The Health Effects of Universal Early Childhood Interventions: Evidence from Sure Start

Sarah Cattan, Gabriella Conti, Christine Farquharson, Rita Ginja and Maud Pecher*

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Abstract

We evaluate the short- and medium-term heath impacts of Sure Start, a large-scale and universal early childhood program in England. We exploit the rollout of the program and implement a difference-in-difference approach, combining data on the exact location and opening date of Sure Start centers with administrative data on the universe of admissions to public-sector hospitals. Exposure to an additional Sure Start center per thousand age-eligible children increases hospitalization by 10% at age 1 (around 6,700 hospitalizations per year), but reduces them by 8-9% across ages 11 to 15 (around 13,150 hospitalizations per year). These findings show that early childhood programs much less intensive than small-scale 'model programs' can deliver significant health benefits, even in contexts with universal healthcare. Impacts are driven by hospitalizations for preventable conditions and are concentrated in disadvantaged areas, suggesting that enriching early childhood environments might be a successful strategy to reduce inequalities in health.

JEL Codes: I10, I14, I18.
Keywords: early childhood intervention, health, difference-in-difference

*Cattan: Institute for Fiscal Studies and IZA (sarah_c@ifs.org.uk). Conti: University College London, Institute for Fiscal Studies, IZA and CEPR (gabriella.conti@ucl.ac.uk). Farquharson: Institute for Fiscal Studies (christine_f@ifs.org.uk). Ginja: University of Bergen and IZA (Rita.Ginja@uib.no). Pecher: Department for Health and Social Care (maud.pecher@dhsc.gov.uk).

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1 Introduction

Large health gaps by socioeconomic status are pervasive, even in countries - like England and continental Europe - with universal healthcare. Even before the COVID-19 pandemic, the ten highest income countries spent, on average, more than 10% of their GDP on health care (OECD, 2021). While medical care is a key input in the production of health (Grossman, 1972), more recent theories of health disparities have emphasized the role of preventive behaviors (Galama and Van Kippersluis, 2019) and of skills and habits developed in the first years of life (Conti, Heckman and Urzua, 2010; Dalgaard, Hansen and Strulik, 2021).

High-quality early childhood interventions targeting disadvantaged children, such as the Perry Preschool and the Abecedarian projects and Head Start, have been shown to generate substantial and long-lasting improvements in the health of their participants, not least through raising take-up of health insurance, and appear a promising policy to reduce health inequalities (Campbell et al., 2014; Carneiro and Ginja, 2014; Conti, Heckman and Pinto, 2016; Muennig et al., 2009). The available evidence on the health effects of early interventions, however, mostly focuses on evaluations of U.S. programs implemented decades ago, serving a very disadvantaged client base and, in the case of 'model' programs, delivering high-intensity services to enrich the child's environment.

While this evidence provides a robust 'proof of concept' for the potential benefits of early intervention, it raises two important questions for today's policy-makers, who are increasingly seeking to implement interventions which benefit the middle class as well as the most deprived. First, can the larger-scale, lighter-touch and less targeted programs that increasingly feature in policy debates in the U.S. and elsewhere deliver benefits similar to the early 'model' programs? Second, can positive health impacts still be achieved in contexts with universal, free-to-access healthcare? Answering these questions is crucial to inform debates about the potential benefits and costs of expanding access to early interventions to less disadvantaged populations. Given the importance of the health insurance mechanism in previous research, evaluating the impact of early interventions in a context with a higher level of baseline health and social services provides a more stringent test of the added value of these programs. This is relevant not only in countries that have already adopted universal healthcare, but also in the U.S., where universal health insurance is increasingly debated and already provided to low-income families.

Our contribution in this paper is to bring novel evidence on these two questions, drawing on the unique experience of England, the first European country to pursue a systematic policy to reduce socio-economic inequalities in health (Mackenbach, 2011). We evaluate the impacts of a large-scale and universal early childhood program on children's hospitalizations in a context with universal free healthcare. We show that the 'Sure Start' program, despite its lighter-touch delivery model, had substantial impacts on children's hospitalizations, lasting up to 10 years after children age out of eligibility. We estimate that the financial benefits of these effects offset a third of the cost of providing the program. Our detailed analysis of hospitalizations for specific conditions suggests that the integration of both health and non-health services offered by Sure Start was key to achieving these impacts, making a strong case for adopting a holistic approach, rather than focusing on a limited set of inputs in the production function of health.

'One of the most innovative and ambitious Government initiatives of the past two decades',[1] Sure Start was introduced in England in 1999 and rolled out over a period of 11 years. At its peak, a network of over 3,500 centers operated as 'one-stop shops' for families with children under the age of 5: the centers brought together health services, parenting support, early education and childcare, and parental employment advice to promote child development in a holistic way. Despite being one of the key pillars of the public service offer for the under-fives in England, Sure Start has received much less international attention than Head Start, the U.S.-based program it took its inspiration from, and evidence on its impact is scarce.[2]

[1] https://publications.parliament.uk/pa/cm200910/cmselect/cmchilsch/130/130i.pdf

[2] Sure Start and Head Start share similarities in terms of service offer but present key differences especially in terms of eligible population. Both programs offered center-based early education, as well as health services, services to strength parent-child relationships, and services to improve family well-being (e.g. continued education and financial security). However, Sure Start targeted all children aged 0-4 living in the center's area, while Head Start mostly targeted low-income children aged 3-4. Sure Start did not have strict eligibility criteria linked to family address or income, while Head Start is a means-tested program. Because of its means-tested nature, Head Start eligible children are also eligible for Medicaid.

We estimate the impact of increasing access to the Sure Start program on children's hospitalization at each age, from 1 to 15, using administrative data on the universe of admissions to publicly funded hospitals.[3] Our identification strategy leverages the variation in the number of centers in the child's Local Authority (LA)[4] induced by the program's rollout across areas of England. Our approach – which controls for LA fixed effects and cohort fixed effects – is motivated by the fact that the rollout of the program, as we show, was mostly determined by local levels of deprivation, which have remained fairly constant over time. We also perform a number of placebo and robustness checks to further probe the validity of our strategy, including testing for the robustness of our results to heterogeneity in treatment effects discussed in recent papers on difference-in-difference models (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021, among others).

Our main estimates show that greater access to Sure Start *increased* hospitalizations during infancy, but subsequently *reduced* them during childhood and adolescence. At age 1, having access to an extra center per thousand children under 5 increased the probability of a hospitalization by 10% - roughly 6,700 additional hospitalizations a year. Sure Start's effects on reducing hospitalizations during childhood and adolescence, however, more than compensate for the increase in admissions at very young ages. At age 5, an additional center per thousand children prevented around 2,900 hospitalizations a year; across all 11- to 15-year-olds, the total was over 13,150 prevented hospitalizations each year.

The multifaceted nature of the program means that the health impacts of Sure Start could be driven by a variety of mechanisms, both direct (such as signposting to appropriate healthcare or promoting vaccinations) and indirect (such as improving children's behavioral development or raising family income). To shed some light on these mechanisms, we estimate impacts of Sure Start access on hospitalizations for specific causes: preventable conditions, infectious illnesses, accidents and injuries, and – among adolescents – mental health. Among infants the increase in hospitalizations is driven mainly by an increase in infectious illnesses. In later primary school years and early adolescence, the reduction in admissions can be attributed to fewer accidents and injuries,

[3] Admissions to publicly funded hospitals account for about 95% of all hospital admissions.
[4] There are 326 Local Authorities in England.

infectious illnesses, and mental health-related conditions. We interpret these patterns as consistent with Sure Start benefiting children via multiple channels: by providing parents with information about children's health and healthcare; strengthening children's immune systems through vaccination campaigns and access to group settings; and improving children's behavioral and emotional development, by promoting better parenting practices and safer home environments. Using additional survey data on the population's labor force, we rule out an increase in parental employment and family income as possible channel.

The literature on large-scale preschool programs often reports heterogeneous effects by children's socio-economic status and gender. In the case of Sure Start, we find that the impacts of the program are strongest among children living in the 30% poorest areas of the country, and entirely null among children living in the 30% richest areas.[5] While boys and girls were equally affected in infancy, the long-lasting impacts of Sure Start are concentrated among boys. This gender asymmetry in the impacts of Sure Start echoes findings for other early childhood interventions, and confirms that disadvantaged boys are more sensitive to their environments (Bertrand and Pan, 2013).

Our paper provides a timely contribution to the literature on the health impacts of early childhood interventions. There is robust evidence that the most intensive programs, like the Perry Preschool and the Abecedarian programs, have had large and long-lasting impacts on health (Campbell et al., 2014; Conti, Mason and Poupakis, 2019; D'Onise, McDermott and Lynch, 2010). Evaluations of Head Start – which operated on a larger scale but was still targeted at disadvantaged children – also tend to find benefits for health in the short and long term (Bailey, Sun and Timpe, 2020; Carneiro and Ginja, 2014; DHHS, 2010, 2012; Frisvold and Lumeng, 2011; Ludwig and Miller, 2007; Thompson, 2018), though questions remain about the extent of fade-out.[6] These programs are all targeted at low-income populations and operated in a context where the existing

[5]Bitler, Hoynes and Domina (2014) also find stronger impacts of Head Start for children at the bottom of the distribution.

[6]Recent research shows that such fade-out might be partly explained by failing to account for the substitution between different types of public services (Kline and Walters, 2016) or by substantial heterogeneity in the effectiveness of Head Start centers (Walters, 2015).

social safety net is relatively weak. Both of these features are potentially important in explaining why they deliver such large impacts on health.

To date, there is much less evidence that speaks to the important question of whether universal programs implemented in contexts with more generous safety nets could be effective. A recent strand of work analyses the long-term health impacts of universal early childhood programs dating back to the origins of the welfare state in Scandinavian countries (Bhalotra, Karlsson and Nilsson, 2017; Bütikofer, Løken and Salvanes, 2019; Hjort, Sølvsten and Wüst, 2017).[7] While all these papers find evidence of meaningful benefits, the provision of public services in these contexts was also quite different from what it is today.

Available evidence from contemporaneous programs in Europe suggests that expanding access to childcare and preschool education can benefit child cognitive and socio-emotional development in modern settings with universal and free healthcare (Black et al., 2014; Felfe and Lalive, 2018; Havnes and Mogstad, 2011).[8] The evidence on the health benefits of such programs is just starting to emerge, with Siflinger and van den Berg (2020) finding that subsidizing childcare in one region of Sweden at age 1 decreases the number of medical visits at ages 4-5 and 6-7.

Our paper fills an important gap in the existing literature by providing evidence that a universal early childhood intervention that is much less intensive than 'model' programs still delivers lasting health benefits, even in a context with free healthcare and a developed social safety net. A key innovation of our analysis is to provide rare evidence on the profile of impacts through the 'missing middle years', i.e. in-between the immediate impacts and the longer-term effects that most existing evidence focuses on (Almond, Currie and Duque, 2018). This evidence is important not only for the cost-benefit analysis of the program, but also because the persistence of the effects in the post-eligibility years provides a stronger basis for predicting longer-term impacts - a key

[7]Bhalotra, Karlsson and Nilsson (2017) study the introduction of universal post-natal health care, information, and support in the 1930s in Sweden. Bütikofer, Løken and Salvanes (2019) evaluate the very long-run impacts of a 1930s program of mother and child health centers and post-natal home visiting in Norway. Hjort, Sølvsten and Wüst (2017) study the long-term health impacts of a universal health visiting intervention in Denmark for all infants.

[8]While the evidence on the expansion of childcare subsidies points to generally positive impacts of these policies on children's cognitive and socio-emotional development, there are some exceptions where childcare is of particularly low quality (e.g. (Baker, Gruber and Milligan, 2008)).

concern of policymakers seeking to justify spending on early intervention. As we illustrate in the paper, tracing out the profiles of program impacts through the medium-term can also shed crucial light on *how* complex and multifaceted programs like Sure Start work. Our findings underline the importance of integrating health with non-health services to exploit the dynamic interactions between various domains of child development.

2 Policy background: the Sure Start rollout and its service offer

2.1 The rollout of Sure Start

First introduced in 1999, Sure Start was conceived as an area-based intervention whose services would be available to all families with a children under five in the neighborhood of the center (without individual means-testing). The initial rollout of the program proposed 250 Local Programmes (SSLPs) in highly disadvantaged areas, anticipated to reach 150,000 children over a decade (Melhuish et al., 2008). Sure Start was given a budget of £450 million over the period 1999-2002 to set up 250 projects (Pugh and Duffy, 2010).

To decide which areas would get funding to open a Sure Start center, a national Sure Start Unit (SSU) developed a set of guidelines for the rollout. The initial 60 'trailblazer' districts invited to submit an application were selected based on the local level of deprivation, augmented with low birth weight and teen pregnancy indicators. The set of trailblazers was also chosen to offer a good spread of different types of areas around the country. 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 (DfEE, 1999). The program proved so popular that it was quickly expanded. A year after it began, the target number of SSLPs was doubled from 250 to 500 (Eisenstadt, 2011).

Four years later, the government announced that Sure Start would transition from a program for disadvantaged neighborhoods into a universalized offer, with "a children's center in every community" by 2010 (DfES, 2003). This expansion also included a rebranding (from 'Local Programmes' to ‘Children’s Centres’) and a greater role for central government in setting out a ‘Core Offer’ of services (Lewis, 2011), which we further describe below. The budget for Sure Start eventually rose from about £500m (about USD 684m) a year to £1.8 billion (in 2018-19 prices; USD 2.46 billion) at its peak in 2009-10, or about a third of overall spending on programs for the under-5s in England (Britton, Farquharson and Sibieta, 2019).

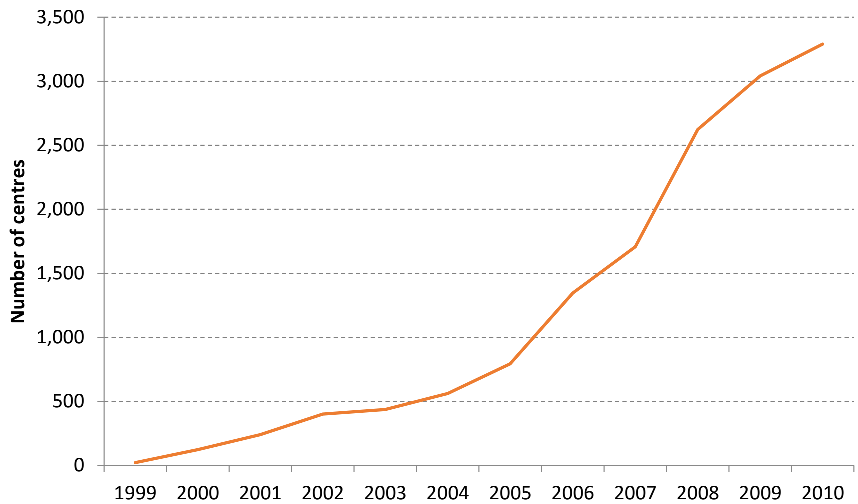
The Department for Education and Skills (DfES) had overall responsibility to establish 3,500 children’s centers by 2010. Again, the rollout was intended to be driven by deprivation, with three distinct phases (House of Commons, 2010). Between 2004 and 2006, there would be approximately 800 ‘Phase 1’ centers to offer full coverage of the 20% most disadvantaged neighborhoods. Of these, around 500 would grow out of existing SSLPs while the rest would be new centers. In the second phase, between 2006 and 2008, 1,700 new centers would open in the 30% most disadvantaged neighborhoods. Finally, the third phase of the rollout would complete the universalization of the program, through the opening of another 1,000 centers in the remaining 70% of areas.[9]

Following the rollout plans, the period between 2005 and 2010 saw a rapid increase in the number of Sure Start centers, with the rollout described by many as ‘haphazard’ and ‘too fast’ (Morris, Barnes and Mason, 2009; Prowse, 2008). By 2010, the overall number of centers reached 3,500, with each center serving a local population of between 600 and 1,200 children depending on the location and level of need (see Figure 1).

The maps in Figure 2, which show the location of Sure Start centers in 2000, 2004, 2006, and 2008, suggest that the deprivation-based guidelines for each of the phases were taken seriously. During the first years of the program rollout, SSLPs were predominantly opened in the most disadvantaged local authorities (shaded in darker green in the figure). By the time all of the SSLPs had been opened, in 2004, the focus on relatively poor areas was even more obvious.

[9]While the national government retained overall control of this phased approach, from 2004 onwards Local Authorities (LAs) also gained more control over decisions about where specifically centers within their jurisdiction were located. LAs were allocated targets and funded to deliver centers based on their under-five population and their level of deprivation.

Figure 1: Number of Sure Start centers in England



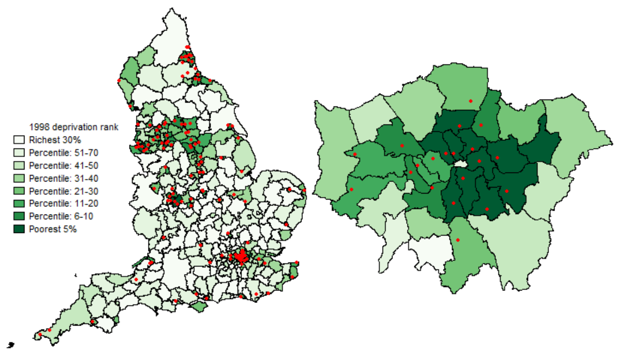
Note: The number of centers is based on centers observed in data received from the Department for Education. Since the treatment of arrangements like satellite sites was not always consistent, these numbers might not exactly match other data sources. We assume that a Sure Start Children's center (SSCC) 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 onward. Source: Authors' calculations using data provided by the Department for Education.

Sure Start since 2010 The new government in 2010 de-prioritized the Sure Start initiative, shifting its early years policy on expanding subsidized childcare. Between 2011 and 2019, national government spending on Sure Start fell by over 60% (Britton, Farquharson and Sibieta, 2019). Following the removal of the funding ring-fence in April 2011, local authorities could choose to respond to these cuts in different ways. Some authorities subsidized Sure Start services from other budget lines; others consolidated several centers into one, cut back on the hours or types of services offered, and/or entirely shut down centers (Smith, 2018). The core mission of the program also became less prescriptive, allowing each center to focus on the outcomes they wanted to achieve for young children and their families (Smith, 2018). In light of these important changes, we focus the empirical analysis presented in this paper on the 1999-2010 period during which the program expanded and delivered a more consistent service offer.[10]

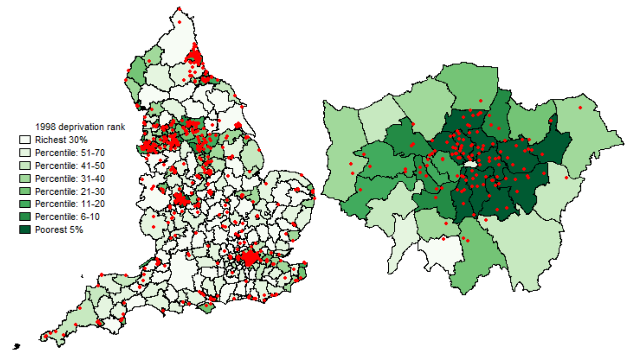
[10]Our empirical strategy, presented in section 4, exploits the variation in access to Sure Start resulting from the rollout to identify the effect of increased access to Sure Start on children's hospitalizations. In principle, the reduction in Sure Start access resulting from center closures could also be used within such an empirical strategy. However, we refrain from doing so because, given the freedom with which LAs could respond to funding cuts, center closures are more likely to be endogenous than their openings. Moreover, we only have imperfect information about center

Figure 2: Sure Start centers around England

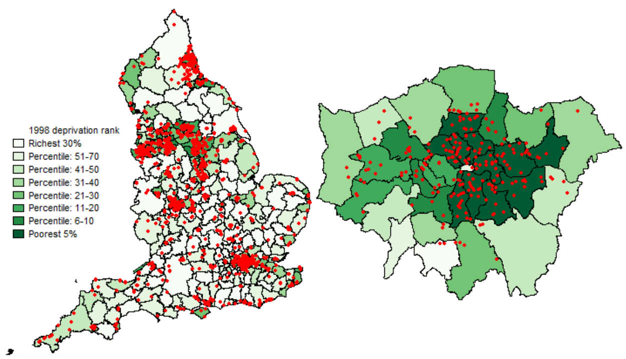
(a) SS centers in 2000



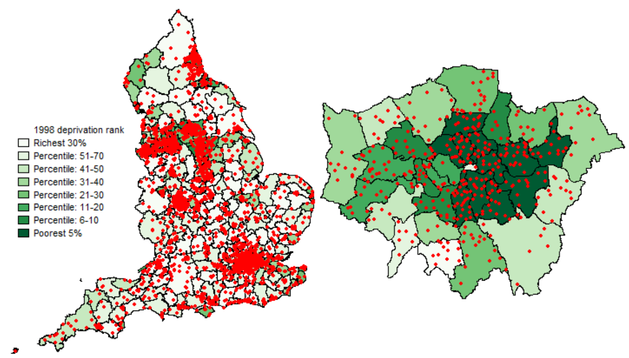
(b) SS centers in 2004



(c) SS centers in 2006



(d) SS centers 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 center (SSLP or SSCC). The maps to the right of the maps of England are zoomed-in maps of London.

2.2 The Sure Start service offer

The overarching aim of the Sure Start initiative was to improve outcomes for young children by bringing together a range of services to support children and their parents. Its approach to child development was based on the recognition that child development is multi-dimensional and that the needs of families, particularly disadvantaged families, often span many traditional areas of support. Sure Start bridged across services in three ways: bringing together existing services under a single roof; streamlining referrals to other, more specialized services; and augmenting services with additional programs to address unmet local needs (DfEE, 1999).

closures, which does not capture 'hollowing out' of services in centers that technically remained open.

In the early years of the program, Sure Start Local Programmes were designed and encouraged to be responsive to local needs, and program managers had wide latitude in the services they offered. While there is no centralized record of the types of services offered in different areas, a survey conducted in 2003-2005 found that the largest single area of spending was play, learning and childcare, which accounted for a third of the SSLPs' budget (Meadows, 2011). A fifth of spending was aimed at community healthcare; this funding was used to supplement existing health services or to provide services that were not available through the public healthcare system (e.g. services for postnatal depression). Another fifth of the budget was spent on parenting support, and a sixth of spending went on outreach and home visiting.[11]

The transition to a universal offer of Children's Centres from 2004 meant that the national government took on a larger role in program design, setting out a 'Core Offer' that centers in the 30% most deprived areas were required to meet. This consisted of outreach to parents; early education and childcare;[12] family and parenting support; child and family health services (such as antenatal support); and links with JobcentrePlus[13] (Lewis, 2011). For centers serving the 70% least disadvantaged areas ('Phase 3' SSCCs), centers could meet some of the requirements of the Core Offer by developing referral links to other services; still, all centers were expected to offer activities for children, health and outreach services, and links to Jobcentre Plus.

Most Children's Centres chose to offer additional services on top of the core offer. Again, no centralized record of these services exists; however, data from a survey of Phase 1 and 2 centers give a sense of the service offer and patterns of usage in 2011, just after our period of interest.

[11] Support for children with special needs accounted for 7% of spending, and the remainder of the budget was spent on premises costs and other activities (Meadows, 2011).

[12] Sure Start centers could meet this requirement by delivering the 'free entitlement', a program offering every 3- and 4-year-old up to 15 hours a week of fully subsidized early education. This meant that much of the funding for childcare hours in practice came through the (separate) free entitlement budget, with Sure Start centers only involved in delivering the program. However, requirements for childcare delivered through Sure Start were more stringent than for the program as a whole: Phase 1 Children's Centres required that a qualified teacher had to be appointed and each local authority was given a target number of childcare places to create. Phase 2 Centres also had to provide access to childcare, with a 0.5 full-time-equivalent qualified teacher post, though there was no target for new childcare places. Phase 3 Centres were not required to provide early learning and childcare places but could do so if the need arose (House of Commons, 2010). In contrast, private nurseries only required that 50% of staff must hold level 2 qualification, which is equivalent to two years post-compulsory schooling.

[13] Jobcentre Plus is a government-funded employment agency and social security office that aims to help people of working age find employment.

Based on these data, Goff and Chu (2013) report that over 90% of the centres offered evidence-based parenting programs; early learning and childcare; informal drop-in play sessions; breastfeeding support; and training for volunteers (often parents). Other common services included midwife and health visitor clinics; sports and exercise for babies and children; advice on accessing welfare benefits, housing, and managing debt; adult learning; parent forums; and antenatal and postnatal classes. On average, sampled centres offered 28 different services (Goff and Chu, 2013).

