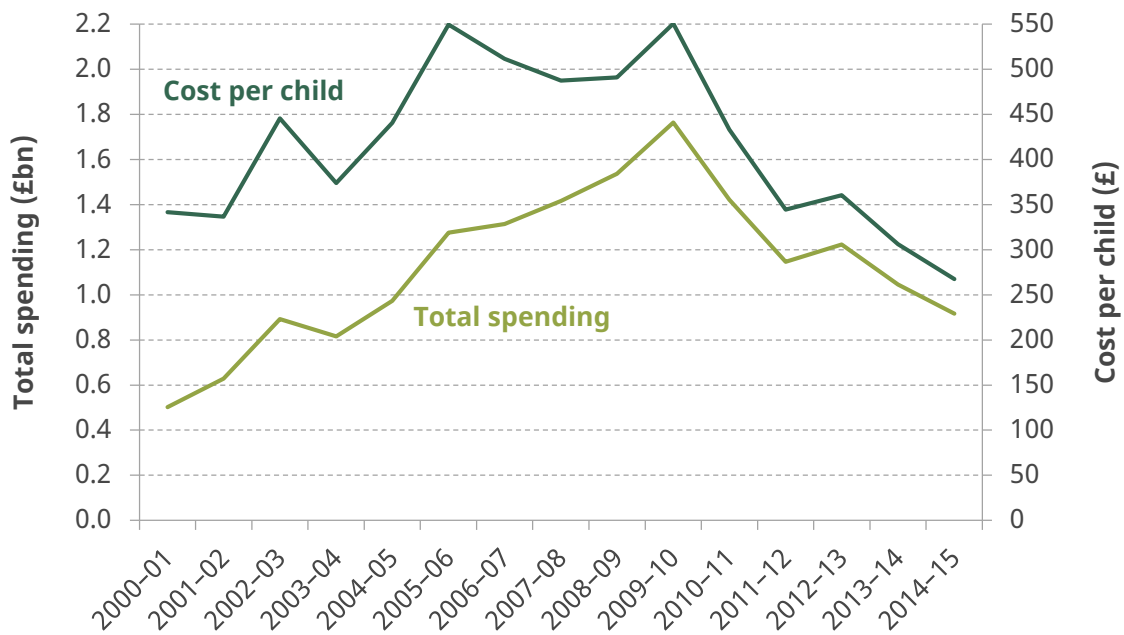
Note: The series of government expenditure on Sure Start is in 2018–19 prices (updated from Belfield, Farquharson and Sibieta (2018) based on the March 2019 GDP deflators). The cost per eligible child is computed as the ratio of spending to the number of eligible children, i.e. children aged 0–4 in the local authorities in which there was at least one centre open in that year.

Source: Belfield, Farquharson and Sibieta (2018) for expenditure data; authors' calculations.

For our cost–benefit analysis, we use the average cost per child over the period covered by our impact analysis (2000–14), which amounts to £416. Based on the average cohort size over this period, the cost of giving access to Sure Start to a representative cohort over the entire young childhood (ages 0–4) is just above £1 billion.

9.3   Valuing Sure Start's benefits

Weighed against Sure Start's cost to taxpayers, we can consider the financial benefits of the hospitalisations that Sure Start averted. In doing this calculation, we only want to consider impacts that are statistically significant (i.e. that we can be confident are not just due to chance). Based on the impact analysis in Chapters 5–8, we can be confident in Sure Start's effects on hospitalisations but not on obesity or maternal mental health.

Our impact analysis considered hospitalisations at ages 5–11, which we extrapolate to ages 12–18 in this cost–benefit analysis. We also want to capture – as well as we can – any potential longer-run benefits. For example, averting a head injury means that the NHS does not need to pay to treat the injury when it happens. But since head injuries also tend to have longer-run consequences (such as a need for follow-up care, or lower attainment in school), preventing the head injury will also generate savings to the public purse in the longer term. In this report, we consider several different benefits related to the reductions in hospitalisations for injuries and infections reported in Chapter 6:

- **Averted healthcare costs**, both in-sample (i.e. at ages 5–11) and extending (where possible) out-of-sample for the ages 12–18.[42] We use specific resource use costs for paediatric fractures, head injuries (separately for intracranial and non-intracranial) and infections.

- **Averted indirect costs**, over the same ages as the healthcare costs, such as costs to the family and the society (e.g. lost income and value of work time lost).

- **Averted long-term costs**, for those cases that would incur sustained costs over the life cycle (such as those deriving from traumatic brain injury or attributable to child maltreatment).

We list the sources of the model inputs in Appendix Table H.1.

9.4   Comparing costs and benefits

The main results of our cost–benefit calculation are reported in Table 9.1, with more detail given in Appendix Table H.2. All costs are in 2018–19 prices, and discounted using a 3.5% discount rate as recommended by the National Institute for Health and Care Excellence (NICE).

The total financial benefit from averted costs, obtained by adding together the direct healthcare costs, indirect costs throughout childhood and long-term costs, amounts to around £65 million. Of this, around £5 million is attributed to direct cost savings to the NHS from fewer hospitalisations at ages 5–11. As expected, the bulk of the total averted cost is attributable to the lifetime costs of traumatic brain injury.

Set against this is the estimated cost of providing an additional Sure Start centre per thousand children to a representative cohort, which we calculate at £1,055 million. On this basis, then, we find that the financial benefits from reducing hospitalisations offset

Table 9.1. Estimated costs and benefits of Sure Start for one cohort of children (2018–19 prices)

Total costs	£1,055 million
Total averted costs	**£65 million**
of which:	
Direct healthcare costs (14%)	£9 million
Indirect costs (3%)	£2 million
Lifetime costs (83%)	£54 million

Source: Authors' calculations using the estimates in Chapter 6 and various sources for the costs data; see Appendix H for details.

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[42] We used the number of averted hospitalisations at age 11 to extrapolate for the ages 12–18. We did this only for fractures (which were significant at later ages) and head injuries (where the effect sizes at later ages were imprecise but big, of similar magnitude to the statistically significant findings at age 5). We assumed constant effects, rather than linearly extrapolating. Given that the treatment effects appear to be increasing over time, this is a conservative assumption.

approximately 6% of the cost of Sure Start provision (with direct savings from the reduction in hospitalisations at ages 5–11 coming to 0.4% of spending on Sure Start). This is comparable to the findings of Meadows et al. (2011) for the NESS evaluation.

In our cost–benefit analysis, we have accounted for both the immediate and the longer-term financial benefits associated with Sure Start's effects on hospitalisations. However, it is important to stress that this simple analysis nevertheless presents some key limitations. First, a focus on hospitalisations means that we only observe the most severe cases; in the absence of data from GP records, we are unable to assess whether Sure Start had effects on the use of other health services. These effects could go in either direction: for example, an overall improvement in health might reduce the need for GP appointments, but a shift from hospitalisation to primary-care treatment could actually increase demands on GPs.

A second important limitation is that we are unable to definitively determine the mechanisms driving the reduction in hospitalisations. This matters because some mechanisms – for example, child maltreatment or changes in children's mental health – are likely to have financial implications over and above their impact on hospitalisations. We have been able to partially account for these using results from the child maltreatment literature. However, to the extent to which other mechanisms are at play, we are likely underestimating the benefits of Sure Start. For example, if the reductions in injuries are driven by improvements in child mental health (e.g. reductions in externalising behaviour leading to fewer broken bones), then we are not accounting for potential benefits that this would also have for outcomes such as education, earnings and crime.

