Children of Neighborhood Renewal: Long-Run Effects of a Revitalization Program

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Abstract

Between 2001 and 2011, England implemented the Neighbourhood Renewal Fund (NRF), an ambitious neighborhood revitalization program targeting the most deprived areas of the country. Using a sharp Regression Discontinuity Design that exploits the cutoff-based program eligibility rule, we study the long-term impact of the NRF on children who grew up in targeted areas. Results show that, compared to the control group who just missed out on program eligibility, the children of neighborhood renewal enjoyed 15-percent higher earnings at age 25. Further results show that stronger attachment to the labor market and better job prospects explain these results, with effects persisting through age 35.

JEL codes: H53, I32, R23.
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1 Introduction

An increasing number of studies show that the neighborhood where a child grows up causally influences a large variety of long-term outcomes, from educational attainment, to involvement in the justice system, health, and labor market prospects (Chyn and Katz, 2021). However, most of this evidence comes from settings where households are nudged or forced to relocate to a better area, while little is known about the long-run impact of improving the neighborhood where families currently reside. Despite this, many governments spend a substantial amount of public money on so-called neighborhood renewal policies, aimed at revitalizing deprived areas.[1] This paper sheds light on the long-term impact of such interventions on incumbent residents by studying an ambitious neighborhood renewal program that was implemented in England between 2001 and 2011 in the most deprived areas of the country.

The first Blair government introduced the Neighbourhood Renewal Fund (NRF) in 2001 with the ultimate goal of narrowing gaps in outcomes between the most deprived neighborhoods and the rest of England. The NRF targeted the 50 most deprived Local Authorities (LAs) in England, based on six numerical measures of deprivation computed for the year 2000. The program was broad in both fiscal and thematic scope, offering targeted areas £500 million in total per year, approximately 3% of LA expenditure, to be allocated across five different areas: education, crime, health, unemployment, and housing.

We study the long-term effects of the NRF on adult outcomes of children who resided in targeted LAs at the time of the program implementation. For identification of causal effects, we use a sharp Regression Discontinuity Design (RDD) that exploits the cutoff-based program eligibility rule. We measure children's outcomes between age 25 and 35 using rich administrative panel data that follows any individual who attended school in England throughout their educational career and subsequently through the labor market, from 2001/02 to 2020/21.

We begin our analysis by studying the impact of childhood exposure to the NRF on income at age 25. Our results show that children growing up in the most deprived neighborhoods of NRF targeted LAs enjoy significantly higher earnings, approximately 15-percent, compared to the control group who just missed out on program eligibility. These positive effects are concentrated among the children from the most disadvantaged backgrounds, and are robust to utilizing alternative measures of income such as the percentile rank within the income distribution. In further robustness checks, we find that our estimates change little when including region fixed effects in our regressions, or excluding, in turn, LAs or neighborhoods that were eligible for other place-based policies

[1] For instance, Germany has spent more than €15 billion on urban renewal projects since the 1970s, Australia invested more than $800 million in the so-called "building better cities program" between 1991 and 1996, and in 2007 France introduced a re-qualification program providing 1.5 billion to 25 historically deprived neighborhoods across the country (ANRU 2007, Neilson 2008, Zabel and Kwon 2021).

operating concurrently to the NRF.

We subsequently delve into mechanisms and find that the income effects are driven by a stronger attachment to the labor market and better job prospects: children exposed to the NRF work 4.9 percent more days per year and are 4.8 percentage points more likely than the control group to work at a large firm - 8 percent of the control group mean. We are also able to study the role of plausible alternative mechanisms. Childhood exposure to the NRF did not significantly affect the probability of growing up in poverty, educational attainment, geographic mobility, or the probability of being convicted of a crime. Furthermore, the NRF did not significantly affect extensive-margin employment. Using the Annual Population Survey (APS), we are also able to exclude that the positive earnings effects arise from the exposure to more affluent neighbors, as we do not find evidence that the NRF changes the socio-economic composition of targeted areas. Taken together, these results suggest that the program operated by enhancing intensive-margin labor supply and improving access to "good" jobs at large firms for individuals active in the labor force. Importantly, these positive effects persist over time, with the impact on earnings and the likelihood of working for a large firm remaining significant up to age 35.

We then dig further into the suggested mechanism of improved access to "good" jobs and investigate if this is a result of firms entering the treated areas using business register data and a difference-in-differences design. We find that, over time, firms in several sectors (agriculture, construction, transportation, and other services) opened a larger number of establishments or increased employment in the most deprived neighborhoods of LAs treated by the NRF. We also verify, using our individual panel data, that these are precisely the sectors that our cohorts seem more likely to be working in as adults. Altogether, this evidence suggests that higher-quality jobs came to areas treated by the NRF, and children who grew up in those areas were better able to access them.

Finally, we study effect heterogeneity and find that attachment to the labor market mostly increases for girls, while the impact on earnings is larger and more precisely estimated for boys. Remarkably, we do not find heterogeneous effects by childhood exposure to the program. These results are consistent with the evidence from our mechanism analysis that the program primarily improved job opportunities in targeted LAs rather than, for instance, school quality. If the program improved adult outcomes through increased human capital accumulation, one would expect larger effects for girls, and for results to differ by exposure to the program.

By providing novel reduced-form evidence on the long-term effects of an important neighborhood renewal policy, our results bring three main contributions to the literature on neighborhood effects and place-based policies. First, while there is mounting evidence that childhood neighborhood of residence causally influences long-run economic, social, and health outcomes (Chyn and Katz, 2021), most of the available evidence on these effects comes from settings where households *change* neighborhood - exploiting differing ages of children at that the time that they move (Chetty

et al., 2016; Chetty and Hendren, 2018a; Deutscher, 2020; Laliberté, 2021), forced moves after public housing demolitions (Chyn, 2018; Haltiwanger et al., 2024) or natural disasters (Nakamura et al. 2022).

As moving all residents of deprived neighborhoods away is simply not feasible, it is especially important from a public-policy perspective to understand the effects of policies aimed at *improving* rather than changing the neighborhoods where households reside, and this paper contributes to this goal. The limited set of evidence from non-mover designs also does not address this key policy question. Damm and Dustmann (2014) exploits the quasi-random assignment of refugees in Denmark between 1986 and 1998 to show that growing up in a high-crime neighborhood increases the probability criminal convictions of male refugees later in life. Baum-Snow et al. (2019) uses a shift-share instrument to show that an increase in the share of college-educated neighbors leads incumbent children to experience an improvement in credit scores as adults. While this paper addresses the question of the effects of neighborhood change on incumbents, it does not study the effects of public policy designed to improve neighborhoods. Our paper directly addresses the policy question, providing evidence that neighborhood renewal policies can improve the outcomes of incumbent children.

Closely related to our paper, Chetty et al. (2026) studies the long-term impact on local residents of an alternative form of neighborhood renewal: demolition of existing public housing units and replacement with a mixture of market-rate and subsidized units. The authors find that children who grew up in the newly revitalized public housing units earned 16% more in adulthood. Importantly, most of these children moved in the targeted neighborhood *after* the demolitions took place, while incumbent children were forced to move to new neighborhoods.[2] Related to this, Chetty et al. (2026) finds that average adult incomes in targeted neighborhoods increased 45% post-treatment due to changes in the composition of resident households. Our paper complements Chetty et al. (2026) by studying the effects of a neighborhood renewal policy which did not result in such large changes in the composition of the neighborhood, and estimating the effects of the policy on incumbent children.

With the notable exception of Chetty et al. (2026), the few other studies that have investigated the effects of neighborhood renewal programs, including the NRF, focus on contemporaneous effects. In the short run, the NRF has been shown to reduce crime (Alonso et al., 2019), increase local employment (Jump and Scavette, 2022), and reduce housing prices (Jump et al., 2025). Also in the UK context, Neri (2024) finds that mixed-income public housing regenerations in London drive an inflow of affluent households into more deprived neighborhoods, and that this, in turn, helps children who originally resided in these areas to perform better in primary school thanks to the exposure to better school peers. Studies from the United States that analyze neighborhood

[2] Chyn (2018) and Haltiwanger et al. (2024) focus on the effects of HOPE VI on displaced children.

renewal programs such as Empowerment Zones (Busso et al., 2013) and Promise Zones (Bryant, 2025) also find positive short-term economic effects. The most notable exception in the literature on neighborhood renewal policies is Damm et al. (2025), which studies long-run effects, but exclusively focuses on the effects of a renewal policy on criminal behavior. Our study complements this evidence by examining the long-term education and labor market effects of such policies on individuals residing in treated areas as children, showing that the NRF continues to have positive impacts - particularly on income - long after its cessation, a result that is important per sé and to make a comprehensive welfare analysis of this type of programs.

More generally, the literature on place-based policies typically focuses on short-term employment effects (Neumark and Simpson, 2015), while studies of long-term effects are rare and limited to programs that created jobs in the manufacturing sector (see Kline and Moretti (2013) and Garin and Rothbaum (2025) for the United States, Mitrunen (2025) for Finland, Incoronato and Lattanzio (2024) for Italy, and Heblich et al. (2022) for China). Given the long-term decline of this sector that has taken across most developed economies in the last 30 years, it becomes especially important to study the effects of programs, such as the NRF, that aim to boost employment by stimulating labor supply and improve neighborhood livability rather than directly incentivizing labor demand in the manufacturing sector, and our third contribution is to provide such evidence.

The paper proceeds as follows. Section 2 presents the institutional context and the aim of the NRF program. Section 3 introduces the data used for the analysis and Section 4 illustrates the empirical strategy. Section 5 discusses the main findings, mechanisms and further results, and presents the robustness checks, and Section 6 outlines our ongoing analysis. Section 7 concludes.

2 Institutional setting

The NRF was introduced by the first Blair government in England beginning in 2001, with the ultimate goal of narrowing gaps in outcomes between the most deprived neighborhoods and the rest of England. The gaps to be targeted by the program were broad, encompassing "common goals of worklessness and crime, and better health, skills, housing and physical environment." (Social Exclusion Unit, 2001). In practice, this meant spending by Local Authorities (LAs) - the administrative units responsible for disbursing the funds through a Local Strategic Partnership (LSP) - was split between a number of areas including crime, education, employment, health, housing/physical environment, and transportation (Cowen et al., 2008).[3] To support such broad-based investment, the NRF program was large in financial scale - with average spending of about £500 million per year until its end in 2011. At its peak, the program averaged 3% of local expenditure in invested

[3]Specifically, 18-20% on crime, 18-20% on education, 15-16% on health, 11-13% on employment, 9–15% on the physical environment (including housing), 16% on cross-cutting activity, and 5% on administration (CLG, 2010).

areas - between £66 and £120 per head (Lupton et al., 2013).

LAs were eligible to receive the funds if they were ranked in the top 50 most deprived (out of 354 total LAs) based on any of six "Indices of Multiple Deprivation" for the year 2000.[4] These included the number of employment-deprived (unemployed, sick, or disabled) persons, the number of income-deprived (on means-tested benefits) persons, the proportion of the LA's population that was living in the 10% most deprived neighborhoods in England, a measure of poverty concentration (weighted average of the ranks of the neighborhoods that contain exactly 10% of the LA's population), average ward deprivation scores within the LA,[5] and the average ward rank within the LA. In 2000, 81 LAs received funding from the NRF on this basis.

The government further extended eligibility to LAs that were in the top 50 most deprived LAs based on the previous measure of deprivation, the 1998 "Index of Local Deprivation" - an index of four alternative deprivation measures. Seven additional LAs were eligible for the NRF on this basis, bringing the total to 88 (Social Exclusion Unit, 2001). Although the eligibility of these seven LAs were described as "transitional" arrangements for three years, these areas received the full funding level of the NRF, and the eligibility of *all* LAs for the NRF was re-determined in 2004 based on the 2004 Indices of Multiple Deprivation. As a consequence, the "transitional" support in practice amounted to the same policy treatment as the one received by the other 81 LAs. Following the 2004 review, five LAs lost NRF eligibility, and three new ones gained it (CLG, 2010).

Importantly, the central government explicitly advised LAs to target the funds towards wards that fell in the top 10% most deprived nationally, and descriptive evidence suggests that LAs aimed to follow these recommendations: during the first year of the program, LAs issued a report detailing how they intended to spend the money. We recovered approximately two thirds of these documents from the UK National Archives and web archive,[6] and most of them indicate that LA planned to direct the NRF to their most deprived neighborhoods. As such, we hypothesize that if the program had any positive impact on children's long-term outcomes, these effects are likely concentrated on the children who were living in the most deprived areas within the local authority, rather than be visible on all children residing in the targeted LAs.

Another important point to note is that there were no comparable neighborhood renewal programs in England prior to the start of the NRF or after it was discontinued. The NRF was preceded by the Single Regeneration Budget Challenge Fund (SRB), which also targeted neighborhood renewal. However, the SRB allocated funding using a competitive bidding process rather than through the centralized and cutoff-based eligibility criterion of the NRF. The NRF was also a higher political priority, with a more hands-on approach taken by the central government (Lupton

[4]Note that LAs did not have to actively apply for NRF money if identified as eligible, they were simply allocated the funding on a per-capita basis.

[5]Wards are smaller geographic units included in LA borders. There were approximately 8400 in England in 2001.

[6]Some reports pertain to the second round of spending beginning in 2004 following the re-assessment of eligibility.

et al., 2013). When the Labour party lost the general elections in 2011, the NRF was discontinued. Afterwards, funding for deprived areas dropped to £32 million per year (Lupton and Fitzgerald, 2015). In our empirical analysis below, we exclusively study the effects of the program on our cohorts post-cessation when their home neighborhoods are no longer being actively treated by the NRF.

While before and after the NRF there were no comparable place-based policies taking place in England targeting deprived neighborhoods, the NRF was part of a larger set of policies that the Labour government put in place to target geographic economic inequality. In particular, during the early 2000s, it also opened Sure Start centers (subsidized child-care centers), with the openings of new centers beginning in deprived neighborhoods (Conti et al., 2021). The government also introduced the New Deal for Communities (NDC), a generous job creation program targeting 39 neighborhoods across the country. Moreover, two other programs that began before the NRF and also targeted deprived areas in England were still running when the NRF was introduced: Regional Selective Assistance (RSA) and EU cohesion policy (Romero and Noble, 2008; Criscuolo et al., 2019; Fritz and van der List, 2025). Finally, the Labour government also introduced the "London Challenge" in 2003, a school improvement program targeting secondary schools in London which, while not explicitly targeting geographic inequality, overlapped thematically with the NRF in the education sector.

These policies either do not perfectly overlap with the NRF geographically (RSA, the NDC, EU cohesion policy, and the London Challenge) or exclusively target age groups outside of our analysis sample (Sure Start). Nonetheless, in Section 5.3, we show that our results remain unchanged when re-running our analysis excluding the areas targeted by these programs.

3 Data

In this Section, we describe our main sources of data. We utilize linked administrative data on individual children's educational outcomes, their earnings in adulthood, the firms where they work, and their criminal records. Through the educational records, we know whether the local authority where they lived in 2001/02 was eligible for the NRF. We also utilize aggregate LA data to undertake tests of the validity of the econometric assumptions of the RD design described in Section 4, and LA expenditure data, coupled with household survey data and administrative business registry data for additional mechanism analysis in Section 5.

NRF Targeting. We identify NRF-targeted LAs based on the original "Indices of Deprivation 2000" and "Index of Local Deprivation 1998" data used by the government. The data are provided for the 354 LAs (district/unitary level) that existed in 2000, and all six (four for the 1998 index)

sub-indicators are available in the data. Figure 1 shows the locations of the 88 LAs that received NRF transfers. Treated areas were concentrated around major cities, but also included seaside towns and rural areas. As shown in the next Section, the NRF cutoff-based targeting rule will allow us to identify Local Authorities that had a similar socio-economic composition to treated areas, but were just below the NRF cutoff based on their index ranks.