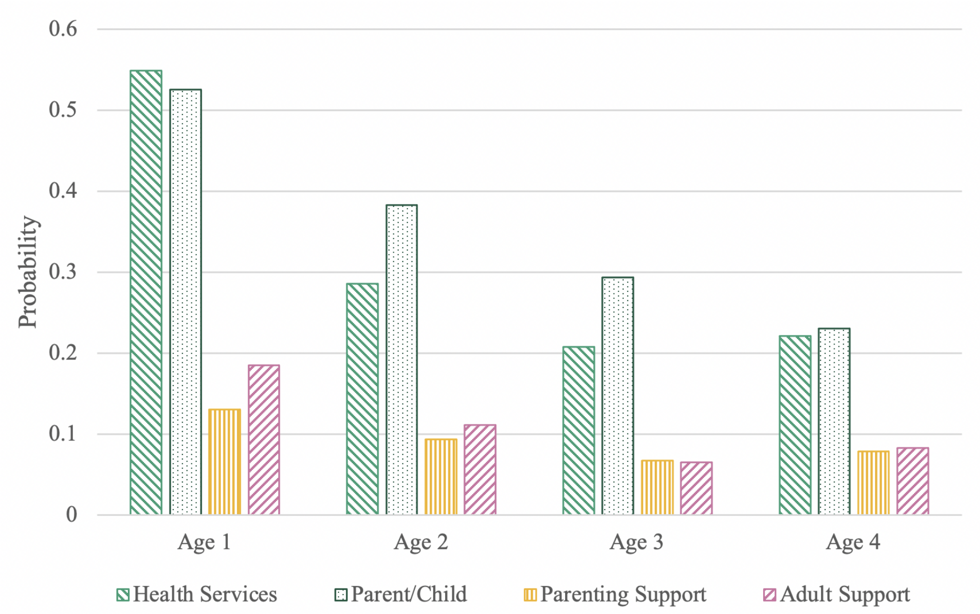
Take-up of Sure Start services Goff and Chu (2013) also collected data on families who were registered with a Phase 1 or Phase 2 center. While these data cannot be used to extrapolate what the take-up of Sure Start services was at the national level, they are the best indication available of the *relative* take-up of services across different groups of families.[14] Figure 3 shows that, across all ages, the services most likely to be used were health services and parent-child services (such as drop-in play sessions or baby classes), though their take-up rapidly declined with the child's age. By contrast, the proportions of families using parenting support and adult support was lower, but more stable across age groups.

3 Expected effects of Sure Start on children's hospitalizations

Given the variety of services offered by Sure Start, the program could have affected children's hospitalizations through a variety of channels. We focus on hospitalizations rather than measures of general health. In part, this is driven by data considerations as existing survey datasets with measures of child health are either too small for this analysis, or they do not offer suitably low-level geographic identifiers. Hospitalizations are an interesting outcome *per se* because one of the targets of the early Local Programmes was precisely to reduce hospitalizations by 10% was

[14] This dataset was collected as part of the Evaluation of Children's Centres in England project (ECCE). The sample is parents who had an infant registered with one of the 128 children's centres selected to participate in the study. Data was collected at three points in time. In the baseline survey, 5,717 parents were surveyed through face-to-face interviews when their child was aged 9-18 months old. All parents who had agreed to be re-contacted and provided a telephone number were invited to take part in a second survey, and 3,588 phone interviews were contacted in 2013 when the selected child was aged about 2 years old. Another round of data was collected through face-to-face interviews of 2,692 parents whose selected child was now about 3 years old. The analysis reported in the paper pools all years of data together so as to reflect almost the entire range of eligible ages.

Figure 3: Take-up of services in Phase 1 and Phase 2 centres



Notes: The figure shows the probability that families in the ECCE sample report making use of each of the four main types of services, by the age of the focus child. This is based on pooling all three waves of ECCE data. Health services include e.g ante-natal classes, breastfeeding groups, midwife/health visitor drop-in session or clinic; Parent/child services include e.g. stay and play, or play and learn drop-in sessions, organized sport or exercise for babies or children, toy libraries: Parenting support include peer support groups (parents supporting other parents), parenting classes, and specialist family or parenting support ; and Adult support includes e.g. benefits and tax credits advice, housing or debt advice, employment support, Basic IT or jobs skills course.

(Stewart, 2013). Furthermore, hospitalizations are costly both for individual welfare and for the public purse. These channels do not necessarily all point in the same direction; for example, short-term hospitalizations might increase as a result of more effective referrals to appropriate healthcare. Equally, the causes and ages of affected hospitalizations will differ across potential mechanisms.

In this section, we therefore set out our hypotheses about how each of the main groups of Sure Start services could have affected health and hospitalizations in the short and medium term, and discuss the mechanisms through which impacts could arise. We consider dynamics across three main periods: early years (when children were eligible to attend Sure Start, i.e. under age 5); middle childhood (when the child is in primary school); and early adolescence (between ages 11 and 15, which is the oldest age we can consider in our analysis). Table 1 summarizes the main groups of Sure Start services and their potential impact on hospitalizations in each period; we have highlighted effects on the same type of hospitalizations in the same color.

Table 1: Expected effects of Sure Start on children's hospitalizations

Mechanism	Services	Potential effect on hospitalizations		
		Early Years (c. age 1-4)	Middle childhood (c. age 5-10)	Adolescence (c. age 11-15)
Screening and referrals to appropriate healthcare	Health services	Ambiguous effect on hospitalizations for preventable diseases	Reduction in hospitalizations for preventable diseases	
Safer home enviroment	Health services	Reduction in hospitalizations for external causes (poisoning and accidents/injuries)		
Improved emotional and behavioral development (through improved parental and formal care)	Parenting support, parent/child activities, childcare	Reduction in hospitalizations for external causes, especially accidents and injuries		
		Reduction in hospitalizations for mental health-related reasons		
Stronger immune systems	Childcare, parent/child activities	Increase in hospitalizations for infections	Reduction in hospitalizations for infections	
Increased family income and parental employment	Adult support	Reduction in hospitalizations as a result of higher family income		
		Increase in hospitalizations from less parental time at home		

Health services All Sure Start centers offered a range of health services, including ante-natal and post-natal support for mothers and babies; advice on accident and injury prevention; advice on obesity, diet and nutrition; and support for mental health and for families with disabilities (e.g. DfE, 2010; DfES, 2003). Sure Start therefore did not substitute for primary care provision (which is freely delivered in England by General Practitioners or GP in the National Health Service), but rather enhanced access to health support and information.

We hypothesize two key mechanisms through which these supplemental health services may have affected hospitalizations. The first is screening children for conditions and referring families to appropriate health care, leading to an increase in hospitalizations for preventable and manageable conditions[15] in the short-term (early years) and a decrease in hospitalizations for the same

[15]Specifically, we consider Ambulatory Care Sensitive conditions, which include conditions like asthma (usually managed with primary healthcare), gangrene (preventable) and measles (vaccinable).

conditions in the longer-term. The second mechanism is enhancing health-promoting parental behavior and the safety of the home environment, leading to a reduction in hospitalizations at all ages. Since Sure Start provided information about accident prevention, 'child-proofing' and safety in the home, we could expect this reduction to be particularly important for accidents and poisonings. Given the nature of the advice focused on very young children, effects may be more pronounced during the early years, although if this information led to sustained changes in parenting behavior, those effects may be longer-lasting.[16]

Parenting support and parent-child services Centers provided evidence-based parenting classes (such as the Triple P and Incredible Years programs) to improve family functioning and positive parenting skills, often with a particular focus on children's mental health and emotional and behavioral issues. They also provided a range of parent-child activities, such as dads' groups, baby massage and drop-in play sessions, aimed at strengthening parent-child relationships.

These services did not target (physical) health specifically, but may nevertheless have had indirect benefits by activating cross-productivities between behavioral and emotional development and health (Cunha et al., 2006). By strengthening child-parent attachment and parenting practices, these services may have led to healthier emotional and behavioral development (Case and Paxson, 2002). Calmer and less fidgety children have been shown to be less prone to injure herself and may be easier for parents to care for (Hoare and Beattie, 2003). Early intervention to improve parent-child bonds may also reduce the chances of parental neglect and maltreatment (Avellar and Supplee, 2013; Eckenrode et al., 2017). Later in life, stronger emotional and behavioral regulation could help children's mental health and reduce their exposure to accidents and injuries from risky or aggressive behavior.

As a result, if these services were effective, we would expect that Sure Start reduced hospitalizations for accidents and injuries during the early years. To the extent that early benefits for

[16]Information was also provided about diet and nutrition, and we could also expect this advice to decrease the incidence of obesity. In a preliminary version of this paper (Cattan et al., 2019), we test for this mechanism directly by employing a similar research design and administrative data on weight and height of all children in primary school at ages 5. We find no evidence of effects of the program on obesity. We rule out this mechanism going forward.

parenting or child development persist, these reductions in hospitalizations could be long-lasting. Moreover, if Sure Start improved children's emotional development, we would also expect a reduction in hospitalizations for mental health-related causes. However, since the prevalence of mental health-related hospitalization is essentially zero before adolescence, we would not expect to pick up an effect on mental-health hospitalizations during the early years or childhood.

Childcare and group-based sessions As discussed in section 2, Sure Start itself was not a childcare program. However, Sure Start centers did play an active role in delivering a separate entitlement to subsidized childcare hours, and the care childcare workforce employed in Sure Start centers was held to higher standards of qualifications than those in the private nurseries that dominate the market.

If Sure Start facilitated the take-up of high-quality childcare, it could have affected health in two ways. First, high-quality childcare can benefit emotional and behavioral development (Heckman, Pinto and Savelyev, 2013). As such, we could expect a reduction in hospitalizations for accidents and injuries similar to those resulting from parenting support and parent-child services. Second, childcare (and, to a lesser extent, other group-based activities) increased the time children spent around other children and hence their potential exposure to infectious diseases. In the short run, this might have led to an increase in the number of sickness episodes. But early exposure to a variety of pathogens also helps to build up the immune system, which might have benefits in the longer run (Henderson et al., 1979; Siflinger and van den Berg, 2020). In this case, we would expect hospitalizations for infections to increase in the short term and drop in the medium term. We would not expect these negative effects to be particularly long lasting, however, all children go to school from the age of 5 in England, meaning that children who were not exposed to Sure Start should see their immune systems catch up once they start spending more time with others.

Adult support The last major set of services offered by Sure Start aimed to support parents, especially in their effort to gain employment (e.g. links with JobcentrePlus to gain job-search assistance and job-related training). A subsequent increase in parental employment could affect children's health through different mechanisms. On the one hand, the associated increase in family income would allow parents to buy more and/or higher quality inputs, such as more nutritious food (Carneiro and Ginja, 2016). On the other hand, employed parents may have less time to spend on health-improving activities (e.g. cooking a home-made meal, accompanying children to the doctor). Finally, parents shifting into employment could result in children spending more time in childcare. Since these different channels push in different directions, the overall effect of Sure Start's employment services on children's health and hospitalizations is ambiguous. Because this channel does not lead to clear testable predictions in the hospitalization data, we present a separate estimation of the effect of Sure Start on parental employment using data from the Labour Force Survey (LFS) (Appendix E). This analysis shows that this mechanism is unlikely to be an important one, which is why we shaded these rows in gray in Table 1.

In sum, as the Table makes clear, the direction of Sure Start's impact on hospital admissions is expected to differ based both on the cause of hospitalization and the age of the child. This means that the *overall* impact on hospitalizations is expected to be ambiguous, particularly earlier in life when higher admissions for infections or preventable conditions may offset reductions in admissions for accidents and injuries. As children grow older, however, we would expect the effect of Sure Start on overall admissions to be more clearly negative. Because we do not have data on service take-up to link to hospitalization data, we are not able to probe directly the mechanisms through which Sure Start worked. Instead, guided by the discussion above, after presenting estimates of impacts on overall admissions, we present estimates of impacts on cause-specific hospitalizations at different ages to suggest what mechanisms were most likely at play (subsection 6.2).

3.1 Existing evidence on the effects of Sure Start

While this paper represents the first effort to evaluate the causal effect of Sure Start over much of its history, there are two previous government-commissioned studies into the program. The first is the National Evaluation of Sure Start (NESS), which collected data on children living in neighborhoods served by the earliest Sure Start Local Programmes in 2001. These children were compared to others surveyed in an earlier national survey (conducted in 2000) who lived in areas not served by the program. The NESS found an increase in parent-reported hospitalizations at 9 months, an increased prevalence of immunizations and a reduced probability of accidental injuries at age 3, and lower Body Mass Index (BMI) and better parent-reported health status by age 5 for children living in the Sure Start neighborhoods (NESS, 2005, 2008, 2010). At age 5 and 7, it also found that this group had better family functioning (e.g. better home learning environment, less chaotic homes), greater social skills, and lower behavioral issues than the group not living in a neighborhood with a Sure Start center.

The second study was the Evaluation of Children's Centres in England (ECCE). Run in 2011, this study collected detailed and extensive data on a sub-sample of Children's Centers and their users, and estimated impacts of Sure Start by comparing the outcomes of children whose families chose to use the services with varying frequency. The authors found no significant association between using Sure Start services and child's health, but did conclude that greater service use was associated with fewer externalizing behavior problems, higher child physical and maternal mental health, and improved family functioning (ECCE, 2015).

Although the methodologies employed by these two evaluations do not support a robust causal interpretation, it is noteworthy that both NESS and ECCE found evidence of a link between Sure Start, child behavior and parenting practices - a key mechanism our findings also suggest was at play. In contrast with previous evaluations, our paper proposes to evaluate the impact of greater access to Sure start by using a robust evaluation methodology exploiting the 11-year rollout of the program and hospitalization administrative data. Moreover, we examine impacts on hospitalizations much beyond the time horizon considered by these evaluations, from age 1 to age 15. We now turn to describing our research design.

4 Data and Empirical Strategy

4.1 Data

The main data we use in this paper combines individual hospitalization records in public hospitals in England with information about when and where each SSLP and SSCC opened.[17] To maximize comparability across cohorts, we restrict our sample to children born within 5 years of the announcement of Sure Start (i.e. those born in 1993 or later) and to children who could only have been exposed to Sure Start before the 2010 change in policy (i.e. those born in 2006 or before). As there is no data available to allow us to estimate the effect of Sure Start on measures of health, we instead rather focus on children's hospitalizations.[18]

Data on Sure Start facilities To measure our treatment variable, we use a unique dataset containing the exact address and date of opening of each Sure Start Local Programme and Children's Centre between 1999 and 2010. Based on this information, we construct our measure of access to Sure Start SS_{dq}, such that it varies across Local Authority d and quarter of birth q (our cohort dimension).[19] Specifically, we define SS_{dq} as the average number of centers per thousand children aged 0-4 that were open during the first 60 months of life of a child born in quarter q and living in Local Authority (LA) d.[20] When estimating models with an outcome measured before age 5, we define SS_{dq} as the average number of centers per thousand children aged 0-4 that were open between the child's birth and the age at which the outcome is measured. Figure 4 plots this variable for each of the 323 LA in England (in grey) and superimposes its average (in blue) across LAs.

[17]We also use a variety of auxiliary data as sources of information on local area characteristics and on policies contemporaneous to Sure Start to perform robustness checks. Those are described in Appendix B.

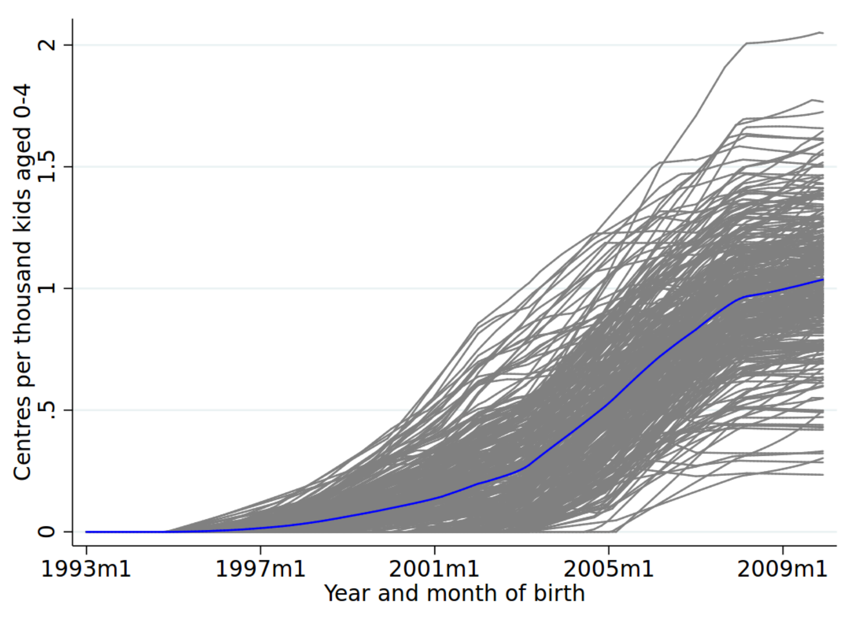
[18]There are two nationally representative surveys that include measures of children's health in England, but each of them have issues that prevent this analysis. The British Household Panel Survey (BHPS) and its follower the UK Household Longitudinal Study have sample sizes that are too small. The Health Survey for England has larger sample sizes, but do not provide researchers access to precise enough geographical identifiers to carry out this analysis.

[19]By 'quarter' of birth, we refer to the combination of a year and quarter. Given that our maximum sample includes children born from January 1 1993 to December 31 2006, we have children born in 52 different quarters of birth or cohorts in the data.

[20]There are 326 Local Authorities (LAs) in England. We exclude three of them from the analysis (the Isles of Scilly, City of London, and West Somerset), which are very small areas with few children aged 0-4 and appear as outliers in terms of Sure Start coverage.

Across the cohorts we consider in the dataset, the number of SS centers per thousand children aged 0-4 increased from 0 to an average of 1.[21]

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



Notes: Each grey line represents one of 323 local authority districts in England (excluding the Isles of Scilly, City of London, and West Somerset). The blue line shows the average for all of England. The lines plot the average Sure Start coverage (centers 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.

Data on hospitalizations We use the Hospital Episode Statistics (HES), an administrative dataset tracking the universe of patients using English public hospitals. Data on inpatient admissions has been collected since April 1997 and we have data up to March 2018. The inpatient data tracks all hospital admissions, providing information on the admission, discharge, clinical diagnoses (up to 20 for each patient), and demographics of each patient.[22] The dataset also includes information

[21] Our treatment variable does not distinguish between Local Programmes and Children's Centres. While the opening dates of all centers are precisely known, pooling in this way requires us to make an assumption when SSLPs transitioned into SSCCs, which has not been recorded in the data. Since over 90% of SSLPs had transitioned into Children's Centres by 2006 (NAO, 2006), we assume that (a) any Local Programme that shares a postcode with a Children's Centre closed at the same time as the associated Children's Centre opened; and (b) all other Local Programmes closed in December 2006.

[22] In this context, inpatient admissions include day cases who are admitted to a hospital bed as well as those who stay overnight. There is a separate register for emergency room attendance, but these data are only considered reliable

on the patient's sex, ethnicity, date of birth, and the Lower-level Super Output Area (LSOA) of residence at the time of admission.[23]

To create our outcomes of interest, we include one record per hospitalization (though patients may have several 'episodes' under different physicians during a single spell of hospitalization) and exclude admissions related to the birth of a child. We then construct counts of all-cause and (primary) cause-specific admissions for each neighborhood (defined at the LSOA level), quarter of birth, sex and age of admission. Cells without admissions are assigned zero. Because a large fraction of cells have zero admissions, we define our main outcome of interest as $D^{ya}_{sql(d)}$, an indicator for whether there is any hospitalization of type y at age a for children of sex s born in quarter q and residing in neighborhood l (of LA d).

4.2 Empirical Specification

Our aim is to estimate the effect of increased access to Sure Start on children's hospitalizations (for any cause and for specific causes). To do so, we exploit the variation in potential exposure to Sure Start across birth cohorts and Local Authority generated by the Sure Start rollout and displayed in Figure 4 in a standard difference-in-difference framework. We operationalize it by way of a two-way fixed effect model, where we control for: i) birth cohort fixed effects to account for secular trends in hospitalization, and ii) neighborhood fixed effects to account for systematic differences in time-invariant area characteristics that may be correlated with both the Sure Start rollout and hospitalizations.

Our main estimating equation has the following specification:

$$D^{ya}_{sql(d)} = \delta^{ya} SS_{dq} + \beta^{ya} X_s + \alpha^{ya} Pop_{al} + \gamma^{ya}_q + \pi^{ya}_{l(d)} + \upsilon^{ya}_{sql(d)}, a = 1, ..., 15 \quad (1)$$

from April 2007, so there is less scope to look at the impacts of Sure Start across the entire life-cycle of the program. Similarly, the register for outpatient data is only reliable from April 2006.

[23]The LSOA is a very small geographic unit. There are around 33,000 LSOAs in England, and the average LSOA has a population of around 1,500 residents. LSOAs are a unit of statistical rather than practical geography, so there are no administrative or electoral responsibilities that are conducted at the LSOA level. However, LSOAs map to Local Authorities (LA), which do have a role in administering a wide range of policies.

where $D_{sql(d)}$ and SS_{dq} are defined as above, X_s is a female dummy and Pop_{al} is the number of children of age a in neighborhood l. γ_q is a set of cohort of birth fixed effects defined at the year-quarter level. The model includes a set of over 32,000 neighborhood fixed effects π_l, which account for time-invariant unobserved heterogeneity across areas. Controlling for neighborhood fixed effects is crucial in light of the evidence presented in section 2 that the rollout of Sure Start was correlated with deprivation and the local potential demand (measured by the number of children aged 0-4). Finally, the error term is denoted $\upsilon^{ya}_{sql(d)}$.

The parameter of interest is the coefficient δ^{ya}, which measures the effect of increasing access to Sure Start by one center per thousand children on the probability that a neighborhood-sex-birth quarter cell experiences at least one hospitalization.[24] As seen on Figure 4, an increase of one center per thousand children is roughly equivalent to the average increase in coverage across the whole rollout period (although individual areas will have seen higher or lower increase). The parameter δ^{ya} is an Intention-To-Treat (ITT) parameter, as it measures the effect of increasing access to rather than actual use of Sure Start. Given that Sure Start was designed as an area-based intervention, this parameter also corresponds to the relevant parameter to compute the net benefits of the policy.

In section 5, we report the estimates of parameters in equation (1) for admissions for any cause and ages $a = 1, ..., 15$. In subsequent sections, we re-estimate this model for cause-specific hospitalizations and for different subgroups. Because the probability of hospitalization can vary quite substantially across ages and cause of admission, we present most of our results with graphs showing the proportionate Effect of an increase in Sure Start coverage, relative to a baseline probability measured in 1996, from ages 1 to 15. Estimates underlying the graphs are reported in the Appendix.

[24]Our main measure of treatment in equation (1) is a "dose-response" model that captures simultaneously the 'extensive' margin of Sure Start (whether there is any center open in the local authority) as well as the 'intensive' margin (how many centers are available). Hence, we also explore non-linearities of the effect to disentangle intensive from extensive margins in section 5.

Inference For all models considered, we present robust standard errors clustered at the level of Local Authority (LA) at the time of admission to account for autocorrelation in the outcomes (Bertrand, Duflo and Mullainathan, 2004). Because we study the effects on a relatively large number of potentially correlated outcomes, we also report the results of a stepwise multiple hypothesis testing procedure that controls for familywise error rate. In particular, we use the procedure in algorithms 4.1 and 4.2 of Romano and Wolf (2005) to account for testing several hypotheses simultaneously; this is an iterative rejection/acceptance method for a fixed level of significance. We use 500 block-bootstrap replications to obtain the adjusted critical values (the block is the LA). The result tables indicate whether the coefficients remain significant at a level of 1, 5, or 10 percent after using this procedure. In line with our discussion of expected effects, when applying this correction we consider the different phases of child development and test simultaneously the impacts for three age groups: 0 to 4 (early years), 5 to 10 (middle childhood) and 11-15 (adolescence).

4.3 Validity of empirical strategy

The interpretation of the parameter δ^a as the causal effect of increasing *access* to Sure Start relies on three crucial assumptions: (1) that greater access to Sure Start increases the probability of participation; (2) the "parallel trends" assumption that the rollout of Sure Start across LA was uncorrelated with time-varying unobservable determinants or shocks to hospitalizations (captured in $\upsilon^{ya}_{sql(d)}$); and (3) that families did not locate selectively to be closer to Sure Start centers as they were rolled out. This section provides evidence supporting the validity of all three assumptions.

Access to Sure Start and participation Sure Start was an area-based intervention, with most Local Programmes and Children's Centres defining a 'reach area' where outreach would be targeted most intensively. Local authorities had a statutory duty to ensure sufficiency of provision for all families living in their area and centers were periodically evaluated by a national regulator on how effectively they were reaching their target population and the extent to which their services were taken up (DfE, 2010).

Unfortunately, for most of Sure Start's history, there was no systematic collection of these take-up figures, which makes it impossible to directly test the assumption that greater access implied greater usage. However, some illustrative figures from the ECCE project confirm that families who took up Sure Start services typically lived close to a children's center: 78% lived within 1.5 kilometers of the center, and 30 per cent within 500 meters (Goff and Chu, 2013). Families were also encouraged to use services from a range of centers, visiting different centers on different days to attend the programming that was most relevant to them. Within LAs, Sure Start centers were often encouraged to operate as part of a network: in 2011, for example, 40% of centers were explicitly organized as part of a wider network (Sylva and Sammons, 2015). In order to reflect the importance of these local networks of services, we focus on an LA-based measure of coverage.[25]

Parallel trends assumption The parallel trends assumption requires that the rollout of Sure Start was uncorrelated with unobserved time-varying determinants of hospitalizations. As discussed in section 2, official guidelines about the Sure Start rollout indicate that new SSLPs were prioritized in areas with relatively high deprivation, high teenage pregnancy rates, and high proportion of low birthweight births (under 2.5kg), while the rollout of SSCCs (from 2002 onwards) was mostly determined by area deprivation. Nevertheless, policymakers' decisions over where and when to open new centers could have been influenced by other factors affecting the supply and demand for the centers. Our identification strategy would be under threat if we found that these factors varied differently across areas and also affected children's hospitalizations.