Linked to this, a third limitation is that we are considering the financial impacts of only a narrow set of potential outcomes and setting them against the total cost of Sure Start. Sure Start may have affected the cognitive and behavioural development of children, in addition to their health. Impacts of this kind could lead to better educational outcomes, which may in turn generate substantial social benefits – for example, through reductions in special educational needs, increased tax revenues and reduced benefit payments. The programme might also have positively affected parents – for example, by moving more mothers into work, which would in turn affect tax revenues. Similarly, if Sure Start affected children or parents in a way that increased costs to the public purse – for example, increasing take-up of specialist services – this should also be taken into account.

As more evidence becomes available on the wider impacts of Sure Start, it will be important to update this cost–benefit analysis to capture a wider set of outcomes.

10. Conclusion

It is no exaggeration to say that Sure Start has had a tumultuous history over its first 20 years. Its rapid roll-out and the subsequent cuts to funding have both helped to reshape the UK early years policy landscape.

It is remarkable that such big changes – both expansions and cuts – have happened with such a limited evidence base. The National Evaluation of Sure Start provided an important initial evaluation of the earliest Sure Start Local Programmes, but the decision to universalise Sure Start was taken before its first impact report was published. The later Evaluation of Children's Centres in England provides vital information about the characteristics of centres and their users, while Smith et al. (2018) have documented the extent of the hollowing out of the programme over the past decade. But there has been very little research that directly investigates the causal impact that Sure Start has had on children and their families.

This report provides one piece of the evidence base for Sure Start, examining its impacts on children's hospitalisations and obesity as well as on the mental health of their mothers. The fall in hospitalisations that Sure Start has caused is substantial; our calculations suggest that providing a centre per thousand children under 5 would mean around 5,500 fewer 11-year-olds are hospitalised each year.

But these results also provide important guidance for policymakers at a time when resources for early intervention are not unlimited. We find no evidence that Sure Start has improved children's hospitalisation outcomes in the richest neighbourhoods; all of the benefits are for children living in poorer areas.

By contrast, decisions around centre closures have by and large been made by local authorities seeking to allocate increasingly limited resources. Some areas have chosen to keep most or all of their centres open, either limiting the services on offer or finding savings elsewhere. Other authorities have closed most of their Children's Centres. Our research suggests that a one-size-fits-all policy within an LA is unlikely to deliver the best value for money – and even less so a patchwork of policies across different LAs.

The benefits that we find – fewer hospitalisations, particularly for injuries and at older ages – translate into some savings for the public purse, though not enough to outweigh the costs of the Sure Start programme. However, it is also important to remember that we consider here just three outcomes: hospitalisations, obesity and maternal mental health. Given the varied nature of Sure Start services and the well-established links between health, academic performance and behaviour, it is certainly possible that future research will reveal other effects of Sure Start that should also be included in these calculations.

Of course, fewer hospitalisations also have a benefit to children and their families, beyond that to the public purse. It is up to policymakers to decide how they weight these benefits against the costs of Sure Start or other early intervention programmes. But since Sure Start has been an important part of the early intervention strategies of successive governments, it is important to understand that it does have benefits for children's health. More broadly, all interested parties – politicians, policymakers and practitioners – should continue to assess how the resources targeted at early intervention can best be used to improve outcomes for children and their families, and how they can best encourage the families who would benefit the most to use the centres.

Appendix A. Official guidance for the roll-out

Sure Start's focus on targeting disadvantaged children translated into a strong focus on area-level measures of deprivation as a determinant of location choices for both SSLPs and SSCCs. However, official guidance also encouraged policymakers to consider other factors. For example, the original 60 SSLP 'trailblazer' districts were selected by central government on the basis of five dimensions (Department for Education and Employment, 1999):

- 1998 Index of Local Deprivation, augmented with low birthweight and teen pregnancy indicators;
- DfEE knowledge of existing projects delivering aspects of the Sure Start strategy, and local projects tackling other social problems;
- geographical spread across English regions;
- spread across different types of areas (inner city, out-of-town, rural, coalfield, coastal, pockets of deprivation);
- links with existing government programmes (a mix of programmes in some areas; some areas with no other initiatives chosen to help evaluate the effect of Sure Start alone).

Following the trailblazer districts in Round 1, there were five more rounds of opening of SSLPs.[43] Information on the criteria for eligibility in subsequent rounds of expansion is scarcer. One notable change was the switch from the 1998 Index of Local Deprivation (measured at local authority district level) used in Rounds 1–3 to the 2000 Index of Multiple Deprivation (measured at ward level) used in Rounds 4–6 of the Local Programme expansion (Ball, 2002). Guidance on the size of programmes also changed over time: SSLPs opened in the first three rounds were expected to select catchment areas encompassing 400 to 1,000 children, while subsequent rounds recommended between 400 and 800 children in the catchment area.

Policymakers were given less guidance on how to run the roll-out of the Sure Start Children's Centres. The pledge to universalise the programme meant that the question was no longer which children would be served, but rather how quickly they would get access to it. In addition, the very tight timeline for programme expansion, heterogeneity in the availability of pre-existing services on which to build (e.g. SSLPs, Jobcentre Plus facilities), and greater local authority control meant that the roll-out of SSCCs was more strongly driven by feasibility than by scores on a standardised set of indicators. Nevertheless, the roll-out of the SSCCs was intended to be driven by deprivation. The expansion of the Children's Centres occurred in three phases, each with different targets (House of Commons Children, Schools and Families Committee, 2010):

- **Phase 1 (2004 to 2006).** There were approximately 800 centres designated in Phase 1, offering full coverage of the 20% most disadvantaged wards.[44] There were individual

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[43] Although there do not appear to be official guidelines on when these rounds should have taken place, the data sets we use for this project suggest that dates were: Round 1 (1999–2000), Round 2 (2000–01), Round 3 (2001), Round 4 (2001–02), Round 5 (2002–03) and Round 6 (2003).

[44] Following the release of the 2004 deprivation indices at lower-layer super output area (LSOA) level, this target was redefined as the 30% most deprived LSOAs (National Audit Office, 2006).

target numbers for children reached for each local authority, and a national target of 650,000 children. According to the Select Committee, 'the majority of [Phase 1 Children's Centres were] already Sure Start Local Programmes or Early Excellence Centres'.[45]

- **Phase 2 (2006 to 2008).** In Phase 2, the overall number of centres was set to reach 2,500, with each centre serving a population of on average 800 children. These would fully cover the 30% most disadvantaged areas, with some centres outside these areas. By Phase 2, all SSLPs (as well as other earlier types of programmes such as Early Excellence Centres and Neighbourhood Nurseries) had transitioned into SSCCs, but most of the Phase 2 centres were new builds.

- **Phase 3 (2008 to 2010).** The final phase of the SSCC roll-out saw the universalisation of the programme, with full coverage of the remaining 70% of areas. The overall number of centres was planned to reach 3,500, with each centre serving a local population of between 600 and 1,200 children depending on the location and level of need.

Despite the differences between them, the SSLPs and the SSCCs shared a focus on improving outcomes, particularly for disadvantaged children. In both Phase 1 and Phase 2 of the programme, the official guidelines for the roll-out included a strong focus on deprivation indicators as a determinant of the timing and intensity of the roll-out.

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[45] Early Excellence Centres (EECs) were introduced in December 1997 and funded through March 2006. They were intended to integrate early education and childcare for children under 5 and to develop models of best practice in existing provision. Two-thirds of pilot EECs were located in wards in the bottom 20% of the distribution of the Index of Local Deprivation (House of Commons Children, Schools and Families Committee, 2010).