Linked Educational and Labor Market Data. To study the impact of the NRF on children's long-term outcomes, we mainly use the Longitudinal Education Outcomes (LEO) data set. The LEO database combines educational data for all individuals who attended school or college in England with information on higher education attendance and labor market outcomes from the school year 2001/02 to 2020/21. Educational data come from the National Pupil Database (NPD) and Individual Learner Record data (ILR). The NPD provides information on the characteristics of schools attended (anonymized school identifier, school, LA) and pupils' socio-economic characteristics (gender, ethnicity, main language spoken at home, eligibility for free school meals). The NPD also provides information on the deprivation level of the local neighborhood where pupils are living - the Income Deprivation Affecting Children Index (IDACI). Specifically, the IDACI index measures the proportion of all children aged 0 to 15 living in income deprived families in a Census Output (CO) area[7]. The more deprived an area, the lower the IDACI score. For example, an IDACI score of 25 means the pupil is living in a CO in the top 25% most deprived nationwide. Crucially, the NPD records information on pupils' LA of residence in each of their school years, which allows us to link LEO to the data on LAs' NRF eligibility. For this, we use the LA where the pupil was living in the school year 2001/02, the earliest year available in the data. While the NPD focuses on compulsory schooling, the ILR provides information on post-compulsory secondary education and apprenticeships.

These two datasets are then linked via an anonymized individual identifier to several additional sources of data. First, the Higher Education Statistics Agency data (HESA) provides information on higher education attendance and completion and the characteristics of the institution attended, available from 2004/05 to 2019/20. Second, His Majesty's Revenues and Customs agency (HMRC) and the Department for Work and Pension (DWP) provide data on employment status (from 1997/98 to 2020/21),[8] earnings (from 2003/04 to 2020/21), and means-tested benefit receipt (from 1999/00 to 2020/21). Third, data from the Inter-departmental Business Register provides information on employer characteristics (industry, employee numbers, and region of headquarters), linked via the employer anonymized identifier to the HMRC/DWP data. In total, LEO follows 38 million individuals, but we focus on the 1985/96 to 1995/96 birth cohorts as these were in com-

[7] A CO area contains between 40 (100) and 250 (625) households (persons).

[8] Data on self-employment are only available from 2013/14.

pulsory schooling in the first available year of LEO and they are at least 25 by the last available year. Importantly, each of these cohorts had a different exposure to the NRF, with the oldest cohort being already in the last year of compulsory schooling when the program started in 2001, while the five youngest cohorts (1991/92 to 1995/96) were still in primary school in that year. In light of the mounting evidence that neighborhood effects increase with the length of a exposure to a better neighborhood (Chetty et al., 2016; Chetty and Hendren, 2018a,b), this data feature will be especially useful to investigate exposure effects in this context.

Local-Authority level Data. To perform balance test of LA characteristics around the treatment cut-off, we use data from the 2001 Census and the 2002 Annual survey of Hours and Earnings aggregated to the pre-2009 Local Authority (district/unitary) level from the UK government's Nomis database. Specifically, we use data on gross weekly pay, hours worked, employment status, occupation groups, usual resident population, migration status, age structure, industrial sector of employment, ethnic group, and educational qualification.

Additional data. To investigate the mechanism(s) through which the NRF may have affected children's long-term outcomes we utilize several additional sources of data. First, we use the linked DfE-MoJ Share, which links the NPD to pupils' criminal records through 2020/21, to study the impact of the NRF on involvement in the criminal justice system. Second, to investigate the potential effects of the NRF on family resources, we exploit the Annual Population Survey (APS), an annual representative survey of UK households available beginning in 2004. In addition, we use the APS to study the long-term effect of the NRF on LAs' socio-economic composition.

We use two additional sources of data, the Annual Survey of Hours and Earnings (ASHE) and the Business Structure Database (BSD), to study the long-term impact of the NRF on the local economy. ASHE is an individual panel dataset that follows 1 percent of the UK workforce over time and provides information concerning employees' job characteristics, including earnings components, weekly hours worked, occupation, industry, and commuting behavior. BSD provides annual data on business creation, death, number of employees, industry, and turnover for approximately 99 percent of business organizations registered in the UK.

4 Empirical Strategy

We estimate the effect of the NRF on educational and labor market outcomes using a sharp RD design with optimal bandwidth selection (Calonico et al., 2017; Cattaneo and Titiunik, 2022) and administrative data linking individual's residence in childhood to adult outcomes from age 25 to 35. Since LAs were eligible to receive the NRF if they ranked in the top 50 most deprived areas on

any of the six 2000 or four 1998 indices of deprivation, we follow Reardon and Robinson (2012) and Alonso et al. (2019) and construct a "binding score" - or the score which determines which LAs received NRF transfers. As an intuitive explanation, consider a LA which is moderately deprived on all six indices of deprivation in 2000, and is the most deprived (rank one) for one of the sub-indices. The binding sub-index for this LA would be the one on which it is ranked the most deprived. This is because it does not matter if this LA is close to the eligibility threshold on other sub-indices or not, its "effective" eligibility status is determined by the sub-index on which it is the most deprived. In the case of a LA which is ineligible for the NRF, the binding score is the rank of the sub-index on which they are closest to the threshold, as that brings it the closest to eligibility.[9] Mathematically, we define this binding score with an eligibility threshold of zero by taking the minimum of the distance of the rank of the sub-index minus fifty.

$$x_l = -\min_s (r_{ls} - 50) \tag{1}$$

where r_{ls} is the rank of LA l on sub-index s of the 2000 and 1998 indices of multiple deprivation. For ease of interpretation, we take the negative of this minimum so a positive value of the binding score implies being treated by the NRF.

Our regression specification is therefore:

$$y_{it} = \beta_0 + \beta_1 NRF_{l(i)} + \beta_2 x_{l(i)} + \beta_3 x_{l(i)} \cdot NRF_{l(i)} + \gamma_c + \epsilon_{it} \tag{2}$$

where i indexes individual and t age (e.g., 25) at which outcome y_{it} is measured. $NRF_{l(i)}$ is an indicator equal to 1 if individual i grew up in an LA l that was NRF-eligible. In all regressions, we include cohort fixed effects γ_c, and use robust standard errors clustered at the LA level.

Importantly, the running variable $x_{l(i)}$ is discrete by construction, given that it is essentially a rank variable, while the canonical RD design assumes that the score that determines the treatment status is a continuous random variable. However, as discussed by Cattaneo et al. (2024), if the running variable is discrete but has a reasonably large number of mass points, i.e. unique values, and for each of them the researcher has access to a large number of individual data - as in LEO - it is still possible to apply standard continuity-based RD methods.[10] In our context, the running variable takes 354 values, corresponding to the number of LAs in England in 2000, and our total sample of children is more than 4.5 million. According to Cattaneo et al. (2024), this number is sufficiently large to conduct a continuity-based RD analysis.

[9] This means our running variable construction differs slightly from Alonso et al. (2019), which uses the underlying indices rather than the ranks to construct the running variable using the binding score. The construction used by Alonso et al. (2019) is outlined in Appendix A, along with discussion of our reasons to use the ranks to construct the running variable.

[10] Although one needs to perform a local linear extrapolation towards the cutoff in order to interpret the estimated effects as the continuity-based RD treatment effect at the cutoff.

Figure 2 shows the location of the Local Authorities that fall into the optimal bandwidth for one of our main outcomes of interest, "Log earnings at age 25". As it could be expected, treated and control areas are geographically concentrated, suggesting that they may have a similar socio-economic composition. In the next paragraph, we test this formally given that this is one of the two requirements for the validity of the RD design.

RDD assumptions. The validity of the continuity-based RD design rests on two main assumptions (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). First, the units or individuals targeted by the policy analyzed should not be able to manipulate the running variable to influence their treatment status. In the case of the NRF, LA eligibility was determined by the UK government based on indices of deprivation computed before the policy was announced, which rules out the risk of manipulation by the LAs. However, in principle, one could think that once the policy was announced, households could move to targeted areas to benefit from it. This would be especially problematic in our case, given that we can only observe children's LA of residence in the school year 2001/02 when the program had just been introduced, and therefore we cannot assign treatment status based on the pre-policy LA of residence. While household endogenous mobility is a priori possible, and the literature on place-based policies provides evidence of gradual changes in household composition in response to these programs, it seems unlikely that a family would move to a deprived area immediately upon announcement of a such a program.

Furthermore, in Table 1, we test for evidence of gradual changes in household composition in response to the NRF program. Specifically, we test for discontinuities around the eligibility threshold in the probability of a pupil's family moving from the Lower Layer Super Output Area (LSOA, a geographic unit consisting of 1,000-3,000 people) of residence in the 2001/02 school year to any other LSOA (including within the same LA) during the years of compulsory schooling. We fail to find evidence that families living in NRF-eligible areas displayed differential moving behavior from those living in non-NRF areas while the NRF was in place. We also test for differential moving behavior for the pupils living in the top-25 and top-10% most deprived neighborhoods as measured by the IDACI score (discussed in Section 3 above), and we also do not observe evidence of discontinuous sorting patterns for the families most likely to be in neighborhoods within the LA targeted by the NRF. Given this, sorting would need to have taken place near-immediately after the announcement of the NRF and completed by the beginning of the 2001/02 school year, which seems implausible.

The second assumption that an RD design needs to satisfy concerns the comparability of baseline characteristics around the policy cutoff. If the assignment of the treatment is as good as random near the program-eligibility threshold, then all those variables determined prior to the realization of the assignment variable should have the same distribution just above and below the

cutoff. In Figure 3, we present balance tests of LA baseline characteristics around the eligibility threshold using optimal bandwidth calculation.[11] The data are drawn from the 2001 Census and the 2002 ASHE aggregated to the LA level. The Figure shows that we fail to find evidence of systematic discontinuous changes in the characteristics of LAs around the eligibility threshold, with only one statistically significant jump in the long-term unemployment rate (p-value of .055) out of the 12 placebo tests reported in the Figure. Furthermore, in Table 2 we show balance tests of individual pupil characteristics around the eligibility threshold using the demographic information provided in the LEO data and optimal bandwidth calculation. There are no statistically significant differences in the gender, ethnicity, language spoken at home, or ages of school children at the threshold.

5 Results

In this Section, we study the long-term impacts of the NRF on individuals exposed to the policy as children. We begin by showing the effect of the program on our main variable of interest, individual earnings at age 25. We then discuss the potential mechanisms behind the main effects, test the robustness of the results to different outcome definitions and regression specifications, present the impact at later ages, and explore effect heterogeneity.

Importantly, as noted in Section 2, in light of the government recommendation to direct the NRF funds towards deprived areas of targeted LAs, we do not necessarily expect to find an effect of the program on all children residing in NRF-eligible LAs - its impact is likely to be concentrated on those living in the poorer areas. We begin our empirical analysis by testing precisely this hypothesis.

5.1 Income at age 25

Figure 4 shows a series of RD plots that depict the relationship between the running variable and the log of individual earnings at age 25 for different groups of individuals. As explained in the previous Section, the running variable is the binding rank that determines LAs' eligibility to receive the NRF, which we refer to as "index of multiple deprivation". As discussed previously, treated observations have a non-negative running variable. As it is typical of RD plots, each graph shows two different estimates of the conditional expectation of the outcome function given the running variable, computed separately for treated and control groups: local sample means of the outcome computed in equal-sized bins of the running variable, with the use of Calonico et al. (2015)'s data-driven approach; and two global polynomial fits of the outcomes on the running variable, estimated

[11] The full distribution is shown in Appendix Figure B.1.

using a linear polynomial and triangular kernel. The top-left graph depicts the relation of interest for the entire sample, while the following graphs progressively restrict the sample to children who lived in more deprived areas in 2001/02. For this, we exploit the so-called "income deprivation affecting children index" (IDACI), the standard measure of neighborhood deprivation provided in the NPD, which, as discussed in detail in Section 3, classified local areas based on the proportion of children under the age of 16 that live in low income households. This implies that we are comparing children in equally deprived neighborhoods living in (geographically larger) LAs that were either just-eligible or just-ineligible for the NRF. In each graph, the sample is further restricted to observations that fall within the MSE-optimal RD bandwidth selected through Calonico et al. (2014)'s approach.

Together, the graphs tell us four things. First, when focusing on the control group in the left-hand side of the graphs, there is a negative relationship between adult earnings and the deprivation rank of the LA where individuals grew up. Unsurprisingly, children growing up in more deprived local authorities have lower incomes as adults. Second, the slope of this relationship gradually changes for the treatment group (right-hand side part of the graph) as we move from the entire sample to more deprived areas of the local authority. Third, it appears that children growing up in a LA that was just eligible for the NRF tend to have higher earnings at age 25 than those who grew up in a LA falling just below the NRF-eligibility cutoff, pointing to a positive effect of the policy on adult income. Finally and most importantly, the magnitude of this discontinuity appears to grow as we progressively focus on children growing up in more deprived areas of their LA.

Table 3 reports the corresponding point estimates of the effect of the NRF on earnings at age 25. In line with the graphs and consistent with the program objectives, the point estimates confirm our hypothesis that the impact of the NRF is concentrated in the most deprived areas: the estimated RD coefficients on earnings are smaller and insignificant in the first two columns, referring, respectively, to the entire sample, and children growing up in neighborhoods ranking up to the 75th percentile of the national distribution of neighborhood deprivation. However, the effect size does grow when examining the 75th percentile (5.8%) compared to the entire sample (1.5%). The point estimate continues to grow in magnitude as we restrict our sample to children growing up in progressively more deprived neighborhoods. For the children growing up in the 50th percentile of the national distribution of neighborhood deprivation, shown in Column (3), the point estimate is larger (9.3%), and is now statistically significant at the 5% level. The effect size seems to peak at the 25th percentile of neighborhood deprivation, shown in Column (4), with point estimates that are economically large (15.4%) and highly significant at the 1% level. The estimated effect size is identical in Column (5), which shows the results for the 10th percentile. Hereafter, we focus our analysis on the sample of children in, Column (4), i.e., those growing up in areas ranking in the

bottom quartile.[12]

Table 4 reports complementary outcomes related to income. Column (1) again reports the estimate from Table 3, Column (4), the point estimate of the RD specification where the dependent variable is log earnings at age 25. Columns (2) and (3) report the NRF effect on the percentile rank of the individual's income within their cohort at age 25, an outcome commonly examined in the intergenerational mobility literature (Chetty et al., 2014, 2024). An advantage of the income rank specification in Column (2) is that unlike the log specification in Column (1), we are able to include individuals with zero earnings in the sample. The result remains large and significant when using this alternative outcome: children growing up in deprived neighborhoods LAs that received the NRF were 4.1 percentage points higher in the income distribution compared to the control group. When restricting the sample to individuals who work in Column (3), the effect is even larger at 5.8 percentage points. We view the smaller magnitude of the effect in Column (2) compared to Column (3) to be suggestive evidence that the mechanism through which the NRF increased incomes is more likely to be on the intensive rather than extensive margin of employment. If the program was pulling individuals into the labor force, we would expect to see the opposite.

Column (4) of Table 4 presents the estimated effect of the program on the probability of being a means-tested benefit recipient at age 25. Although the point estimate is negative, as would be expected with an increase of incomes, the effect is not statistically significant. This is also consistent with the hypothesis that the program operates along the intensive margin of employment, since individuals may be more likely to lose their entitlement to benefits when they find a job compared to when adjusting their working hours.

Overall, we find clear and consistent evidence that the NRF had a large, approximately 15%, effect on the adult incomes of individuals exposed to the policy as children, and specifically for those individuals who grew up in the top 25% most deprived neighborhoods in England. To benchmark the size compared to other place-based interventions, this effect is near-identical in magnitude to the effect of public housing revitalization on adult incomes of children growing up in treated units estimated by Chetty et al. (2026), comparable to that of Finland's post-war forced industrialization studied by Mitrunen (2025), 2.5 times larger than the effect of WWII US-government-led industrialization analyzed by Garin and Rothbaum (2025), and half the size of the Moving to Opportunity (MTO) impact on young cohorts estimated by Chetty et al. (2016). In the next Section, we study the mechanisms behind this sizable positive effect of the policy on children's adult income.

[12] We focus on the 25th rather than 10th percentile because our control group becomes much smaller in size compared to the treatment group when studying the 10th percentile. This is because, as one could expect, in control LAs there are relatively much fewer neighborhoods in the top 10% of deprivation nationally.