To better understand whether that could be the case, we gather data on a large number of factors that could have affected the rollout of the SSLPs and SSCCs (these come from various sources described in Appendix B). In addition to data on known determinants of the rollout (overall deprivation, teenage pregnancy rates, and proportion of low birthweight births), we also gathered LA and year-level information about: local labor market conditions (male and female weekly full-time earnings, and job seeker allowance claimants rate, which is a measure of unemployment),

[25] Using an LA-based measure of coverage rather than a distance-based treatment measure also reduces measurement error, since there is no precise postcode information in the HES data.

potential demand (number of children aged 0-4 in the LA, and proportion of children aged 0-4 looked after), health indicators (infant mortality rate, and number of GPs per 1000 inhabitants in each LA), and proportion of 3 year old children with a funded part-time childcare place (to be potentially used in Sure Start centers), and political variables (share of local council seats held by the Labour party, which was the party in power during the expansion of this policy). Importantly, while all these factors could have affected decisions about where to open new centers, they could all potentially affect hospitalizations.

We start by showing the extent to which the change in Sure Start coverage is explained by time-invariant characteristics and time-varying demand and supply factors. To do so, we regress Sure Start coverage rate (defined at the LA and quarter of birth cohort) for potentially exposed cohorts (born between 1996 and 2006) on LA fixed effects, cohort fixed effects (i.e. year-quarter fixed effects), and the potential determinants of the SS rollout described above. Area and cohort fixed effects explain 86% of the variation in SS coverage, and while only 4% of the variation can be explained by time-varying demand and supply factors. This suggests that the rollout was mostly determined by time-invariant area characteristics, which our empirical strategy controls for via neighborhood fixed effects.

We further investigate how the variation in SS exposure we exploit in our empirical strategy correlates with baseline area characteristics. Following Bhuller et al. (2013), we estimate the following equation:

$$\Delta SS_{dq} = \rho_d + [\beta_q \times c_{d,1998}]' \Phi_q + \epsilon_{dq} \tag{2}$$

where $\Delta SS_{dq} = SS_{dq} - SS_{dq-1}$ and $c_{d,1998}$ is the vector of LA characteristics described above and measured in 1998, the year preceding the opening of the first SSLP. We plot the estimated coefficients Φ_q and their 95% confidence intervals in a series of graphs shown in Appendix Figure A.1. Consistent with official guidelines about the SS rollout, we find that the expansion of Sure Start coverage is positively associated with deprivation, teenage pregnancy rate, and proportion of low birth weight births for cohorts born between 1996 and 2002 (i.e. cohorts mostly exposed to SSLPs). We also find a positive and significant correlation between the expansion in SS coverage and local unemployment rate until 2003 (conditional on deprivation level), which is perhaps unsurprising given that one of the core objectives of Sure Start was to help increase parental employment. From 2003 onward, there is no correlation between any of the variables we consider and the local rollout of the program.

Overall, this evidence suggests that the variation we use to estimate the impact of increased access to Sure Start on hospitalizations is unlikely to be correlated with unobservable determinants of hospitalizations. Nevertheless, in subsection 5.2, we provide further reassurance that differential trends across areas are not driving our results by checking the robustness of our estimates to alternative specifications allowing for differential pre-trends across areas.

A final threat to the validity of the identifying assumption may arise if hospitalizations were subject to other, confounding policy shocks that correlated with the rollout of Sure Start. One crucial set of potential confounders is the local service offer. Over the decade that Sure Start was rolled out, the national government also made a number of reforms to the benefit system, the health system and the early years system. In-work benefits became more generous while out-of-work benefits were reduced (Gregg, 2008); health spending rose from 5% to 7.5% of GDP (Stoye and Zaranko, 2019); and the government introduced and expanded a new part-time childcare entitlement for 3- and 4-year-olds (Blanden et al., 2016). While our empirical strategy allows for cohort effects, many of these reforms may have impacted local authorities differently. We will therefore present a robustness check that controls for a range of time-varying LA-level characteristics, including the rollout of funded childcare places; the number of physicians per capita, a proxy for health service availability; and local labor market characteristics (to reflect changes in the benefit system incentivizing employment). In addition to these measures of the policy environment, we will also incorporate a wide range of other characteristics that may be related to both the rollout of Sure Start and the incidence of hospitalizations. These include local demographics; vital statistics; and labor market characteristics (see Appendix B for the full list of variables and sources). If any of these characteristics is confounding our results - or is correlated to another unobserved characteristic that is confounding our results - we would find that our results are not robust to these specifications. As we show in the results of these checks in subsection 5.2, this is not the case, thus providing reassuring evidence that the effect of Sure Start access we estimate is not confounded by the effect of other policies.

Selective migration A final requirement for our empirical strategy is that there is no selective migration into high-coverage areas. Using data from the British Household Panel Survey, we show in Appendix C that there is no relationship between migration and Sure Start coverage. Furthermore, the overall degree of inter-LA migration is relatively low, with around 4% of families moving LA each year after children turn 5. Since out treatment is defined by the child's LA of residence at the time of admission, this provides further reassurance that inter-LA migration is not a major source of measurement error in our context.

5 Sure Start's effects on overall hospitalizations

5.1 Main estimates

Table 2 reports the estimates of the effect of a one-center (per thousand children) increase in access to Sure Start on hospitalizations for any cause between the ages of 1 and 15. These effects are estimated separately from 15 regressions (one for each age of admission). Figure 5 plots these estimates re-scaled by the baseline probability of any hospitalization at the corresponding age to enable comparison of relative effects across ages.[26]

These results show that, during the earliest years of life, an increase in Sure Start coverage resulted in an increase in hospital admissions. In particular, an additional center per thousand children raises the probability of any hospitalization at age 1 in a cell by 2.6 percentage points, a 10% rise relative to the pre-Sure Start baseline (when 26% of LSOA-sex-quarter of birth cells had at least one hospitalization). This translates into approximately 6700 additional yearly hospitalizations.[27] Figure 5 also shows clearly that these early increases in hospitalizations are followed

[26]We use the mean from the cohort born in 1996 as our baseline, also reported in Table 2.

[27]To compute the number of yearly averted or additional all-causes hospitalizations engendered by the presence of an additional center per thousand children, we multiply the estimates of parameter δ^{ya}, as defined in model 1 and

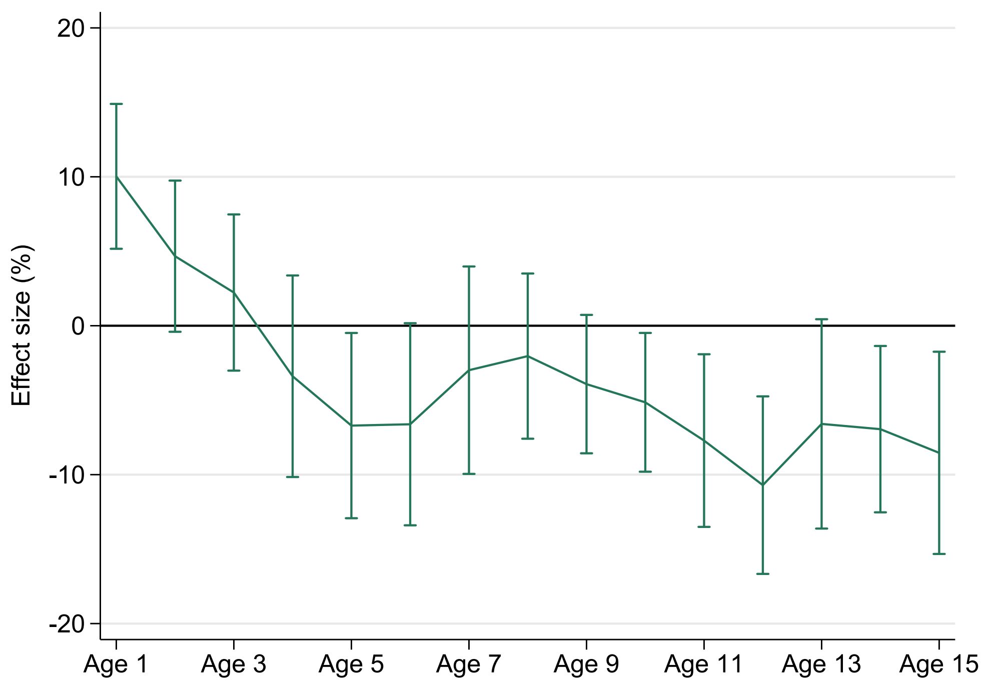
Table 2: Effect of an increase in Sure Start coverage on probability of hospitalization for any cause

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
SS Cov	0.0256***	0.0095	0.0040	-0.0057	-0.0109*	-0.0095	-0.0038	-0.0024	-0.0044	-0.0055*	-0.0084**	-0.0125***	-0.0081	-0.0091**	-0.0120**
	(0.0075)+++	(0.0063)	(0.0057)	(0.0069)	(0.0061)	(0.0059)	(0.0053)	(0.0039)	(0.0032)	(0.0031)	(0.0038)+	(0.0042)+++	(0.0052)	(0.0045)	(0.0058)
Baseline mean	0.2552	0.2044	0.1791	0.1687	0.1623	0.1438	0.1260	0.1160	0.1125	0.1078	0.1089	0.1172	0.1229	0.1311	0.1410
N	2,822,176	3,084,704	3,347,232	3,609,760	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,478,496	3,215,968	2,953,440	2,690,912	2,428,384
Earliest cohort	Apr.96	Apr.95	Apr.94	Apr.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93
Latest cohort	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Mar.06	Mar.05	Mar.04	Mar.03	Mar.02

Note: The table shows coefficients from regression analysis at each outcome age. Observations are cells defined by the LSOA, quarter-year of birth, and sex. The model regresses an indicator for any hospitalization in a cell on Sure Start coverage, the population at the relevant age in the LSOA, an indicator for female and fixed effects for quarter–year of birth and for the LSOA of residence. Sure Start coverage SS_{qd} is defined as the number of centers per thousand children aged 0–4 in the local authority for outcomes at age 5 and older, and as the number of centers per thousand children aged 0-4 that were open in the LA d when the child was aged $a-1$ for outcomes at ages $a = 1, ..., 4$. The baseline mean (3rd row) is measured in 1996. 'Earliest' and 'latest' cohorts refer to the first and last birth cohort included in each regression. Standard errors are shown in parentheses clustered by LAD. *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively; $+$, $++$ and $+++$ indicate significance at the 10%, 5% and 1% level, respectively, after adjusting inference following the procedure described in algorithms 4.1 and 4.2 of Romano and Wolf (2005).

by substantial decreases in the probability of admission through childhood and early adolescence. Once children turn 5 and stop being age-eligible to use Sure Start services, the overall impact on hospitalizations becomes consistently negative, with larger impacts during the first few years of schooling (ages 5-6) and then from age 10 onward. Exposure to an additional center per thousand children at ages 0-4 averts around 7% of hospital admissions at age 5, 8% by the end of primary school at age 11, and 8.5% by age 15 (the final age we study). This represents around 2,860 fewer yearly hospitalizations at age 5 and over 13,150 prevented hospitalizations of 11-to 15-year-olds each year. Table 2 also indicates whether the estimates are still significant after adjusting inference to multiple hypothesis testing: the increase in admissions among infants and the reductions at ages 11 and 12 survive this adjustment.

Figure 5: Effect of an increase in Sure Start coverage on probability of any hospitalization in the neighborhood, rescaled by baseline probability



Note: Effect sizes are constructed by rescaling the estimates by the pre-Sure Start baseline probability of a hospitalization at each age. Vertical bars indicate 90% confidence intervals. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

presented in Table 2, by the number of observations per year, which is 262,528. Our analysis is carried out at the LSOA-quarter and year of birth-gender level, with 32,816 LSOAs thus generating 262,528 yearly observations.

5.2 Robustness checks

As discussed in subsection 4.3, our difference-in-difference design relies on the assumption that cohorts' exposure to Sure Start is uncorrelated with time-varying unobservable shocks to hospitalizations. We have already presented some evidence suggesting that the factors that determined the Sure Start rollout did not vary much over time, but in this section we further challenge the validity of our identifying assumption by presenting a series of alternative specifications and placebo analyses.

Differential time trends We first augment the model to allow for differential trends in hospitalizations across Local Authorities in two different ways. If the results of these specifications are similar to those obtained for the main model and described above, we can be more confident that trends in outcomes have similar slopes across LAs and are not driving our main results.

Specifically, we first estimate LA-specific linear time trends in the pre-treatment period, i.e. using cells on cohorts for which $SS_{dq} = 0$. For each LA, we obtain a slope estimate ρ_d. We then linearly extrapolate this pre-treatment time trend for all the cohorts in the sample and include this estimated trend as a control in our main model (equation 1). By estimating these trends only on pre-treatment data, we avoid controlling for any impact that Sure Start itself has had on LA trends.[28] These estimates are presented in Appendix Figure A.2 and are similar to our main estimates of Figure 5.

The second way we probe the robustness of our results to differential LA trends is by including in the benchmark model interactions between the cohort fixed effects and the baseline characteristics that we showed affected the rollout of Sure Start (1998 deprivation levels, teen conception rate and incidence of low birth weight). In Appendix Table A.1 we additionally show that our estimates remain unchanged when we do that.

Finally, we also estimate a version of model (1) that includes a wide range of time-varying local area characteristics, including measures of other public services that changed over this period. We

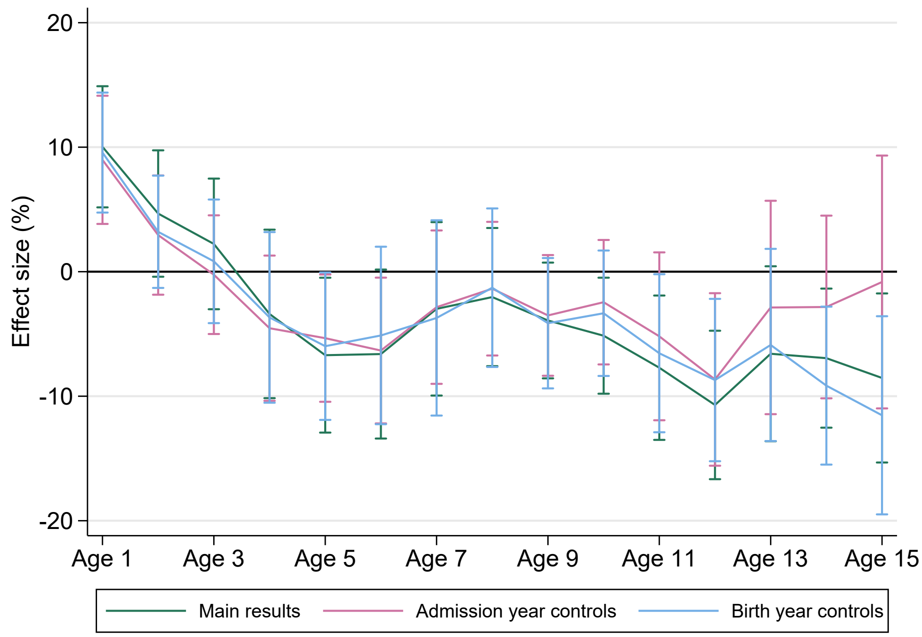
[28] Given the relatively large effects that we estimate in our main model and the staggered rollout of the program, we would expect Sure Start to have a substantial impact on trends in hospitalizations post-implementation.

conduct two versions of this robustness check, measuring these local area characteristics either in the child's year of birth or contemporaneously in the year that the outcomes are measured. The former version tests for confounding variables that were tied to the Sure Start rollout and may have influenced children's early health, such as the teen conception rate or the health service offer. The latter specification tests whether our estimates are confounded by a correlation between the rollout of Sure Start and subsequent changes in local characteristics or the local service offer, for example from policymakers seeking to 'follow up' early intervention with later services. Figure 6 shows that both of these robustness checks yield very similar results to our main estimates (though there are differences at age 15). We interpret the robustness of our results to this wide range of local characteristics as evidence that the internal validity of our research design is not compromised by most plausible confounders.

Placebo checks In order to gain more conclusive evidence that causality drives our estimates, we conduct a randomization inference test (Duflo, Glennerster and Kremer, 2006). To do so, we randomly reassign the Sure Start rollout profiles across LAs and re-estimate equation (1) under this placebo assignment of the treatment. We conduct this analysis for hospitalizations at all ages 500 times to derive a distribution of the placebo "treatment effects". Following MacKinnon and Webb (2020), we present in Figure A.3 in the Appendix the distributions of the t-statistics of the placebo treatment effect and the actual t-statistics for δ^{ya} in equation (1) are the vertical green lines (the dashed lines are the 5th and 95th percentiles of the distribution of placebo treatment effects). The placebo distributions in Figure A.3 are smooth and centered at zero and the solid lines allow to reject the null of no effect at ages 1, 5, 10, 12, 14 and 15 as in Table 2. Therefore, we can reject the null hypothesis that any combination of program rollout would generate the same magnitude of treatment effects that are displayed in Table 2.

Finally, we subject our results to an additional placebo check, by considering any admissions due to congenital chromosomal defects before age 1: since these are genetic conditions, they cannot be plausibly affected by Sure Start. We therefore expect our estimates to show no impact of

Figure 6: Effect of an increase in Sure Start coverage on probability of any hospitalization, rescaled by baseline probability: Robustness to inclusion of time-varying controls



Note: Figure shows coefficients from separate regressions for each outcome age. Coefficients are rescaled by the baseline (1996) mean for each age. Specification including time-varying controls contains controls for: the teenage conception rate; the share of births with low birth weight; the total period fertility rate; the LA population density; the share of primary school students with English as an Additional Language; the rate of Children Looked After among infants and among children aged 1-4; the Jobseeker's Allowance receipt rate; the number of GPs per capita in the LA; the number of JobcentrePlus per capita in the LA; and the take-up rate for funded childcare places for 3- and 4-year-olds in the LA. Vertical bars indicate 90% confidence intervals. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start. Area characteristic sources are in Appendix Table B.1.

Sure Start on admissions for these conditions. Indeed, Table A.2 shows that increasing SS coverage is unrelated to changes in the likelihood of hospitalizations due to congenital chromosomal defects.

5.3 Specification checks

5.3.1 Sensitivity of the results to sample selection

As indicated at the bottom of Table 2, the sample of analysis is not constant across outcomes. This is because we have sought to maximize our sample at each age, within constraints imposed by the data and the need for comparability across cohorts.[29] In order to check that our results are not driven by changes in the composition of the sample across ages, we re-estimate our main specification on two common cohorts. The first common cohort covers ages 1 to 4 and uses data for children born between April 1996 and December 2006. As Figure A.4 shows, the results on this early years cohort are virtually identical to our main estimates. The second common cohort covers ages 11 to 15 and uses data for children born between January 1993 and March 2002. Figure A.5 shows that the results on the common teen cohort are statistically indistinguishable from our main estimates.

5.3.2 Non-linearities in treatment effects

Our main model assumes a linear effect of Sure Start coverage on children's hospitalizations. It may however be possible that the effect is non-linear, for example if there needs to be a critical mass of children exposed to Sure Start to start picking up effects on hospitalizations or if effects arise only once families have access to several centers in their vicinity. We explore this possibility by re-estimating our model to distinguish between no Sure Start coverage, medium coverage (fewer than 0.25 centers per thousand children), and high coverage (more than 0.25 centers per thousand).[30] Figure A.6 shows that the impacts of high coverage are of greater magnitude than the impacts of medium coverage, but other than at age 1, the impacts of medium and high coverage are statistically indistinguishable.

5.4 Robustness to heterogeneous treatment effects

Recent work has emphasized that the estimand recovered in the linear model with Two Way Fixed Effects (TWFE), as we employ here, is a weighted sum of the average treatment effects (ATE) in each group and period, with weights summing to one but some possibly negative (Borusyak, Jaravel and Spiess, 2021; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021).

[29] Many younger cohorts are not yet old enough to have data for hospitalizations at later ages. Further, since the inpatient data are only collected from 1997, some older cohorts will not be observed at younger ages.

[30] The cut-off point between medium and high coverage is approximately the median coverage among those with positive coverage.

Negative weights arise when groups that are treated earlier are used as controls for groups that are treated later, and hence are more likely to be assigned to long-run ATEs. As discussed in these papers, if treatment effects are heterogeneous across group and period, the presence of negative weights creates an issue in that the treatment parameter recovered in the standard TWFE model can be negative even if all ATEs are positive. Moreover, the TWFE estimand might not necessarily be the aggregation scheme that researchers might find to be most relevant to focus on (Callaway and Sant'Anna, 2020).

In the context of Sure Start, heterogeneity of treatment effects across groups and time is a possibility (and indeed, we will show heterogeneity across neighborhoods with different levels of deprivation). Moreover, because we exploit the staggered rollout of Sure Start, it is possible that our estimand puts negative weights on some treatment effects. Although we rely on a continuous treatment measure, and not a discrete treatment as most papers in this literature consider, Callaway, Goodman-Bacon and Sant'Anna (2021) do show that the same type of issues would apply in our case.

As no alternative estimator for continuous treatments have been developed yet, we assess the severity of these issues by discretizing our treatment variable and comparing the binary treatment effect estimates we obtain using the TWFE estimator with those we obtained using the efficient imputation estimator proposed by Borusyak, Jaravel and Spiess (2021) (BJS henceforth).[31] We consider three different binary treatment variables defined as indicators for whether SS_{dq} is above 0, 0.1 and 0.25. The results comparing the effects of these binary treatment effects are reported in Appendix Table A.3. In general the TWFE and the BJS estimates are very similar to each other, suggesting that negative weights are unlikely to be an important issue in our context. For infants, Table A.3 shows the BJS estimator yields slightly larger impacts in magnitude on hospitalizations than the TWFE estimator, though they are not statistically significant from each other. From age

[31] We choose the imputation estimator of Borusyak, Jaravel and Spiess (2021) over the ones proposed by de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2020) because it is more efficient than the others under heteroskedasticity. The gain in efficiency comes from the fact that the imputation estimator of BJS uses all non-treated periods to impute the counterfactual outcome for each group, while the alternative estimators only use the one period before the group becomes treated as counterfactual.

10 onward the TWFE and the BJS estimators yield similar estimates also for treatment indicators of 0.1 or 0.25 centers per 1,000 under 5 children, though we note that the BJS model often yields more precise etimates than the TWFE (at least in the case of binary treatment effects).

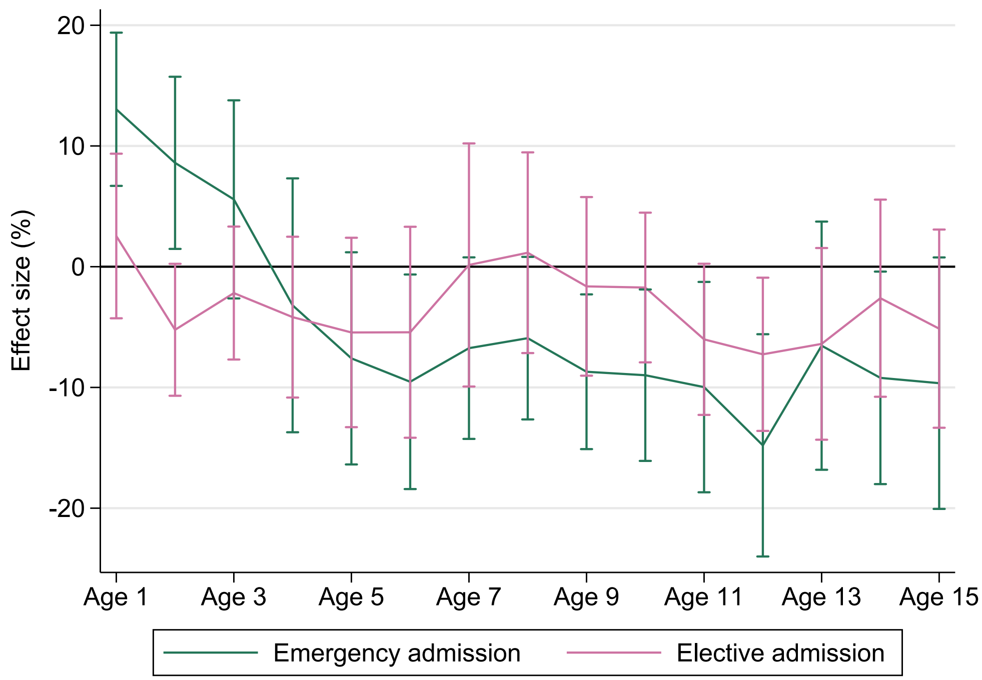
6 Mechanisms

Having shown in section 5 that greater access to Sure Start increased the hospitalizations of infants and toddlers but reduced admissions among older children, we now turn to discussing the mechanisms potentially underlying these impacts. As mentioned earlier, there is not center- or local authority-level data available to provide direct evidence of the mechanisms underlying our main results. Instead, we provide two types of evidence. First, we analyze the impact of Sure Start on hospitalizations by different admission routes and for different causes and follow our discussion in section 3 to assess which mechanisms are most likely to underlie the results. Second, we use another dataset - the Labour Force Survey (LFS) - to directly estimate the impact of Sure Start on parental employment.