Appendix B. Sources of data used in the roll-out analysis

This appendix provides further detail on the sources, years of measurement and geographic levels of the local characteristics used in our quantitative analysis of the roll-out of Sure Start in Chapter 2 as well as in the robustness checks in Appendix D.

As Table B.1 makes clear, for most characteristics we have data covering the entire period between 1999 and 2010 (the period covered by our roll-out analysis in Chapter 2). The two major exceptions to this are the share of primary school pupils with English as an additional language (where data are not available between 2000 and 2003) and the employment rate (where data from the Labour Force Survey at the local authority district (LAD) level are not available after 2004). In both of these cases, we have imputed the data from these missing years with a constant and included a 'missing' dummy to avoid dropping these observations.

In addition, many of the data series have casewise missingness, where data are unavailable for some area–year combinations (but not more generally for the entire year or for the same area in every year). We use linear interpolation to reduce missingness in these data by imputing the missing data as an average of the non-missing observations in the same area in the year before and after. We apply this procedure in cases where up to five years of data are missing. Within the 323 local authority districts that we consider in the main impact analysis (dropping the City of London, Isles of Scilly and West Somerset, which were all strong outliers in Sure Start coverage), no casewise missing data remain after this procedure.

Table B.1. Information about data on local characteristics

Category	Variable	Source	Years	Geography
Deprivation	Percentile of rank distribution of Index of Local Deprivation	Department of the Environment, Transport and the Regions[a]	1998	LAD
Health indicators	Under-18 conception rate (conceptions per 1,000 women aged 15–17)	Child and Maternal Health Intelligence Network[b]	1998–2014	LAD
	Proportion of births below 2.5kg	ONS Vital Statistics[c]	1991–2014 (interpolated in 2008 and 2009)	LAD
Potential demand for services	Total period fertility rate	ONS Vital Statistics[c]	1990–2014	LAD
	Density	ONS population density[d]	1990–2014	LAD
	% of primary school pupils with English as an additional language	National Association for Language Development in the Curriculum (NALDIC)	1999; 2004–13	County

Category	Variable	Source	Years	Geography
	Children looked after per 1,000: under 1	Department for Education	1992–2014	County
	Children looked after per 1,000: 1–4	Department for Education	1992–2014	County
Labour market	Total employment rate	Labour Force Survey[e]	1995–2004	LAD
	Rate of jobseeker's allowance receipt	Jobseeker's allowance[f]	1992–2014	LAD
	Median weekly pay, full-time women/men[g]	Annual Survey of Hours and Earnings, workplace series[h]	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 GP practices per 1,000 population	Constructed with HSCIC data[i]	1990–2014	LAD
	Number of Jobcentre Plus per 1,000 population	Department for Work and Pensions	2001–14	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

[a] Downloaded 20 November 2015, http://www.legco.gov.hk/yr99-00/english/bc/bc09/papers/1471e01.pdf.

[b] Downloaded 2 November 2015, http://atlas.chimat.org.uk/IAS/dataviews/view?viewId=96.

[c] Obtained 24 November 2015 from the ONS Vital Statistics Outputs Branch, with help from Laura Todd.

[d] Downloaded 18 January 2016 from ONS.

[e] Downloaded 2 December 2015 from NOMIS.

[f] Downloaded 16 December 2015 from NOMIS.

[g] Deflated to constant 2015 pounds using the Consumer Prices Index, downloaded from ONS Consumer Price Indices, 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.

[h] Downloaded 15 December 2015 from NOMIS.

[i] Health and Social Care Information System, 'GPs, GP practices, nurses, and pharmacies', downloaded 26 November 2015.

Appendix C. Hospitalisation falsification tests

In Chapters 5 and 6, we present results showing that Sure Start has a strong, statistically significant and economically meaningful effect on reducing hospitalisations, especially for injuries. That analysis uses the methodology described in Chapter 4 to estimate the causal relationship between Sure Start and hospitalisations. In order to test whether our methodology is appropriate – and the effects it estimates can genuinely be considered causal impacts of Sure Start – we can conduct placebo and falsification tests. These tests are not designed to tell us anything about Sure Start itself; rather, they provide information about the quality of the model and the methodology that we use. Failing these tests would cast doubt on the appropriateness of our model and therefore the validity of our results in Chapters 5 and 6. Passing the falsification tests increases our confidence in our main results because it suggests that our model has overcome some potential pitfalls. But it is important to remember that these tests *cannot* prove that our model (or any model) is definitively 'right'.

The first falsification test we do involves changing the *outcome* that we consider. Our main results look at both overall hospitalisations and hospitalisations for causes that might plausibly be influenced by Sure Start. In our falsification test, we change the outcome to consider hospitalisations due to congenital chromosomal defects. Because we do not want to capture repeated hospitalisations later in life – which could potentially be influenced by the quality of primary care in the community – we restrict our outcome to hospitalisations in the first 2 to 12 months of life. However, we look at two sets of cohorts, 'maximum cohorts' (which include children born between 1996 and 2009, an endpoint analogous to our age 5 results) and 'minimum cohorts' (running between 1996 and 2003, an endpoint analogous to our age 11 results). We choose congenital chromosomal defects because these conditions are genetic: there is no plausible mechanism through which Sure Start could influence hospitalisations for these reasons. We therefore expect our estimates to show no impact of Sure Start on admissions for these conditions. If there is an impact, it suggests that our methodology is flawed, and our main estimates are picking up something other than the causal effect of Sure Start coverage.

Table C.1 shows the results from this falsification test. Not only are the results not statistically significant; the coefficients themselves are zero out to at least four decimal places, meaning we find precisely estimated zero impacts.

A second type of falsification test manipulates our definition of Sure Start *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 actual impacts of Sure Start. This is analogous to a placebo test in medicine: we are checking whether a set of results that uses a false treatment variable (in the medical context, a sugar pill) will still find benefits. If we do find benefits, it suggests there might be selection into areas that do and do not open Sure Start centres and when they do that is related to hospitalisations and that we are not controlling for well in our model. This would mean the effects of Sure Start that we discuss above would be biased.

For this placebo test, we take the roll-out of Sure Start experienced by all 323 local authorities and shuffle it randomly across LAs. In this way, we preserve the overall shape

of the roll-out 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, so that within each LA we pretend that Sure Start was rolled out differently from what actually happened. Since our methodology relies on comparing LAs that opened Sure Start centres earlier or later, shuffling the roll-out profiles across LAs essentially means that we will be looking for effects at the wrong time. Because of this, we should expect to see that the

Table C.1. Falsification test: Sure Start's effect on hospitalisations for congenital chromosomal defects between ages 2 and 12 months

	Maximum cohorts	Minimum cohorts
Sure Start coverage	0.0000	0.0000
	(0.0000)	(0.0000)
No. of observations	10,632,384	5,906,880
First cohort	1996	1996
Last cohort	2009	2003

Note: As for Table 5.1. Rather than all-cause admissions, the outcome for this model is hospitalisations for congenital chromosomal defects (which are not plausibly affected by Sure Start).