5.2 Mechanisms

The NRF was a composite policy that aimed to improve deprived neighborhoods by intervening across five main areas: education, crime, health, housing/physical environment (including transportation), and unemployment. Therefore, it is especially important to investigate whether the positive effects of the NRF on childrens' earnings are driven by a specific channel, such as increased educational attainment or improved job access, or by improvements across different dimensions. In this Subsection, we demonstrate that the positive effects of the NRF are most likely driven by increased attachment to the labor market and improved access to better-quality jobs. We also present evidence that family income during childhood, educational attainment, migration patterns, reduced involvement in the criminal justice system, and changes in the neighborhood socio-economic composition do not explain the income effects described above.

Figure 5 reports the estimated RD impact of the policy on outcomes related to a number of potential mechanisms for children growing up in the most deprived neighborhoods (IDACI rank $\leq$ 25pct).[13] The first way the NRF could have a long-lasting impact on adult outcomes is by improving the economic conditions of the household during childhood, since it is well known that the home environment has long-term effects on children's outcomes (Duncan et al., 2023). Although we cannot test this outcome directly using the LEO data, we are able to proxy for household poverty status using free school meal eligibility (FSME) during the school years. Figure 5 shows that we fail to find significant evidence that children exposed to the NRF were less likely to receive free meals in school - though the negative coefficient is consistent with improved economic circumstances of their households during childhood. In Appendix Table B.3 we present complementary evidence from the Annual Population Survey (APS) on the economic well-being of households with children between 2005 and 2010, while the NRF was in place. These results show, consistent with the free school meal results from the LEO, that households in LAs just eligible for the NRF did not seem to be better off compared to those in ineligible LAs. If anything, the estimates point to the opposite.[14]

The second potential mechanism the NRF could operate through is increasing educational attainment, which we also investigate in Figure 5. Again, we fail to find strong evidence that this is a key channel. In particular, although both coefficients are directionally positive, treated students are not significantly more likely to have graduated from upper secondary school or from university compared to the control group.

Another channel through which the NRF could have improved adult outcomes is by reducing

[13] Exact point estimates are shown in Appendix Table B.1.

[14] Note that the sample size of the APS survey is too small to draw definitive conclusions, with between 2500 and 8800 observations per specification. Moreover, the effective sample size is even smaller, given that the observation counts in the Table refer to the total sample size, rather than the number included in the optimal bandwidth.

involvement in the criminal justice system. This is an important channel to investigate, since our point estimates for income at age 25 are of very similar magnitude to estimates of the income effects of having a criminal record in Agan et al. (2022), and Alonso et al. (2019) finds that the NRF contemporaneously reduced *aggregate* crime rates in treated LAs. At the same time, the net effect of the NRF on the probability of *individuals* having a criminal record is a priori ambiguous, because the interventions implemented by LAs in this area targeted both prevention and enforcement. Thus, it could be the case that, even despite the aggregate decreases in crime shown in Alonso et al. (2019), increased enforcement could still lead to increases in observed criminal records. Figure 5 shows the effect of the NRF on the probability of having a criminal record between the ages 10-19 and 20-35, for boys and girls separately.[15] The only significant result (at the 10% level) is a positive effect of .043 p.p. (17 percent compared to the control mean) for boys between ages 20 and 35. Although the magnitude of this estimate seems too large to be explained only by enforcement effects, it does seem to exclude the possibility the income effects we observe for our cohorts are related to reductions in the probability of having a criminal record.

The NRF could have also enhanced geographic mobility, enabling the treatment group to access improved economic opportunities outside of the LA where they resided in childhood. Again, in Figure 5, we fail to find strong evidence in support for this channel. Although all coefficients are directionally consistent with increased economic sorting, the policy does not significantly change the probability of living in the same LA or macroeconomic region in adulthood as in childhood, or the probability of living in London as an adult.

The NRF could have also changed the socio-economic composition of the treated areas. Such socio-economic changes have been shown to improve the outcomes of incumbent or economically disadvantaged children who remain in the neighborhood in previous research (Baum-Snow et al., 2019; Chetty et al., 2026). Although in our case this seems unlikely, given that we do not observe differential moving behavior among our cohorts during childhood in Table 1 or during adulthood in Figure 5, we are able to test the effect of the NRF on household characteristics within LAs using the APS and 2021 Census. The APS has the advantage of taking place annually and includes information about the IDACI rank of the neighborhood, enabling us to track potential neighborhood change in the most deprived areas of LAs over the lifetime of the NRF program. However, it has the disadvantage of a small sample size (particularly when restricting to deprived neighborhoods) as discussed above. The 2021 Census, on the other hand, is only available for one year aggregated to the LA level, but the underlying number of observations per LA is much higher than in the APS data.

The results using the APS for households in living in deprived neighborhoods are shown in

[15] Results are separated by gender since boys are much more likely to be involved in criminal activity at all ages, as can be seen in the control means at the bottom of Appendix Table B.1.

Table 5.[16] The NRF did not have clear and consistent effects on any of the examined outcomes, though some results are statistically significant in some years. In particular, there are significant results for more than one year for the probability of being under the age of 35 (increased probability), being a benefit recipient (reduced probability), working in a high-skilled job (reduced probability), and hourly pay (negative in 2008-2010, positive in 2014-2017). The results are a mixed bag, some suggesting positive effects (benefit recipient), whiles others suggest negative effects (working in a high-skilled job). Results for the 2021 Census are shown in Table 6. Using the census data, we do not find any statistically significant effects on demographic composition or aggregate labor market outcomes. Overall, these results from both datasets that the aggregate effect of the NRF on neighborhood socio-economic composition was not clearly or uniformly positive, meaning that is is unlikely to be the mechanism underlying the improved outcomes we observe for our cohorts.

The final mechanism through which the NRF could improve adult labor market outcomes is more direct: by increasing extensive- or intensive-margin labor supply. Recall that our main analysis in above suggested that the intensive-margin effects of the policy were stronger than the extensive-margin effects. In Table 7, which report the estimated impact of the policy on the quantity and quality of work undertaken by individuals throughout the year, we find further evidence from the LEO data consistent with that hypothesis. In Column (1), we rule out extensive-margin labor supply effects, as the NRF has no effect on employment during the tax year, with a coefficient very close to zero and statistically insignificant. Instead, Columns (2)-(4) show that the policy does affect intensive-margin labor supply, by enhancing labor market attachment and job quality. In particular, affected cohorts of children are significantly more likely to work more days throughout the year (Column 2, +4.9 percent). Although this does not translate into a significantly higher probability of working for a single employer (Column 3), Column (4) shows that the probability of working for a large firm is significantly larger (4.8 percentage points or 8 percent compared to the control group) for the treated group.

In Appendix Table B.4, we provide complementary evidence on job characteristics and commuting behavior from the Annual Survey of Hours and Earnings (ASHE) from 2005 to 2019. The survey data is for individuals living in the LA at the time of the survey, so the estimated coefficients are a mixture of policy and sorting effects. The evidence on working for a larger firm (Column 6) and for higher pay (Column 9) are consistent with the results from the LEO, with positive and sometimes significant point estimates.[17] Column (2) also shows some suggestive evidence that individuals are less likely to be working in their home LSOA. Given that the NRF included investments in transportation infrastructure, this would also be consistent with those investments facilitating access to "better" jobs for individuals living in the most deprived areas of the LAs.

[16] The corresponding results for all households in the LA are shown in Appendix Table B.2.

[17] The same caveats about sample size discussed above with respect to the APS results apply here.

Overall, these results suggest that, rather than stimulating human capital accumulation, the NRF boosted young cohorts' income primarily through intensive-margin labor supply effects. It increased overall labor supply conditional on working and facilitated access to higher paid jobs at large firms. This mechanism is consistent with the results of Garin and Rothbaum (2025), who show that the long-run positive impacts of large manufacturing plant openings on children growing up in treated areas are driven by increased access to better jobs rather than higher human capital attainment. At the same time, considering that the NRF did not include incentives for firms to relocate to targeted areas, this result raises the important question of how the program succeeded at generating job opportunities for young cohorts. We now turn to exploring this question in further detail.

5.2.1 Firm Entry

We investigate the effect of the NRF on firm entry decisions in more detail using the Business Structure Database (BSD) data. Since the BSD data are available beginning in 1997, we use a difference-in-differences (DiD) design rather than an RDD.[18]

Our outcomes of interest y_{jlt} are the number of establishments and the log number of employees that parent firm j has in IDACI 25 areas of LA l in year t.[19] To ensure the sample of LAs is the same as in our RDD design, we limit our sample to LAs in the optimal bandwidth of our main "income at age 25" results in Section 5.1. We undertake firm-level event studies of the form:

$$y_{jlt} = \alpha_l + \delta_t + \sum_{\substack{k=-4 \\ k\neq -1}}^{17} \gamma_k \cdot NRF_l \cdot 1(t = 2001 + k) + \kappa_j + \epsilon_{jlt} \tag{3}$$

Where α_l are LA fixed effects, δ_t are year fixed effects, and $1(t = 2001 + k)$ are the event-time dummies. In addition, we control for firm fixed effects κ_j. We omit the dummy for the year prior to beginning of the NRF, 2000, so γ_k identifies the changes in outcomes of firms in NRF compared to non-NRF LAs relative to the same difference at $k = -1$.[20]

In Figure 6, we show the results for the agriculture, construction, transportation and communication, and other services sectors, while in Appendix Figure B.3 we report the results for

[18] Using the DiD design allows us to take full advantage of our sample size. As discussed in Cattaneo et al. (2024), when implementing mass point RDD estimation, the local polynomial estimation behaves as if each mass point (LA) was a single observation. This means our effective sample size is smaller when using the RDD design, and therefore the DiD design is preferred when pre-treatment data are available.

[19] In our main specifications, to ease computation burden we only include in our analysis sample firms-LA pairs where the firm is present. I.e., we do not include lines of data with zeros in the locations where the firm does not operate.

[20] Note that since the NRF began in 2001 in all treated LAs, we do not have concerns around staggered treatment adoption (Callaway and Sant'Anna, 2021) and implement our estimation using two-way fixed effects.

the five sectors with insignificant outcomes (energy, manufacturing, wholesale/retail trade, finance/banking, and public administration/education/health). In each of these sectors, we observe statistically significant increases in either the number of establishments (construction, transportation, and other services sectors) or employment (agriculture, transportation) in the post-NRF period. Results emerge over long time horizons, most likely because firm adjustment to changing conditions in the NRF-eligible LAs is slow.

This is suggestive evidence that some sectors responded to improving conditions in the most deprived areas of the treated LAs by entering or expanding into these areas. These firms then provide the access to the better jobs driving the improved results for the children growing up in treated LAs discussed previously.

In Figure 7, we complement these results by estimating with the LEO data the NRF RD effect on the probability that our cohorts are working in a particular industrial sector. Consistent with the firm-level results, we find that the sectors where we observed increased firm entry in the BSD (agriculture, construction, transportation and communication, and other services) all have positive point estimates, though the effect is only statistically significant for the agriculture sector.

5.3 Robustness Checks

In this Section, we perform a series of robustness checks to test the sensitivity of our results to perturbations of our regression specification. First, we re-run the analysis including region fixed effects in addition to the cohort fixed effects included in our main specification. Then we re-estimate our results excluding LAs or neighborhoods that are eligible for other public transfers during the years the NRF is in place. In the interest of space, we present these estimates graphically in Figure 8, and report the corresponding point estimates in Appendix Table B.5. Our results are very similar to the main estimates across all robustness checks.

Region Fixed Effects. We do not include region fixed effects in the main specification because it is extremely demanding given the number of LAs included in the optimal bandwidth of our RD design. As can be seen in Figure 2, the geographic distribution of the LAs in the optimal bandwidth means that when including region fixed effects, the effective sample size drops to comparing children in approximately 2-4 local authorities to one another. However, given that there may be time-invariant region-specific characteristics, such as the distance from London, that may influence the outcomes of interest and potentially be correlated with the NRF-targeting, we re-run our regressions including region fixed effects. As shown in Figure 8, with the exception of the effect on the probability of working at a large firm, which becomes small and insignificant when including region fixed effects, the estimated impact on all the other key outcomes is similar to our main

specification.

Concurrent Programs. The NRF targeted the most deprived local authorities in England, but was part of a wider effort by the then Labour government to tackle geographical inequality. As part of this larger initiative, the government implemented a package of different policies throughout the first decade of the 2000s, and some of them partially targeted the same areas of the NRF. Moreover, some of the NRF-targeted areas were also eligible for other sources of funding that pre-dated the NRF, such as Regional Selective Assistance (RSA), aimed at boosting job creation, and EU cohesion policy funds for disadvantaged areas. We first describe each of these programs in more details, and then compare our main results with estimates obtained by excluding, in turn, the LAs (or neighborhoods within LAs) that were eligible for these different sources of funding.

London Challenge. In an effort to boost the performance of London secondary schools, the UK government implemented an intense improvement program between 2003 and 2011, which was named the "London Challenge".[21] This program mainly focused on five London Boroughs, namely Lambeth, Southwark, Hackney, Haringey and Islington. The London Challenge aimed to tackle under-performance through expert counseling on school local planning, a school leadership training program available across all the city's schools, and a large increase in funding - at its peak it had a budget of 40 million a year (Kidson and Norris, 2015). While several reports provide descriptive evidence that the London Challenge decisively contributed to the improvement in performance that London schools experienced throughout the first decade of the years 2000s, many experts conclude that isolating the causal impact of this program from that of other initiatives taking place at the same time is impossible (Burgess et al., 2014; Greaves et al., 2014). Here, we take into consideration the role of this program by running a specification where we exclude the five boroughs that received most of the London Challenge support. As shown in Figure 8, our results change very little when performing this robustness check.

New Deal for Communities. One of the programs that the first Blair government put in place at the same time as the NRF is the New Deal for Communities (NDC). The program targeted 39 small geographic areas (neighborhoods) with approximately 10,000 residents with large-scale investment of £50 million each (Lupton et al., 2013). Importantly, despite its generosity and partial overlaps of goals with the NRF, the NDC is unlikely to explain our results considering its very local nature.[22] Furthermore, the majority of these 39 neighborhoods are located in LAs which are sufficiently

[21] Following the perceived success of the program, in 2008 the government extended it first to primary schools and then to schools in Greater Manchester and the Black Country.

[22] With each neighborhood having 10,000 residents, approximately 390,000 persons (mostly adults) were exposed to the NDC program, while our main sample includes more than 4.7 million children)

deprived that they are not close to the NRF cutoff - only eight of them are within the optimal bandwidth for our main "earnings at age 25" results. Nonetheless, to ensure that our results are not driven by this concurrent program, we re-run our results excluding the areas that receive NDC funding. As shown in Figure 8, all results are similar when using this subsample.

Regional Selective Assistance. Finally, there were two other sources of funding that pre-dated the NRF, were in still in place when it was introduced, and had partial geographic overlap with the NRF. The first policy that was already in place when the NRF was introduced was the Regional Selective Assistance (RSA), which was a UK firm-investment-support program targeting the manufacturing sector (see Criscuolo et al. (2019) for further details about the RSA). The majority of areas targeted by the RSA were in Scotland and Wales, while the NRF was an England-specific program, but there was some geographic overlap with the NRF. By its nature, this policy was much more limited in scope than the NRF, targeting only firms in the manufacturing sector. Moreover, Criscuolo et al. (2019) show that the program increased employment at treated firms, but not wages. Therefore, the only plausible mechanism through which the RSA could affect our cohorts, who were children at the time of the policy, is by providing employment opportunities for their parents. To take into account this possibility, we repeat our analysis excluding RSA-eligible LSOAs from the sample and find very similar results, as shown in Figure 8, with the exception of the log days per year shown in Panel C, which is no longer significant.

