JUE *Insight*: The labor market effects of place-based policies: Evidence from England's Neighbourhood Renewal Fund

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Abstract

Neighborhood renewal programs are a type of place-based policy that aim to revive underperforming localities. The literature on place-based policies has found mixed results regarding their effects on local labor market outcomes, but there are relatively few studies of policies that aim to improve local labor supply. In this paper we examine the labor market effects of the Neighbourhood Renewal Fund, which targeted 88 of the most deprived areas in England during the early 2000s as part of the Labour government's National Strategy for Neighbourhood Renewal. The fund disbursed almost £3 billion for spending on community safety, education, healthcare and worklessness, with supply-side interventions making up the bulk of the program's spending on worklessness. Using a difference-in-differences approach, we find statistically significant impacts on local employment. Our results suggest that policy interventions to improve local labor supply can be a successful strategy for neighborhood renewal.

Keywords: Place-Based Policies, Urban Economics, Labor Supply, Employment.

JEL Codes: J21, J22, J48, R10, R58.

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1 Introduction

Place-based policies aim to revive underperforming localities through tax incentives or subsidies that encourage development goals such as poverty reduction, local business formation, human capital investment, or employment growth. Neighborhood renewal programs (along with enterprise zones, empowerment zones, and industrial cluster policies) have been implemented in various forms across Europe and the United States since the end of the Second World War. A notable example is the National Strategy for Neighbourhood Renewal, which was introduced in the United Kingdom by the Labour Government in 2001. The goal of this program was to target the most deprived areas of England using a variety of policy interventions, and to improve relative outcomes in these areas with respect to health, education, crime, and employment (Weaver, 2001).

The main source of funding for the National Strategy for Neighbourhood Renewal was the Neighbourhood Renewal Fund, which distributed money to 88 of the most deprived local areas in England. Between 2001 and 2008, nearly £3 billion was disbursed across these areas, with a number of agencies (both government and non-profit) in charge of coordinating the program's implementation. While there was flexibility in the exact allocation of funds per locality, estimates from 2006 suggest that roughly 19 percent of the money was spent on community safety, 19 percent on education, 16 percent on health, and 12 percent on worklessness, with the remainder being spent on the environment, cost-cutting activity, and administration (Dept. for Com. and Local Gov't, 2010).

The literature on place-based policies has found mixed results regarding their effects on local labor market outcomes (Neumark & Simpson, 2015). However, many of these policies are primarily focused on stimulating labor demand via tax cuts and credits to hire workers within specific localities (e.g., urban enterprise zones), which may attract in-migrants from outside of those local areas rather than its existing residents. Bartik (2012) argues that there has been much less work on regional policies that aim to improve the quality of local labor supply and their effect on the quantity and quality of local employment. These are policies that fall under Helen Ladd's definition of 'place-based people strategies', which seek to assist disadvantaged residents within targeted geographies through efforts such as job search and workforce development training (Ladd, 1994).

In this paper, we examine the labor market effects of the Neighbourhood Renewal Fund (henceforth, NRF). As discussed in an independent evaluation, most of the labor market interventions funded by the NRF focused on advice, guidance, and training for unemployed or marginalized workers, as well as transitional employment schemes. While some support to businesses was provided, this appears to have often been targeted at self-employment or social enterprises (Cowen et al., 2008). Thus, by examining the effects of the NRF, we shed light on the extent to which improvements in local labor supply, through investments in education, environment, and community safety, can result in increased employment and decreased worklessness.

Mid-program evaluations concluded that progress was being made (Lupton & Power, 2005), and post-program evaluations were generally positive. Around two thirds of the NRF outcomes are thought to be directly attributable to the program (Lupton et al., 2013), with a range of new local services being provided as a result. In terms of worklessness, the official post-program evaluation concluded that there were nearly 70,000 fewer workless people in NRF areas by 2007 than there would have been without the policy, or around 750 persons per district (Dept. for Com. and Local Gov't, 2010, pp. 59). However, unlike the evaluation of Alonso et al. (2019) on the effects of the NRF on crime, or the official evaluation of the NRF on education, the report does not use a difference-in-differences or similarly robust approach. Moreover, the official evaluation only examined the labor market impacts of the NRF on worklessness, and not on measures of employment.