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

Figure C.1. Distribution of placebo coefficients at each outcome age
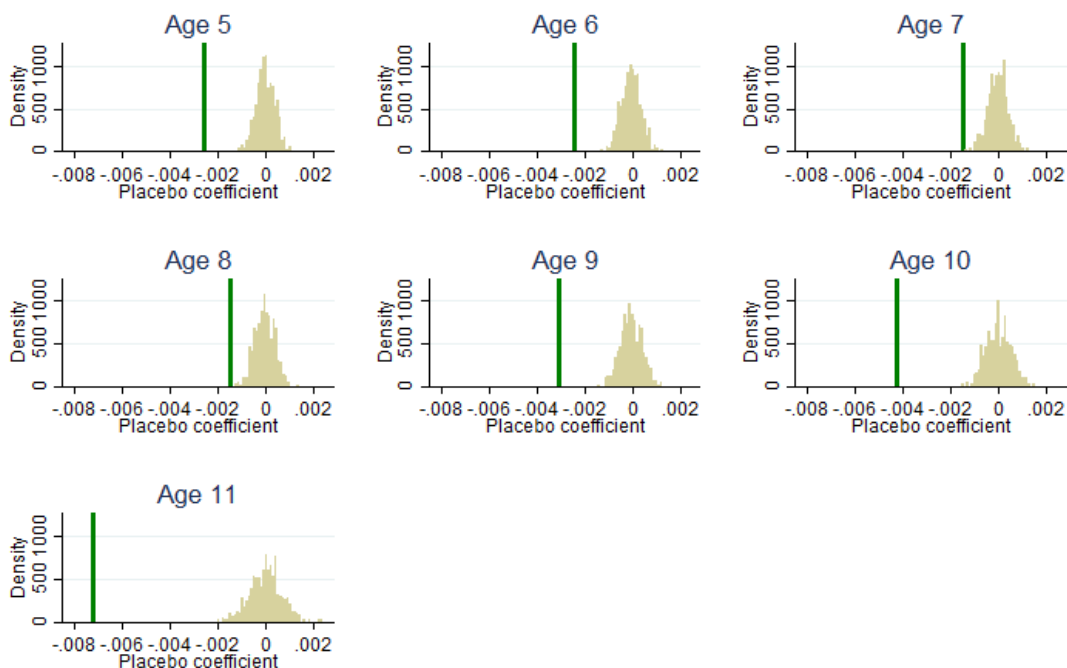
Note: Graphs show histograms of the distribution of placebo coefficients (in beige), with a bin width of 0.0001. The placebo roll-out is simulated 500 times by randomly reshuffling the roll-out profiles of the 323 local authorities. The true estimate is shown in green at each age.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

estimates are on average close to zero and not statistically significant.[46] We repeat this exercise 500 times, randomly reshuffling the roll-out profiles each time. This allows us to build up a distribution of placebo coefficients. Figure C.1 plots these placebo coefficients (in beige) for each outcome age, along with the true coefficient in green. The distribution of placebo coefficients is centred close to zero at each age and it is clear that our actual estimates are quite far outside the distributions of placebo estimates. These results indicate that our model is picking up the true differences in hospitalisation outcomes caused by the roll-out of Sure Start, increasing our confidence in our results.

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[46] In the cleanest version of a placebo test, the placebo estimates should be exactly zero on average. But in our approach, although different local authorities get a (random) different profile of treatment, the profiles are all somewhat related to one another – they all have coverage growing over time. This means that the random placebo profile of any one LA will still be somewhat informative about the direction of the actual Sure Start roll-out in that area, and so there might still be some information picked up in the placebo estimates.

Appendix D. Hospitalisation robustness tests

As discussed in Chapter 4, our main model of Sure Start's impact on hospitalisations identifies the causal impact of Sure Start under several conditions. Two of the most important are the following:

- There are no time-varying characteristics that influence both the roll-out of Sure Start and the trend in hospitalisations in different local authorities.

- While local authorities might have different levels of hospitalisations, the growth rate (trend) is the same in different local authorities.

In this appendix, we present results from models that relax these assumptions. Encouragingly, these results are substantially the same as in our main model; this suggests that these two assumptions are reasonable and that they are not driving the results in our main model.

## Controlling for time-varying characteristics

Our first robustness check relaxes the first assumption above, that there are no time-varying characteristics that influence both the roll-out of Sure Start and the trend in hospitalisations. It is important to note that characteristics that *do not* change over time – both those that we observe in the data and other unobserved characteristics – are already accounted for in our main model (by the inclusion of LSOA fixed effects). So our concern here is with characteristics that change from year to year. For example, if one local authority had a spike in low birthweight, this might increase both the number of hospitalisations (since children are on average less healthy) and the number of Sure Start centres (since local authorities might open more centres in response). In this case, our main model would incorrectly attribute part of the rise in hospitalisations to the increase in Sure Start coverage, when in reality both were driven by a third factor.

It is not possible to test every possible characteristic that could vary over time; many of these are not observable to researchers – for example, the personal beliefs of the council leadership are typically not recorded in any data set. However, we can use economic theory to identify a wide range of potentially important characteristics that are the most likely to pose a problem to our model. These are the characteristics that formed part of the official guidance for the roll-out of Sure Start (e.g. deprivation) as well as other local area characteristics that were significant in the roll-out analysis. The characteristics that we examine include:

- labour market characteristics (e.g. employment and unemployment rates, wages);
- vital statistics (e.g. fertility, low birthweight, teenage pregnancy);
- existing service provision (e.g. GPs, funded childcare);
- other markers of need (e.g. rates of children looked after and of children with English as an additional language);
- political alignment of the local council.

Appendix B lists these variables, their sources, and the years and geographic level at which they are available.

Figure D.1. Effect of an increase in Sure Start coverage on probability of any hospitalisation in the neighbourhood, rescaled by baseline probability: main results and robustness to inclusion of time-varying characteristics
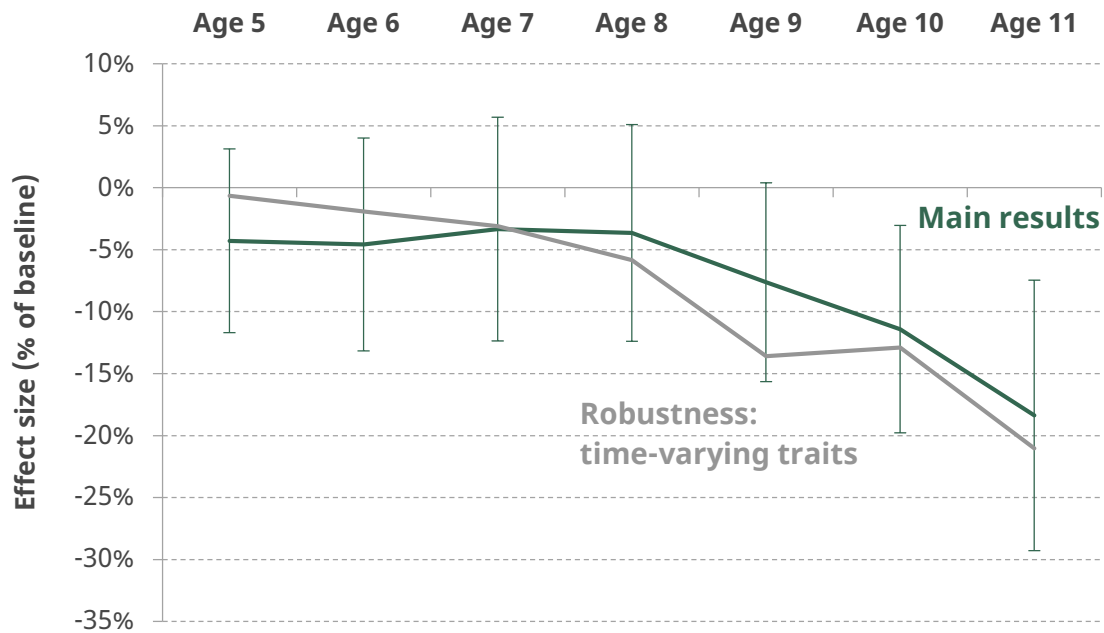
Note: Effect sizes for main results are constructed by rescaling the estimates in Table 5.1 by the pre-Sure-Start baseline probability of a hospitalisation at each age. Vertical bars indicate 95% confidence intervals for the main results; where they do not cross the horizontal axis, Sure Start has a statistically significant impact on the probability of hospitalisation. Results for the robustness check come from a model that adds controls for the time-varying characteristics (measured in the year before birth) summarised in Appendix B.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start. Data sources for the time-varying characteristics used in the robustness check are outlined in Appendix B.