6.1 Admission route

We first examine whether effects are heterogeneous across the two possible routes through which patients can be admitted to hospital: via the emergency room or via the elective route (which in England can only be accessed following a referral by a NHS GP). The results are shown in Figure 7 (underlying coefficients are presented in Appendix Table A.4). The change in admissions resulting from an increase in Sure Start coverage is driven by Sure Start's impacts on emergency admissions, with null effects on elective admissions for most ages. This suggests that Sure Start is affecting the incidence of illness or injury, not just families' propensity to seek health care for underlying or longer-term conditions.

Figure 7: Effect of an increase in Sure Start coverage on probability of any hospitalization, re-scaled by baseline probability: Emergency and elective admission routes



Note: The figure shows coefficients from separate regressions for each outcome age. Coefficients are re-scaled by the baseline (1996) mean for each age. Vertical bars indicate 90% confidence intervals. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

6.2 Cause-specific hospitalizations

To help understand what these wider effects could be, we next consider hospitalizations for a range of specific causes. Following the discussion in section 3, we focus on conditions that are likely to result in emergency rather than elective admissions and that are most likely to have been affected by Sure Start: preventable conditions, infectious illnesses, external causes, and (among adolescents) mental health.[32] We measure preventable conditions as Ambulatory Care Sensitive (ACS) conditions, which include chronic conditions that can typically be managed outside of hospital (e.g. asthma); acute conditions where serious illness could have been prevented by early intervention

[32]Hospital admissions in the HES data can have up to 20 causes, recorded via ICD-10 codes. In these results we classify admissions based on the primary diagnosis recorded; however, our results are similar when we instead look for any diagnosis matching the criteria.

(e.g. gangrene); and conditions that arise from vaccinable diseases (e.g. measles).[33] We present results graphically in Figure 8 and refer the reader to Appendix Table A.5 for tables containing point estimates and p-values adjusted for multiple hypothesis testing.

Starting with preventable (ACS) conditions, Figure 8(a) shows that Sure Start substantially increases hospitalizations for these conditions at younger ages, with a 20% increase over baseline levels at Age 1. However, as children age, greater access to Sure Start instead reduces ACS admissions, with a 20% reduction over baseline levels by age 11. This pattern is consistent with Sure Start providing information and signposting to help parents learn how to manage their child's conditions earlier in life, thereby reducing hospitalizations later on.

Figure 8(b) shows Sure Start's impacts on hospitalizations for any infectious illness (which include infectious and parasitic diseases and respiratory illness). We find that greater access to Sure Start substantially increases hospitalizations for infectious illnesses in infancy; however, there are significant and substantial falls in hospitalizations (of up to 18% of the baseline) at ages 5 and 6, just after children age out of Sure Start eligibility and start school. In line with the discussion in section 3, the results presented here are consistent with exposure to pathogens through Sure Start activities such as childcare: children who are more exposed early in life are initially more vulnerable to infectious illness, but then build up a stronger immune response which protects them compared to their peers when the entire cohort enters school. These effects then fade out in the longer term, as the start of universal schooling sees other children's immune systems 'catch up'.

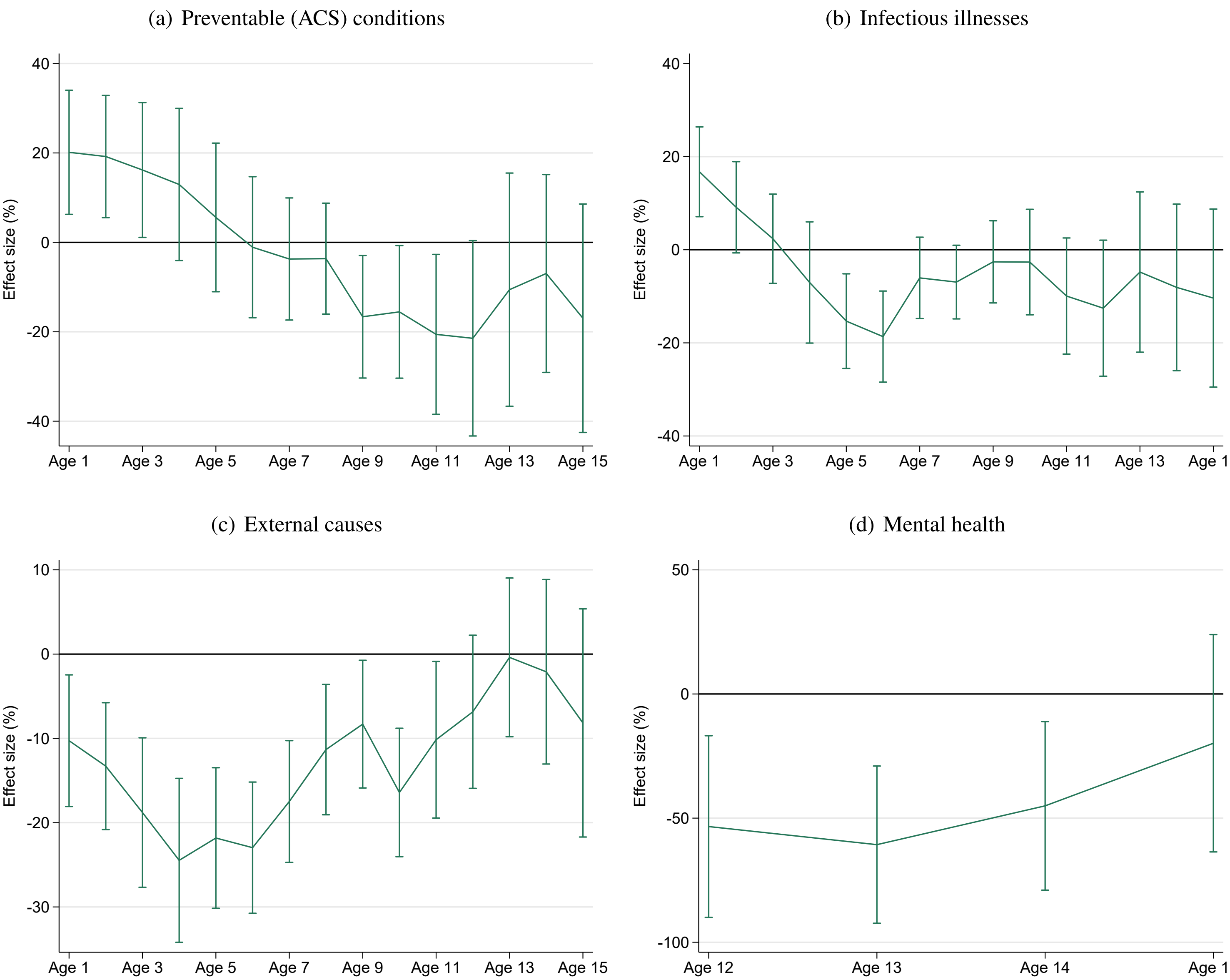
Next, we turn to hospitalizations for external causes.[34] Figure 8(c) shows that there is a very large, significant decline in hospitalizations for external causes at almost all ages we consider. Unlike the infectious outcomes discussed above, these effects are always negative; even at the youngest ages the probability of an externally caused hospitalization falls by 10% or more with greater access to Sure Start. At younger ages, these results offset some of the increase in hospitalizations due to infectious illnesses.

To understand the mechanisms underlying those impacts, we further analyze the impacts of

[33] See Blunt (2013) for a full list of ICD-10 codes that are included in this definition.

[34] Those correspond to ICD-10 groups S, T, V and Y

Figure 8: Effect of an increase in Sure Start coverage on probability of hospitalization for specific causes, re-scaled by baseline probability



Note: Figure shows coefficients from separate regressions for each outcome age. Coefficients are re-scaled by the baseline (1996) mean for each age. Vertical bars indicate 90% confidence intervals. Cause-specific results are based on the primary diagnosis at the time of admission. See Blunt (2013) for a list of all relevant ICD-10 codes included in ACS conditions. Infectious illnesses are composed of infectious and parasitic diseases (ICD-10 groups A and B) and respiratory illnesses (ICD-10 group J). External admissions include ICD-10 codes in groups S, T, V and Y. Mental health admissions relate to ICD-10 codes beginning with F.

Sure Start on different categories of external causes. Table A.6 shows that greater access to Sure Start significantly reduces poisonings from ages 1 to 3, consistent with information about or direct provision of safer environments for young children. However, by far the main driver of reductions in hospitalizations for external causes is a reduction in injuries, which decline with greater access to Sure Start during almost all years in childhood.[35]

We interpret the magnitude and persistence of these effects on injuries as potential evidence for sustained impacts on children's emotional and behavior development. Indeed, several studies report a correlation between children's behavioral issues (e.g. hyperactivity and aggressive behavior) and hospitalizations for injuries (e.g. Hoare and Beattie, 2003). In Appendix D, we complement this evidence using data from a nationally representative cohort of children born in 2000-2002 and show that having fewer externalizing behavior problems (such as aggression or hyperactivity) is correlated with a reduced probability of injury in middle childhood and early adolescence, even conditional on a wide range of demographics and family circumstances. The effect of Sure Start on reducing injuries could also reflect the effect of the program on reducing child maltreatment (through parenting and broader family support provided by the program). Indeed, reductions in hospitalizations for injuries are commonly interpreted in the home visiting literature as signs of reductions in child maltreatment (Kitzman et al., 1997).[36]

Lastly, we look directly at the impact of Sure Start on children's mental health. There are significant limitations to our data: we only observe hospital admissions, so our measure of mental health is very extreme and does not capture young people who are receiving services in the community, through their schools or through non-hospital providers. Previous work has also raised concerns about the accuracy of mental health diagnosis coding, especially for conditions such as depression or anxiety (Davis, Sudlow and Hotopf, 2016). Relatedly, recorded mental health hospitalizations among young people are very rare. Among children they are so rare, occurring in just 0.02% of cells, that we cannot estimate results. In Figure 8(d) (and Appendix Table A.7), we show the impact of additional access to Sure Start on mental health admissions among teenagers (ages

[35] Injuries (ICD-10 groups V and Y) account for between 70 and 80% of external admissions; most of the rest are accounted for by poisonings (codes T15-T98).

[36] While previous research has identified a subset of conditions that can be used as proxies for potential maltreatment, unfortunately the incidence of these is too low to reliably estimate Sure Start's impacts on these outcomes (González-Izquierdo et al., 2010) directly.

12 to 15). We find a statistically significant decrease in mental health-related admissions at ages 12 to 14, which is again consistent with potential longer-run impacts on children's socio-emotional development via enriched early environment and improved parenting practices.

6.3 Sure Start's effects on parental employment

In addition to their focus on children's health and development, Sure Start centers also brought together existing services to support parental (especially maternal) employment. As discussed in section 3, there are several channels through which an increase in parental employment resulting from Sure Start could affect children's hospitalizations. In order to tease out whether this is likely to be an important mechanism underlying our results, we use another dataset, the UK Labour Force Survey, to directly estimate whether increased Sure Start access had an effect on maternal labor market outcomes. These data have a quarterly frequency and a rotating panel structure at the household level (similar to the Current Population Survey in U.S.). As a result, we need to adapt our estimation strategy, though we aim to keep it as much in line as possible with the framework implemented in the HES data. Appendix E describes the data and estimation framework and presents the results. We find no robust evidence that Sure Start had an effect on maternal labor supply, either when children were aged 0-4 or later on. We conclude from this analysis that it is unlikely that the effects we observe on children's hospitalizations are driven by an increase in maternal employment (and family income).

In sum, the expansion of Sure Start through the 2000s led to significant changes in the hospitalizations of children from infancy all the way to adolescence. At the youngest ages, greater access to Sure Start increased hospitalizations, driven mainly by an increase in infectious illnesses. The increase was partly offset by a fall in hospitalizations from external causes and poisonings in the early years. Later, during early primary school, hospitalizations related to infectious illness fell. In later primary school years and early adolescence, we again observe a statistically fewer admissions to hospital for mental health reasons.

These patterns are consistent with Sure Start improving children's health and other dimensions of development through a number of key mechanisms: providing parents with greater information about children's health and healthcare; strengthening children's immune systems; improving children's behavioral and emotional development, by improving parenting practices and/or providing high-quality childcare. This evidence suggests that early childhood interventions focusing on these channels can deliver lasting health benefits, even in contexts with universal free health care.

7 Impact heterogeneity by gender and deprivation

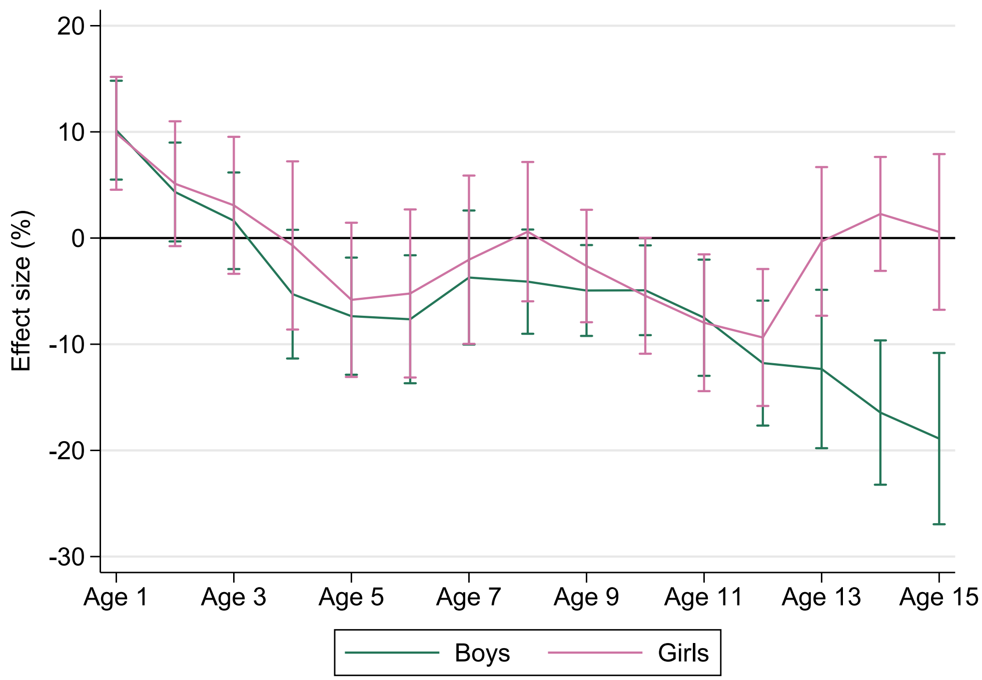
The literature evaluating early childhood interventions often report the presence of heterogeneous impacts across different groups of children. We explore whether impacts of Sure Start on hospitalizations are heterogeneous by gender and by areas with different levels of deprivation. The latter dimension is particularly interesting when looking at the case of universal interventions, given on-going policy debates about targeted vs. universal interventions.

7.1 Heterogeneity by gender

Figure 9 (and the point estimates in Table A.8) shows how the effects of Sure Start on all-cause hospital admissions vary between girls and boys. While the profile of effects is fairly similar for girls and boys up to age 10, during adolescence the impacts diverge. While there is no impact on girls in their teen years, the impact on boys grows steadily. By age 15, an additional Sure Start center per thousand children during the first five years of life reduces the probability of hospitalization among boys by 20%, with no effect among girls.

The greater impacts on boys in adolescence are consistent with the results of other early childhood intervention evaluations, such as the Abecedarian program (Conti, Heckman and Pinto, 2016), Head Start (Carneiro and Ginja, 2016) and the Boston Preschool program Gray-Lobe, Pathak and Walters (2021). Interestingly, in the case of Sure Start, we find similar impacts for boys and girls early on, suggesting that the gender difference in impacts during adolescence is unlikely to be due to differences in the take-up of services. An analysis of gender-specific effects on hospitalizations for different causes reveals that the gender difference in the program impacts is entirely driven by the greater impact of Sure Start reducing hospitalizations for injuries for boys (see Figure 10). Behavioral problems being more frequent among boys than girls (Bertrand and Pan, 2013), this finding could be further indication that an important channel through which the program worked was by improving children's behavioral development.

Figure 9: Effect of an increase in Sure Start coverage on probability of any hospitalization, rescaled by baseline probability: Differences by gender

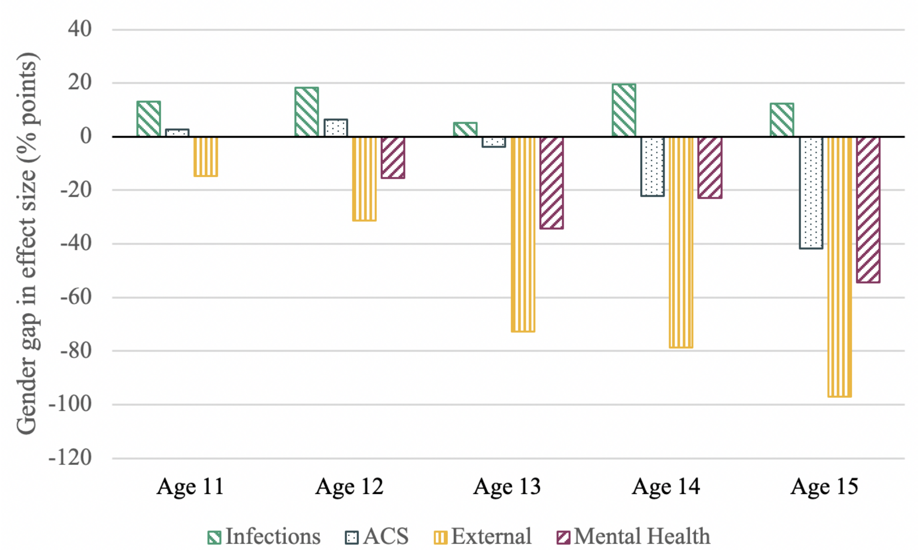


Note: The Figure shows coefficients from separate regressions for each outcome age, with Sure Start treatment interacted with gender. Coefficients are rescaled by the gender-specific baseline (1996) mean for each age. Vertical bars indicate 90% confidence intervals. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

7.2 Heterogeneity by level of deprivation

Sure Start started as an intervention targeting highly disadvantaged areas, but the program was universalized from 2004 onwards. Many large-scale early childhood interventions have been found to benefit more disadvantaged populations (see Almond, Currie and Duque (2018) for a review),

Figure 10: Gender gap in the Effect of an increase in Sure Start coverage on probability of cause-specific hospitalizations



Note: The Figure shows the percentage point difference the estimated effect size of Sure Start on the probability of hospitalization between boys and girls. The difference in effect size between both genders is statistically significant at the 90% level at ages 11-12 for infections, age 15 for ACS and ages 11-15 for external. Table A.9 and Table A.10 display the original cause-specific point estimates and p-values by gender for ages 1-15.

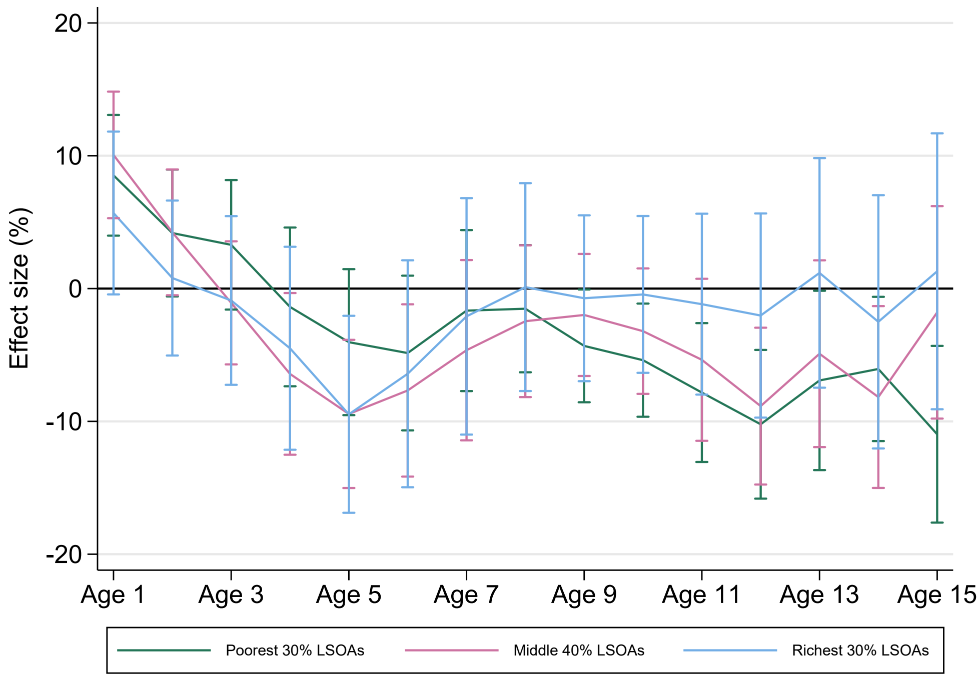
and we now turn to study whether the program's impacts varied by socioeconomic status.

Because we do not have information on family income or parental education in the hospitalization data, we rely on the level of deprivation of the neighborhood of residence.[37] In particular, we allow for heterogeneity of Sure Start effects for the three groups of neighborhoods (defined at the LSOA level): the 30% most deprived neighborhoods, the 30% least deprived neighborhoods, and those in the middle of the distribution of disadvantage. As Figure 11 and Table A.11 illustrate, the increase in admissions among infants detected for the whole sample in Figure 5 is driven by those residing in neighborhoods falling into the poorest 30% of the deprivation distribution. From ages 10 to 15, the drop in hospital admissions attributed to SS is even more concentrated in the areas

[37]To classify neighborhoods into these three groups, we use the 2004 Index of Multiple Deprivation (IMD). The IMD 2004 contains seven domains of deprivation: Income deprivation, Employment deprivation, Health deprivation and disability, Education, skills and training deprivation, Barriers to Housing and Services, Living environment deprivation and Crime. Each Domain contains a number of indicators, which are aggregated by principal component analysis. The IMD is a widely used measure of small area deprivation and its 2004 version was the measure used to classify areas into the different stages of the rollout of Sure Start Children's Centres.

with the highest levels of disadvantage, with imprecise impacts at the middle of the distribution (and zero effects at the top of the distribution).[38]

Figure 11: Effect of an increase in Sure Start coverage on any hospitalization, rescaled by baseline probability: Differences by area deprivation



Note: Figure shows coefficients from separate regressions for each outcome age, with Sure Start treatment interacted with the three disadvantage categories. Coefficients are rescaled by the deprivation-specific baseline (1996) mean for each age. Vertical bars indicate 90% confidence intervals.

The stronger impacts of Sure Start among more deprived neighborhoods could be a result of a number of different and non-mutually exclusive factors having to do with service quality, the quality of the counterfactual environment, and service take-up. On the one hand, there is ample evidence that disadvantaged children grow up in less safe and stimulating environments and that disadvantaged parents make less use of healthcare (Currie, 2006). Disadvantaged families may

[38] As discussed in section 2, the rollout of Sure Start started in the poorest areas and progressively expanded into richer areas. Given that we use hospitalization data until 2017, the sample we use to measure impacts on hospitalizations at age 15 only includes cohorts born up until 2003 (cohorts born until 2004 for age age 14, etc.). This means that there may be less variation in exposure among the least deprived cohorts than among the most deprived cohorts to identify the effects of Sure Start on hospitalizations during adolescence. We check whether this is the case by plotting the variation in each of the three subgroups in Appendix Figure A.7, which shows that there is still a lot of variation in exposures to Sure Start for relevant cohorts in the middle and richest neighborhoods.

therefore have had more scope to benefit from the information and services to support parents that Sure Start provided. The services offered to families in more deprived areas may also have been of higher quality or intensity than those offered in less deprived areas. As mentioned in section 2, Phase 1 and Phase 2 SSCCs (in the 30% most deprived areas) had more stringent requirements in terms of the qualifications of the childcare staff than Phase 3 SSCCs. If staff qualifications are indeed an important input for the production of high-quality childcare (Sylva et al., 2010), children in more deprived areas may have benefited from higher-quality childcare than those in less deprived areas.

Finally, the stronger impacts of Sure Start in deprived areas may also reflect a higher take-up of services by more disadvantaged families. Survey data on service offer and service take up presented earlier does suggest that this could be the case for parenting support services, which evidence presented so far suggests was a particularly important component of the program (see Figure A.8 and Sammons, Goff and Smith (2015)).