We estimate the impact of the NRF on employment and earnings using data on 352 local areas in England between 1999 and 2008. We use standard difference-in-differences and continuous treatment approaches following Alonso et al. (2019), as well as the synthetic difference-in-differences estimator of Arkhangelsky et al. (2021) to deal with potential pre-treatment differences between treated and control units. We also estimate spillover effects using the spatial difference-in-differences model of Delgado & Florax (2015), and we assess the robustness of our results to interval censoring, missing data, confounding policies, and gentrification in an online appendix.

Our major contributions are to provide the first evaluation of the effects of the NRF on employment and earnings, and the first evaluation of the labor market effects of the NRF which controls for geographic spillovers, differential trends, and confounding policies. Our findings suggest that the NRF had a significant impact on local employment, increasing the number of employed residents in treated areas by 2.5%, and the number of self-employed residents by 8.2%. These increases are associated with a small decrease in average earnings, consistent with the NRF working through improvements to labor supply. Notably, we do not find evidence of spatial spillovers nor improvements to job counts, but a sensitivity analysis suggests significant falls in out-of-work benefits claimants.

Our results are robust to a variety of checks, imply that the Neighbourhood Renewal Fund was a cost-effective intervention in deprived parts of England in the early 2000s, and suggest that 'place-based people strategies' to improve local labor supply can be a successful strategy for neighborhood renewal.

2 The Neighbourhood Renewal Fund

The Labour Party was elected to government in 1997, following a landslide victory in which the outgoing Conservative Party lost 178 seats in the House of Commons. While Labour's manifesto was relatively light on the topic of neighborhood renewal, concerns about declining localities had been building for some time. To address these concerns, the newly-formed Social Exclusion Unit was asked to produce a report on neighborhood problems during Labour's first year in office (Lupton & Power, 2005). While the Social Exclusion Unit worked on this report, a variety of place-based policies continued to be implemented, or were newly introduced. Notable examples of the former include further rounds of the outgoing Conservative government's Single Regeneration Budget; notable examples of the latter include Sure Start Centres and the New Deal for Communities.

The National Strategy for Neighbourhood Renewal was announced in January 2000, and was significantly larger in size and scope than its predecessors. The New Deal for Communities, for example, was essentially a pilot study covering 39 of around 4,000 small neighborhoods identified as deprived by the Social Exclusion Unit (Romero, 2009). The National Strategy for Neighbourhood Renewal, in comparison, encompassed the entirety of England, and attempted to focus existing nationwide policies on the poorest areas (Lupton & Power, 2005; Lupton et al., 2013). As part of the broader program, Local Strategic Partnerships between local governments, public authorities and civil society organizations were set up in 88 of the most deprived districts in England, and tasked with the creation of local neighborhood renewal strategies. These local strategies were supported by the NRF, which disbursed almost £3 billion between 2001 and 2008. The NRF was replaced by the Working Neighbourhoods Fund in 2008 (Dept. for Com. and Local Gov't, 2015).

Eligibility for the NRF was determined by the Index of Multiple Deprivation (IMD), which was commissioned by the Office of the Deputy Prime Minister to measure the distribution of well-being across small geographies (Jackson, 2005). The IMD's original design utilized scores along six domains – income, employment, health, education, housing, and access to services[1] – based on ward rankings across England. Each domain was assigned a weight and combined to produce a single deprivation score, such that higher scores represent more distressed districts. To be eligible for NRF funding, a local authority district needed to score in the top 50 most deprived on any of the domains. This criteria ultimately yielded 81 NRF-eligible local authority districts out of a total of 352 in England. However, seven further areas that would be eligible by these criteria using the 1998 Index of Local Deprivation were given transitional funding for three years, raising the total number of NRF districts to 88 (Social Exclusion Unit, 2001). NRF funding was allocated to each of the eligible districts

[1]For the 2004 IMD, crime and living environment were added as domains while access to services and housing were combined into one domain.