In Figure D.1, we reproduce Figure 5.2 (in dark green) but add another line which is from a model that also controls for these time-varying characteristics (in grey). The results of these two models are very similar; at each age, the estimates from the robustness check model are within the 95% confidence interval of the main results. The differences in the effect size are biggest at age 9, but even here the difference is just 6 percentage points. In addition, the results of the robustness check are statistically significant at the same ages as the main results. This analysis suggests that the assumption in our main model that there are no time-varying characteristics that affect both the Sure Start roll-out and hospitalisations is valid, at least for the rich set of characteristics that we are able to check.

Checking common trends across local authorities

The other important assumption underlying our main model is that different local authorities are on a 'common trend' of hospitalisations. That is, LAs might have different rates of hospitalisations, but changes over time are similar.

We can test how sensitive our results are to this assumption by including an LA-specific linear time trend in our model. This trend allows each local authority to have a different (linear) trend in hospitalisations; for example, if hospitalisations in Camden grew an extra

3% each year compared with hospitalisations in Islington, this model would correct for these differences.

When incorporating these trends in the model, we do not want to strip out any difference in trends that is due to the effects of Sure Start itself. We therefore estimate each LA's own linear trend based on data from before its first Sure Start centres open, and extrapolate this through the Sure Start period.

Figure D.2 replicates our main results (from Figure 5.2) along with the results from this robustness check. The results from the model that includes the LA-specific time trends (in grey) are almost identical to our main results. Once again, the results from the robustness check are within the confidence intervals of the main results in each case, and the two models produce statistically significant results at the same outcome ages. This means that we can be confident that our main results are not driven by different linear trends in hospitalisations in each local authority.

Figure D.2. Effect of an increase in Sure Start coverage on probability of any hospitalisation in the neighbourhood, rescaled by baseline probability: main results and robustness to inclusion of LA-specific linear time trends
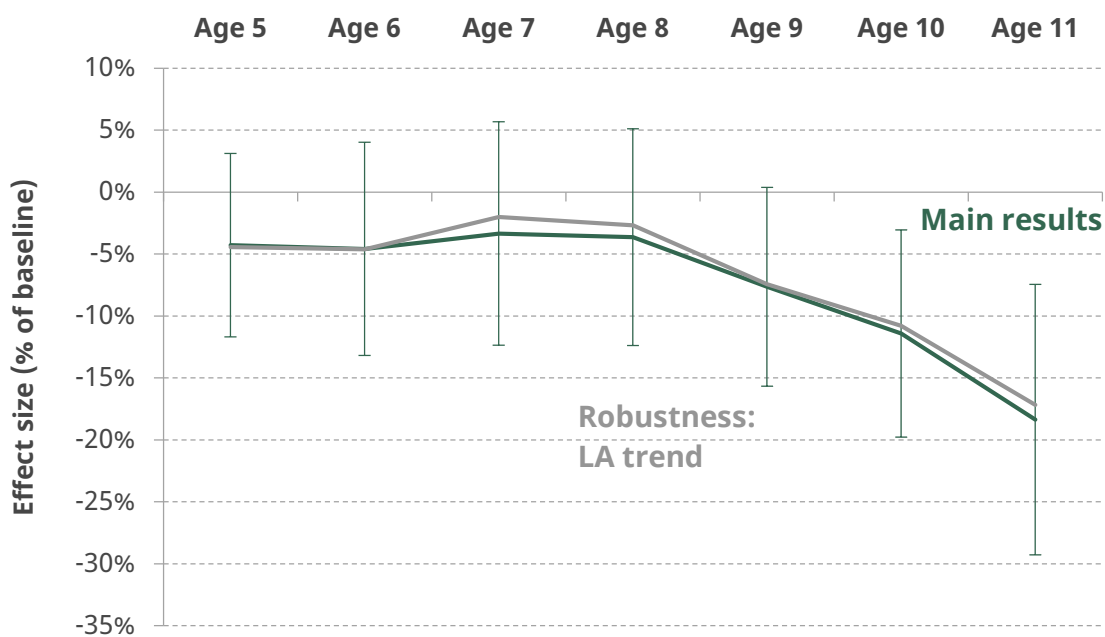
Note: Effect sizes for main results are constructed by rescaling the estimates in Table 5.1 by the pre-Sure-Start baseline probability of a hospitalisation at each age. Vertical bars indicate 95% confidence intervals for the main results; where they do not cross the horizontal axis, Sure Start has a statistically significant impact on the probability of hospitalisation. Results for the robustness check come from a model including projected LA-specific linear time trends, estimated on pre-treatment data.
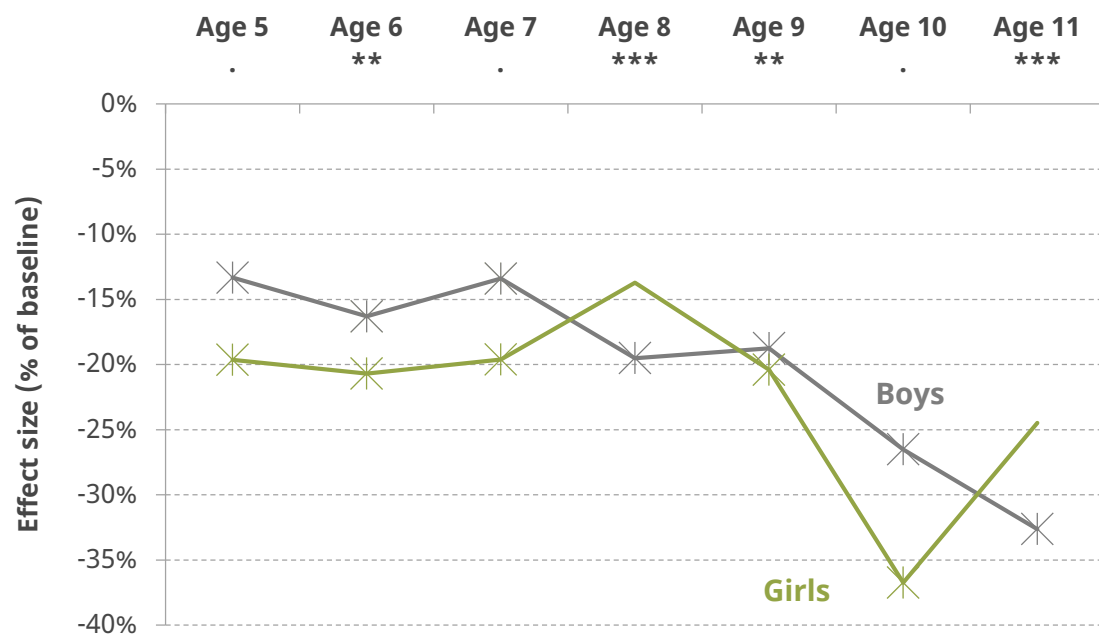
Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

Appendix E. Heterogeneity of injury-related hospitalisations

As we showed in Section 5.2, the overall impact of Sure Start on hospitalisations is not the same for all groups. Analogous to that analysis, Figures E.1 and E.2 show how impacts differ by gender and deprivation using injury-related admissions as an outcome. As with the all-cause results, Figure E.1 shows that there is no clear pattern for whether boys or girls experience bigger reductions in injury-related hospitalisations, nor are the effects consistently different in a statistical sense.