8 Conclusion and cost-benefit analysis

Robust evidence on the impacts of 'model' early childhood programs and of Head Start shows that early childhood interventions targeted at low-income children in the US can deliver substantial health benefits. Much remains to be known whether these impacts translate into less targeted and intensive programs, especially in contexts with more generous safety nets. The contribution of this paper is to show that a fully universal, scaled-up, area-based early childhood intervention can deliver significant and long-lasting health benefits, even in a context with free healthcare. We exploit a unique social experiment - the rollout of Sure Start, an area-based program offering health and non-health services to families with a child under 5 in England. We use administrative data on the universe of hospital admissions in publicly funded hospitals and unique data about when and where every Sure Start centers opened between 1999 and 2010. This paper presents the first robust evaluation of the causal impacts of increasing access to Sure Start throughout the history of the program.

We find that greater access to Sure Start increased hospitalizations during infancy, but subsequently reduced them during childhood and adolescence. Among infants, having access to an extra center per thousand children increased the probability of a hospitalization in the neighborhood cohort by 10% of the baseline at age 1. Once children turn 5 and stop being age-eligible to use Sure Start services, the overall impact on hospitalizations becomes consistently negative. Exposure to an additional center per thousand children under five averts hospital admissions by 8% by the end of primary school (age 11) and by 8.5% by age 15.

The profile of impacts on overall hospitalizations mask a lot of heterogeneity in the profiles of impacts on hospitalizations for specific causes. Our cause-specific results are consistent with Sure Start working through a number of mechanisms, including: strengthening children's immune systems, fostering children's behavioral and emotional development, and improving parenting practices and the safety of the home environment. Overall, these results speak to the importance of integrating health services with early education and childcare and parenting services to promote child development in a holistic way. The persistence of impacts into middle childhood and adolescence is driven by the reduction in hospital admissions for injuries and mental health. We argue that these impacts are likely to reflect an improvement in children's behavioral and emotional development, which speaks the importance of cross-productivities between different domains of development (Cunha, Heckman and Schennach, 2010; Cunha et al., 2006).

A simple cost-benefit analysis shows that the financial benefits from reduced hospitalizations offset approximately 31% of the provision cost of Sure Start (see Appendix F). While this figure should be interpreted as a lower bound of the program benefits because the program is likely to have affected other outcomes, our results suggest that the overall effectiveness of Sure Start intervention might have come despite, rather than because of, its universality. Indeed, impacts are concentrated in the 30% most disadvantaged neighborhoods. In line with evaluations of other universal preschool programs, some form of targeting might therefore have been desirable to reach a higher value for money. The case of Sure Start however does suggest that area-based targeting may be an attractive alternative to individual means-testing, as it could potentially reduce individual stigma associated with attending a targeted program. The results presented in this paper are relevant to current proposals to expand investments in early childhood interventions in the US and continental Europe as a way to decrease health expenditures and reducing health inequalities.

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The Health Effects of Universal Early Childhood Interventions: Evidence from Sure Start

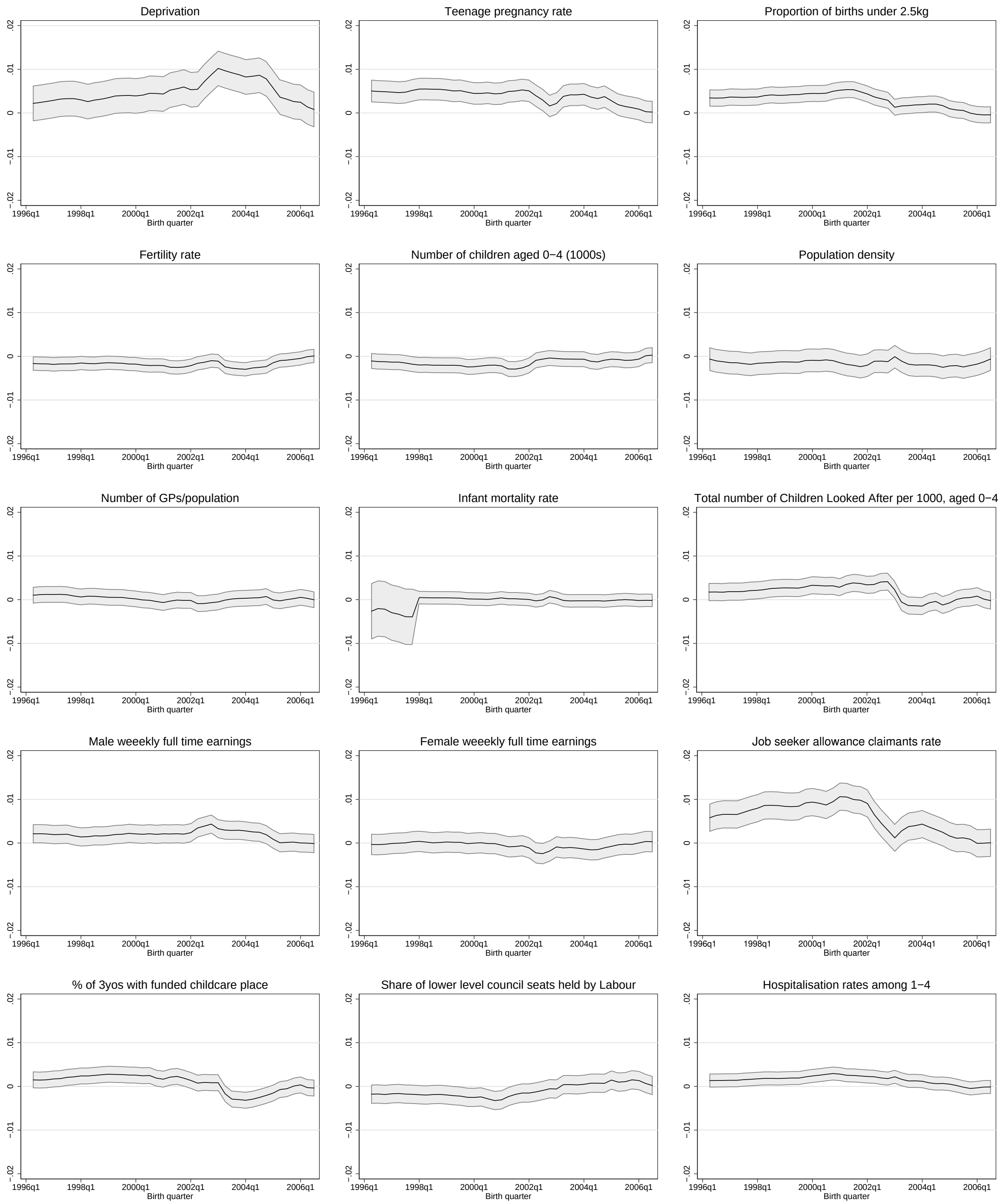
Sarah Cattan, Gabriella Conti, Christine Farquharson, Rita Ginja and Maud Pecher

November 15, 2021

MATERIAL FOR ONLINE APPENDIX

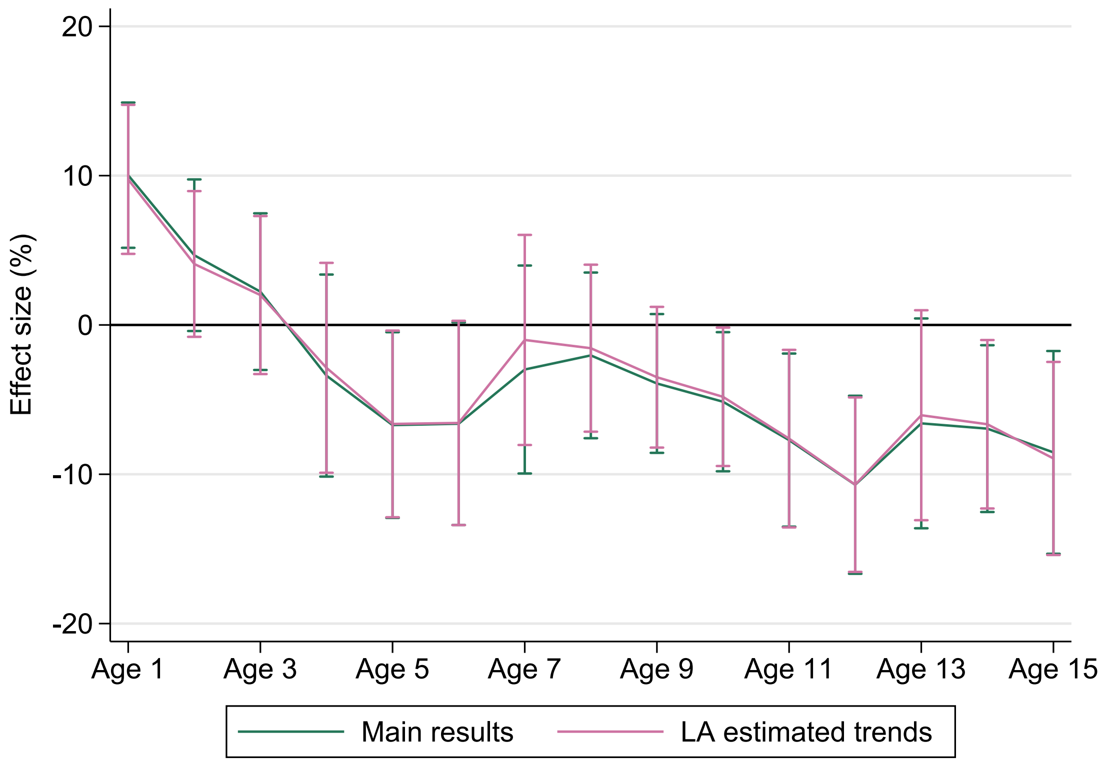
A Appendix Tables and Figures

Figure A.1: Regression of the change in Sure Start coverage on baseline Local Authority characteristics, 1998 - 2006



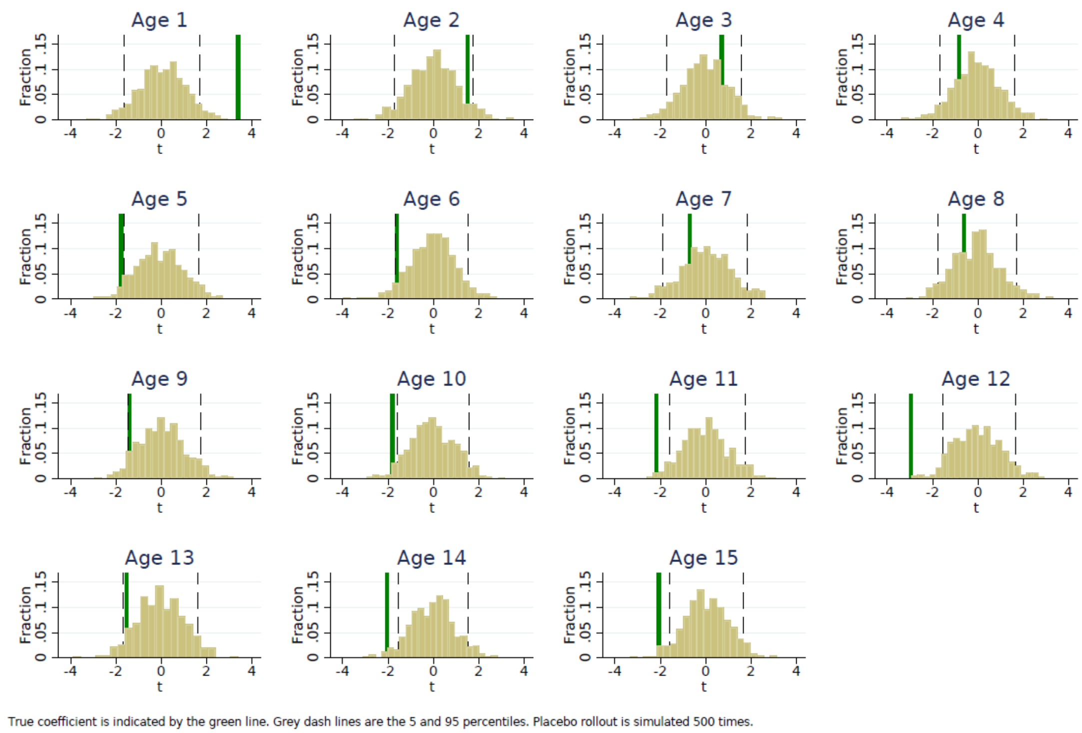
Note: These figures plot the coefficients obtained from a regression of the changes in Sure Start coverage on Local Authority specific baseline characteristics (measured in 1998) interacted with quarter-year dummies, controlling for Local Authority fixed effects. The figures plot the interaction terms for each variable.

Figure A.2: Effect of an increase in Sure Start coverage on probability of any hospitalization, re-scaled by baseline probability: Baseline estimates and controlling for linear local authority trends



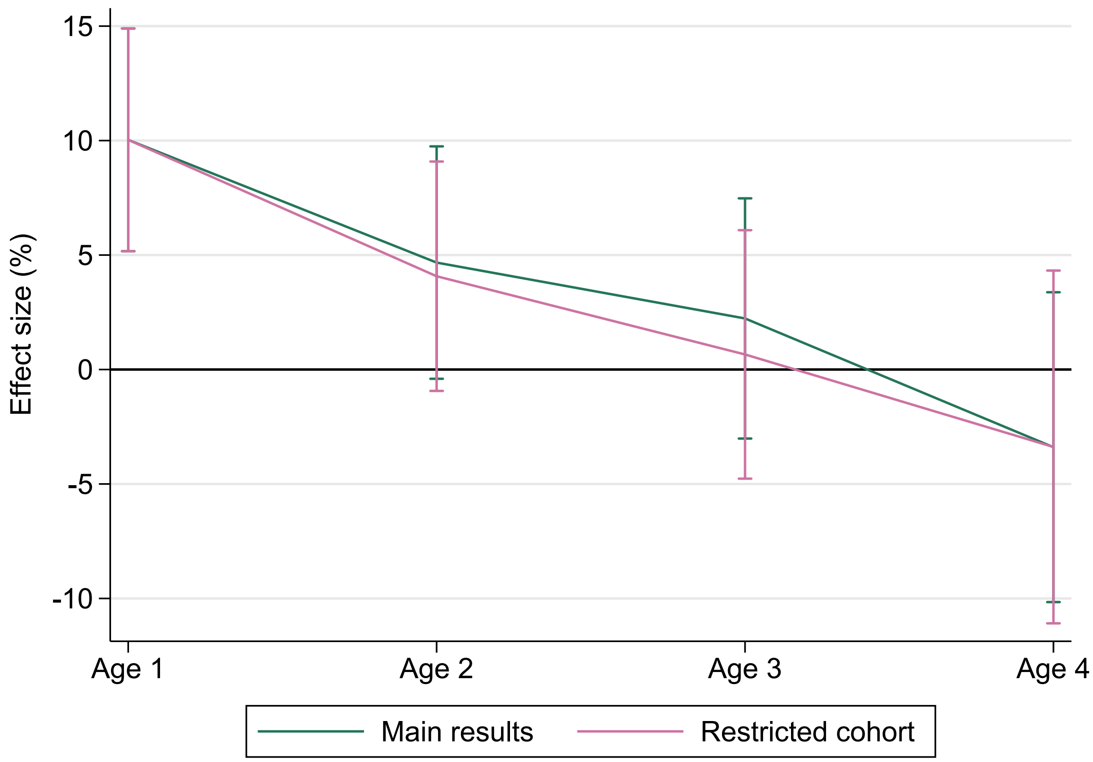
Note: Effect sizes are constructed by re-scaling the estimates by the pre-Sure Start (1996) baseline probability of a hospitalization at each age. Vertical bars indicate 90% confidence intervals. Results with LAD estimated trends additionally control for a local authority-specific linear time trend, estimated based on pre-treatment hospitalization data for each LA. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

Figure A.3: Distribution of t-statistics from random permutation inference exercise



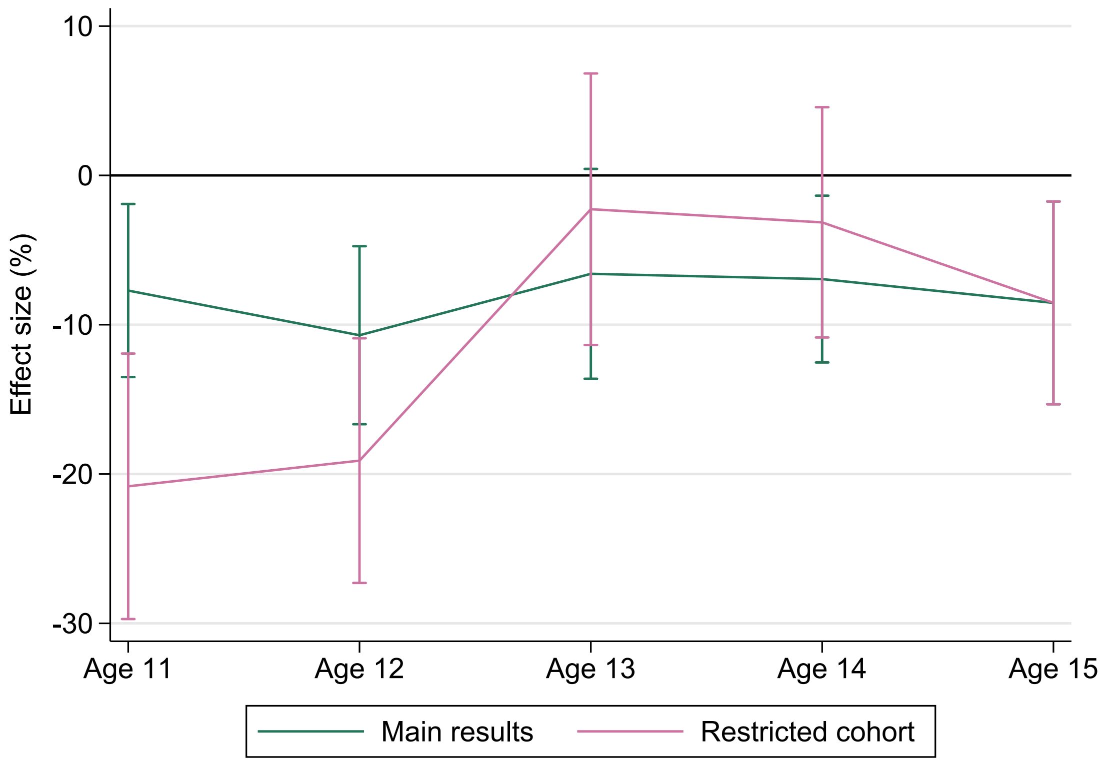
Note: Graphs show histograms of the distribution of t-statistics for the impact of Sure Start coverage, estimated for 500 random permutations of treatment (in beige). The 5th and 95th percentiles are indicated by the grey vertical lines and the true t statistic is shown in green. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

Figure A.4: Effect of an increase in Sure Start coverage on probability of any hospitalization, re-scaled by baseline probability: Baseline estimates and estimates on a common cohort for 1- to 4-year-olds



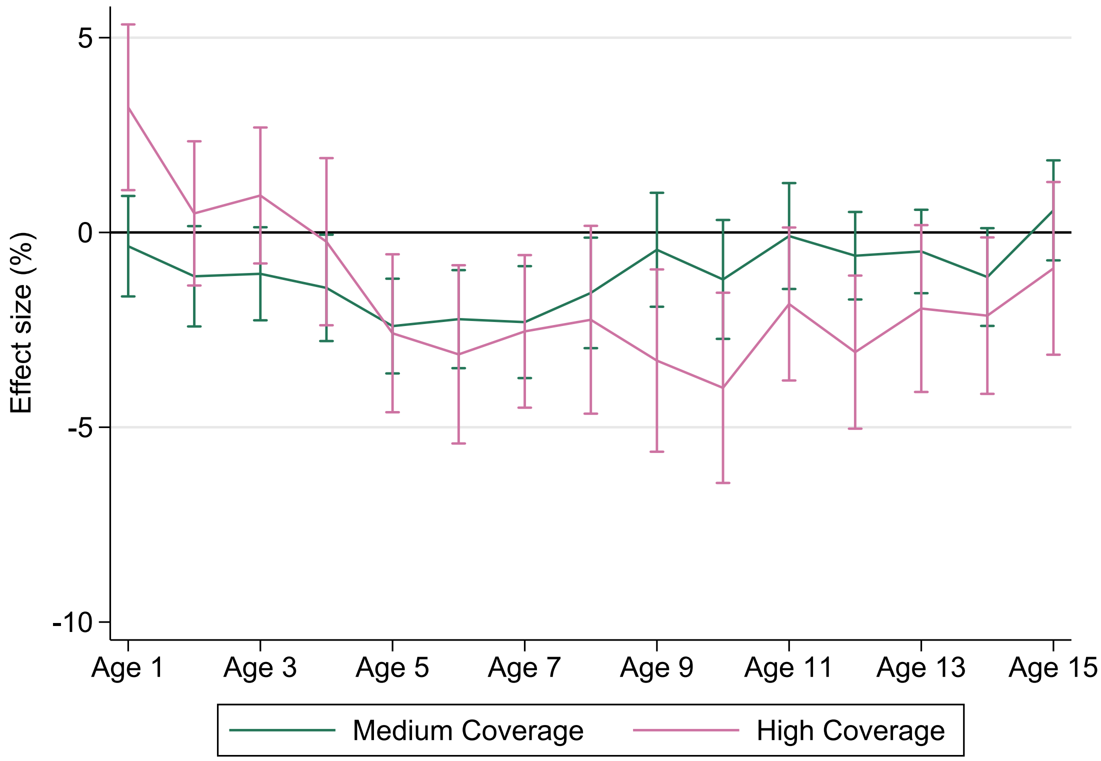
Note: The figure shows coefficients from separate regressions for each outcome age. Coefficients are re-scaled by the baseline (1996) mean for each age. Main results are estimated on cohorts as listed in Table 2. Common cohort results use a cohort of children born between April 1996 and December 2006. Vertical bars indicate 90% confidence intervals. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

Figure A.5: Effect of an increase in Sure Start coverage on probability of any hospitalization, re-scaled by baseline probability: Baseline estimates and estimates on a common cohort for 1- to 4-year-olds



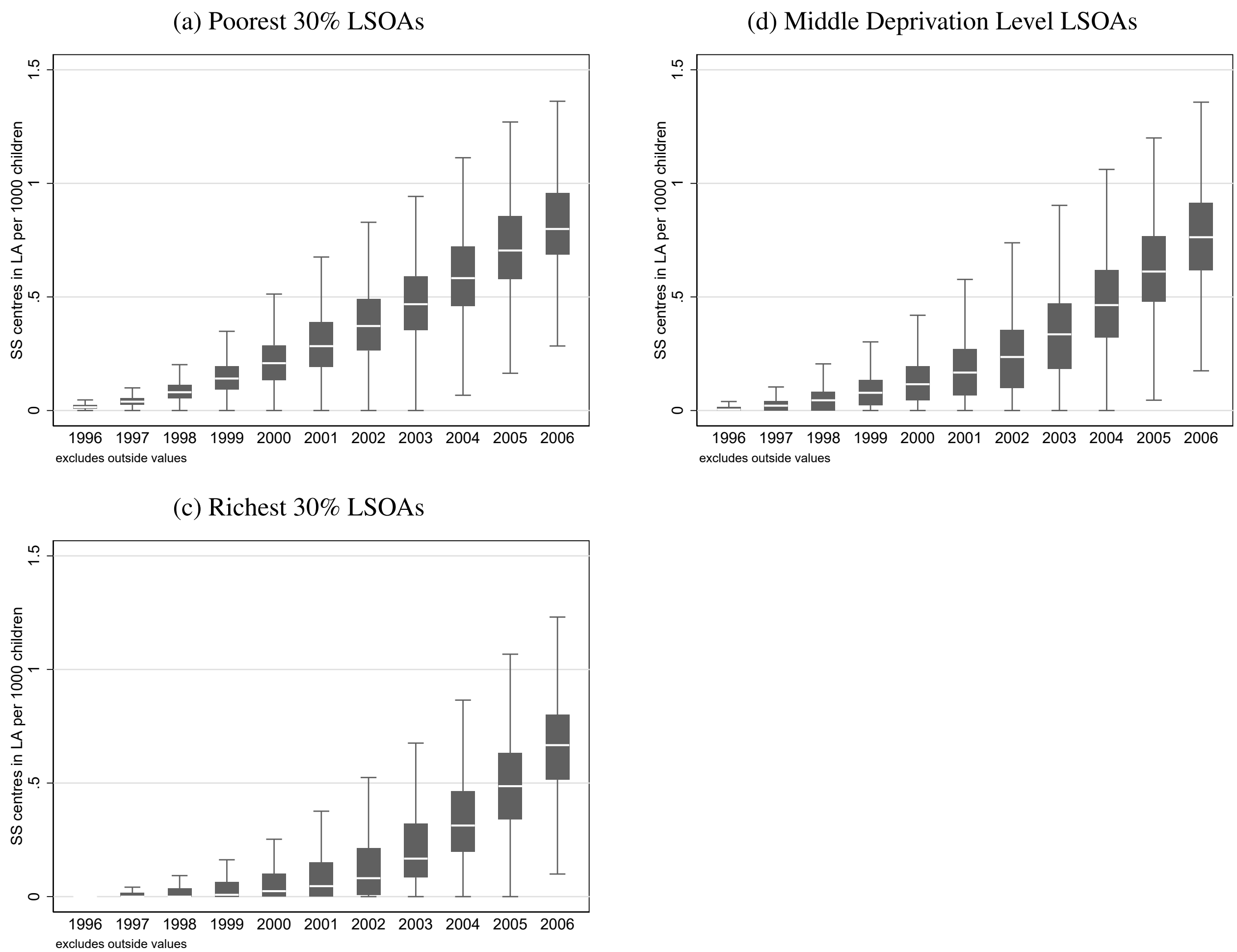
Note: The figure shows coefficients from separate regressions for each outcome age. Coefficients are re-scaled by the baseline (1996) mean for each age. Main results are estimated on cohorts as listed in Table 2. Common cohort results use a cohort of children born between January 1993 and March 2002. Vertical bars indicate 90% confidence intervals. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