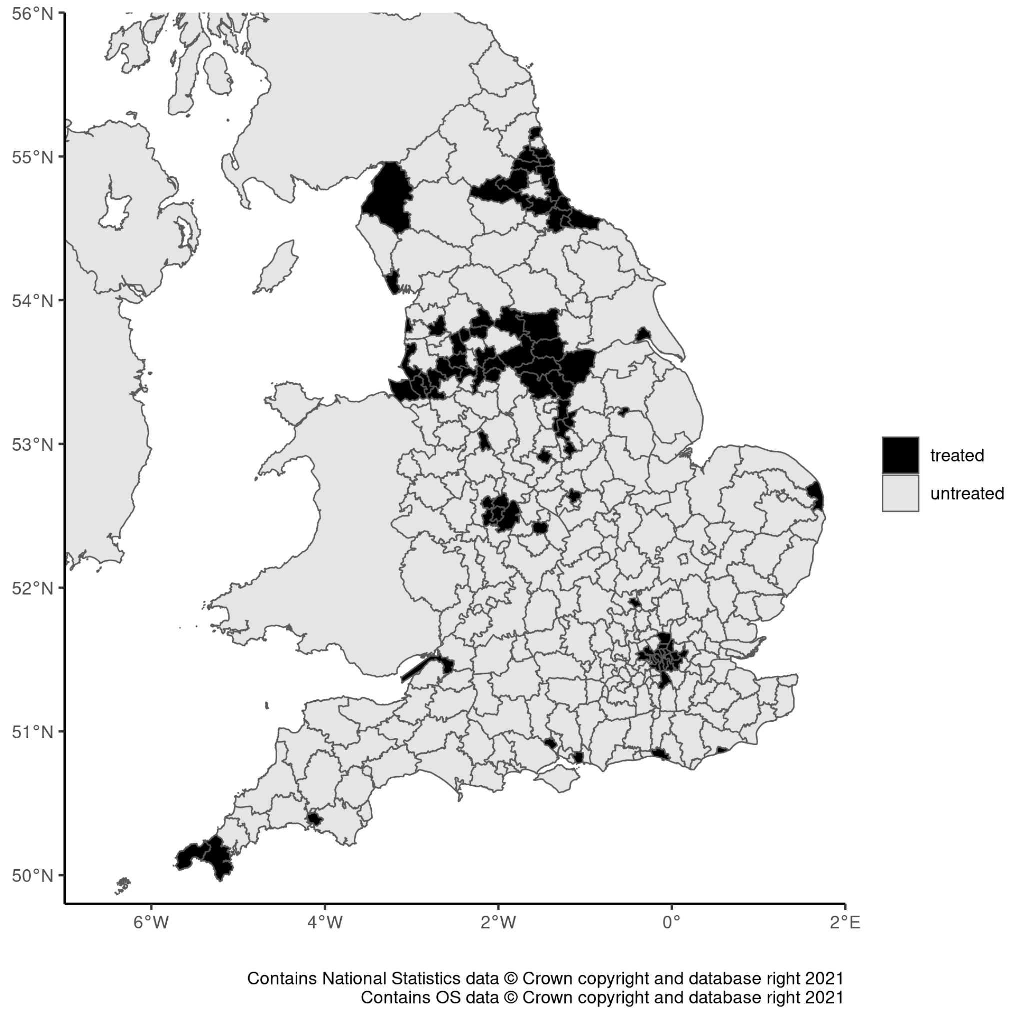

Figure 1: Map of England (with surrounding geography included for reference), with Neighbourhood Renewal Fund treatment areas highlighted in black.

based on the number of residents living in deprived neighborhoods (Alonso et al., 2019).