Figure E.2 illustrates how the impact of Sure Start on injury-related hospitalisations differs by the neighbourhood's level of disadvantage. Among children in disadvantaged neighbourhoods, the probability of an injury-related hospitalisation in a neighbourhood–sex–birth-month–birth-year cell falls by between 14% and 31% depending on the age, with the biggest effects at ages 10 and 11. The impacts for children in the middle and richest neighbourhoods look more similar, with the effects for the richest in particular lacking a clear pattern across ages.

Figure E.1. Sure Start's effect on probability of hospitalisation for injuries, rescaled by baseline probability: differences by gender

Note: As for Figure 5.4.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.
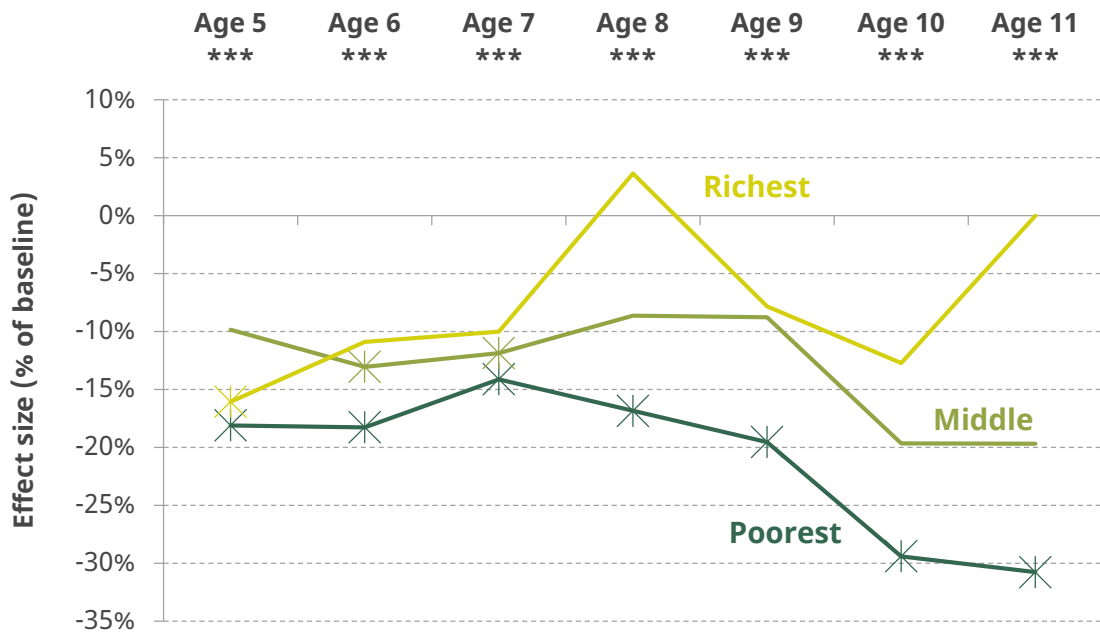
Figure E.2. Sure Start's effect on probability of hospitalisation for injuries, rescaled by baseline probability: differences by disadvantage

Note: As for Figure 5.5.

Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997–2014) and the Department for Education's data on the roll-out of Sure Start.

Appendix F. Child behaviour and injuries

In Chapter 6, we show that one of the biggest drivers of Sure Start's overall effect on hospitalisations is the reduction in 'external' admissions, specifically admissions for injuries. These effects grow bigger at older ages, suggesting that the mechanism underlying them is more likely to be a long-lasting change. One plausible mechanism is a change in child behaviour, and particularly 'externalising' behaviours such as conduct disorders or hyperactivity.

In this appendix, we present evidence from the Millennium Cohort Study (MCS) showing that child behaviour is indeed strongly related to hospitalisations and to injuries. This evidence is only about the association between behaviour and injuries; we cannot say from these data that the behaviour causes more hospitalisations or injuries.

We use data from three waves of the MCS: wave 3 (age 5), wave 4 (age 7) and wave 5 (age 11). These correspond to the ages that we study in the hospitalisation impact analysis in Chapters 5 and 6. However, the MCS data are taken from a single cohort born in 2000–01 and so will not necessarily be representative of all the cohorts in our impact analysis.

At each wave, parents report hospitalisations (for any cause) and injuries sustained by their child since the last wave (so 'age 5' results consider hospitalisations between ages 4 and 5, 'age 7' results for ages 6 and 7, and 'age 11' results for ages 8–11). We use the number of hospitalisations or injuries reported as our outcome.

We have two measures of child behaviour, both reported by the mother using the widely used Strengths and Difficulties Questionnaire (SDQ). The 'externalising behaviour' score comes from the SDQ subscales on hyperactivity and conduct disorders. The 'internalising behaviour' score comes from the SDQ subscales for emotional problems and peer problems. Both indices are scored out of 20, with a higher score indicating more problems in that domain of behaviour.

Since behavioural difficulties are also linked to many other characteristics that are in turn linked to hospitalisations – for example, gender or socio-economic status – we run a multivariate regression that controls for a range of characteristics about the child and family:

- child's sex (an indicator for whether the child is female);
- child's ethnicity (indicators for mixed, Indian, Bangladeshi or Pakistani, black or black British, and other ethnicity; the omitted category is white);
- mother's age (in years) when the child was born;
- mother's education (indicators for education to A level, to GCSE level or below, and unknown education; the omitted category is degree-level education);
- mother's work status (indicators for working part-time (5–25 hours per week) or full-time (25+ hours per week) and for unknown work status; the omitted category is working fewer than five hours per week);
- household net earnings (this includes the net earnings of all parents in the household from employment, self-employment and other sources; it is reported in thousands of pounds in the prices of the relevant year);

- the Home Learning Environment, which is a standardised index constructed using factor analysis on a series of parental time inputs (e.g. how often the parents read to the child, visit the library, play games with the child, etc.); this index is standardised to have a mean of 0 and a standard deviation of 1;
- all regressions also control for fixed effects for the child's age in months at the time of interview, their region of residence at the time of interview, and the season of the interview.

This means that our results give the association between behaviour and hospitalisations or injuries over and above these characteristics.