Figure A.6: Effect of an increase in Sure Start coverage on probability of any hospitalization, re-scaled by baseline probability: Non-linear estimates



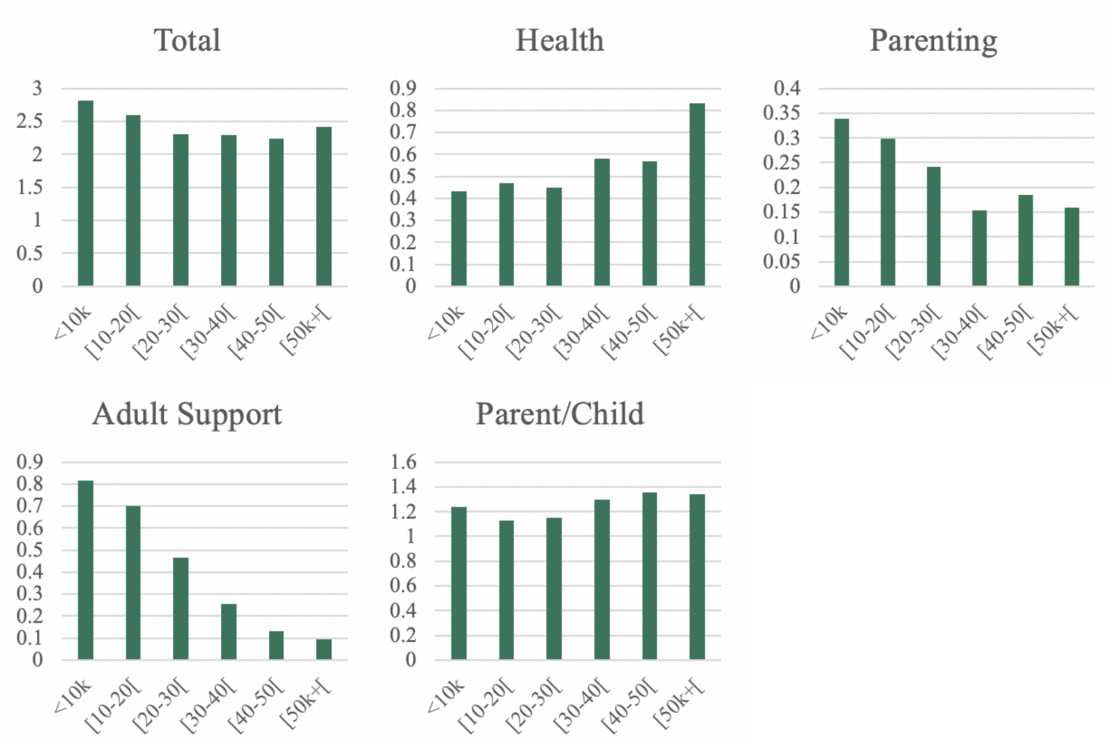
Note: The figure shows coefficients from separate regressions for each outcome age. Treatment is a pair of indicators for whether the cell experienced medium treatment (strictly positive coverage, but less than 0.25 centers per thousand children) or high treatment (more than 0.25 centers per thousand children). The omitted category is low treatment (untreated). Coefficients are re-scaled by the baseline (1996) mean for each age. Results marked with a star are significant at the 5% level. Source: Authors' calculations using data from the Hospital Episode Statistics inpatient data (1997-2017) and the Department for Education's data on the rollout of Sure Start.

Figure A.7: Average coverage over the first 60 months of life, by local authority and month and year of birth: By level of deprivation



Note: The figure presents the average Sure Start coverage (centers 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 per LA according to the level of deprivation in the LSOA of residence in 2004. Source: Authors' calculations using data from the Department for Education and ONS population estimates.

Figure A.8: Hours spent per week at different Sure Start services by family income, 2011



Note: The figure is based on information collected by the Evaluation of Children's centers in England (ECCE) in 2011 on hours spent per week by families using different services.

Table A.1: Effect of an increase in Sure Start coverage on probability of hospitalization for any cause: Robustness to inclusion of linear trends in baseline characteristics

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
Panel A: Main Specification															
SS Cov	0.0256***	0.0095	0.0040	-0.0057	-0.0109*	-0.0095	-0.0038	-0.0024	-0.0044	-0.0055*	-0.0084**	-0.0125***	-0.0081	-0.0091**	-0.0120**
	(0.0075)	(0.0063)	(0.0057)	(0.0069)	(0.0061)	(0.0059)	(0.0053)	(0.0039)	(0.0032)	(0.0031)	(0.0038)	(0.0042)	(0.0052)	(0.0045)	(0.0058)
Baseline mean	0.2552	0.2044	0.1791	0.1687	0.1623	0.1438	0.1260	0.1160	0.1125	0.1078	0.1089	0.1172	0.1229	0.1311	0.1410
Panel B: controlling for conception rate only															
SS Cov	0.0308***	0.0156**	0.0043	-0.0104	-0.0137*	-0.0105	-0.0054	-0.0042	-0.0064	-0.0059	-0.0096**	-0.0121**	-0.0099	-0.0097	-0.0079
	(0.0096)	(0.0073)	(0.0070)	(0.0086)	(0.0070)	(0.0070)	(0.0064)	(0.0048)	(0.0039)	(0.0038)	(0.0046)	(0.0053)	(0.0067)	(0.0064)	(0.0083)
Panel C: controlling for deprivation (ild 1998) dummies only															
SS Cov	0.0196**	0.0079	-0.0003	-0.0088	-0.0125*	-0.0088	-0.0027	-0.0013	-0.0016	-0.0023	-0.0046	-0.0078*	-0.0043	-0.0069	-0.0041
	(0.0077)	(0.0063)	(0.0059)	(0.0074)	(0.0065)	(0.0064)	(0.0059)	(0.0044)	(0.0035)	(0.0032)	(0.0041)	(0.0042)	(0.0053)	(0.0050)	(0.0062)
Panel D: controlling for low birth weight only															
SS Cov	0.0191**	0.0030	-0.0027	-0.0149**	-0.0186***	-0.0154**	-0.0089	-0.0073*	-0.0073**	-0.0082**	-0.0106**	-0.0144***	-0.0101*	-0.0122**	-0.0106
	(0.0089)	(0.0072)	(0.0063)	(0.0073)	(0.0063)	(0.0062)	(0.0056)	(0.0042)	(0.0035)	(0.0034)	(0.0042)	(0.0045)	(0.0058)	(0.0050)	(0.0069)
Baseline mean	0.2552	0.2044	0.1791	0.1687	0.1623	0.1438	0.1260	0.1160	0.1125	0.1078	0.1089	0.1172	0.1229	0.1311	0.1410
N	2822176	3084704	3347232	3609760	3675392	3675392	3675392	3675392	3675392	3675392	3478496	3215968	2953440	2690912	2428384

Note: See notes to Table 2. Panels B to D expand the baseline model to include also linear trends on the LA characteristics that were the official guidelines of rollout of Sure Start measures at the baseline (1998). *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Table A.2: Effect of an increase in Sure Start coverage on probability of hospitalization for congenital chromosomal defects between 2 and 11 months

	(1) Coverage at birth	(2) Avg. coverage ages 0-4
SS coverage	0.0011	0.0006
	(0.0011)	(0.0010)
N	2,625,280	2,625,280
Baseline mean	0.0237	0.0237
Earliest cohort	Apr. 1997	Apr. 1997
Latest cohort	Dec. 2006	Dec. 2006

Note: See notes to Table 2. The first column defines Sure Start treatment based on the number of centers per thousand children in the LA at the time of the child's birth. The second column uses the average coverage over the first five years of life, as we use in our main results (note that this means some treatment postdates the outcome, which is measured between 2 and 11 months). *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Table A.3: Comparison of binary treatment effect estimates using the TWFE and Borusyak, Jaravel and Spiess (2021) estimators

Age of admission	Estimator	$1(SS_{dq} > 0))$		$1(SS_{dq} > 0.1)$		$1(SS_{dq} > 0.25)$	
Age 1	TWFE	-0.001	(0.002)	0.005**	(0.002)	0.009***	(0.003)
	BJS	0.006**	(0.003)	0.008***	(0.003)	0.011***	(0.003)
Age 2	TWFE	-0.002	(0.002)	0.000	(0.002)	0.003*	(0.002)
	BJS	0.006**	(0.003)	0.005*	(0.002)	0.005**	(0.002)
Age 3	TWFE	-0.002*	(0.001)	0.001	(0.001)	0.004***	(0.001)
	BJS	0.006**	(0.002)	0.000	(0.003)	0.004	(0.002)
Age 4	TWFE	-0.003*	(0.001)	0.001	(0.001)	0.002	(0.002)
	BJS	0.002	(0.002)	0.003	(0.002)	0.002	(0.003)
Age 5	TWFE	-0.004***	(0.001)	0.001	(0.001)	0.000	(0.002)
	BJS	0.002	(0.002)	0.004**	(0.002)	-0.002	(0.002)
Age 6	TWFE	-0.003***	(0.001)	0.001	(0.001)	-0.001	(0.002)
	BJS	0.000	(0.002)	0.001	(0.003)	0.001	(0.003)
Age 7	TWFE	-0.003**	(0.001)	0.002	(0.001)	0.000	(0.001)
	BJS	0.001	(0.002)	0.003	(0.003)	0.002	(0.004)
Age 8	TWFE	-0.002	(0.001)	0.002	(0.001)	-0.001	(0.001)
	BJS	-0.001	(0.002)	0.001	(0.002)	-0.001	(0.002)
Age 9	TWFE	-0.000	(0.001)	-0.000	(0.001)	-0.003**	(0.001)
	BJS	0.004**	(0.002)	-0.001	(0.001)	-0.003**	(0.001)
Age 10	TWFE	-0.001	(0.001)	-0.001*	(0.001)	-0.003**	(0.001)
	BJS	0.000	(0.003)	-0.002	(0.001)	-0.002	(0.002)
Age 11	TWFE	0.000	(0.001)	-0.003***	(0.001)	-0.002	(0.001)
	BJS	0.002	(0.002)	0.002	(0.002)	-0.001	(0.001)
Age 12	TWFE	-0.000	(0.001)	-0.004***	(0.001)	-0.003**	(0.001)
	BJS	0.000	(0.003)	-0.003*	(0.001)	-0.003**	(0.001)
Age 13	TWFE	-0.000	(0.001)	-0.001	(0.001)	-0.002	(0.001)
	BJS	-0.004**	(0.002)	-0.002	(0.001)	-0.002*	(0.001)
Age 14	TWFE	-0.001	(0.001)	-0.001	(0.001)	-0.001	(0.001)
	BJS	-0.003**	(0.001)	-0.002	(0.001)	-0.002	(0.001)
Age 15	TWFE	0.001	(0.001)	-0.001	(0.001)	-0.002	(0.001)
	BJS	-0.001	(0.001)	-0.003**	(0.001)	-0.003**	(0.001)

Note: This table reports the coefficients associated with a binary measure of Sure Start coverage estimated in the TWFE model and using the Borusyak, Jaravel and Spiess (2021) estimator. We consider three different definitions of this binary measure of Sure Start coverage: an indicator for whether SS_{dq} is above 0 (results reported in column 3 of the table), an indicator for whether SS_{dq} is above 0.1 (column 4) and an indicator for whether it is above 0.25 (column 5). With both estimators, we control for a gender dummy and the number of individuals of age a when the dependent variable measures hospitalizations at age a. The TWFE model also controls for neighborhood (defined at the LSOA level) and cohort (defined as the year-quarter of birth) level. *, ** and *** indicate significance at the 10%, 5% and 1% level respectively.

Table A.4: Effect of an increase in Sure Start coverage on probability of hospitalization: By admission route

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
Panel A: Emergency admissions															
SS Cov	0.0276***	0.0132**	0.0063	-0.0029	-0.0060	-0.0065*	-0.0042	-0.0037	-0.0056**	-0.0057**	-0.0064*	-0.0101***	-0.0045	-0.0069*	-0.0079
	(0.0082)+++	(0.0066)	(0.0056)	(0.0057)	(0.0042)	(0.0037)	(0.0028)	(0.0026)	(0.0025)	(0.0028)	(0.0034)	(0.0038)++	(0.0043)	(0.0040)	(0.0052)
Baseline mean	0.2118	0.1531	0.1132	0.0897	0.0789	0.0677	0.0623	0.0627	0.0646	0.0639	0.0639	0.0681	0.0692	0.0753	0.0817
Panel B: Elective admissions															
SS Cov	0.0019	-0.0039	-0.0019	-0.0041	-0.0055	-0.0049	0.0001	0.0007	-0.0009	-0.0009	-0.0033	-0.0044*	-0.0042	-0.0018	-0.0039
	(0.0031)	(0.0025)	(0.0029)	(0.0040)	(0.0048)	(0.0048)	(0.0046)	(0.0032)	(0.0026)	(0.0020)	(0.0021)	(0.0023)	(0.0032)	(0.0035)	(0.0038)
Baseline mean	0.0741	0.0753	0.0875	0.0982	0.1014	0.0909	0.0756	0.0641	0.0583	0.0539	0.0556	0.0606	0.0664	0.0697	0.0756
N	2,822,176	3,084,704	3,347,232	3,609,760	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,478,496	3,215,968	2,953,440	2,690,912	2,428,384
Earliest cohort	Apr.96	Apr.95	Apr.94	Apr.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93
Latest cohort	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Mar.06	Mar.05	Mar.04	Mar.03	Mar.02

Note: See notes to Table 2. Emergency admission routes include emergency room services or emergency referrals from GPs; elective admissions include pre-planned inpatient care. *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively; +, ++ and + + + indicate significance at the 10%, 5% and 1% level, respectively, after adjusting inference following the procedure in algorithms 4.1 and 4.2 of Romano and Wolf (2005).

Table A.5: Effect of an increase in Sure Start coverage on probability of hospitalization due to specific conditions

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
Panel A: Any hospitalisation for infectious illnesses															
SS Cov	0.0202***	0.0074	0.0015	-0.0035	-0.0071**	-0.0067***	-0.0017	-0.0016	-0.0006	-0.0005	-0.0016	-0.0020	-0.0007	-0.0011	-0.0015
	(0.0071)++	(0.0048)	(0.0037)	(0.0039)	(0.0029)++	(0.0021)++	(0.0015)	(0.0011)	(0.0012)	(0.0013)	(0.0012)	(0.0014)	(0.0015)	(0.0015)	(0.0017)
Baseline mean	0.1208	0.0808	0.0635	0.0494	0.0466	0.0361	0.0275	0.0231	0.0225	0.0187	0.0160	0.0159	0.0145	0.0136	0.0147
Panel B: Any hospitalisation for ACS related cause															
SS Cov	0.0123**	0.0072**	0.0038*	0.0021	0.0007	-0.0001	-0.0003	-0.0003	-0.0015**	-0.0013*	-0.0016*	-0.0016	-0.0008	-0.0005	-0.0013
	(0.0051)++	(0.0031)+	(0.0022)	(0.0017)	(0.0013)	(0.0010)	(0.0007)	(0.0007)	(0.0008)	(0.0008)	(0.0008)	(0.0010)	(0.0011)	(0.0009)	(0.0012)
Baseline mean	0.0609	0.0375	0.0235	0.0162	0.0129	0.0101	0.0089	0.0093	0.0092	0.0086	0.0076	0.0075	0.0072	0.0071	0.0079
Panel C: Any hospitalisation for an external cause															
SS Cov	-0.0041**	-0.0052***	-0.0057***	-0.0062***	-0.0056***	-0.0056***	-0.0040***	-0.0024**	-0.0017*	-0.0036***	-0.0023*	-0.0018	-0.0001	-0.0007	-0.0029
	(0.0019)+	(0.0018)+++	(0.0016)+++	(0.0015)+++	(0.0013)+++	(0.0012)+++	(0.0010)+++	(0.0010)++	(0.0010)	(0.0010)+++	(0.0013)	(0.0014)	(0.0017)	(0.0022)	(0.0029)
Baseline mean	0.0397	0.0395	0.0305	0.0255	0.0257	0.0245	0.0227	0.0214	0.0207	0.0218	0.0231	0.0260	0.0291	0.0335	0.0357
N	2,822,176	3,084,704	3,347,232	3,609,760	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,478,496	3,215,968	2,953,440	2,690,912	2,428,384
Earliest cohort	Apr.96	Apr.95	Apr.94	Apr.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93
Latest cohort	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Mar.06	Mar.05	Mar.04	Mar.03	Mar.02

Note: See notes to Table 2. Cause-specific results are based on the primary diagnosis at the time of admission. External admissions include ICD-10 codes in groups S, T, V and Y. *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively; +, ++ and + + + indicate significance at the 10%, 5% and 1% level, respectively, after adjusting inference following the procedure in algorithms 4.1 and 4.2 of Romano and Wolf (2005).

Table A.6: Effect of an increase in Sure Start coverage on probability of hospitalization due to specific conditions

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
Panel A: Any hospitalisation for poisoning															
SS Cov	-0.0033***	-0.0025**	-0.0024**	-0.0008	-0.0002	-0.0001	-0.0004	0.0003	0.0006*	-0.0003	0.0003	0.0006	0.0004	0.0015	-0.0009
	(0.0011)+++	(0.0012)++	(0.0010)++	(0.0007)	(0.0006)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0005)	(0.0009)	(0.0014)	(0.0019)
			2.4												
Baseline mean	0.0187	0.0188	0.0114	0.0070	0.0058	0.0046	0.0039	0.0034	0.0031	0.0028	0.0031	0.0041	0.0065	0.0110	0.0139
Panel B: Any hospitalisation for injuries															
SS Cov	-0.0010	-0.0030**	-0.0033***	-0.0056***	-0.0054***	-0.0055***	-0.0035***	-0.0026***	-0.0022***	-0.0033***	-0.0026**	-0.0024*	-0.0006	-0.0018	-0.0027
	(0.0012)	(0.0012)++	(0.0010)+++	(0.0013)+++	(0.0011)+++	(0.0010)+++	(0.0009)+++	(0.0009)+++	(0.0009)+++	(0.0010)+++	(0.0013)	(0.0014)	(0.0014)	(0.0018)	(0.0022)
Baseline mean	0.0216	0.0214	0.0194	0.0187	0.0201	0.0200	0.0189	0.0181	0.0178	0.0191	0.0202	0.0221	0.0230	0.0232	0.0227
Panel C: Any hospitalisation for fractures															
SS Cov	-0.0007*	-0.0009**	-0.0007*	-0.0008	-0.0016***	-0.0028***	-0.0009*	-0.0012**	-0.0013***	-0.0013***	-0.0018***	-0.0010	0.0005	0.0008	0.0005
	(0.0004)	(0.0004)	(0.0004)	(0.0006)	(0.0005)+++	(0.0006)+++	(0.0005)	(0.0005)+	(0.0005)+++	(0.0005)+++	(0.0007)+++	(0.0007)	(0.0009)	(0.0010)	(0.0013)
Baseline mean	0.0054	0.0065	0.0068	0.0080	0.0101	0.0111	0.0108	0.0100	0.0102	0.0115	0.0121	0.0130	0.0138	0.0134	0.0119
Panel D: Any hospitalisation for head injuries															
SS Cov	-0.0007	-0.0016*	-0.0010	-0.0028***	-0.0019***	-0.0005	-0.0006*	-0.0007	-0.0006**	-0.0006*	0.0000	-0.0005	-0.0004	-0.0011	-0.0017*
	(0.0008)	(0.0008)	(0.0006)	(0.0007)+++	(0.0006)+++	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0004)	(0.0005)	(0.0005)	(0.0007)	(0.0009)
Baseline mean	0.0084	0.0079	0.0066	0.0052	0.0046	0.0037	0.0031	0.0029	0.0028	0.0029	0.0031	0.0038	0.0044	0.0049	0.0051
N	2,822,176	3,084,704	3,347,232	3,609,760	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,478,496	3,215,968	2,953,440	2,690,912	2,428,384
Earliest cohort	Apr.96	Apr.95	Apr.94	Apr.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93
Latest cohort	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Mar.06	Mar.05	Mar.04	Mar.03	Mar.02

Note: See notes to Table 2. Cause-specific results are based on the primary diagnosis at the time of admission. External admissions include ICD-10 codes in groups S, T, V and Y. Poisonings include ICD-10 codes T15-T98; injuries include codes S00-T14; fractures include codes S02, S12, S22, S32, S42, S52, S62, S72, S82, S92, T02, T10, T14; and head injuries include codes S00-S09. *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively; +, ++ and + + + indicate significance at the 10%, 5% and 1% level, respectively, after adjusting inference following the procedure in algorithms 4.1 and 4.2 of Romano and Wolf (2005).

Table A.7: Effect of an increase in Sure Start coverage on probability of hospitalization for mental health

	(1) Age 11	(2) Age 12	(3) Age 13	(4) Age 14	(5) Age 15
SS Cov	-0.0003	-0.0007**	-0.0016***	-0.0019**	-0.0010
	(0.0002)	(0.0003)++	(0.0005)+++	(0.0009)++	(0.0013)
Baseline mean	0.0007	0.0013	0.0026	0.0042	0.0049
N	3,478,496	3,215,968	2,953,440	2,690,912	2,428,384
Earliest cohort	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93
Latest cohort	Mar.06	Mar.05	Mar.04	Mar.03	Mar.02

Note: See notes to Table 2. Cause-specific results are based on the primary diagnosis at the time of admission. Mental health admissions are based on ICD-10 group F. Results for younger ages are omitted because of very low prevalence. *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively; +, ++ and +++ indicate significance at the 10%, 5% and 1% level, respectively, after adjusting inference following the procedure in algorithms 4.1 and 4.2 of Romano and Wolf (2005).

Table A.8: Effect of an increase in Sure Start coverage on probability of hospitalization for any cause, by gender

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
SS Cov: Boys δ_B	0.0291***	0.0102	0.0034	-0.0105	-0.0138**	-0.0126**	-0.0053	-0.0053	-0.0062*	-0.0060*	-0.0092**	-0.0153***	-0.0158***	-0.0212***	-0.0249***
	(0.0081)+++	(0.0066)	(0.0058)	(0.0073)	(0.0063)+	(0.0061)	(0.0054)	(0.0039)	(0.0033)	(0.0031)	(0.0041)++	(0.0046)+++	(0.0058)++	(0.0053)+++	(0.0065)+++
SS Cov: Girls δ_G	0.0221***	0.0089	0.0046	-0.0010	-0.0080	-0.0064	-0.0022	0.0006	-0.0026	-0.0051	-0.0077**	-0.0098**	-0.0004	0.0030	0.0009
	(0.0072)+++	(0.0062)	(0.0058)	(0.0067)	(0.0061)	(0.0059)	(0.0053)	(0.0041)	(0.0032)	(0.0031)	(0.0038)	(0.0041)++	(0.0050)	(0.0043)	(0.0067)
p-values:															
$H_A : \delta_B \neq \delta_G$	0.016	0.616	0.588	0.000	0.000	0.000	0.024	0.000	0.003	0.466	0.311	0.010	0.000	0.000	0.000
H_A : Diff. effect size	0.798	0.592	0.384	0.004	0.253	0.106	0.247	0.003	0.062	0.717	0.746	0.184	0.000	0.000	0.000
Baseline mean:															
Boys	0.2863	0.2348	0.2099	0.1982	0.1871	0.1652	0.1419	0.1302	0.1255	0.1223	0.1220	0.1300	0.1283	0.1292	0.1320
Girls	0.2241	0.1739	0.1482	0.1392	0.1375	0.1225	0.1101	0.1018	0.0995	0.0933	0.0959	0.1044	0.1174	0.1331	0.1499
N	2,822,176	3,084,704	3,347,232	3,609,760	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,478,496	3,215,968	2,953,440	2,690,912	2,428,384
Earliest cohort	Apr.96	Apr.95	Apr.94	Apr.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93
Latest cohort	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Mar.06	Mar.05	Mar.04	Mar.03	Mar.02

Note: See notes to Table 2. Each regression interacts Sure Start coverage with indicators for whether the cell contains boys or girls (coverage on its own is not included in this model.) 'Difference p-value' tests the equality of the coefficients for coverage interacted with boys and with girls. 'Effect size difference p-value' tests the equality of the effect size (coefficients weighted by subgroup baseline mean) for coverage interacted with boys and with girls. *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively; $+$, $++$ and $+++$ indicate significance at the 10%, 5% and 1% level, respectively, after adjusting inference following the procedure in algorithms 4.1 and 4.2 of Romano and Wolf (2005).