The districts funded by the NRF are displayed in figure 1. These districts tended to be in or around major cities, which is very different from recent attempts to 'level up' towns and peripheral areas in England (Jennings et al., 2021; Tomaney & Pike, 2021). In the late 1990s, however, Britain's cities were still seen as problem areas suffering from job loss and population decline, while smaller towns and rural districts were seen as growth areas (Fothergill & Houston, 2016). This view would ultimately be reflected in both the NRF funding areas and New Labour's wider 'urban renaissance agenda' (Colomb, 2007)

As noted above, estimates from 2006 suggest that roughly 19 percent of the NRF money was spent on community safety, 19 percent on education, 16 percent on health, and 12 percent on worklessness, with the remainder being spent on the environment, cost-cutting activity, and administration (Dept. for Com. and Local Gov't, 2010). Of the funding targeting worklessness, the majority appears to have been spent on programs to increase labor supply, including advice, guidance, and training for unemployed or marginalized workers. Of the £519,000 of NRF funds spent by Kensington and Chelsea borough council on 'work and business' projects between 2004 and 2006, for example, £224,000 was spent on a project to encourage disadvantaged residents to find work by improving childcare support, and a further £200,000 was spent on advice, job search, CV preparation, and application assistance for unemployed people in the borough. A relatively small sum of £15,000 was spent on a consultant to investigate barriers to social enterprises and local groups wanting to develop as enterprises, which was the only funded project with any connection to labor demand (Kensington and Chelsea Partnership Steering Group, 2007).

As well as the independent evaluation in Cowen et al. (2008), there was also an official post-program evaluation that covered labor market outcomes. However, the quantitative part of this evaluation only assessed the NRF's impact on worklessness. Moreover, the authors used a 'transitions model' to achieve this, which identified factors that might affect outcomes in NRF-treated districts, held those factors constant, and then isolated the change in worklessness that could be attributed to the NRF (Dept. for Com. and Local Gov't, 2010). Importantly, the authors of this report emphasized that their analysis was not credibly causal, and suffered from limited data availability. Given the recent resurgence of interest in place-based policies to improve labor market outcomes, we aim to provide a more comprehensive and robust analysis of the NRF's impact on employment, wages, jobs, and unemployment claimants. Our primary methodology follows Alonso et al. (2019), which studies the impact of the NRF on property and violent crime using difference-in-differences models and finds improvements of 10 to 25 percent in treated districts.

3 Data

We use data on various different measures of labor market activity for the local authority districts that existed in England in the early 2000s, other than the Isles of Scilly and City of London, which are extremely small and have very different local economies from the rest of the country. Of the 352 districts in our sample, 88 received NRF funding, of which 7 only received 'transitional funding' – see Social Exclusion Unit (2001). For our main results, we use data on total employees, total self-employment, and average weekly earnings.

English local authority districts had an average population of around 140,000 during the sample period. The treatment and population data are from the replication files for Alonso et al. (2019), kindly provided to us by the authors. Both the treatment indicator and treatment intensity variables are on a British fiscal year basis, so the observations corresponding to 2002, for example, refer to the period between April 2001 and March 2002.

The employment and self-employment variables are from the NOMIS 'Local Area Labour Force Survey' dataset between 1999/00 and 2003/4, and from the (comparable) 'Annual

Population Survey' dataset between 2004/5 and 2007/8, and are defined on a residence basis. These data are fiscal year averages, with the Labour Force Survey averaging over March–February observations and the Annual Population Survey averaging over April–March. These data run from 2000 to 2008.

The earnings figures are from the 'Annual Survey of Hours and Earnings', and are defined as mean gross earnings for full-time employees within local authority districts. These data are observed as of April, so, for example, our 2002 observations for average weekly earnings correspond to earnings as of April 2002. These should, therefore, be interpreted as earnings at the end of each fiscal year. Comparable figures exist between 1997 and 2004, after which the survey methodology changed.

Unfortunately, detailed population estimates are no longer available for English districts as they existed between 1999 and 2008 (see e.g., Calvert Jump, 2020, for a discussion of local government restructuring in Britain), so we only have access to total population figures. This means that we work with absolute numbers of employed persons in the bulk of the paper, as working age population figures no longer exist. However, we discuss the likely effects of population movements and internal migration in section 7, and leave a description of those data until that point. The employment data involve interval censoring, and there are some missing values, which we also discuss in section 7.