EU Cohesion Policy. The second policy that was already in place when the NRF was introduced is EU Cohesion Policy (henceforth cohesion policy), which is the main place-based policy of the European Union, investing heavily in so-called "less-developed" regions, those with a GDP per capita less than 75% of the EU average. Cohesion policy is similarly (or more) broad-based in terms of goals compared to the NRF; furthermore, the financial scope of cohesion policy is immense (see von Ehrlich and Overman (2020) and Fritz and van der List (2025) for further details concerning cohesion policy). However, cohesion policy is an EU program, while the NRF was an England-specific policy. As a consequence, because Scotland and Wales are typically less wealthy compared to England, the majority of UK regions that were eligible for the policy were in these two countries. Nonetheless, given that there was some geographic overlap in Cornwall and South Yorkshire, in Figure 8, we repeat our analysis excluding from the sample LAs that are located in regions that were eligible for cohesion policy funds.[23] These results are very similar to our main estimates, with near-identical point estimates and confidence intervals with the exception of the log days per year shown in Panel C, which is no longer significant.

[23] Specifically, we exclude areas of England that were eligible for Objective 1 funding or transitional support (European Union, 2005).

5.4 Effect Persistence and Heterogeneity

The results discussed so far have shown that the NRF improved children's labor market prospects at age 25. An important policy question is whether these effects persist over even longer periods of time. The LEO data allow us to investigate this question, as we can follow older cohorts of children until age 35, and at least some of the youngest ones until age 28. To maximize the sample size, when analyzing the effects at older ages, we pool the data between age 30 and 35.[24]

We present these results in Table 8, with Panel A focusing on age 28, and Panel B on ages 30-35. First, in Column (1), we confirm that that there are also no significant extensive-margin labor supply effects at older ages. In Columns (2) to (3) we study the persistence of job stability improvements observed at age 25. There is scant evidence for the persistence of these effects. The impact on log days worked per year in Column (2) is not significant at either age range, but the probability of working for a single employer after turning 30, shown in Column (3), is statistically larger with a point estimate of .026. Overall, the job stability and labor market attachment effects seem weaker at older ages compared to at age 25. However, the probability of working for a large firm, shown in Column (4), remains significantly higher for the treatment group compared to the control group both at ages 28 and from 30-35. This is, once again, consistent with the idea that improvements in income, which are shown in Column (5) and remain significant and of similar magnitude, stem from access to "better" jobs at larger employers.

Additionally, Columns (6)-(7) present evidence that improved labor market outcomes are not being driven by increases in entrepreneurial activity, since the probability of self-employment does not increase.[25] Furthermore, conditional on self-employment, earnings do not increase as shown in Column (7). Finally, Column (10) shows the probability of being on benefits. Again, we find results consistent with our findings at age 25, that the probability of being on means-tested benefits does not significantly decrease as a result of the NRF.

We conclude our main analysis by investigating potential heterogeneous effects of the NRF along two dimensions. First, we test whether the program affects boys and girls differently, considering both the ample evidence on gender gaps in educational attainment (in favor of girls) and labor market outcomes (in favor of boys) (Reeves, 2022). Since the effects of the policy seem to be due to increases in intensive-margin labor supply rather than increases in human capital accumulation, we would ex-ante expect that the effects would be larger for boys - who are more likely to participate in the labor force. Second, we study whether the NRF has larger effect on younger

[24] Due to the pooling, the outcomes slightly change compared to those of previous ages. In particular, we study the probability of ever being employed between age 30 and 35, average (log) days worked per year, average yearly (log) earnings, the likelihood of ever working for at most 1 (2) employer(s), the probability of ever been self-employed, average yearly self-employment (log) earnings, and the likelihood of ever being on benefits between 30 and 25.

[25] This margin could not be investigated in our main mechanism analysis at age 25 because data on self-employment in the LEO is not available for earlier years.

cohorts, in light of the mounting evidence that the longer children are exposed to a better neighborhood, the stronger its beneficial effect is (Chetty et al., 2016; Chetty and Hendren, 2018a,b). The LEO data allow us to study the effect of the NRF on 11 cohorts of individuals who were between 6 and 16 years old when the program was introduced, and compare its impact across those who were already in secondary school in 2001 and those who were still in primary school at that point. Importantly, since our effects seem to operate along a job-access channel, we might expect less heterogeneity by exposure in our setting.

Our results separated by gender, shown in Table 9 are consistent with our hypotheses. Panel A shows the results for the entire sample, Panel B the results for girls, and Panel C the results for boys. The effects on increased number of days within the year worked are shown in Columns (1), (4), and (7) for ages 25, 28, and 30-35, respectively. Consistent with on-average lower intensive-margin labor supply among women (and therefore more flexibility to adjust hours) the increase in the number of days worked at age 25 is driven by girls. The probability of working for a large firm, shown in Columns (2), (5), and (8) for ages 25, 28, and 30-35, respectively, significantly increases for both boys and girls at ages 25 and 30-35, but the point estimates are approximately twice the size for boys. The same is true for the earnings results, shown in Columns (3), (6), and (9) for ages 25, 28, and 30-35, respectively. In particular, the impact of the NRF on earnings is only significant for boys, and is very large at approximately 18-19% across all ages. Again, this evidence is consistent with a job access rather than human capital accumulation mechanism, as in the latter case one would expect the results to be larger for girls.

As for exposure effects, Table 10 reveals that the NRF does not have strong heterogeneous effects along this dimension. Panel A shows the results for the entire sample, Panel B for the less-exposed cohorts (born 1985/66 to 1990/91), and Panel C the results for the more-exposed cohorts (born 1991/92 to 1995/96). At age 25 (Columns 1-3), all results are significant for both the more- and less-exposed cohorts, although the point estimates for log days per year in Column (1) and the probability of working for a large firm in Column (2) are somewhat smaller for the more exposed cohorts. The point estimates for the main outcome of log earnings in Column (3) are significant and very similar in magnitude for both the more- and less-exposed cohorts. The main difference between the results is at age 28, when the effect on the probability of working for a large firm becomes insignificant for older cohorts (Column 5). However, there are still large and significant effects of the NRF on log earnings (Column 6) for both groups of cohorts. Altogether, this is another piece of evidence that the program operates through a job access channel.

Overall, the persistence of the effects and the patterns of effect heterogeneity we observe in our results suggest that the positive economic impacts of the NRF persist over long periods of time, and that access to high-quality jobs at large firms is a particularly important driver of these effects.

6 Next Steps

We have received access to LA expenditure data for the period the NRF was in place, which we plan to use to understand how targeted LAs actually spent the NRF funding allocations. With these data, we will also undertake further heterogeneity analysis using the estimator of Calonico et al. (2025), which will enable us to study if some categories of interventions undertaken by the LAs were more effective compared to others.

Beyond the mechanism analysis, we plan to undertake a number of further robustness checks exploring technical econometrics issues. In particular, to take into account that our running variable is discrete, we plan to replicate our analysis employing both the local randomization method formalized in Cattaneo et al. (2015) and the method proposed by Kolesár and Rothe (2018).

Finally, we plan to conclude by performing a cost-benefit analysis of the NRF, using the Marginal Value of Public Funds (MVPF) method (Finkelstein and Hendren 2020, Hendren and Sprung-Keyser 2020).

7 Conclusion

An increasing number of studies show that the neighborhood where a child grows up causally influences a large arrays of long-term outcomes (Chetty et al. 2016, Chetty and Hendren 2018a, Chetty and Hendren 2018b, Chyn 2018, Chyn and Katz 2021, Haltiwanger et al. 2024). Despite this, we know very little about how government programs aimed at revitalizing deprived neighborhoods impact the long-term trajectories of children who grow up in targeted areas. This paper contributes to answering this key policy-relevant question by studying an ambitious neighborhood renewal program that was implemented in England between 2001 and 2011 in the most deprived areas of the country.

The first Blair government (1997-2001) introduced the Neighbourhood Renewal Fund (NRF) in 2001 with the ultimate goal of narrowing gaps in outcomes between the most deprived neighborhoods and the rest of England. The program was large both in size and scope, offering targeted areas £500 million per year, to be allocated across five different categories of interventions, namely education, crime, health, unemployment, and housing. This paper estimates the long-term effects of the NRF on the labor market outcomes of children who resided in targeted areas at the time of the program implementation, using a sharp RDD design that exploits the cutoff-based program eligibility rule.

Our results show that children who were still in compulsory school when the NRF was introduced enjoy 15-percent higher earnings at age 25 compared to the control group who just missed out on the program eligibility. These effects persist to even older ages, through the end our

our analysis period at age 35. Mechanism analysis shows that an increased attachment to the labor market and enhanced job prospects, rather than increased human capital accumulation, explain these results.

Given these results, an important question for future research is how the NRF succeeded attracting firms to treated areas, considering that the program did not include incentives for firms to relocate. Potentially, targeted LAs may have been able to attract businesses in their deprived neighborhoods by creating the perception of a safer environment. Alternatively, the program may have pushed young people to look for jobs in the richer parts of their LAs. Distinguishing between these channels is especially important to policymakers designing neighborhood revitalization programs.

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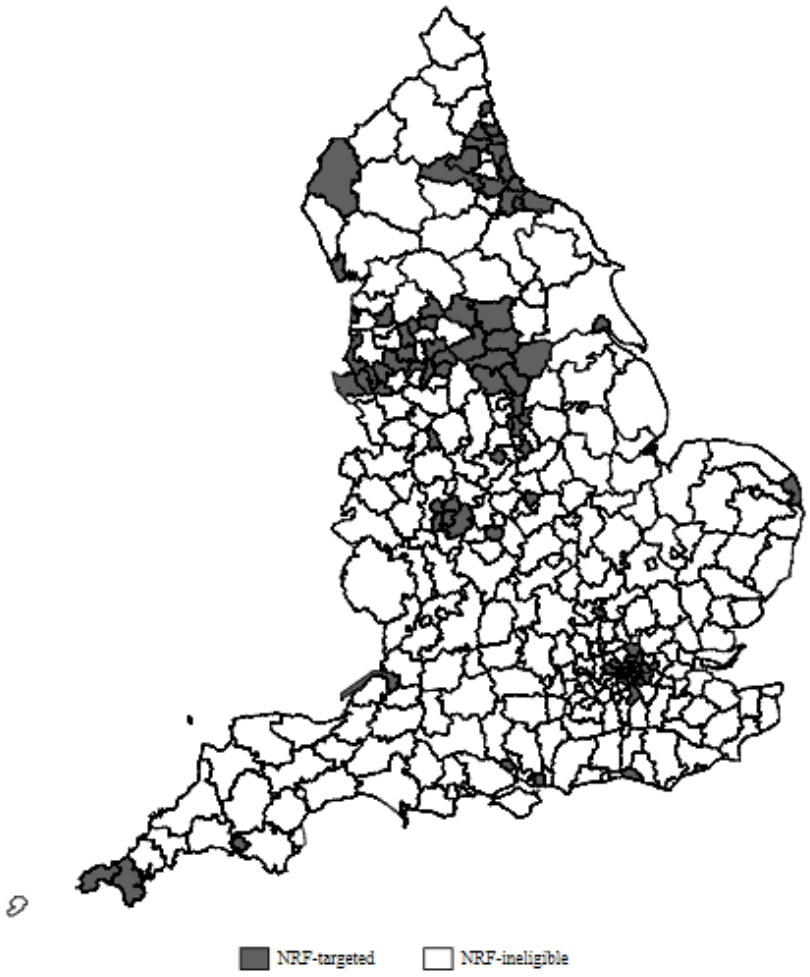
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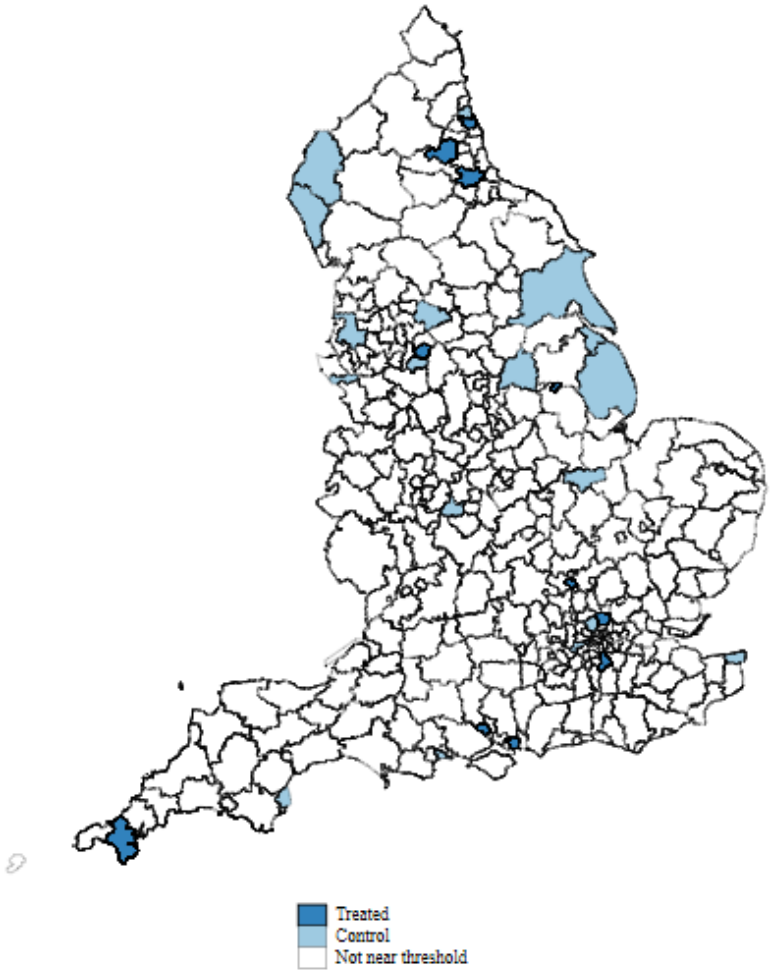
Figures

Figure 1: NRF Targeting



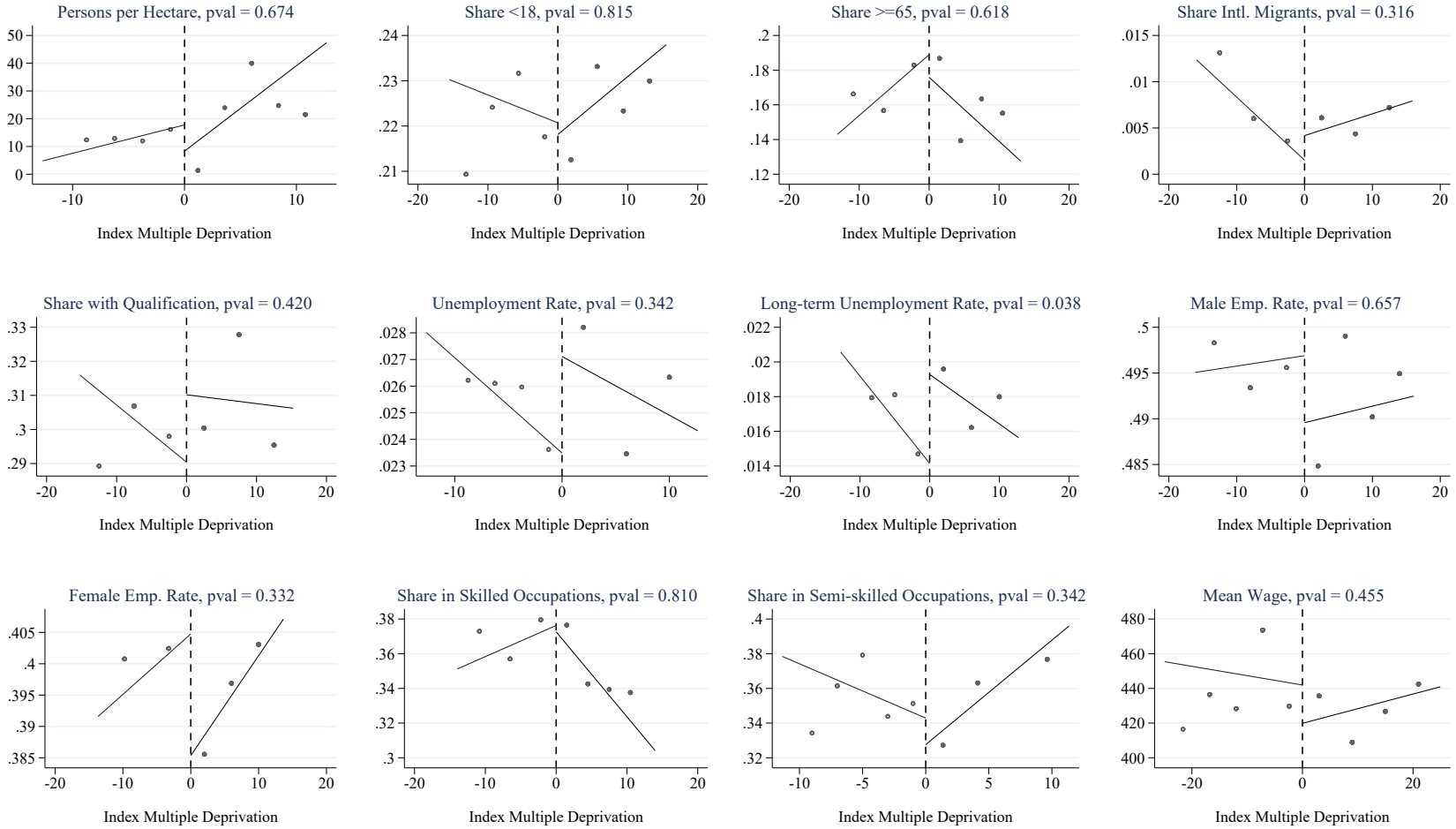
Source: ONS, 1998-2000.
Notes: This map shows the location of English Local Authorities that received NRF funding, based on based on their 1998 Index of Local Deprivation and 2000 Index of Multiple Deprivation.