Table A.9: Effect of an increase in Sure Start coverage on probability of hospitalization for specific conditions, by gender

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
Panel A: Any hospitalisation for infectious illnesses															
SS Cov: Boys δ_B	0.0255***	0.0102**	0.0052	-0.0020	-0.0056*	-0.0058**	-0.0002	-0.0009	0.0003	0.0002	-0.0005	-0.0005	-0.0003	0.0003	-0.0004
	(0.0078)	(0.0052)	(0.0040)	(0.0043)	(0.0030)	(0.0023)	(0.0015)	(0.0011)	(0.0013)	(0.0013)	(0.0013)	(0.0016)	(0.0016)	(0.0017)	(0.0019)
SS Cov: Girls δ_G	0.0150**	0.0045	-0.0022	-0.0049	-0.0087***	-0.0077***	-0.0032**	-0.0023**	-0.0015	-0.0012	-0.0026**	-0.0035***	-0.0011	-0.0026	-0.0027
	(0.0065)	(0.0046)	(0.0035)	(0.0036)	(0.0028)	(0.0021)	(0.0015)	(0.0011)	(0.0012)	(0.0013)	(0.0012)	(0.0013)	(0.0016)	(0.0016)	(0.0019)
p-values:															
$H_A : \delta_B \neq \delta_G$	0.000	0.001	0.000	0.016	0.000	0.011	0.000	0.015	0.002	0.015	0.001	0.000	0.383	0.034	0.165
H_A : Diff. effect size	0.086	0.069	0.000	0.002	0.000	0.000	0.000	0.007	0.001	0.013	0.001	0.000	0.425	0.062	0.299
Baseline mean:															
Boys	0.1387	0.0944	0.0747	0.0585	0.0538	0.0404	0.0289	0.0245	0.0232	0.0194	0.0161	0.0155	0.0138	0.0121	0.0123
Girls	0.1029	0.0672	0.0522	0.0403	0.0394	0.0319	0.0260	0.0216	0.0218	0.0181	0.0160	0.0163	0.0152	0.0152	0.0172
Panel B: Any hospitalisation for ACS related cause															
SS Cov: Boys δ_B	0.0138**	0.0086***	0.0049**	0.0030*	0.0014	0.0003	0.0001	-0.0000	-0.0013	-0.0010	-0.0016*	-0.0015	-0.0009	-0.0012	-0.0028**
	(0.0055)	(0.0033)	(0.0023)	(0.0018)	(0.0014)	(0.0011)	(0.0008)	(0.0007)	(0.0008)	(0.0008)	(0.0009)	(0.0011)	(0.0012)	(0.0010)	(0.0013)
SS Cov: Girls δ_G	0.0107**	0.0058*	0.0027	0.0012	0.0000	-0.0005	-0.0008	-0.0007	-0.0018**	-0.0016**	-0.0015*	-0.0017*	-0.0006	0.0003	0.0001
	(0.0049)	(0.0031)	(0.0021)	(0.0016)	(0.0013)	(0.0009)	(0.0007)	(0.0007)	(0.0007)	(0.0008)	(0.0008)	(0.0010)	(0.0012)	(0.0010)	(0.0014)
p-values:															
$H_A : \delta_B \neq \delta_G$	0.106	0.025	0.007	0.002	0.003	0.128	0.025	0.109	0.176	0.076	0.785	0.741	0.557	0.061	0.008
H_A : Diff. effect size	0.871	0.681	0.277	0.045	0.024	0.119	0.016	0.087	0.042	0.010	0.578	0.368	0.576	0.052	0.003
Baseline mean:															
Boys	0.0679	0.0432	0.0271	0.0188	0.0148	0.0115	0.0099	0.0102	0.0099	0.0095	0.0083	0.0082	0.0073	0.0066	0.0069
Girls	0.0538	0.0318	0.0198	0.0137	0.0110	0.0088	0.0080	0.0085	0.0085	0.0076	0.0068	0.0069	0.0071	0.0075	0.0089
Panel C: Any hospitalisation for External related cause															
SS Cov: Boys δ_B	-0.0029	-0.0039**	-0.0044**	-0.0064***	-0.0059***	-0.0060***	-0.0039***	-0.0037***	-0.0031***	-0.0042***	-0.0046***	-0.0060***	-0.0097***	-0.0135***	-0.0204***
	(0.0021)	(0.0020)	(0.0018)	(0.0016)	(0.0014)	(0.0012)	(0.0011)	(0.0010)	(0.0010)	(0.0011)	(0.0014)	(0.0017)	(0.0019)	(0.0026)	(0.0033)
SS Cov: Girls δ_G	-0.0052***	-0.0066***	-0.0070***	-0.0061***	-0.0053***	-0.0052***	-0.0041***	-0.0012	-0.0004	-0.0030***	-0.0001	0.0024*	0.0094***	0.0121***	0.0145***
	(0.0018)	(0.0018)	(0.0016)	(0.0015)	(0.0013)	(0.0012)	(0.0010)	(0.0011)	(0.0010)	(0.0010)	(0.0013)	(0.0014)	(0.0018)	(0.0025)	(0.0036)
p-values:															
$H_A : \delta_B \neq \delta_G$	0.049	0.007	0.002	0.585	0.252	0.174	0.733	0.000	0.000	0.046	0.000	0.000	0.000	0.000	0.000
H_A : Diff. effect size	0.004	0.000	0.000	0.015	0.019	0.031	0.006	0.053	0.002	0.505	0.004	0.000	0.000	0.000	0.000
Baseline mean:															
Boys	0.0443	0.0441	0.0349	0.0301	0.0309	0.0296	0.0274	0.0260	0.0247	0.0267	0.0302	0.0356	0.0385	0.0402	0.0402
Girls	0.0350	0.0349	0.0260	0.0209	0.0204	0.0194	0.0181	0.0167	0.0167	0.0169	0.0160	0.0165	0.0198	0.0268	0.0313
N	2,822,176	3,084,704	3,347,232	3,609,760	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,675,392	3,478,496	3,215,968	2,953,440	2,690,912	2,428,384
Earliest cohort	Apr.96	Apr.95	Apr.94	Apr.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93	Jan.93
Latest cohort	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Dec.06	Mar.06	Mar.05	Mar.04	Mar.03	Mar.02

Note: See notes to Table 2. Each regression interacts Sure Start coverage with indicators for whether the cell contains boys or girls (coverage on its own is not included in this model.) ‘Difference p-value’ tests the equality of the coefficients for coverage interacted with boys and with girls. ‘Effect size difference p-value’ tests the equality of the effect size (coefficients weighted by subgroup baseline mean) for coverage interacted with boys and with girls. *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively.

Table A.10: Effect of an increase in Sure Start coverage on probability of hospitalization for mental health, by gender

	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
SS Cov: Boys δ_B	-0.0008***	-0.0016***	-0.0018**	-0.0019
	(0.0003)	(0.0005)	(0.0009)	(0.0013)
SS Cov: Girls δ_G	-0.0006*	-0.0017***	-0.0020**	-0.0000
	(0.0003)	(0.0006)	(0.0010)	(0.0015)
p-values:				
$H_A : \delta_B \neq \delta_G$	0.206	0.802	0.707	0.060
H_A : Diff. effect size	0.143	0.08	0.06	0.026
Baseline mean:				
Boys	0.0013	0.0019	0.0030	0.0035
Girls	0.0013	0.0034	0.0054	0.0062
N	3,215,968	2,953,440	2,690,912	2,428,384
Earliest cohort	Jan-93	Jan-93	Jan-93	Jan-93
Latest cohort	Mar-05	Mar-04	Mar-03	Mar-02

Note: See notes to Table 2. Each regression interacts Sure Start coverage with indicators for whether the cell contains boys or girls (coverage on its own is not included in this model.) ‘Difference p-value’ tests the equality of the coefficients for coverage interacted with boys and with girls. ‘Effect size difference p-value’ tests the equality of the effect size (coefficients weighted by subgroup baseline mean) for coverage interacted with boys and with girls. *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively.

Table A.11: Effect of an increase in Sure Start coverage on probability of hospitalization for any cause, by neighborhood deprivation

	(1) Age 1	(2) Age 2	(3) Age 3	(4) Age 4	(5) Age 5	(6) Age 6	(7) Age 7	(8) Age 8	(9) Age 9	(10) Age 10	(11) Age 11	(12) Age 12	(13) Age 13	(14) Age 14	(15) Age 15
Cov: Poorest 30% δ_P	0.0286***	0.0113	0.0078	-0.0031	-0.0086	-0.0091	-0.0027	-0.0022	-0.0061*	-0.0073**	-0.0102**	-0.0142***	-0.0097*	-0.0091*	-0.0181***
	(0.0093)+++	(0.0079)	(0.0070)	(0.0081)	(0.0071)	(0.0066)	(0.0059)	(0.0043)	(0.0036)	(0.0035)	(0.0041)++	(0.0047)+++	(0.0058)	(0.0049)	(0.0067)++
Cov: Middle 40% δ_M	0.0236***	0.0079	-0.0018	-0.0099*	-0.0142***	-0.0102*	-0.0055	-0.0027	-0.0021	-0.0032	-0.0055	-0.0097**	-0.0058	-0.0102*	-0.0024
	(0.0068)+++	(0.0054)	(0.0046)	(0.0057)+	(0.0051)+++	(0.0053)	(0.0048)	(0.0038)	(0.0029)	(0.0029)	(0.0038)	(0.0039)+	(0.0050)	(0.0052)	(0.0065)
Cov: Richest 30% δ_R	0.0115	0.0013	-0.0013	-0.0060	-0.0120**	-0.0073	-0.0022	0.0001	-0.0007	-0.0004	-0.0011	-0.0021	0.0013	-0.0030	0.0016
	(0.0076)	(0.0057)	(0.0054)	(0.0062)	(0.0057)	(0.0059)	(0.0056)	(0.0045)	(0.0036)	(0.0033)	(0.0040)	(0.0049)	(0.0059)	(0.0070)	(0.0080)
p-values:															
$H_A : \delta_P \neq \delta_M$	0.459	0.561	0.054	0.120	0.125	0.695	0.311	0.855	0.080	0.069	0.063	0.148	0.285	0.813	0.015
$H_A : \delta_P \neq \delta_R$	0.054	0.175	0.135	0.604	0.427	0.630	0.872	0.433	0.046	0.012	0.005	0.002	0.019	0.362	0.011
Effect size diff *p-values*:															
Poor vs. Middle	0.499	0.982	0.048	0.011	0.001	0.090	0.177	0.625	0.194	0.240	0.274	0.577	0.480	0.566	0.037
Poor vs Rich	0.412	0.326	0.208	0.333	0.045	0.601	0.885	0.603	0.175	0.061	0.027	0.016	0.039	0.507	0.033
Baseline mean:															
Poorest 30%	0.3355	0.2708	0.2361	0.2233	0.2138	0.1874	0.1605	0.1472	0.1409	0.1346	0.1304	0.1394	0.1409	0.1499	0.1647
Middle 40%	0.2341	0.1866	0.1645	0.1541	0.1500	0.1333	0.1175	0.1085	0.1051	0.1003	0.1017	0.1098	0.1172	0.1253	0.1338
Richest 30%	0.2027	0.1613	0.1411	0.1332	0.1269	0.1141	0.1027	0.0946	0.0938	0.0908	0.0970	0.1046	0.1123	0.1200	0.1267
N	2822176	3084704	3347232	3609760	3675392	3675392	3675392	3675392	3675392	3675392	3478496	3215968	2953440	2690912	2428384
Earliest cohort	apr.96	apr.95	apr.94	apr.93	jan.93	jan.93	jan.93	jan.93	jan.93	jan.93	jan.93	jan.93	jan.93	jan.93	jan.93
Latest cohort	des.06	des.06	des.06	des.06	des.06	des.06	des.06	des.06	des.06	des.06	mar.06	mar.05	mar.04	mar.03	mar.02

Note: See notes to Table 2. Each regression interacts Sure Start coverage with indicators for whether the LSOA is in the poorest 30%, the richest 30%, or in between (coverage on its own is not included in this model). Indicators for these different groups are time-invariant and so absorbed by the LSOA fixed effects. ”p-value: Poor vs. Middle” tests the equality of the coefficients for coverage interacted with the indicators for being in the poorest 30% or the middle. ‘p-value: Poor vs. Rich’ does the same, testing the equality of the coefficients relating to the poorest and richest 30%. ’Effect size p-value’ does the same, but for the effect sizes (coefficients weighted by subgroup baseline mean). *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively; $+$, $++$ and $+++$ indicate significance at the 10%, 5% and 1% level, respectively, after adjusting inference following the procedure in algorithms 4.1 and 4.2 of Romano and Wolf (2005).

B Data sources for local authority characteristics

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 rollout of Sure Start in section 5.1.1

Table B.1 shows that for most characteristics we have data covering the entire period between 1999 and 2010. A major exception to this is the share of primary school pupils with English as an additional language (where data are not available between 2000 and 2003). In this case, 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: Covariates used in the rollout analysis

Category	Variable	Source	Years	Geography
Deprivation	Percentile of rank distribution of Index of Local Deprivation	Department of Environment, Trade, and the Regions[1]	1998	LAD
Health Indicators	Under-18 conception rate (conceptions/1,000 women aged 15-17)	Child and Maternal Health Intelligence Network[2]	1998-2018	LAD
	Proportion of births below 2.5kg	ONS Vital Statistics[3]	1991-2018 (interpolated in 2008 and 2009)	LAD
Potential Demand	Total period fertility rate	ONS Vital Statistics[4]	1990-2018	LAD
for services	Density	ONS Population Density[5]	1990-2018	LAD
	% of primary school pupils with English as an Additional Language	National Association for Language Development in the Curriculum (NALDIC)	1999; 2004-2018	County
	Children Looked After per thousand (under 1)	Department for Education	1992-2018	County
	Children Looked After per thousand (1 to 4)	Department for Education	1992-2018	County
Labour Market	Rate of Jobseekers Allowance receipt	Jobseekers Allowance[7]	1992-2018	LAD
Pre-Existing Services	Number of GPs per 1,000 population	Constructed with HSCIC data[10]	1990-2018	LAD
	Number of JobcentrePlus per 1,000 population	Department for Work and Pensions	2001-2018	LAD
	Free entitlement take-up rate among 3 and 4-year-olds	Department for Education Statistical Returns	1997-2018	County

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

C Evidence on migration between local areas

In our main analysis, we assess how early exposure to Sure Start in a child's local authority of residence affects the probability of hospitalization between ages 1 and 15. We define children's local authority based on their residence at the time of hospitalization, since residence at the time of birth is not reliably measured for cohorts born before 2003.

There are two potential difficulties in using a child's residence-at-admission as the basis for defining their exposure to Sure Start. First, mobility across local authorities could introduce measurement error if we assign children's treatment based on the wrong local authority. Second, to the extent that mobility is selective (for example, with more motivated families electing to move into areas with greater access to Sure Start), our strategy will yield biased estimates of Sure Start's effectiveness.

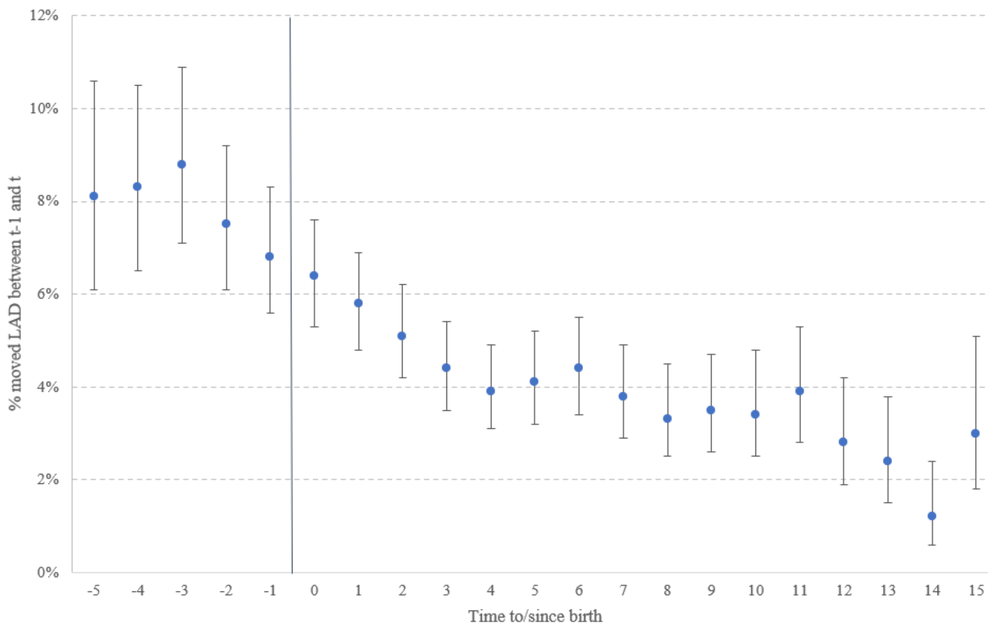
In this appendix, we use data restricted-access data with geographic identifiers from the British Household Panel Survey (BHPS) to assess both the overall extent of inter-LA mobility during childhood and the extent to which it is correlated with Sure Start provision. The BHPS data are ideal for this analysis: they follow a representative panel annually for 18 years, meaning that we can observe families' mobility before the birth of their child as well as afterwards. Our sample consists of primary caregivers who had a child while in the BHPS sample. We then follow these primary caregivers (henceforth parents) up to five years before the child's birth, and up to 15 years after birth.

Figure C.1 shows that overall inter-LA mobility is relatively low and declining as children age: around 7-8% of families move LA each year in the five years before their child is born, but this declines to 4% of families moving by the time a child is aged 3. This means that measurement error related to mobility between LAs is relatively small, particularly after children turn 5 and age out of Sure Start eligibility entirely.

We also find that what inter-LA mobility there is does not systematically relate to Sure Start availability. In Table C.1, we show that children living at time t in LAs with greater access to Sure Start are no more likely to have moved between t-1 and t than those living in lower-coverage areas.

This provides reassurance that families are not systematically relocating into high-Sure Start local authorities.

Figure C.1: Share of families who moved LA in the past year, by age of child



Note: Mobility is indexed based on the wave in which the family's first birth was observed. Source: Authors' calculations using data from the British Household Panel Survey, 1991-2009.

Table C.1: Association between Sure Start coverage and inter-LA mobility in the previous year

	(1) Age 0	(2) Age 1	(3) Age 2	(4) Age 3	(5) Age 4	(6) Age 5
SS Coverage (at time t)	0.037	0.000	0.020	0.004	0.000	0.003
	(0.041)	(0.026)	(0.021)	(0.019)	(0.018)	(0.017)
Observations	1,017	1,106	1,193	1,134	1,060	1,004
R-squared	0.002	0.000	0.001	0.000	0.000	0.000
Outcome mean	0.072	0.062	0.055	0.046	0.043	0.043

Note: *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively. The outcome is an indicator for whether the family had moved LA since the previous wave. Sure Start coverage is measured contemporaneously in the LA of residence at time t. Source: Authors' calculations using data from the British Household Panel Survey, 1991-2009.

D Evidence on the relationship between children's behavioral and emotional difficulties and hospitalization

In section 6, we show that Sure Start substantially reduces hospitalizations for accidents and injuries between ages 1 and 11. The impacts at younger ages may be driven by information and support in reducing risk in the home environment; indeed, we find that poisonings fall up to age 3. However, these direct informational effects are not a plausible mechanism for the longer-term falls in external hospitalizations that we observe. In this appendix, we present evidence from the Millennium Cohort Study (MCS) on the correlates of parent-reported injuries, highlighting that child behavior - and particularly externalizing behavior - is strongly associated with injuries through middle childhood and early adolescence.

We use data from three waves of the MCS: wave 3 (age 5), wave 4 (age 7) and wave 5 (age 11).[1] At each wave, parents report injuries sustained by their child since the last wave (so 'age 5' results consider hospitalizations between ages 4 and 5, 'age 7' results for ages 6 and 7, and 'age 11' results for ages 8–11). We use as an outcome whether the parent reports any injury since the previous wave.[2]

The richness of the MCS data allows us to consider the link between child behavior and injuries while controlling for a wide range of other potential correlates. These include child demographics (sex, ethnicity); maternal demographics (age at child's birth, education); economic circumstances (maternal employment, household net earnings); and the home learning environment.[3] In our specification with covariates, we also control for fixed effects for the child's region of residence at the time of interview[4] and the season of the interview. All regressions control for fixed effects of

[1]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.

[2]The associations documented here are similar when we consider the number of injuries sustained since the previous wave. We focus on the 'any injury' indicator since it is more analogous to the outcomes in our main results.

[3]We construct a standardized measure of the home learning environment using factor analysis on a series of parental time inputs (for example, how often the parents read to the child, visit the library, or play games with the child).

[4]There are 12 regions in total: 9 in England, Wales, Scotland and Northern Ireland. Since regional data is not available in the public-access version of MCS at wave 4 (age 7), we use the child's wave 3 (age 5) region of residence instead.

the child's age in months at the time of interview.

We have two measures of child behavior, both reported by the mother through the widely used Strengths and Difficulties Questionnaire (SDQ). The 'externalizing behavior' score comes from the SDQ subscales on hyperactivity and conduct disorders. The 'internalizing behavior' 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 behavior.

Table D.1 presents the associations between behavior and whether the child has sustained an injury since the previous wave. Columns 1, 3 and 5 show a significant relationship between externalizing behavior problems and injuries at ages 5, 7 and 11. An additional point on the externalizing scale (out of 20) is associated with roughly a roughly 1-percentage point increase in the probability that a child has sustained an injury since the previous wave. By contrast, there is little association between internalizing behavior and injuries at any age.

Columns 2, 4 and 6 show that these associations are robust to the inclusion of a wide range of additional controls. While these results should not be interpreted as causal, they do provide suggestive evidence of a relationship between externalizing behavior and injuries that cannot be explained by the child's demographics or family circumstances. This supports the hypothesis that a Sure Start-induced change in child behavior is a plausible mechanism for the reduction in injury-related hospitalizations through middle childhood and early adolescence. This is also in line with findings from the ECCE project, which identified a reduction in externalizing behavior over time as one of the main benefits of using Sure Start services (Sammons, Goff and Smith, 2015).

Table D.1: Association between child behavioral problems and any parent-reported injury

	(1)	(2)	(3)	(4)	(5)	(6)
	Age 5		Age 7		Age 11	
Externalizing behavior	0.009***	0.006***	0.009***	0.007***	0.011***	0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Internalizing behavior	0.001	0.002	-0.001	0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Female		-0.043***		-0.033***		-0.032***
		(0.011)		(0.010)		(0.012)
Ethnicity: Mixed		-0.052*		-0.038		-0.055*
		(0.029)		(0.028)		(0.031)
Ethnicity: Indian		-0.043		-0.056**		-0.180***
		(0.028)		(0.027)		(0.031)
Ethnicity: Pakistani or Bangladeshi		-0.088***		-0.095***		-0.179***
		(0.021)		(0.020)		(0.023)
Ethnicity: Black		-0.075**		-0.043		-0.147***
		(0.030)		(0.029)		(0.032)
Ethnicity: Other		-0.068*		-0.054		-0.203***
		(0.040)		(0.038)		(0.043)
Mother's age at birth		-0.001		0.000		-0.000
		(0.001)		(0.001)		(0.001)
Mother cohabiting		-0.035**		-0.012		-0.024
		(0.015)		(0.014)		(0.015)
Mother's education: A level		0.010		-0.002		-0.004
		(0.017)		(0.016)		(0.018)
Mother's education: GCSE or below		0.017		0.016		-0.006
		(0.013)		(0.012)		(0.014)
Mother's education: Missing		0.034		-0.003		-0.043
		(0.025)		(0.025)		(0.030)
Mother's work status: Part-time		0.005		-0.009		-0.018
		(0.013)		(0.013)		(0.015)
Mother's work status: Full-time		0.001		0.021		-0.015
		(0.014)		(0.013)		(0.016)
Mother's work status: Unknown		-0.109*		0.017		-0.055
		(0.061)		(0.052)		(0.044)
Household net earnings		0.000		-0.000		-0.000
		(0.000)		(0.000)		(0.000)
Home learning environment		0.011*		0.012**		0.018***
		(0.006)		(0.005)		(0.006)
Constant	0.219***	0.320***	0.190***	0.246***	0.315***	0.469***
	(0.009)	(0.044)	(0.008)	(0.042)	(0.009)	(0.047)
Observations	6,971	6,960	6,974	6,963	6,947	6,924
R-squared	0.007	0.019	0.009	0.020	0.010	0.036
Mean	.26	.26	.228	.228	.358	.358
Age in months FE?	Yes	Yes	Yes	Yes	Yes	Yes
Region FE?		Yes		Yes		Yes
Interview quarter FE?		Yes		Yes		Yes

Note: The outcome is an indicator for whether the parent reports that the child has sustained an injury since the last wave (i.e. from ages 3-5 for age 5 results; from ages 6-7 for age 7 results; and from ages 8-11 for age 11 results). *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively.