As discussed above, the NRF funded 88 of the most deprived areas in England, which suggests that there might be differences in pre-treatment labor market characteristics between treated and non-treated areas. To explore this possibility, figure 2 plots histograms of our labor market variables in fiscal year 2000, and scatters against the average ward rank of the Index of Multiple Deprivation.

While there is a significant amount of overlap between the distributions, pre-treatment employees and self-employment per capita are lower in more deprived areas than less deprived areas, which is to be expected. Interestingly, however, while the top half of the average wage distribution is mainly accounted for by non-treated areas, some of the most deprived parts of the country also have very high average weekly earnings. These are generally those parts of London in which widespread deprivation co-exists with affluence, including Tower Hamlets (which incorporates the Canary Wharf financial centre) and the City of Westminster. Kensington and Chelsea, which we have already mentioned, is another example. There are, therefore, some pre-treatment differences between the treated and control units, which our empirical strategy takes into account.

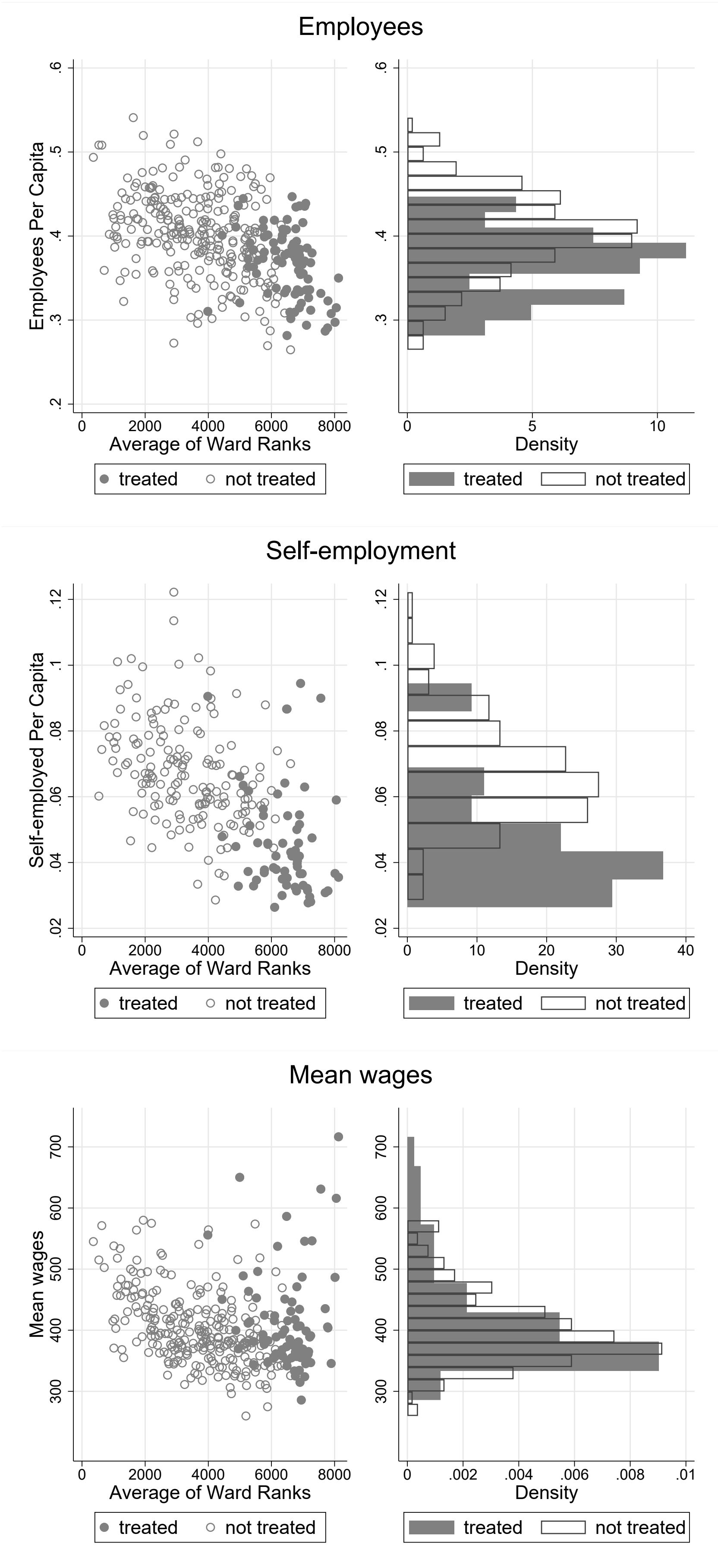

Figure 2: Histograms and scatter diagrams of pre-treatment (fiscal year 2000) labor market variables, deflated by total population where appropriate, against the average ward rank of the index of multiple deprivation (higher rank indicates higher deprivation).

4 Methods

Our main results use three specifications: a difference-in-differences model, a continuous treatment variable model, and a synthetic difference-in-differences model. The difference-in-differences model includes area and time fixed effects to estimate the impact of the NRF on labor market outcomes:

$$y_{it} = \alpha_i + \delta_t + \beta D_{it} + \epsilon_{it}. \tag{1}$$

The dependent variable is the natural log of total employees, self-employed persons or average weekly earnings in district i, $i = 1, ..., 352$, and year t, in which $t = 2000, ..., 2008$ for employees and self-employment, and $t = 1997, ..., 2004$ for average weekly earnings. The area and time fixed effects are denoted by α_i and δ_t, respectively, and the dummy variable D_{it} equals one for NRF treated areas after 2001 and zero otherwise. Therefore, the control group consists of all districts that were not eligible for funds from the NRF, as in Alonso et al. (2019). The area effects control for time-invariant differences in local labor market outcomes from unobservable factors that vary across localities, while the time effects capture common time trends that are shared across localities. Standard errors are clustered by district.