Table F.1 presents the results of our regressions. When considering hospitalisations in general, internalising behaviour is a more statistically significant predictor than externalising behaviour, which is significant only at age 5. However, when looking at injuries specifically, externalising behaviour is a better predictor than internalising behaviour. Its significance also increases as children get older: at age 5, a one-unit increase in the externalising behaviour index is associated with an increase of 0.012 injuries, or 3.6% of the outcome mean; by age 11, the same size of increase is associated with 0.025 more injuries, or 4.7% of the outcome mean.[47]

Table F.1. Association between child behaviour and hospitalisations and injuries

	Hospitalisations			Injuries		
	Age 5	Age 7	Age 11	Age 5	Age 7	Age 11
Externalising behaviour	0.011*** (0.003)	−0.001 (0.002)	−0.002 (0.003)	0.012*** (0.003)	0.014*** (0.003)	0.025*** (0.004)
Internalising behaviour	0.013*** (0.003)	0.013*** (0.003)	0.018*** (0.004)	0.008** (0.004)	0.003 (0.003)	0.005 (0.004)
Female	−0.042*** (0.016)	−0.037*** (0.013)	−0.028 (0.020)	−0.051*** (0.017)	−0.056*** (0.016)	−0.084*** (0.024)
Ethnicity: mixed	−0.034 (0.042)	−0.049 (0.035)	−0.034 (0.053)	−0.092* (0.047)	−0.063 (0.043)	−0.120* (0.064)
Ethnicity: Indian	0.042 (0.042)	−0.055 (0.034)	−0.056 (0.052)	−0.101** (0.047)	−0.093** (0.042)	−0.310*** (0.064)
Ethnicity: Pakistani or Bangladeshi	0.071** (0.031)	0.024 (0.025)	0.011 (0.039)	−0.148*** (0.034)	−0.159*** (0.031)	−0.322*** (0.047)
Ethnicity: black or black British	−0.001 (0.045)	−0.046 (0.036)	−0.031 (0.055)	−0.119** (0.050)	−0.062 (0.045)	−0.259*** (0.067)
Ethnicity: other	0.039 (0.059)	0.068 (0.048)	−0.133* (0.073)	−0.126* (0.065)	−0.084 (0.059)	−0.318*** (0.089)
Mother's age at birth	0.002 (0.001)	0.001 (0.001)	0.000 (0.002)	−0.003* (0.002)	−0.002 (0.001)	−0.002 (0.002)

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[47] The mean and standard deviation of the externalising behaviour score are reasonably similar over time: the mean is 4.6 at age 5, 4.5 at age 7 and 4.1 at age 11; the standard deviation is 3.4, 3.6 and 3.6 respectively. This means that a one-unit increase is fairly comparable over time.

	Hospitalisations			Injuries		
	Age 5	**Age 7**	**Age 11**	**Age 5**	**Age 7**	**Age 11**
Mother cohabiting	−0.013	−0.009	−0.043*	−0.032	−0.033	−0.091***
	(0.023)	(0.018)	(0.025)	(0.025)	(0.022)	(0.031)
Mother's education: A level	−0.017	0.038*	0.030	−0.004	0.004	−0.030
	(0.025)	(0.020)	(0.030)	(0.028)	(0.025)	(0.037)
Mother's education: GCSE or below	−0.001	0.026*	0.007	0.007	0.026	−0.058**
	(0.019)	(0.015)	(0.023)	(0.021)	(0.019)	(0.029)
Mother's education: missing	0.066*	−0.006	0.018	0.054	−0.007	−0.095
	(0.037)	(0.032)	(0.051)	(0.041)	(0.039)	(0.062)
Mother's work status: part-time	−0.028	0.001	−0.034	−0.024	−0.026	−0.052*
	(0.019)	(0.016)	(0.026)	(0.021)	(0.020)	(0.031)
Mother's work status: full-time	−0.008	0.010	−0.013	−0.008	0.019	−0.046
	(0.021)	(0.017)	(0.026)	(0.024)	(0.021)	(0.032)
Mother's work status: missing	0.080	−0.054	−0.005	−0.179*	−0.015	−0.081
	(0.090)	(0.066)	(0.075)	(0.100)	(0.082)	(0.091)
Household net earnings (£'000s)	−0.000	−0.000	−0.000	−0.000	0.000	−0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Home Learning Environment	0.023***	0.018***	0.017*	0.021**	0.019**	0.031**
	(0.008)	(0.007)	(0.010)	(0.009)	(0.008)	(0.012)
Constant	0.047	0.076	0.283***	0.434***	0.350***	0.811***
	(0.065)	(0.053)	(0.079)	(0.072)	(0.066)	(0.097)
No. of observations	6,964	6,963	6,928	6,960	6,963	6,924
R^2	0.016	0.014	0.013	0.023	0.027	0.040
Outcome mean	0.173	0.118	0.181	0.333	0.291	0.537
Age fixed effects (FE)	Yes	Yes	Yes	Yes	Yes	Yes
Region FE	Yes	Yes	Yes	Yes	Yes	Yes
Season of interview FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Results are associations and should not be interpreted as causal. Internalising and externalising behaviour indices are based on the Strengths and Difficulties Questionnaire (peer problems and emotional difficulties, and hyperactivity and conduct disorder subscales respectively). The indices are scored out of 20 with a higher score indicating greater problems. Results at 'age 5' consider hospitalisations/injuries at ages 4 or 5; similarly, 'age 7' results cover ages 6 and 7 while 'age 11' refers to ages 8–11. See the text of this appendix for a description of all control variables and their scale.

Source: Authors' calculations using the Millennium Cohort Study.

These are not particularly big numbers in and of themselves. But it is striking that externalising behaviour is strongly statistically significant at all three ages when many other characteristics are not. In particular, socio-economic characteristics such as the mother's age at birth, maternal education, maternal work status and household net earnings are neither as statistically significant nor as consistent a predictor of injuries as externalising behaviour. This result supports the hypothesis that a Sure Start-induced change in child behaviour is a plausible mechanism for the big and growing reduction in injury-related hospitalisations.

Appendix G. Sensitivity for analysis of maternal mental health

As we highlight in Chapter 8, we had to make several modelling decisions when estimating the impact of Sure Start on mothers' outcomes. In this appendix, we examine the sensitivity of our analysis to changing two of these choices:

- using fixed effects at the neighbourhood, rather than individual, level;
- including controls for time-varying family demographics.

The first of these extensions seeks to test the sensitivity of our results to using the same type of fixed effects as we do in the main impact analysis in Chapters 5 and 6. Including time-varying demographic controls is a more conservative specification, but risks controlling for some characteristics that might have been affected by Sure Start (and therefore understating the overall impacts we estimate).

We also present the results from a model that investigates whether the impacts of Sure Start coverage are different at different ages. This builds on Table 8.3 by considering families where the eldest child is between 5 and 11 years old (analogous to the age group that is covered in our analysis of hospitalisations).

Results with neighbourhood fixed effects

The results in Table G.1 look very similar to the results of our preferred specification presented in Table 8.1. The estimated impact that Sure Start has on the GHQ score is larger when using LSOA fixed effects (0.9 points compared with 0.6 points in Table 8.1) and the estimated decline in mental health problems is smaller (1.6 percentage points compared with 5.0 percentage points). But the directions of these effects are the same, and none is statistically significant.

Table G.1. Estimated impact of Sure Start coverage on maternal mental health in a model including neighbourhood fixed effects

	GHQ score	Mental health problem
Sure Start coverage (eldest)	0.920	−0.016
	(0.605)	(0.107)
No. of observations	13,794	13,794
R^2	0.536	0.498
Baseline mean	2.026	0.494

Note: As for Table 8.1. The model controls for neighbourhood (LSOA) fixed effects rather than individual fixed effects as in Table 8.1. It also controls for the sex and ethnicity (white, black, Asian, other or missing) of the eldest child and for dummies of the eldest child's year–month of birth.

Source: Authors' calculations using the pooled BHPS–UKHLS data, 1993 to 2014 and the Department for Education's data on the roll-out of Sure Start.