Figure 2: Treatment and Control Local Authorities



Source: ONS, 1998-2000.
Notes: This map shows the location of English Local Authorities that are included in the RD analysis of the impact of the NRF on earnings at age 25 if the child's neighbourhood's IDACI rank is less thank 25, based on Calonico et al. (2014)'s MSE-optimal bandwidth approach.

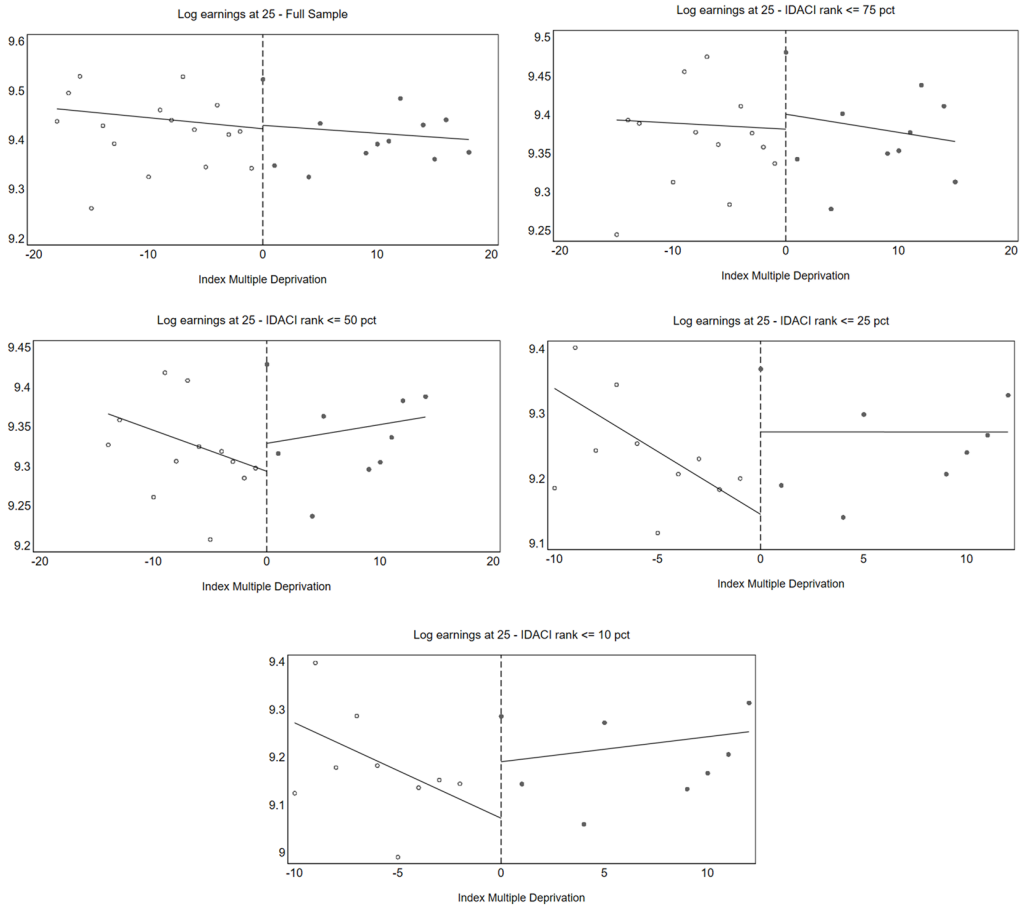
Figure 3: LA characteristics around eligibility cutoff



Source: NOMIS, 2000.

Notes: This Figure presents a series of RD balance tests for LA characteristics, as measured just before the introduction of the NRF. In each graph, the estimation sample includes the Local Authorities that fall within the MSE-optimal bandwidth for the RD analysis of the impact of the NRF on the variable considered.

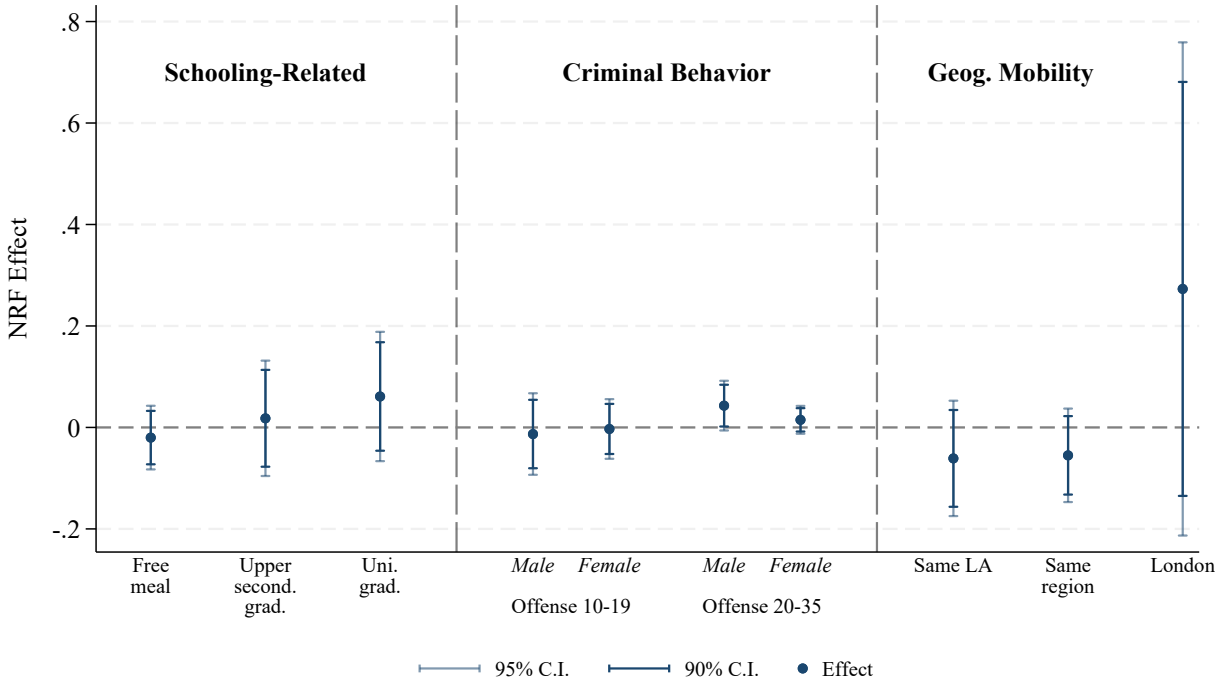
Figure 4: Log earnings at age 25



Source: LEO, 2001/02-2020/21.

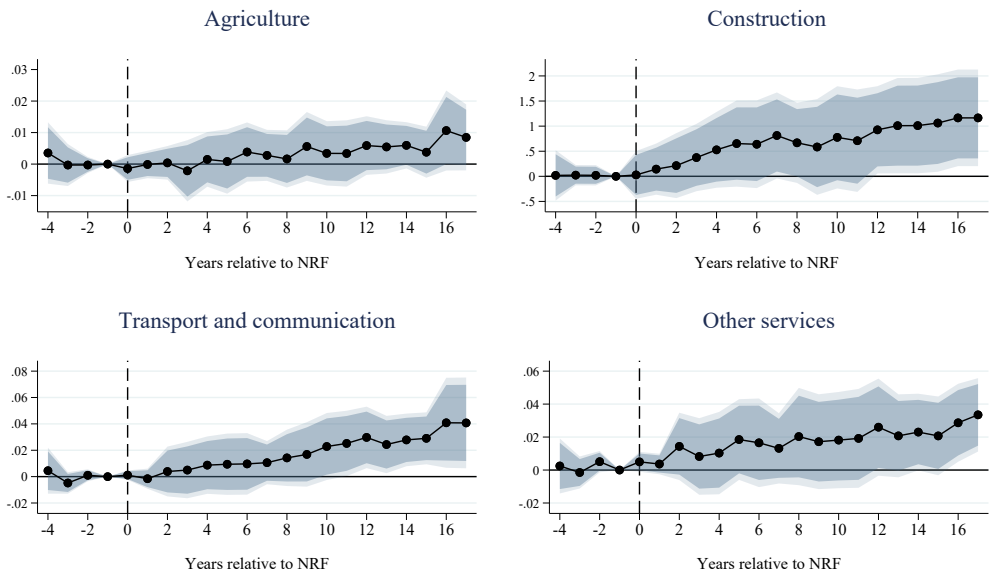
Notes: These graphs present the RD impact of the NRF on children's earnings at age 25. The top-left graph refers to the entire sample, while in the following graphs, the sample is progressively restricted to children growing up in more deprived areas of their LA, based on their neighborhood IDACI rank, which classifies local areas based on the proportion of children under the age of 16 that live in low income households.

Figure 5: Effects on Non-Labor-Market Outcomes

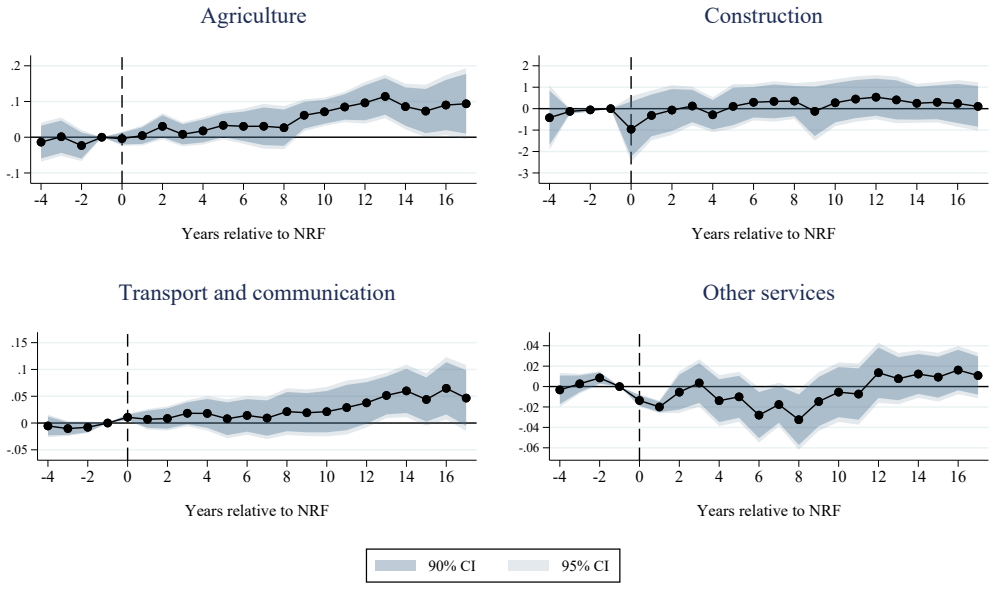


Source: LEO and DfE-MoJ Share, 2001/02–2020/21.
Notes: This figure presents the RD impact of the NRF on child poverty, educational attainment, geographic mobility, and criminal offenses. Results for criminal offense outcomes are separated by gender since boys are much more likely to be involved in criminal activity at all ages (see the control means at the bottom of Appendix Table B.1).The sample includes children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a deprived neighborhood of either treated or control LAs included the RD optimal bandwidth for the outcome considered. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Robust standard errors clustered at the LA level are displayed in parentheses.

Figure 6: Event Studies: Firm Entry and Employment



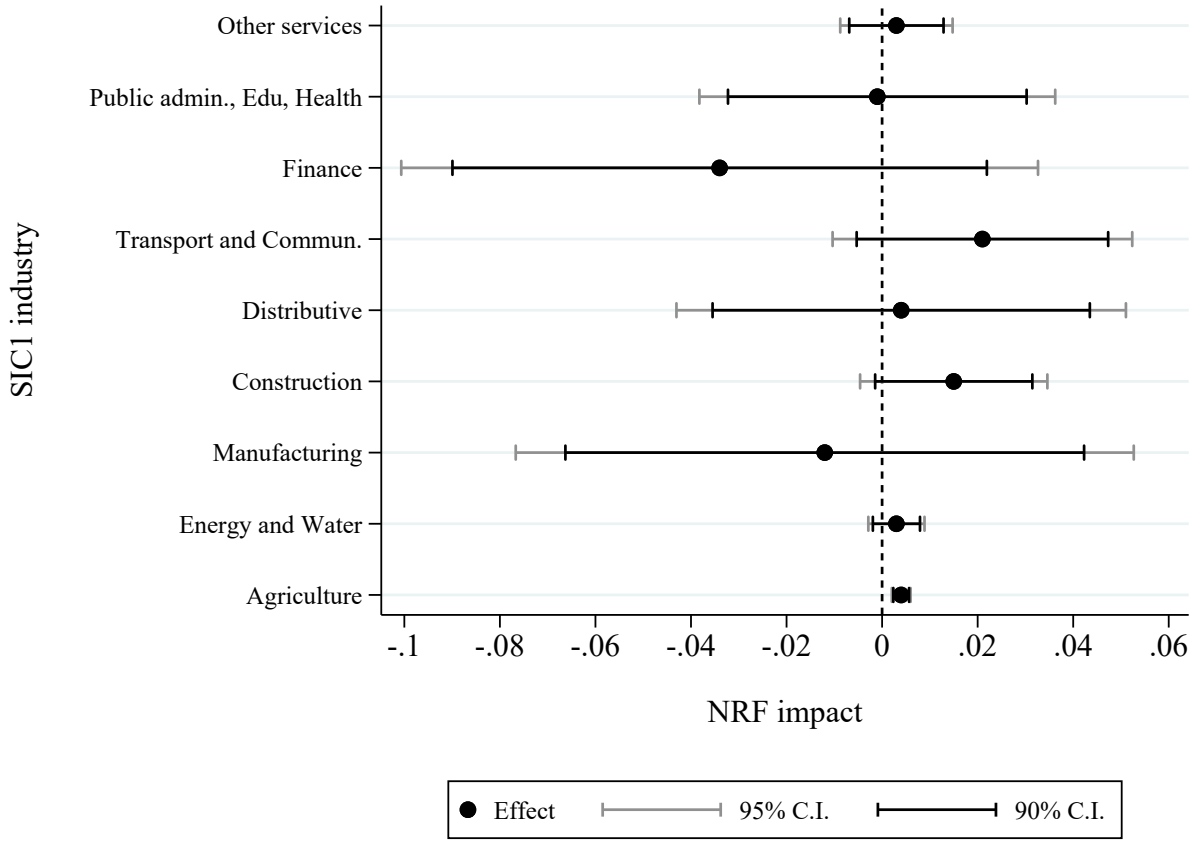
(A) Number of Establishments



(B) log Employment

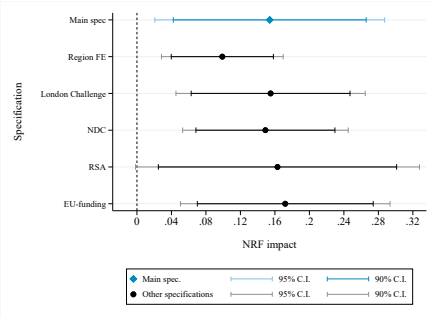
Source: BSD, 1997-2019.
Notes: Panels A and B report the regression coefficients and the associated percent confidence intervals for the difference between treated and control local authorities relative to the year before the NRF, the γ_k from equation (3). The coefficients at $k = -1$ are normalized to zero. The x-axis indexes event time. The outcome variables are the number of establishments (Panel A) and log employment (Panel B). The unit of observation is at the firm-by-local-authority level, not including pairs with zero values.