As the treatment period is uniform for all treated areas, it is worth noting that the problems with two-way fixed effects estimators recently highlighted by Goodman-Bacon (2021) and others are not relevant to our results. We also report results from a generalized (or event-study) difference-in-differences model that captures lead and lag effects of the NRF, to provide an informal visual test of parallel trends.

The continuous treatment variable model is specified as follows:

$$y_{it} = \alpha_i + \delta_t + \gamma TI_{it} + \epsilon_{it}. \tag{2}$$

Treatment intensity TI is proxied by the amount of NRF funds allocated per inhabitant of district i in year t. The amount of funding per district varied according to the number of inhabitants and was determined by the UK government, rather than the local authority districts themselves. The sample in these models ends in 2007, due to the availability of population data. As above, standard errors are clustered by district.

As is well-known, difference-in-differences models rely on the assumption of parallel pre-treatment trends. Given the pre-treatment relationships between jobs, employment, self-employment and deprivation documented above, this assumption is possible, but is not certain. To allow for potentially different pre-trends among the treated and control units, we use the synthetic difference-in-differences estimator of Arkhangelsky et al. (2021), which uses unit and time weights to ensure that weighted pre-treatment outcomes for control units are approximately parallel, on average, to pre-treatment outcomes for treated units, and that the average post-treatment outcome for the control units differs by a constant amount from the weighted average of the pre-treatment outcomes for the same control units (Arkhangelsky et al., 2021, pp.4090).

The synthetic difference-in-differences estimator is described by equation (3):

$$\left(\hat{\alpha}, \hat{\beta}, \hat{\delta}, \hat{\mu}\right) = \arg\min_{\alpha,\beta,\delta,\mu} \sum_{i=1}^{N} \sum_{t=1}^{T} (Y_{it} - \mu - \alpha_i - \delta_t - \beta D_{it})^2 \hat{\omega}_i \hat{\lambda}_t. \tag{3}$$

We estimate the average treatment effect on the treated (ATT) using (3) as outlined by Clarke et al. (2023). The ATT, $\hat{\beta}$, is estimated by a two-way fixed effect regression of the dependent variable, Y_{it}, observed for each unit i in each period t where the binary policy variable of interest is denoted D_{it} as before and μ is a constant term. Cross-sectional and year fixed effects are also denoted by α_i and δ_t as before, and we calculate standard errors using the boostrap method discussed in Clarke et al. (2023), which re-estimates multiple $\hat{\beta}$s using re-sampling with replacement. This (and the estimation procedure) are included in the `sdid` Stata command, see Pailañir & Clarke (2023).