Results with time-varying family demographics

The results in Table G.2 control for a range of time-varying family demographics, including:

- **maternal education:** indicators for whether the mother is qualified to A level, GCSE level, holds no formal qualifications, or has missing or foreign qualifications (relative to the baseline of degree-educated);
- **marital status:** indicators for whether the mother is separated or widowed, is single, or has missing marital status (relative to the baseline of married or cohabiting);
- **benefit receipt:** indicators for whether the family is receiving disability benefits and whether the family is receiving tax credits.

Table G.2. Estimated impact of Sure Start coverage on maternal mental health in a model including time-varying family demographics

	GHQ score	Mental health problem
Sure Start coverage (eldest)	0.449	−0.068
	(0.674)	(0.097)
No. of observations	13,652	13,652
R^2	0.504	0.473
Baseline mean	2.032	0.495

Note: As for Table 8.1. The model also controls for individual fixed effects and for time-varying demographics: maternal education, maternal marital status, and family receipt of tax credits and/or disability benefits.

Source: Authors' calculations using the pooled BHPS–UKHLS data, 1993 to 2014, and the Department for Education's data on the roll-out of Sure Start.

These results also look very similar to the results of our preferred specification presented in Table 8.1. The estimated impact that Sure Start has on the GHQ score is slightly smaller when including these time-varying characteristics (0.4 points compared with 0.6 points in Table 8.1) and the estimated decline in mental health problems is bigger (6.8 percentage points compared with 5.0 percentage points). But the directions of these effects are the same, and none is statistically significant.

Age-specific results for older children

The results in Table G.3 replicate the analysis in Table 8.3 on an older age group. Rather than children who are still eligible for Sure Start – and so whose coverage is changing from one year to the next – we here consider families where the eldest child is between 5 and 11 years old.

For these families, the coverage experienced by the eldest child between the ages of 0 and 4 is the same each time they are interviewed (since the children have already aged out of Sure Start). This means that the overall baseline effect of Sure Start coverage is a characteristic that does not change over time, and so it is stripped out of the model by individual fixed effects. Instead of estimating this overall effect, the model investigates whether the impact that the Sure Start treatment has differs as the eldest child gets older.

For both of our outcomes, we find no statistically significant estimates from interacting Sure Start coverage with indicators for the age in years of the eldest child. This means that, as in Table 8.3, there is no evidence of the impacts of Sure Start on maternal mental health fading out or getting stronger as children age.

Table G.3. Estimated impact of Sure Start coverage on maternal mental health, by age of eldest child

	GHQ score	**Mental health problem**
SS coverage (eldest) × 6 years old	−0.208	−0.085
	(0.446)	(0.077)
× 7 years old	0.364	0.042
	(0.451)	(0.080)
× 8 years old	0.063	0.018
	(0.512)	(0.083)
× 9 years old	0.532	0.077
	(0.585)	(0.097)
× 10 years old	0.053	−0.064
	(0.730)	(0.108)
× 11 years old	−0.684	−0.078
	(0.787)	(0.136)
No. of observations	8,928	8,928
R^2	0.563	0.529

Note: As for Table 8.3. The model is estimated on mothers whose eldest child was born between 1993 and 2014 and is between 5 and 11 years old, rather than 4 years old or younger as in Table 8.3, at the time of the interview. Coefficients are relative to the impact of the individual fixed effect, which cannot be separately identified from the treatment effect at age 5.

Source: Authors' calculations using the pooled BHPS–UKHLS data, 1993 to 2014, and the Department for Education's data on the roll-out of Sure Start.

Appendix H. Inputs for the cost–benefit analysis

Table H.1. **Inputs for the cost–benefit analysis**

Cost	Value updated (2018–19 prices)	Base value (year prices)	Source
Unit cost, paediatric infection	£1,029	£1,011 (2017–18)	National Schedule of Reference Costs (NSRC)
Unit cost, paediatric fracture	£989	£972 (2017–18)	NSRC
Unit cost, paediatric head injury (non-intracranial)	£640	£629 (2017–18)	NSRC
Unit cost, paediatric head injury (intracranial)	£2,376	£2,335 (2017–18)	NSRC
Indirect costs of child infections	£157	£111 (2001–02)	Lorgelly et al., 2008
Indirect costs of child injuries (fractures, head)	£204	£185 (2012)	Cooper et al., 2016
Lifetime cost of child maltreatment (non-fatal)	£94,737	£89,270 (2015)	Conti et al., 2017
Lifetime cost of child maltreatment (fatal)	£998,371	£940,758 (2015)	Conti et al., 2017
Lifetime cost of traumatic brain injury	£5,400,349	£4,890,000 (2012)	Child Accident Prevention Trust, 2012

Note: From the National Schedule of Reference Costs (NSRC), we computed the national average unit cost of non-elective long-stay and non-elective short-stay admissions for paediatric injuries (separately for intracranial injuries, non-intracranial head injuries and non-head injuries) and infections. The cost measure in Lorgelly et al. (2008) includes over-the-counter (OTC) medicines, lost income, parental costs and value of work time lost. The cost measure in Cooper et al. (2016) only includes the parental costs (OTC medications, aids, formal and informal childcare, time off work, travel); it has been obtained as the average of the midpoint of two ranges of costs, for shorter admissions (lasting ≤ 1 day) and admissions lasting 2 or more days. The lifetime cost of non-fatal child maltreatment excludes the short-term healthcare costs, to avoid double-counting. The cost measure in Child Accident Prevention Trust (2012) includes education, missed employment, and cost to the government for lost tax revenue and increased transfers.

Table H.2. Detailed breakdown of averted costs for the cost–benefit analysis

Costs	£1,054.9 million
Averted healthcare costs of infections, age 5	£683,000
Averted healthcare costs of injuries (fractures + head injuries), ages 5–11	£3,939,000
Averted healthcare costs of injuries (fractures + head injuries), ages 12–18	£4,597,000
Averted indirect costs of injuries, ages 5–11	£0.8 million
Averted indirect costs of injuries, ages 12–18	£0.9 million
Averted indirect costs of infections, age 5	£0.1 million
Averted lifetime costs of non-fatal child maltreatment (related to fractures)	£0.7 million
Averted lifetime costs of non-fatal child maltreatment (related to head injury)	£1.4 million
Averted lifetime costs of fatal child maltreatment	£1.3 million
Averted lifetime costs of traumatic brain injury	£50.4 million
Benefits: sum of all averted costs	**£64.8 million**

Note: Figures for averted healthcare costs are rounded to the nearest thousand and all other figures are rounded to the nearest hundred thousand (this reflects the greater precision in the unit cost of healthcare). Figures may not add due to rounding. For head injuries, we use the specific resource use costs from the National Schedule of Reference Costs (NSRC) and the actual prevalence in Hospital Episode Statistics (HES) 1998 at the specific ages 5–11 to cost separately intracranial and head non-intracranial injuries. We use the result in Gonzalez-Izquierdo et al. (2010) to attribute 4.7% of head injuries and 2% of fractures at age 5 to child maltreatment (non-fatal) that entails long-term costs. We also derive from Trefan et al. (2016) the mortality rate of 1 in 250 due to fatal child maltreatment (abusive head trauma) or motor vehicle accidents, and use it on the age 11 estimates to compute the long-term costs. Lastly, we use the actual prevalence in HES 1998 at age 11 for the proportion of traumatic brain injury, and use it on the age 11 estimates to compute the long-term costs.

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