Figure 7: Industrial Sector of Employer

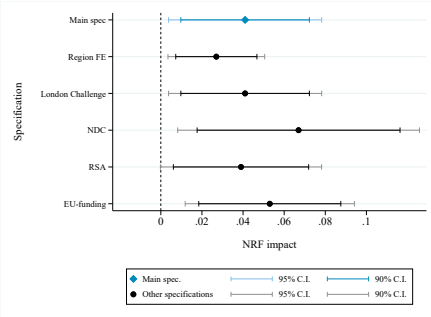


Source: LEO, 2001/02-2020/21.
Notes: This figure presents the RD impact of the NRF on the industrial sector of employment. The sample includes children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a deprived neighborhood of either treated or control LAs included the RD optimal bandwidth for the outcome considered. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Robust standard errors clustered at the LA level are displayed in parentheses.

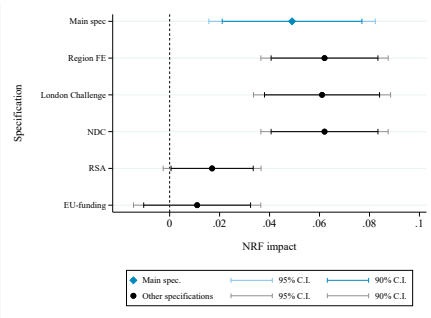
Figure 8: Robustness checks - age 25



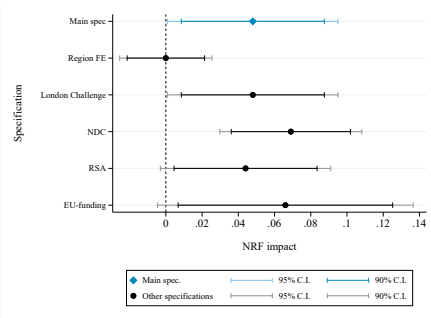
(A) Log Earnings



(B) Earning rank



(C) Log Days per year



(D) Large firm

Source: LEO, 2001/02-2020/21.
Notes: These graphs present a series of robustness checks on the RD impact of the NRF on children's outcomes at age 25. See Section 5.3 for more details on each specification.

Tables

Table 1: Probability of Family Move During School Years

	Entire sample	IDACI rank	
		≤ 25	≤ 10
	(1)	(2)	(3)
NRF impact	-0.013	-0.031	-0.019
	(0.031)	(0.027)	(0.035)
Observations	6,210,855	1,552,800	621,290
Control mean	0.265	0.348	0.372

Source: LEO, 2001/02-2020/21.

Notes: This table reports the RD impact of the NRF on the probability of moving out of the LSOA where the child lived in the 2001/02 school year, for different samples of individuals. The first column includes all children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a treated or control LA that falls within the RD optimal bandwidth for the outcome considered. In the subsequent columns, the sample is progressively restricted to children growing up in more deprived areas of their LA, based on their neighborhood IDACI rank, which classifies local areas based on the proportion of children under the age of 16 that live in low income households. Robust standard errors clustered at the LA level are displayed in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table 2: Baseline Pupil Characteristics Around Eligibility Cutoff

	Male	White British	English native	Age at census
	(1)	(2)	(3)	(4)
NRF impact	0.002	-0.144	-0.124	0.002
	(0.002)	(0.107)	(0.078)	(0.003)
Observations	6,210,685	6,184,185	6,167,090	6,211,995
Cohort FE	✓	✓	✓	✓
Control Mean	0.516	0.919	0.953	10.36

Source: LEO, 2001/02-2020/21

Notes: This table reports the RD impact of the NRF on outcome indicated in the column header. Robust standard errors clustered at the LA level are displayed in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table 3: Log Earnings at age 25

	Entire sample	IDACI rank			
		≤ 75	≤ 50	≤ 25	≤ 10
	(1)	(2)	(3)	(4)	(5)
NRF impact	0.015	0.058	0.093**	0.154***	0.154**
	(0.043)	(0.043)	(0.042)	(0.056)	(0.064)
Observations	4,705,985	3,448,460	2,227,870	1,066,365	412,370
Cohort FE	✓	✓	✓	✓	✓
Control mean	9.498	9.441	9.367	9.276	9.202

Source: LEO, 2001/02-2020/21.

Notes: This table reports the RD impact of the NRF on log earnings at age 25, for different samples of individuals. The first column includes all children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a treated or control LA that falls within the RD optimal bandwidth for the outcome considered. In the subsequent columns, the sample is progressively restricted to children growing up in more deprived areas of their LA, based on their neighborhood IDACI rank, which classifies local areas based on the proportion of children under the age of 16 that live in low income households. Robust standard errors clustered at the LA level are displayed in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 4: Income sources at age 25 - IDACI rank $\leq$ 25pct

	Log earnings	Income rank		Benefit recipient
		All	Conditional on employment	
	(1)	(2)	(3)	(4)
NRF impact	0.154***	0.041**	0.058***	-0.046
	(0.056)	(0.019)	(0.021)	(0.029)
Observations	1,066,365	1,552,730	1,066,365	1,552,800
Cohort FE	✓	✓	✓	✓
Control mean	9.276	0.396	0.440	0.305

Source: LEO, 2001/02-2020/21.

Notes: This table reports the RD impact of the NRF on income sources at age 25 for children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a deprived neighborhood of either treated or control LAs included the RD optimal bandwidth for the outcome considered. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Robust standard errors clustered at the LA level are displayed in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effect of the NRF on Socio-Economic Composition - IDACI ≤ 25 (APS)

	Demographic composition					Labor Market			
	White British (1)	Under 35 (2)	HH with children (3)	Single Parent HH (4)	University graduate (5)	On benefits (6)	Employed (7)	High skill job (8)	Log real hourly pay (9)
A. 2005–2007									
RD Estimate	-0.149	0.058	0.072*	0.009	0.001	-0.046***	-0.031	-0.040*	-0.078
	(0.124)	(0.039)	(0.040)	(0.024)	(0.018)	(0.014)	(0.026)	(0.021)	(0.051)
Observations	32206	33898	33898	12087	28830	18824	25312	14174	4703
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.951	0.226	0.328	0.130	0.136	0.0565	0.733	0.306	2.538
B. 2008–2010									
RD Estimate	-0.242	0.064	0.073	-0.043	-0.075**	0.008	-0.039	-0.073**	-0.097***
	(0.170)	(0.050)	(0.045)	(0.045)	(0.034)	(0.026)	(0.040)	(0.031)	(0.030)
Observations	22955	28514	26142	8884	14963	21436	18351	11885	5574
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.936	0.218	0.326	0.143	0.256	0.0776	0.799	0.318	2.540
C. 2011–2013									
RD Estimate	-0.200	0.070	0.087	0.044	-0.024	-0.007	-0.020	-0.027	-0.039
	(0.153)	(0.045)	(0.054)	(0.068)	(0.037)	(0.025)	(0.034)	(0.041)	(0.047)
Observations	22644	20873	21782	4979	14600	14728	18825	11599	5235
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.850	0.218	0.325	0.145	0.292	0.0783	0.796	0.316	2.497
D. 2014–2017									
RD Estimate	-0.268	0.037	0.086	-0.035	-0.011	-0.001	-0.065	0.022	0.048**
	(0.177)	(0.040)	(0.084)	(0.045)	(0.038)	(0.013)	(0.049)	(0.030)	(0.022)
Observations	30205	37609	29122	12645	25283	37285	20014	22672	7064
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.921	0.216	0.322	0.136	0.320	0.133	0.818	0.323	2.512
E. 2018–2019									
RD Estimate	-0.252	0.068*	0.120	-0.073*	-0.029	0.026	-0.035	-0.075***	-0.003
	(0.191)	(0.035)	(0.076)	(0.040)	(0.035)	(0.022)	(0.056)	(0.015)	(0.025)
Observations	13607	16159	16159	3734	11451	13607	10316	7078	3031
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.917	0.208	0.313	0.131	0.331	0.240	0.830	0.332	2.542
F. 2020–2021									
RD Estimate	-0.186	0.102***	0.041	-0.085	0.054	-0.055*	-0.053	0.020	-0.017
	(0.136)	(0.039)	(0.069)	(0.054)	(0.042)	(0.031)	(0.044)	(0.041)	(0.058)
Observations	12634	10982	12329	2334	12329	8779	7312	11011	2954
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.929	0.168	0.265	0.117	0.344	0.211	0.835	0.330	2.620

Notes: This table presents the RD impact of the NRF on households in deprived neighbourhoods during the years the program was in place. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Robust standard errors clustered at the LA level are displayed in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Effect of the NRF on LA Socio-Economic Composition (2021 Census)

	Demographic composition				Labor Market			
	Share White (1)	Share under 18 (2)	Share 65 (3)	Share with Uni. Degree (4)	Share in Skilled Occs. (5)	Share in Semi-skilled Occs. (6)	LFPR (7)	Unemp. Rate (8)
A. 2021 Census								
RD Estimate	-0.205	-0.023	-0.059	-0.005	-0.018	-0.018	-0.071	0.050
	(0.249)	(0.047)	(0.067)	(0.025)	(0.025)	(0.014)	(0.120)	(0.052)
Observations	354	354	354	354	354	354	354	354
Control mean	0.760	0.172	0.181	0.334	0.447	0.292	0.520	0.0815

Notes: This table presents the RD impact of the NRF on local authority socio-economic composition using the 2021 Census. Robust standard errors are displayed in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Labor market outcomes at age 25 - IDACI rank $\leq$ 25 pct

	Employed	Log days per year	1 employer only	Large firm
	(1)	(2)	(3)	(4)
NRF impact	0.014	0.049***	-0.01	0.048**
	(0.016)	(0.017)	(0.019)	(0.024)
Observations	1,552,800	1,090,150	1,066,435	1,056,760
Cohort FE	✓	✓	✓	✓
Control Mean	0.715	5.669	0.699	0.599

Source: LEO, 2001/02-2020/21.

Notes: This table presents the RD impact of the NRF on labor market outcomes at age 25. The sample includes children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a deprived neighborhood of either treated or control LAs included in the RD optimal bandwidth for the outcome considered. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Robust standard errors clustered at the LA level are displayed in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table 8: Impact at older ages - IDACI rank $\leq$ 25 pct

	Employed (1)	Log days per year (2)	1 employer only (3)	Large firm (4)	Log earnings (5)	Self-employment (6)	Log self-employment earnings (7)	On benefits (8)
Age 28								
NRF Impact	0.016	0.004	-0.003	0.037*	0.154**	0.007	0.067	-0.044
	(0.014)	(0.016)	(0.016)	(0.022)	(0.068)	(0.011)	(0.116)	(0.028)
Observations	1,167,130	762,925	744,670	734,670	744,620	1,012,315	56,990	1,167,130
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.664	5.708	0.728	0.594	9.423	0.0636	9.047	0.272
Age 30–35								
NRF Impact	0.016	0.019	0.026***	0.049***	0.149*	0.012	-0.167	-0.028
	(0.011)	(0.034)	(0.006)	(0.019)	(0.082)	(0.013)	(0.117)	(0.03)
Observations	868,855	576,640	566,585	485,535	566,575	868,855	70,535	868,855
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.672	5.603	0.9	0.59	9.483	0.089	8.995	0.395

Source: LEO, 2001/02–2020/21.

Notes: This table presents the RD impact of the NRF on economic outcomes at age 28 and between ages 30 and 35 for children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a deprived neighborhood of either treated or control LAs included the RD optimal bandwidth for the outcome considered. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Robust standard errors clustered at the LA level are displayed in parentheses.

Table 9: Impact by gender - IDACI rank $\leq$ 25 pct

	Age 25			Age 28			Age 30-35		
	Log days per year (1)	Large firm (2)	Log earnings (3)	Log days per year (4)	Large firm (5)	Log earnings (6)	Log avg days per year (7)	Ever large firm (8)	Log avg earnings (9)
A. Entire sample									
NRF impact	0.049***	0.048**	0.154**	0.004	0.037*	0.154**	0.019	0.049***	0.149*
	(0.017)	(0.024)	(0.085)	(0.016)	(0.022)	(0.068)	(0.034)	(0.019)	(0.082)
Observations	1,090,150	1,056,760	1,066,365	762,925	734,670	744,620	576,640	485,535	566,575
Control Mean	5.669	0.599	9.276	5.708	0.594	9.423	5.603	0.618	9.483
B. Girls									
NRF impact	0.065***	0.033*	0.115	0.003	0.028	0.099	0.02	0.032*	0.097
	(0.011)	(0.019)	(0.085)	(0.014)	(0.02)	(0.113)	(0.043)	(0.017)	(0.117)
Observations	527,315	510,910	515,220	367,595	353,510	357,975	278,875	238,560	273,635
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	5.695	0.614	9.179	5.723	0.617	9.248	5.611	0.618	9.265
C. Boys									
NRF impact	0.022	0.063**	0.186***	0.002	0.045	0.194***	0.007	0.069**	0.184***
	(0.019)	(0.03)	(0.068)	(0.015)	(0.03)	(0.069)	(0.021)	(0.034)	(0.061)
Observations	562,505	545,535	550,825	395,100	380,945	386,430	297,555	246,810	292,735
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	5.646	0.586	9.363	5.694	0.573	9.58	5.595	0.564	9.684

Source: LEO, 2001/02-2020/21.

Notes: This table presents the RD impact of the NRF by gender. The sample includes children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a deprived neighborhood of either treated or control LAs included the RD optimal bandwidth for the outcome considered. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Robust standard errors clustered at the LA level are displayed in parentheses.

Table 10: Impact by exposure - IDACI rank $\leq$ 25 pct

	Age 25			Age 28			Age 30-35		
	Log days per year (1)	Large firm (2)	Log earnings (3)	Log days per year (4)	Large firm (5)	Log earnings (6)	Log avg days per year (7)	Ever large firm (8)	Log avg earnings (9)
A. Entire sample									
NRF impact	0.049***	0.048**	0.154**	0.004	0.037*	0.154**	0.019	0.049***	0.149*
	(0.017)	(0.024)	(0.085)	(0.016)	(0.022)	(0.068)	(0.034)	(0.019)	(0.082)
Observations	1,090,150	1,056,760	1,066,365	762,925	734,670	744,620	576,640	485,535	566,575
Control Mean	5.669	0.599	9.276	5.708	0.594	9.423	5.603	0.618	9.483
B. Less exposed cohorts									
NRF impact	0.078***	0.059**	0.156***	0.004	0.049**	0.144**	0.019	0.049***	0.149*
	(0.013)	(0.027)	(0.060)	(0.016)	(0.023)	(0.073)	(0.034)	(0.019)	(0.082)
Observations	590,685	574,900	575,160	595,675	576,970	580,295	576,640	485,530	566,575
Control Mean	5.667	0.610	9.222	5.712	0.592	9.404	5.603	0.590	9.483
C. More exposed cohorts									
NRF impact	0.020*	0.039*	0.147***	-0.003	-0.005	0.193***			
	(0.011)	(0.022)	(0.055)	(0.019)	(0.025)	(0.063)			
Observations	499,465	481,860	491,205	167,250	157,705	164,325			
Control Mean	5.672	0.586	9.338	5.694	0.598	9.488			

Source: LEO, 2001/02-2020/21.