E Estimation of Sure Start effects on maternal employment

In addition to their focus on children's health and development, Sure Start centers also brought together existing services to support parental employment. Children's centers were required to develop links with JobcenterPlus, an existing network of government-run agencies to support the unemployed in finding work. Children's centers were also required to signpost parents towards existing childcare programs, most notably the entitlement to a part-time free childcare place for 3- and 4-year-olds.[5] Many Sure Start centers also offered information about further education and basic skills courses.

There is a large literature establishing that childcare subsidy programs can affect parental employment in some contexts, but typically only for mothers whose youngest child is eligible for the program (e.g. Gelbach (2002); Cascio (2009); Brewer et al. (2020)). While these parental employment outcomes are important in their own right, an increase in parental employment may also impact on children's development through higher family income and/or less parental time with children. To investigate the likely importance of this channel, we use the UK's Labour Force Survey (LFS) to analyze how access to Sure Start affected maternal employment.

Data and outcomes The LFS is collected in a staggered five-quarter rolling panel, with households entering the survey at different points in the year and then remaining in the sample for five consecutive quarters. We use a secure access version of the LFS that contains information both on the household's local authority of residence and on the precise birth date of all household members. To mirror our hospitalization analysis, we focus on mothers whose children were all born between 1993 and 2006. To avoid mothers of newborn children (who most often take several months of maternity leave), we further restrict the sample to mothers who did not give birth during the period that they were in the LFS sample.

As our primary outcome, we focus on an indicator for whether a mother is in paid work at the

[5] The free entitlement was first introduced in 1997, offering a free childcare place to 4-year-olds for 12.5 hours per week, 33 weeks of the year. The program was extended to cover 3-year-olds in April 2004, and the generosity of the system was increased in a series of reforms: by September 2010 it covered a 15-hour place for 38 weeks of the year.

time she is surveyed by the LFS. As secondary outcomes, we consider whether mothers work part-time (fewer than 30 hours a week) or full-time, and whether they are in full-time education. Since mothers are observed up to five times in the LFS, each mother can be included multiple times in our model.

Sure Start treatment Since existing evidence suggests that the strongest effects should be found among mothers whose youngest child is eligible for support, we focus on the treatment a mother experiences in respect of her youngest child. Specifically, we use the same measure of Sure Start coverage as in our hospitalization analysis (centers per thousand children aged 0-4 in the local authority, averaged over the child's first 60 months of life[6]). We assign this measure of Sure Start coverage to mothers based on the year and month of birth of their youngest child and their local authority of residence when they are first observed in the LFS.

Specification To evaluate the impact of access to Sure Start on maternal employment, we estimate Equation 3 by OLS:

$$y^a_{iwdmqt} = \alpha + \delta^a SS_{dmt} + \pi_{wd} + \lambda_q + \gamma^{a_m}_{iq} + \phi^{a,k} g^k_{iq} * Ki\beta^a X_i + \epsilon^a_{wdmt}, a = 0, ..., 15 \qquad (3)$$

where y^a_{iwdmt} is the outcome variable, an indicator for whether a mother i is in work. We estimate the model separately for mothers whose youngest child is a years old, for each age from 0 to 15. SS_{dmt} is the average Sure Start coverage of the mother's youngest child, based on when they were born (year t and month m) and where the family resides when they enter the LFS (ward w of local authority d). We include quarter-year fixed effects q to control for contemporaneous labour market conditions. We control flexibly for the ages of children in the household: $\gamma^{a_m}_{iq}$ is a set of fixed effects for the youngest child's age in months at the time mother i is observed in quarter-year q. We also control for the presence and ages of up to four older children k through a continuous measure of the older child's age in years g^k_{iq} interacted with an indicator K_i for whether there is

[6] Where a child is less than five years old, we average coverage only over the months in which they have actually been alive.

such a child in the household.

Unlike our main hospitalization regressions, Equation 3 is estimates at the individual level. This means we are able to control for individual characteristics X_i. We include characteristics pre-determined at the time of potential Sure Start exposure, namely mothers' ethnicity and age; in alternate specifications we also include education and partnership status. However, because Equation 3 is estimated on individual-level survey data, we cannot include LSOA-level fixed effects (since there are not sufficient observations in each LSOA). We instead control for around 9,000 ward fixed effects (π_{wd}). Using individual rather than neighborhood cell level, also allows to use a more precise (monthly rather than quarterly) measure of Sure Start treatment, and to control for the youngest child's age in months rather than quarters.

E.1 Results: Maternal employment

We first consider the impact that Sure Start had on the probability that a mother is working. These results are presented in Figure E.1, which reports the estimates from 15 separate regressions, based on the age of the mother's youngest child. To account for the different baseline probabilities of employment at different ages, Figure E.1 then rescales each of these coefficients by the baseline employment rate of women whose youngest child was that age in 1996.

Figure E.1 shows no clear pattern in Sure Start's impacts on maternal employment. While there are statistically significant positive impacts at ages 1, 6 and 15 (and a significant negative effect at age 7), there is no clear overall pattern of results across ages. We present the full set of results in Column 2 of Tables E.1 to E.3.[7]

Column 1 presents the raw correlation between Sure Start coverage and maternal employment. Unsurprisingly, mothers with greater access to Sure Start - whose children are on average older - tend to have higher rates of employment.[8] In Column 2 we control for ward fixed effects and

[7]We conduct similar analysis for subgroups of mothers: single mothers, partnered mothers, and by maternal education (those with less than high school vs. mothers with high school or more). We find no consistent patterns of impacts among any of these subgroups. Results available on request.

[8]This is because Sure Start treatment is generally weakly increasing over a child's first five years, as new centers open in the child's local authority. Therefore, as children who are still age-eligible for Sure Start get older, their average level of access to Sure Start tends to increase.

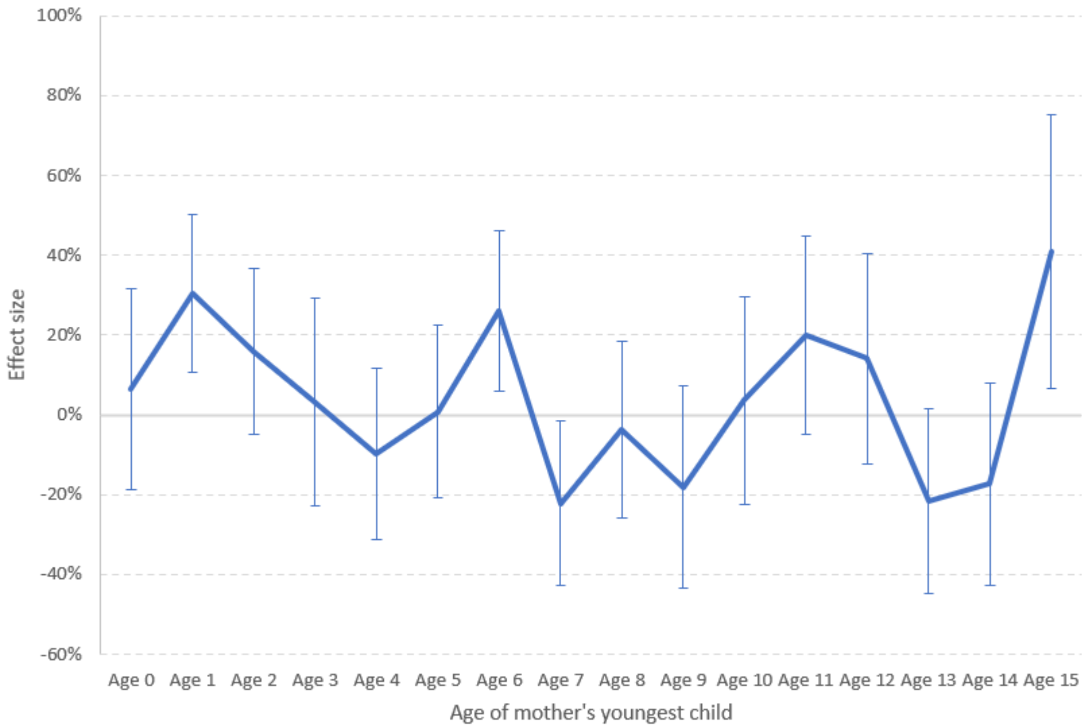
for the set of basic controls shown in Equation 3. In the next three columns we present additional robustness checks. In Column 3 we allow for a local authority-specific linear time trend, estimated based on pre-treatment data and extrapolated to the post-Sure Start period. The inclusion of these estimated trends has very little impact on our results. In Column 4 we additionally control for characteristics of the mother that were potentially influenced by Sure Start exposure (education and partnership status); characteristics of the local labor market at the time of data collection (male and female median weekly full-time earnings and the local unemployment benefit claiming rate); and a range of local characteristics that may have helped to determine Sure Start's rollout, measured at the birth of the youngest child.[9] In general, the inclusion of this extended set of controls does not change the overall conclusion of mixed impacts of Sure Start on maternal employment, with mostly non-significant effects.

As a final robustness check, in Column 5 we estimate our main equation (Column 2) on the subgroup of mothers with only one child. This sample restriction allows us to examine maternal employment in the simplest case, without the possibility of unmeasured spillovers from older children's treatment. Our results become substantially less precise, but we find similar patterns in terms of the direction and statistical significance of effects, except at the oldest ages.

We also present the results of a specification check in Column 6. Here, we exploit the panel aspect of the LFS to control for mother fixed effects. This allows us to look within mothers at whether higher Sure Start coverage increases the probability that a mother is working. Because Sure Start coverage only varies during a child's first five years of life (as the average coverage is updated to include additional months of treatment), this specification is only possible where the youngest child is aged 4 or below (Table E.1). This specification substantially decreases the precision of our estimates, but again we find statistically significant employment impacts only at age one. These effects are very large - implying that a mother gaining an additional center per thousand children was nearly 30 percentage points more likely to be working - but they once again come in a context of insignificant and inconsistent results at other ages.

[9] This is the same set of characteristics used in the robustness checks for our hospitalization analysis.

Figure E.1: Effect of Sure Start coverage on probability of maternal employment, rescaled by baseline probability



Note: The table shows coefficients from separate regressions for each outcome age. Coefficients are rescaled by the employment rate of mothers whose youngest child was born in 1996. Vertical bars indicate 90% confidence intervals. Source: Authors' calculations using data from the UK Labour Force Survey and the Department for Education's data on the rollout of Sure Start.

Table E.1: Effect of an increase in Sure Start coverage on probability of maternal employment: Youngest child aged 0-4

	(1)	(2)	(3)	(4)	(5)	(6)
Age 0	-0.014	0.032	0.022	0.005	0.033	-0.139
	(0.022)	(0.077)	(0.089)	(0.085)	(0.102)	(0.130)
N	28,190	28,190	28,190	28,190	16,087	28,190
Baseline mean	0.5036	0.5036	0.5036	0.5036	0.5349	0.5036
Age 1	-0.044**	0.165**	0.150**	0.147**	0.280***	0.285**
	(0.020)	(0.065)	(0.073)	(0.074)	(0.084)	(0.128)
N	45,595	45,595	45,595	45,595	25,147	45,595
Baseline mean	0.5429	0.5429	0.5429	0.5429	0.5883	0.5429
Age 2	-0.063***	0.087	0.05	0.091	0.007	0.105
	(0.021)	(0.069)	(0.068)	(0.068)	(0.119)	(0.142)
N	37,605	37,605	37,605	37,605	19,065	37,605
Baseline mean	0.5449	0.5449	0.5449	0.5449	0.5825	0.5449
Age 3	-0.063***	0.018	0.02	0.028	-0.028	-0.055
	(0.020)	(0.091)	(0.090)	(0.095)	(0.149)	(0.175)
N	31,162	31,162	31,162	31,162	14,282	31,162
Baseline mean	0.5774	0.5774	0.5774	0.5774	0.6178	0.5774
Age 4	-0.070***	-0.063	-0.063	-0.046	-0.206	0.166
	(0.018)	(0.083)	(0.083)	(0.081)	(0.158)	(0.186)
N	27,028	27,028	27,028	27,028	11,473	27,028
Baseline mean	0.6411	0.6411	0.6411	0.6411	0.6732	0.6411
Fixed effects		Ward	Ward	Ward	Ward	Mother
Trends?			Estimated	Estimated	Estimated	
Basic Controls?		Y	Y	Y	Y	Y
Extended Controls?				Y		
Sample restrictions?					Only children	

Note: *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively. Standard errors are clustered at the LA level.

Table E.2: Effect of an increase in Sure Start coverage on probability of maternal employment: Youngest child aged 5-10

	(1)	(2)	(3)	(4)	(5)
Age 5	-0.092***	0.005	0.009	-0.047	-0.159
	(0.016)	(0.092)	(0.090)	(0.089)	(0.203)
N	24,247	24,247	24,247	24,247	9742
Baseline mean	0.7013	0.7013	0.7013	0.7013	0.69
Age 6	-0.063***	0.183**	0.179**	0.221**	-0.006
	(0.017)	(0.086)	(0.089)	(0.089)	(0.204)
N	22,292	22,292	22,292	22,292	8727
Baseline mean	0.7039	0.7039	0.7039	0.7039	0.7028
Age 7	-0.034**	-0.165*	-0.161*	-0.122	0.199
	(0.017)	(0.093)	(0.093)	(0.091)	(0.228)
N	21,148	21,148	21,148	21,148	8247
Baseline mean	0.7453	0.7453	0.7453	0.7453	0.7045
Age 8	-0.014	-0.028	-0.024	-0.134	0.247
	(0.017)	(0.101)	(0.101)	(0.111)	(0.324)
N	20,610	20,610	20,610	20,610	7956
Baseline mean	0.7487	0.7487	0.7487	0.7487	0.7265
Age 9	-0.022	-0.137	-0.118	-0.185	-0.377
	(0.018)	(0.116)	(0.119)	(0.136)	(0.259)
N	19,834	19,834	19,834	19,834	7538
Baseline mean	0.7571	0.7571	0.7571	0.7571	0.738
Age 10	0.004	0.028	0.000	0.059	0.156
	(0.017)	(0.123)	(0.123)	(0.127)	(0.271)
N	19,116	19,116	19,116	19,116	7228
Baseline mean	0.7795	0.7795	0.7795	0.7795	0.7906
Fixed effects		Ward	Ward	Ward	Ward
Trends?			Estimated	Estimated	Estimated
Basic Controls?		Y	Y	Y	Y
Extended Controls?				Y	
Sample restrictions?					Only children

Note: *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively. Standard errors are clustered at the LA level.

Table E.3: Effect of an increase in Sure Start coverage on probability of maternal employment: Youngest child aged 11-15

	(1)	(2)	(3)	(4)	(5)
Age 11	0.012	0.157	0.152	0.178	0.113
	(0.018)	(0.119)	(0.120)	(0.139)	(0.297)
N	18,784	18,784	18,784	18,784	7357
Baseline mean	0.7886	0.7886	0.7886	0.7886	0.7794
Age 12	-0.007	0.113	0.112	0.008	-0.01
	(0.018)	(0.128)	(0.126)	(0.122)	(0.290)
N	18,809	18,809	18,809	18,809	7651
Baseline mean	0.8007	0.8007	0.8007	0.8007	0.8316
Age 13	0.014	-0.174	-0.169	-0.028	-0.582*
	(0.017)	(0.113)	(0.111)	(0.112)	(0.311)
N	17,854	17,854	17,854	17,854	7650
Baseline mean	0.806	0.806	0.806	0.806	0.7816
Age 14	-0.026	-0.14	-0.159	-0.072	-0.657**
	(0.023)	(0.124)	(0.121)	(0.124)	(0.302)
N	16,385	16,385	16,385	16,385	7829
Baseline mean	0.8053	0.8053	0.8053	0.8053	0.769
Age 15	-0.014	0.324*	0.348**	0.459***	0.213
	(0.028)	(0.165)	(0.164)	(0.177)	(0.265)
N	14,835	14,835	14,835	14,835	8064
Baseline mean	0.7932	0.7932	0.7932	0.7932	0.7613
Fixed effects		Ward	Ward	Ward	Ward
Trends?			Estimated	Estimated	Estimated
Basic Controls?		Y	Y	Y	Y
Extended Controls?				Y	
Sample restrictions?					Only children

Note: *, ** and *** indicate significance at the 10%, 5% and 1% level, respectively. Standard errors are clustered at the LA level.

F Cost-Benefit Analysis of Sure Start

This section reports the details of the cost–benefit calculation we report in section 8 of the paper. We do so by combining official data on government expenditures on Sure Start to compute the cost of Sure Start, with the estimates obtained in the previous sections and results from the best published literature to compute the benefits. We compute the averted costs in terms of hospitalizations attributable to providing access to Sure Start to 1,000 more children (i.e. from opening one more center at the peak coverage level).

We are not the first to try to quantify the monetary benefits of Sure Start. Meadows (2011) calculated that 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 up to age 4); and that by the time children had reached the age of 5, SSLPs had already delivered economic benefits between £279 and £557 per eligible child (coming from reduction in work-less households), which is 6–12% of the total cost of the program. The authors concluded that this is a large impact, given the early stage at which it is measured, but that there was 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 support, childcare, finance and work support, and training and education. The average cost per user per hour (the value of resources used to deliver one hour of a service to a child) ranged from £6 for childcare to £55 for finance and work support, while the mean cost per family using the service (which accounts for the hours of usage) ranged from £958 for parent support to £8,454 for childcare. The authors then combined estimates on the associations between the use of different types of SSCC services and improved family outcomes with existing evidence from the literature on long-term effects. They found that some SSCC services provide positive value for money, i.e. the monetary valuation of improved outcomes exceeds the cost of delivery.

Costs We opted to compute in an alternative way the cost of Sure Start. Our choice is informed by different factors. First, we have not collected detailed costs data as was done in the NESS and ECCE evaluations. Second, given that we evaluate the effects of Sure Start using the whole period it was in place, it would be difficult to compute a measure of costs valid for both SSLPs and SSCCs. Third, our measure of costs needs to be consistent with the methods we use in the estimation of the impacts, 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, dividing the overall government expenditures on Sure Start by the number of eligible children, i.e. the number of children aged 0–4 in the local authorities in which Sure Start was in place in that particular year. This is consistent with the aim of the government (especially at program maturity) to provide Sure Start to every age eligible child, and the fact that Sure Start was area-based, rather than means-tested. The cost per child computed in this way amounts to £415.9 per eligible child, on average. This cost per child is much lower than the costs of other programs studied in the literature (see Figure F.1).[10]

Benefits Weighed against Sure Start's cost to taxpayers, we consider the financial benefits of the hospitalizations that Sure Start averted. In doing this calculation, we only consider impacts that are statistically significant at the 10% or less after accounting for multiple hypothesis testing, for the following conditions: injuries and poisoning (a subset of external), respiratory, parasitic/intestinal, and mental health. We consider three types of costs:

- Averted direct healthcare costs. We use specific NHS resource use costs for each of these conditions, taking the average cost among the different categories for non-elective long and short stay.

- Averted indirect costs, over the same ages as the healthcare costs, such as costs to the family

[10] Although information on Sure Start usage is scare, we can also use the Action for Children (2019) estimate of 2.2 million yearly users in 2013 to compute the cost per child using the services, which amounts to around £480. The costs of a large-scale intervention per child eligible (or attending) are a fraction of the cost of provision of some of the major successful targeted early-childhood interventions. In 2019, Head Start's $9.8 billion budget covered provision to 87,300 children (U.S. Department of Health & Human Services, 2019). Other programs, such as the Abecedarian and the Perry School cost over £10,000 per child per year (Barnett and Masse, 2007; Heckman, 2010), while the cost of the Boston School program falls just short of this figure (all in 2020 GPB prices) (Gray-Lobe, Pathak and Walters, 2021).

and to 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, or for mental health conditions).

The main results of our cost–benefit calculation are reported in Table F.1. 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 £330 million. Of this, around £3.9 million is attributed to direct cost savings to the NHS from fewer hospitalizations at ages 1–15. As expected, the bulk of the total averted cost is attributable to the lifetime costs of traumatic brain injury and mental health conditions. Set against this is the estimated cost of providing an additional Sure Start center 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 hospitalizations offset approximately 31% of the cost of Sure Start provision (with direct savings from the reduction in hospitalizations at ages 1–15 amounting to 0.37% of spending on Sure Start).

Of course, the benefits of Sure Start may extend to other domains beyond health since the program was designed to promote child development in a holistic way and through a variety of services. To accurately measure the full benefits of Sure Start against its cost, it will therefore be crucial to look at additional outcomes that the program could have improved. In work in progress we will study the impacts of Sure Start on children's attainment, use of social care, and offending behavior.

Table F.1: Estimated costs and benefits of Sure Start for one cohort of children (2018–19 prices)

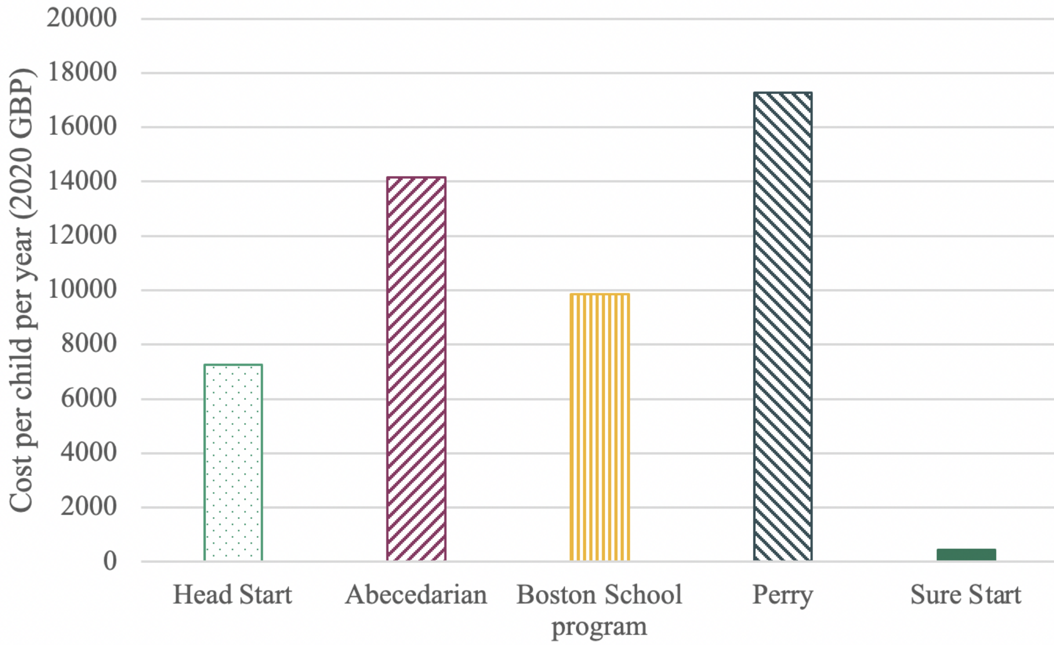
Total program expenditures	£1,055 million
Total costs from averted hospitalizations	£330 million
Of which:	
Direct healthcare costs (1.2%)	£3.9 million
Indirect costs (1.3%)	£4.3 million
Long-term costs (97%)	£322 million

The total averted costs of Sure Start were calculated using the estimated effect of Sure Start on hospital admissions for poisoning, head injuries, fractures, respiratory illnesses, infections and parasitic conditions and mental health. The direct healthcare costs are calculated using the National Schedule of NHS Costs (2018/2019). To compute the indirect costs, we use Cooper et al. (2016)'s estimated mean short term family costs resulting from injury and poisoning hospitalizations; Stevens et al. (2003)'s family borne cost of respiratory admissions and Telford et al. (2012)'s estimated mental health educational cost. The sources used to calculate the lifetime costs of averted hospitalizations are the following:

- We compute the share of head injuries and fracture hospital admissions being due to child maltreatment using González-Izquierdo et al. (2010); and calculate their lifetime costs based on Conti et al. (2017).
- The proportion of traumatic brain injury admissions is calculated using Trefan et al. (2016). The medical and lifetime costs of a pediatric traumatic brain injury are based on Kendrick et al. (2017) and Child Accident Prevention Trust (2013), respectively.
- We use Friedli and Parsonage (2009)'s estimates to compute the lifetime cost of averted mental health admissions.
- In our computations, we only use the program impacts that survive adjustment of inference

for multiple hypothesis testing.

Figure F.1: Cost of early years interventions programs per year per child



Sources: The cost of provision per year per child attending Head Start as of 2004 and in 2004 USD : $7,000 (U.S. Department of Health and Human Services, 2004). The cost of yearly provision per child of the Abecedarian project is estimated to be around $13,000 in 2002 USD (Barnett and Masse, 2007). The Boston School program was predicted to cost $13,000 annually (in 2020 USD) for every child enrolled full-time by the DESE (Gray-Lobe, Pathak and Walters, 2021; Massachusetts Department of Elementary and Secondary Education , 1995). The Perry project cost $17,759 (2006 USD) per year per child (Heckman, 2010).

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