Notes: This table presents the RD impact of the NRF by exposure to the program during compulsory schooling. The entire sample includes children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a deprived neighborhood of either treated or control LAs included the RD optimal bandwidth for the outcome considered. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Less (more) exposed cohorts comprise the 1985/86 to 1990/91 (1991/92 to 1995/96) cohorts who were in secondary school (primary) when the NRF was introduced in 2001. All regressions include cohort fixed effects. Robust standard errors clustered at the LA level are displayed in parentheses.

Online Appendix

Children of Neighborhood Renewal: Long-Run Effects of a Revitalization Program

Emma Duchini Catherine van der List

A Alternative Binding Score Construction

As explained in Section 2, the rank indices used to construct the running variable are based on corresponding continuous indices of deprivation for the year 2000 and the year 1998. Alonso et al. (2019) build the running variable for the RD analysis of the NRF impact on crime using these continuous indices rather than the corresponding ranks. For this, the authors standardize the indices so each have a mean of zero and a standard deviation of one:

$$z_{ls} = \frac{w_{ls} - \mu_s}{\sigma_s} \tag{A.1}$$

where w_{ls} is the value for sub-index s in local authority l, μ_s is the mean of sub-index s, and σ_s is its standard deviation. Next, they calculate the maximum of the distance to the cut-off for each sub-index:[A.1]

$$x_l = \max_s \left(z_{ls} - z_{50,s}\right) \tag{A.2}$$

where z_{ls} is the standardized score of LA l for sub-index s calculated in equation (A.1), and $z_{50,s}$ is the standardized score of the LA ranked 50th (the eligibility cut-off) for sub-index s.

Although the two running variables are highly correlated (correlation coefficient = 0.79), our preferred measure is the one calculated using the rank. This is for several reasons. First, the ranks were the variable used to determine NRF eligibility, not the underlying sub-indices. Second, by construction, the rank-derived variable has a larger support than the continuous score both above and especially below the NRF cutoff (see Appendix Figure A.1). In fact, although the latter has the advantage of having a continuous distribution, this is very compact, especially for control LAs. This is because, even though the standardization procedure ensures all variables have a mean of zero and a standard deviation of one, it does not alter the underlying *shapes* of the distributions to make them more comparable to one another. This means that, when using optimal bandwidth procedures, some sub-indices are much more likely to be the "binding score" by construction, and which sub-index is chosen as the "binding score" systematically differs between the treatment and control groups.

As an illustration, Appendix Figure A.2 shows the distributions of two of the underlying

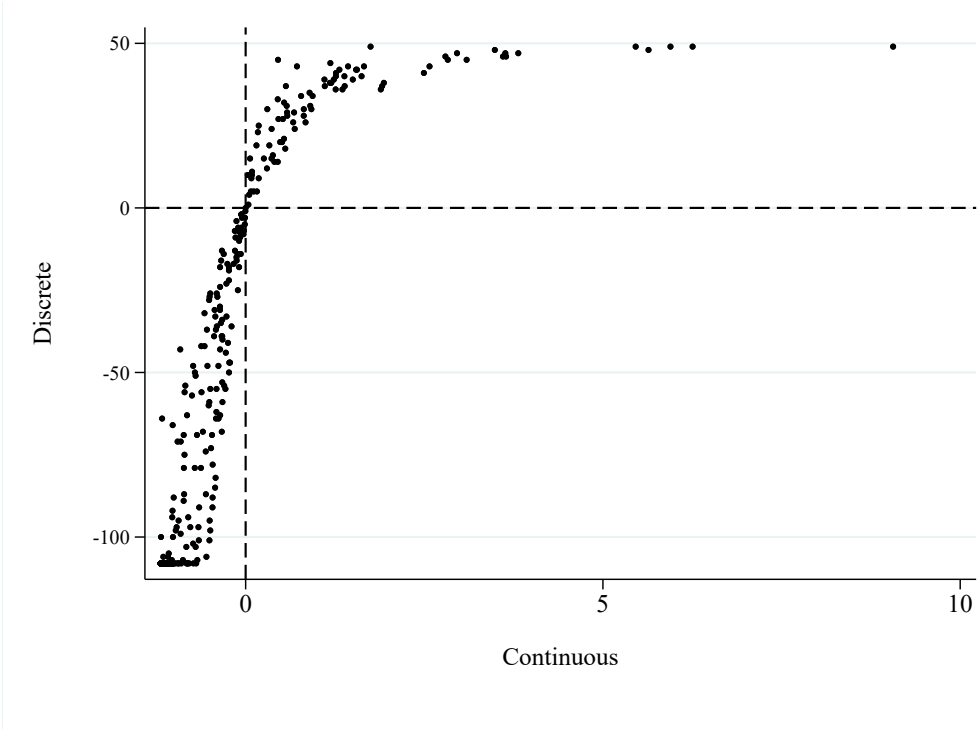
[A.1] In this case, the maximum must be used instead of the minimum (as was the case in equation (1)) since for ranks lower ranks imply higher deprivation, whereas for the underlying sub-indicators higher values imply higher deprivation.

sub-indices after the standardization procedure: the Concentration Score and the Average of Ward Ranks. As shown in the Figure, the distributions of the two sub-indices remain very different from one another despite the standardization procedure. In particular, the distribution of the Average of Ward Ranks is much more evenly distributed around zero, while the concentration score is much more right skewed. This leads to systematically different percentages of local authorities within a theoretical optimal bandwidth for each underlying sub-index. These differing distributions are also reflected in which sub-indices(s) are chosen as the "binding score" when using the procedure in Equation (A.2).

To further illustrate this point, Appendix Table A.1 shows the sub-index chosen as the binding score for both the continuous running variable (using a theoretical optimal bandwidth of .5) and the rank-based measure (using a theoretical optimal bandwidth of 38 to include approximately the same total number of LAs as the continuous measure). The Table also shows this decisive variable separately for non-NRF ("control") and NRF ("treatment") local authorities. The Table confirms that there are systematic differences in which sub-index binds between treatment and control LAs when using the continuous measure. For example, the Concentration score is the binding variable for 69.62% of the local authorities in the control group, but only for 7.14% of the LAs in the treatment group. There are not such large systematic differences when using the rank-based measure, with a more even distribution across treatment and control. Second, the table also shows that when using the continuous measure, the more compressed distribution of the running variable for the non-NRF LAs leads to a much larger pool of control (79) compared to treated (28) when using an optimal bandwidth. The rank-based measure does not have such issues, with similar numbers of treatment and control local authorities when using optimal bandwidths.

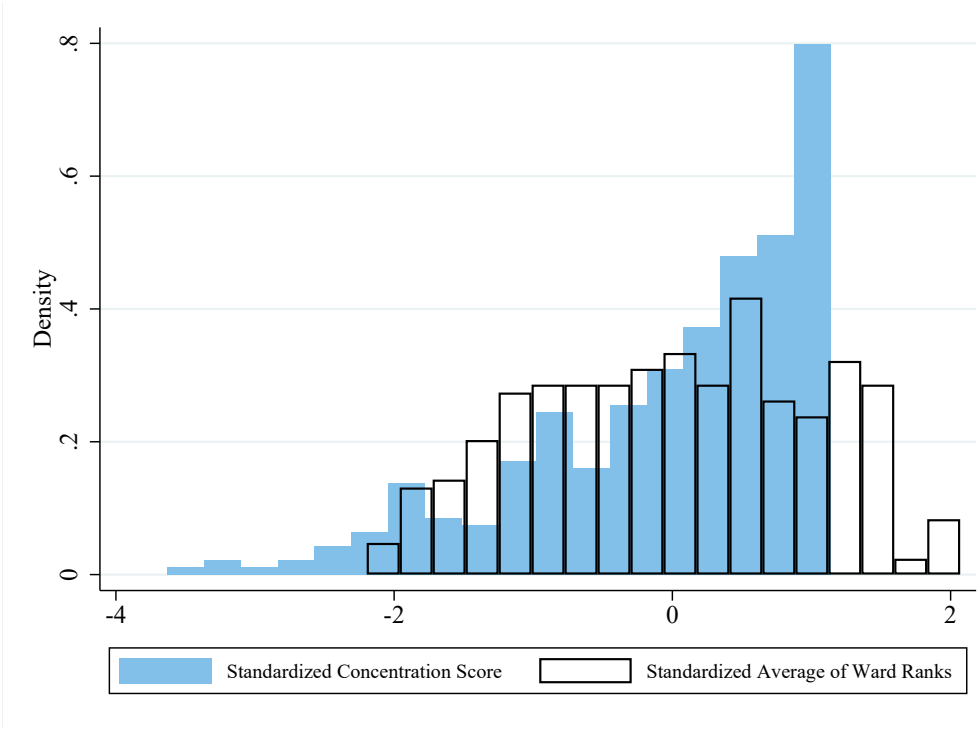
In practice, this implies that very affluent areas like Oxford may be included in the control group when using the continuous running variable because of the systematic bias in which index denotes the binding score and the uneven density of the distributions, particularly for LAs that did not receive the NRF. As a consequence, the econometrician risks underestimating the impact of the NRF when using the continuous running variable because LAs that are generally richer are more likely to be included in the control group than in our main specification.

Figure A.1: Correlation between rank and scale running variables



Source: ONS, 1998-2000.
Notes: This graph presents the correlation between the discrete running variable used in the main specification, and based on the rank binding score, and the continuous running variable used by Alonso et al. (2019) and constructed using the underlying continuous sub-indices of deprivation. Each dot refers to one of the 354 LAs present in England in 2000.

Figure A.2: Comparison of (Standardized) Index Distributions



Source: Authors' calculations using NRF eligibility data.
Notes: This Figure shows the distributions of two of the underlying sub-indices of Local Deprivation for the year 2000 after the standardization procedure: the Concentration Score and the Average of Ward Ranks.

Table A.1: Decisive Variable by Running Variable Construction and Treatment Status

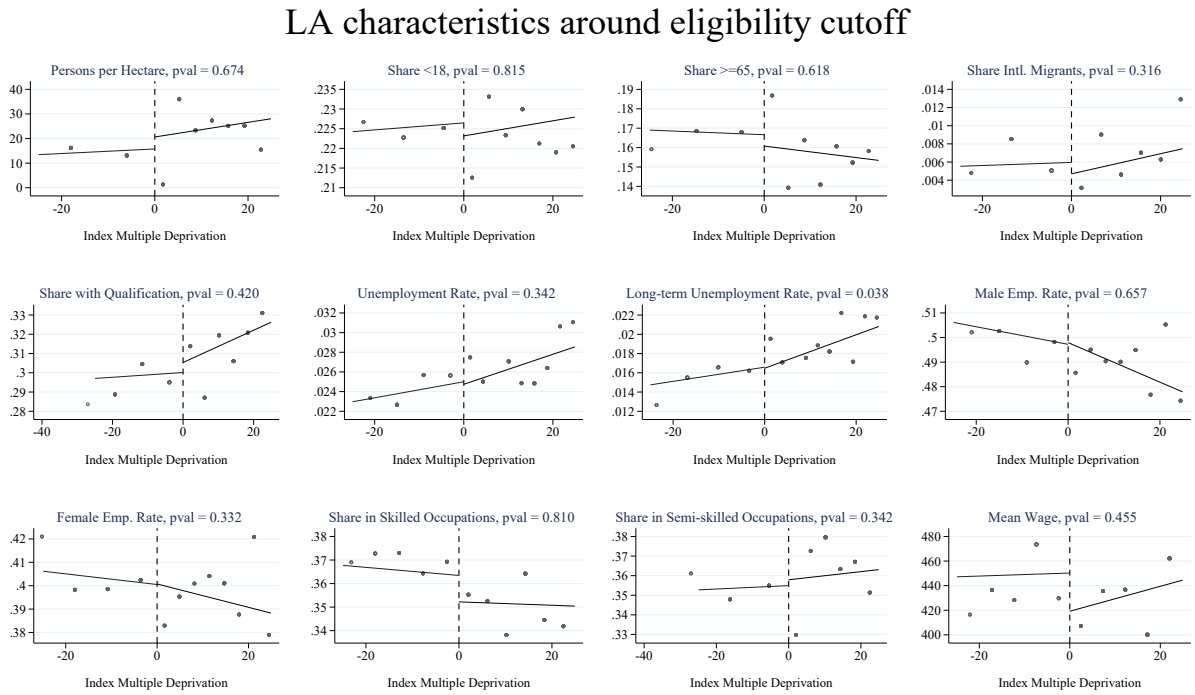
	Treatment		Control	
	N	Percent	N	Percent
Panel A: Continuous sub-index				
Average of Ward Ranks	6	21.43	10	12.66
Concentration	2	7.14	55	69.62
Employment Deprivation	2	7.14		
Income Deprivation	4	14.29	5	6.33
Pop. Sh. in 10% Most Dep. Wards	6	21.43		
Degree (1998)	2	7.14	1	1.27
Education (1998)	1	3.57	1	1.27
Extent (1998)	2	7.14		
Intensity (1998)	3	10.71	7	8.86
Total	28	100.00	79	100.00
Panel B: Rank-based				
Average of Ward Ranks	13	23.21	11	20.75
Average of Ward Scores	1	1.79		
Concentration	9	16.07	8	15.09
Employment Deprivation	4	7.14	2	3.77
Income Deprivation	7	12.50	9	16.98
Pop. Sh. in 10% Most Dep. Wards	5	8.93	4	7.55
Degree (1998)	5	8.93	1	1.89
Education (1998)	3	5.36	5	9.43
Extent (1998)	6	10.71	7	13.21
Intensity (1998)	3	5.36	6	11.32
Total	56	100.00	53	100.00

Source: Authors' calculations using NRF eligibility data.

B Additional Figures and Tables

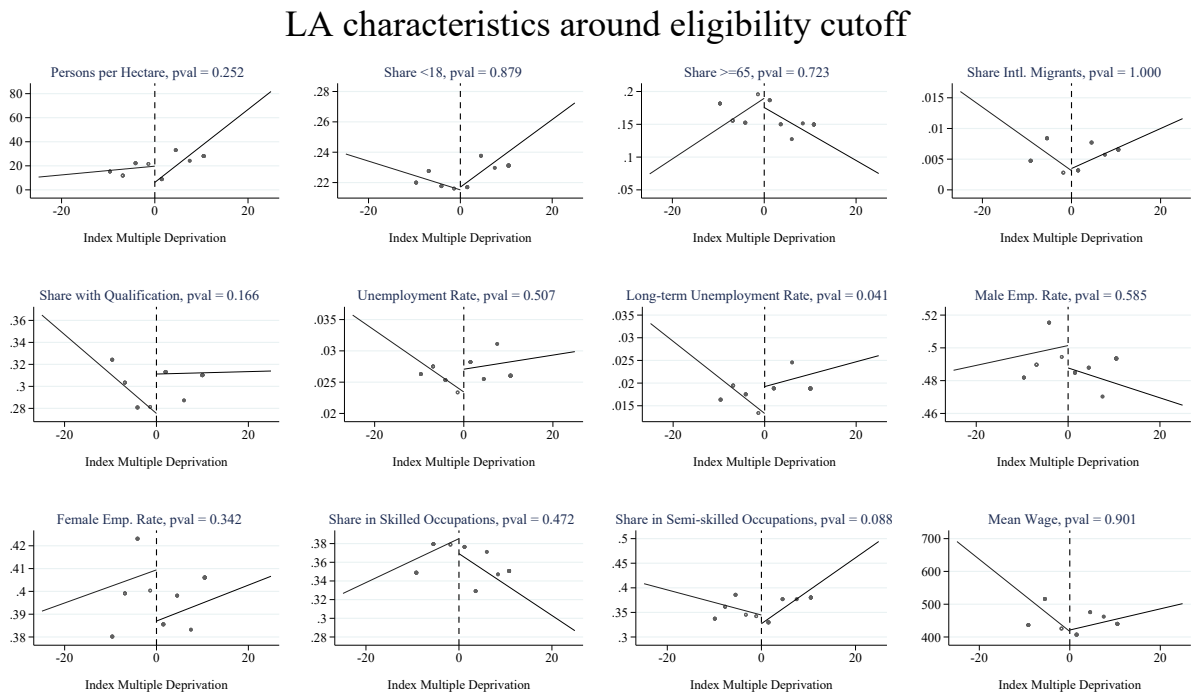
B.1 Additional Figures

Figure B.1:



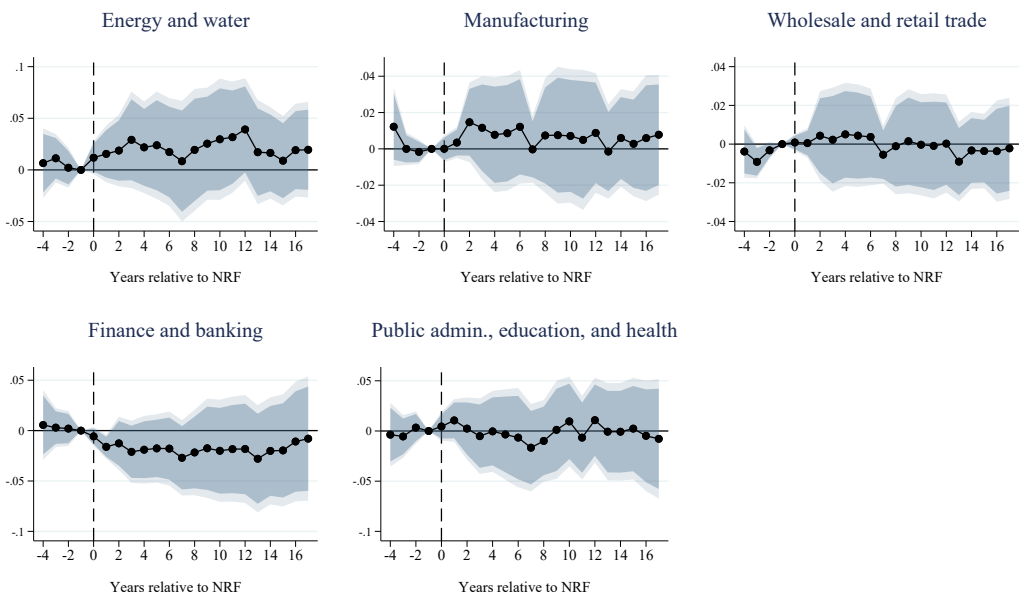
Source: NOMIS, 2000.
Notes: This Figure presents a series of RD balance tests for LA characteristics, as measured just before the introduction of the NRF. In each graph, the estimation sample includes all Local Authorities.

Figure B.2:

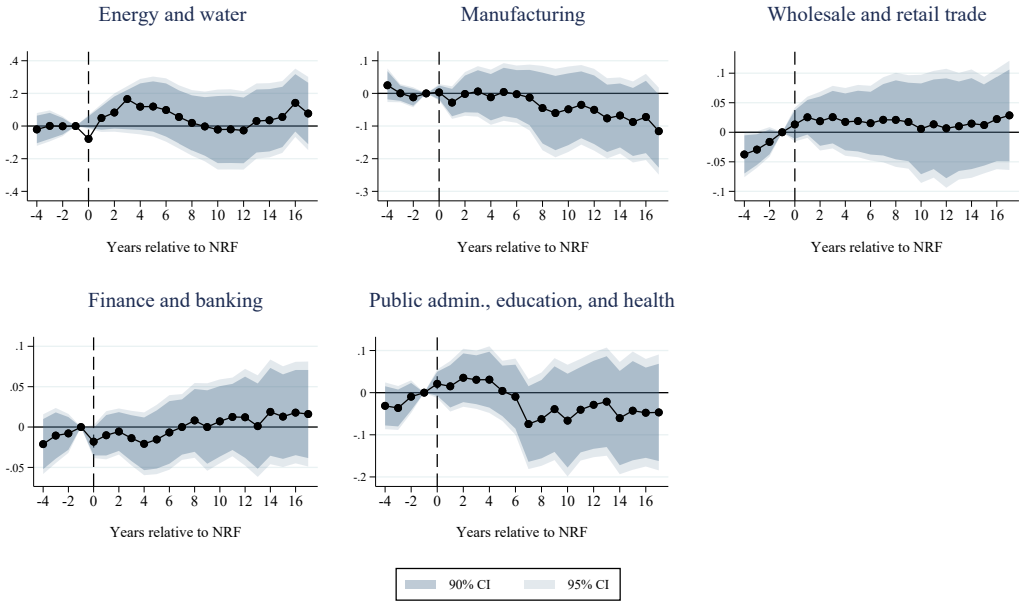


Source: NOMIS, 2000.
Notes: This Figure presents a series of RD balance tests for LA characteristics, as measured just before the introduction of the NRF. In each graph, the estimation sample includes the Local Authorities that fall within the MSE-optimal bandwidth for the RD analysis for "earnings at age 25".

Figure B.3: Event Studies: Firm Entry and Employment



(A) Number of Establishments



(B) log Employment

Source: BSD, 1997-2019.
Notes: Panels A and B report the regression coefficients and the associated percent confidence intervals for the difference between treated and control local authorities relative to the year before the NRF, the γ_k from equation (3) The coefficients at $k = -1$ are normalized to zero. The x-axis indexes event time. The outcome variables are the number of establishments (Panel A) and log employment (Panel B). The unit of observation is at the firm-by-local-authority level, not including pairs with zero values.

B.2 Additional Tables

Table B.1: Mechansims - age 25 - IDACI rank $\leq$ 25 pct

	Child poverty	Educational attainment		Geographic mobility			Criminal offenses			
	FSME in school	Upper Secondary schooling	University graduate	Same LA	Same region	London	Boys, 10–19	Girls, 10–19	Boys, 20–35	Girls, 20–35
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
NRF impact	-0.02	0.018	0.061	-0.061	-0.055	0.273	-0.013	0.043*	-0.003	0.015
	(0.032)	(0.058)	(0.065)	(0.058)	(0.047)	(0.248)	(0.041)	(0.025)	(0.030)	(0.014)
Observations	1,552,800	1,552,800	1,552,800	1,429,535	1,448,700	1,448,700	818,897	818,897	788,575	788,575
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.242	0.439	0.154	0.745	0.873	0.132	0.303	0.246	0.146	0.0759

Source: LEO, 2001/02-2020/21.

Notes: This table presents the RD impact of the NRF on child poverty, educational attainment, geographic mobility, and criminal offenses. The sample includes children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a deprived neighborhood of either treated or control LAs included in the RD optimal bandwidth for the outcome considered. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Robust standard errors clustered at the LA level are displayed in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table B.2: Effect of the NRF on LA Socio-Economic Composition (APS)

	Demographic composition					Labor Market			
	White British (1)	Under 35 (2)	HH with children (3)	Single Parent HH (4)	University graduate (5)	On benefits (6)	Employed (7)	High skill job (8)	Log real hourly pay (9)
A. 2005–2007									
RD Estimate	-0.094	0.083**	0.011	-0.005	0.000	-0.032*	-0.049***	-0.035	0.016
	(0.072)	(0.038)	(0.027)	(0.023)	(0.014)	(0.017)	(0.019)	(0.030)	(0.080)
Observations	111232	99871	134164	23113	135447	72132	93245	85450	30172
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.951	0.226	0.328	0.130	0.136	0.0565	0.733	0.306	2.538
B. 2008–2010									
RD Estimate	-0.157	0.076	0.020	0.002	-0.005	0.005	-0.051*	-0.033	0.019
	(0.105)	(0.048)	(0.043)	(0.027)	(0.031)	(0.013)	(0.029)	(0.042)	(0.085)
Observations	97874	82052	101290	33277	123440	123440	68254	56920	26377
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.936	0.218	0.326	0.143	0.256	0.0776	0.799	0.318	2.540
C. 2011–2013									
RD Estimate	-0.124	0.069*	0.046	0.020	-0.004	0.003	-0.043**	-0.027	-0.008
	(0.090)	(0.040)	(0.036)	(0.022)	(0.029)	(0.011)	(0.022)	(0.038)	(0.057)
Observations	89468	96044	89468	40171	115096	115096	59709	80376	41442
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.850	0.218	0.325	0.145	0.292	0.0783	0.796	0.316	2.497
D. 2014–2017									
RD Estimate	-0.177*	0.084**	0.055	0.002	0.024	0.004	-0.036	0.005	-0.012
	(0.107)	(0.041)	(0.042)	(0.022)	(0.035)	(0.009)	(0.031)	(0.033)	(0.058)
Observations	124671	105488	124671	40658	150131	158555	56183	128839	57812
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.921	0.216	0.322	0.136	0.320	0.133	0.818	0.323	2.512
E. 2018–2019									
RD Estimate	-0.190	0.102***	0.076*	-0.023	0.028	0.016	-0.025	0.001	-0.005
	(0.116)	(0.030)	(0.042)	(0.025)	(0.032)	(0.016)	(0.029)	(0.035)	(0.051)
Observations	56092	56092	52324	16061	67156	70248	35340	61411	26103
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.917	0.208	0.313	0.131	0.331	0.240	0.830	0.332	2.542
F. 2020–2021									
RD Estimate	-0.124	0.066**	0.014	0.022	0.013	-0.014	-0.008	0.011	-0.028
	(0.087)	(0.030)	(0.034)	(0.023)	(0.039)	(0.015)	(0.027)	(0.040)	(0.074)
Observations	45141	43628	31520	13330	52008	49585	17329	48867	15733
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control mean	0.929	0.168	0.265	0.117	0.344	0.211	0.835	0.330	2.620

Notes: This table presents the RD impact of the NRF on households during the years the program was in place. Robust standard errors clustered at the LA level are displayed in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.3: Households with children - IDACI rank $\leq$ 25 pct - APS

	Married/ cohabitating (1)	University graduate (2)	Benefit recipient (3)	Employed (4)	High-skilled job (5)	Full time (6)	Log hourly pay (7)
A. 2005-2007							
NRF impact	-0.062*	-0.006	-0.027	-0.06	0.045	0.019	0.187
	(0.033)	(0.016)	(0.023)	(0.043)	(0.034)	(0.029)	(0.12)
Observations	10153	13446	9065	12010	6410	5919	2613
Year FE	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.631	0.096	0.108	0.619	0.306	0.639	2.082
B. 2008-2010							
NRF Impact	-0.004	-0.119***	0.066	-0.147***	-0.130***	0.053*	-0.046
	(0.024)	(0.042)	(0.05)	(0.055)	(0.031)	(0.03)	(0.05)
Observations	8816	5662	5851	5625	3578	5315	2467
Year FE	✓	✓	✓	✓	✓	✓	✓
Control Mean	0.63	0.171	0.153	0.651	0.308	0.64	2.163

Source: APS, 2005-2010.

Notes: This table presents the RD impact of the NRF on households with children during the years the program was in place. The sample is restricted to households residing in a deprived neighborhood of either treated or control LAs included the RD optimal bandwidth for the outcome considered. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Robust standard errors clustered at the LA level are displayed in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table B.4: Workers - IDACI rank $\leq$ 25 pct - ASHE

	Geographic mobility			Job characteristics					Log hourly pay
	Same LA (1)	Same LSOA (2)	Deprived LSOA (3)	N employers (4)	Private sector (5)	Large firm (6)	High-skilled job (7)	Full time (8)	Log hourly pay (9)
A. 2005									
NRF Impact	-0.095	-0.021	-0.164	-0.005	-0.080*	0.090**	-0.013	-0.028	0.035
	0.071	0.015	0.194	0.008	0.045	0.043	0.031	0.024	0.078
Observations	3516	4731	3075	3401	3516	4737	3401	4737	3216
Control Mean	0.583	0.05	0.278	1.017	0.708	0.683	0.29	0.726	2.43
B. 2008									
NRF Impact	-0.115*	-0.040***	-0.087	0.007	-0.047	0.076**	-0.041	-0.08	0.032
	0.065	0.015	0.161	0.015	0.036	0.038	0.034	0.067	0.065
Observations	2821	4877	2369	2821	5614	2936	2821	2648	2927
Control Mean	0.561	0.0512	0.259	1.019	0.729	0.687	0.312	0.715	2.441
C. 2011									
NRF Impact	-0.03	-0.015	-0.151	0.067*	0.001	-0.02	0.023	-0.040*	0.092*
	0.064	0.018	0.19	0.041	0.008	0.04	0.034	0.024	0.049
Observations	3832	4681	3215	3972	5730	5960	6707	3623	3932
Control Mean	0.546	0.0451	0.259	0.687	1.03	0.701	0.316	0.698	2.4
D. 2014									
NRF Impact	0.192	-0.019*	-0.162	0.004	0.045	0.051	-0.057	-0.014	0.053
	0.128	0.011	0.178	0.012	0.036	0.043	0.044	0.029	0.047
Observations	3123	6799	3545	4780	3123	3946	3731	6042	3906
Control Mean	0.561	0.0434	0.277	1.027	0.809	0.667	0.29	0.69	2.351
E. 2017									
NRF Impact	0.118	-0.021*	-0.207	0.004	0.044	0.019	-0.035	-0.029	0.037
	0.147	0.013	0.172	0.009	0.039	0.03	0.047	0.027	0.05
Observations	2962	3743	3377	7467	3540	3901	5275	5574	3702
Control Mean	0.553	0.0457	0.276	1.023	0.815	0.65	0.291	0.69	2.397
F. 2019									
NRF Impact	0.278**	0.02	-0.059	0.02	-0.041	0.008	-0.029	0.043	0.031
	0.121	0.02	0.208	0.018	0.028	0.043	0.05	0.033	0.053
Observations	4433	3836	4986	5665	10372	5316	5318	6887	5618
Control Mean	0.556	0.0505	0.271	1.044	0.834	0.655	0.295	0.67	2.423

Source: ASHE, 2005-2019.

Notes: This table presents the RD impact of the NRF on employed individuals. The sample is restricted to individuals residing in a deprived neighborhood of either treated or control LAs included the RD optimal bandwidth for the outcome considered. We consider as deprived neighborhoods those output areas whose IDACI rank falls in the bottom quartile of the national distribution. Robust standard errors clustered at the LA level are displayed in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table B.5: Robustness checks

			Excluding areas targeted by			
	Main specification	Region fixed effects	London Challenge	NDC	RSA	EU funding
	(1)	(2)	(3)	(4)	(5)	
A. Log earnings						
NRF impact	0.154***	0.099***	0.155***	0.149*	0.163*	0.172***
	(0.068)	(0.036)	(0.056)	(0.059)	(0.084)	(0.062)
Observations	1,066,365	1,066,365	998,050	910335	505,140	939,565
Control mean	9.276	9.276	9.276	9.276	9.294	9.278
B. Earnings rank						
NRF impact	0.041**	0.027**	0.041**	0.039*	0.067**	0.053**
	(0.019)	(0.012)	(0.019)	(0.20)	(0.03)	(0.021)
Observations	1,552,730	1,552,730	1,452,445	1,321,430	731,355	1,366,700
Control mean	0.396	0.396	0.396	0.397	0.401	0.397
C. Log days per year						
NRF impact	0.049***	0.062***	0.061***	0.062***	0.017*	0.011
	(0.017)	(0.013)	(0.014)	(0.013)	(0.010)	(0.013)
Observations	1,090,150	1,090,150	1,019,870	930,500	517,205	960,585
Control mean	5.699	5.669	5.669	5.669	5.670	5.669
D. Large firm						
NRF impact	0.048**	-0.000	0.048**	0.044*	0.069***	0.066***
	(0.024)	(0.013)	(0.024)	(0.024)	(0.02)	(0.020)
Observations	1,452,445	1,056,760	989,140	902,225	500,525	939,565
Control mean	0.599	0.599	0.599	0.599	0.599	0.600

Source: LEO, 2001/02-2020/21.

Notes: This table presents a series of robustness checks on the impact of the NRF on different outcomes. The first column reports the main specification, run on the sample of children who were between 6 and 16 when the NRF was introduced in 2001 and resided in a treated or control LA that falls within the RD optimal bandwidth for the outcome considered. The second column adds region fixed effects, while the following ones exclude, in turn, areas that were targeted by other programs running at the same time as the NRF. All regressions include cohort fixed effects. Robust standard errors clustered at the LA level are displayed in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.