While we use the synthetic difference-in-differences estimator to allow for potential differences in pre-treatment trends between the treatment and control groups, we do not make use of pre-treatment deprivation itself in our empirical strategy. This is because the rule for allocating districts to the NRF was highly non-linear, with districts allocated to the NRF if they were within the top 50 most deprived areas on any of the following six metrics within the Index of Multiple Deprivation: the employment scale (number of people in receipt of unemployment-related benefits), the income scale (number of people in receipt of income-related benefits), the population-weighted average of the employment and income scales for the wards in a district, the population-weighted average of the combined employment and income ranks for the wards in a district, the proportion of the population living in wards which rank within the most deprived 10% of wards in the country, and the population-weighted average of the ranks of a district's most deprived wards that contain exactly 10% of the district's population (Alonso et al., 2019). Instead of attempting to model this mechanism, we control for pre-trends non-parametrically following Arkhangelsky et al. (2021).

5 Main results

The results from our simple difference-in-differences models and synthetic difference-in-differences models are presented in the top panel of figure 3. These results suggest that there was a positive treatment effect of the NRF on treated areas for total employees and self-employment, and a negative treatment effect for average weekly earnings. Specifically, our results suggest that the NRF was associated with a 2.47% increase in employees, an

8.2% increase in self-employment, and a 1.18% decrease in average weekly earnings. As the median treated district had 78,000 employees and 10,000 self-employed persons in 2001, these results suggest that the NRF led to an increase of around 1,900 employees and 800 self-employed persons in the median treated area between 2002 and 2008.

The results from our continuous treatment variable model are presented in the bottom panel of figure 3. The coefficients indicate the effect of a £1 per capita increase in NRF spending per locality over the treatment period. The results suggest that £1 per capita of NRF funding is associated with a 0.14% increase in employees, a 0.25% increase in self-employment, and a 0.05% decrease in average weekly earnings, although this result is not statistically significant. As the median annual disbursal was around £16 per capita from the NRF over the treatment period, these results suggest that the NRF led to an increase of around 1,750 employees and 400 self-employed persons in the median treated area between 2002 and 2007, which is consistent with the previous results.

There are two interesting takeaways from the results in figure 3. First, Cowen et al. (2008) observe that the levels of interest shown in self-employment within targeted groups – our largest effect size – were an unexpected benefit of the NRF (Cowen et al., 2008, pp.44). Second, the fact that employment increased while wages decreased supports our conjecture that the Neighbourhood Renewal Fund operated through an increase in labor supply, rather than labor demand.

The similarity between the simple and synthetic difference-in-differences models in the top panel of figure 3 suggests that differential pre-trends are not pronounced in our data. These pre-trends are explored further in figure 4, which presents the results from generalized difference-in-differences models using the same dependent variables as in figure 3, in which $t = 0$ corresponds to fiscal year 2002 and the effect sizes are difference-in-differences relative to fiscal year 2001. The obvious issue here is that we only have two pre-treatment observations for the employment variables, so we cannot conclusively demonstrate the absence of pre-trends for employees and self-employment. However, the lack of pre-trends in average weekly earnings, for which we have 5 years of pre-treatment data, hopefully alleviates this concern to some extent.

Over a longer time span, as illustrated in figure 5, economic activity rates slowly diverged between treated and untreated districts prior to the introduction of the Neighbourhood Renewal Fund. While the gap increased by around a quarter of a percentage point per year, figure 4 suggests that this process was arrested by the turn of the millennium, and reversed as a result of the Neighbourhood Renewal Fund.

Our main results suggest a positive treatment effect of the NRF on employment, alongside reductions in average weekly earnings consistent with a supply-side mechanism. In the next two sections we explore the implications of these results for job counts and claimant counts,

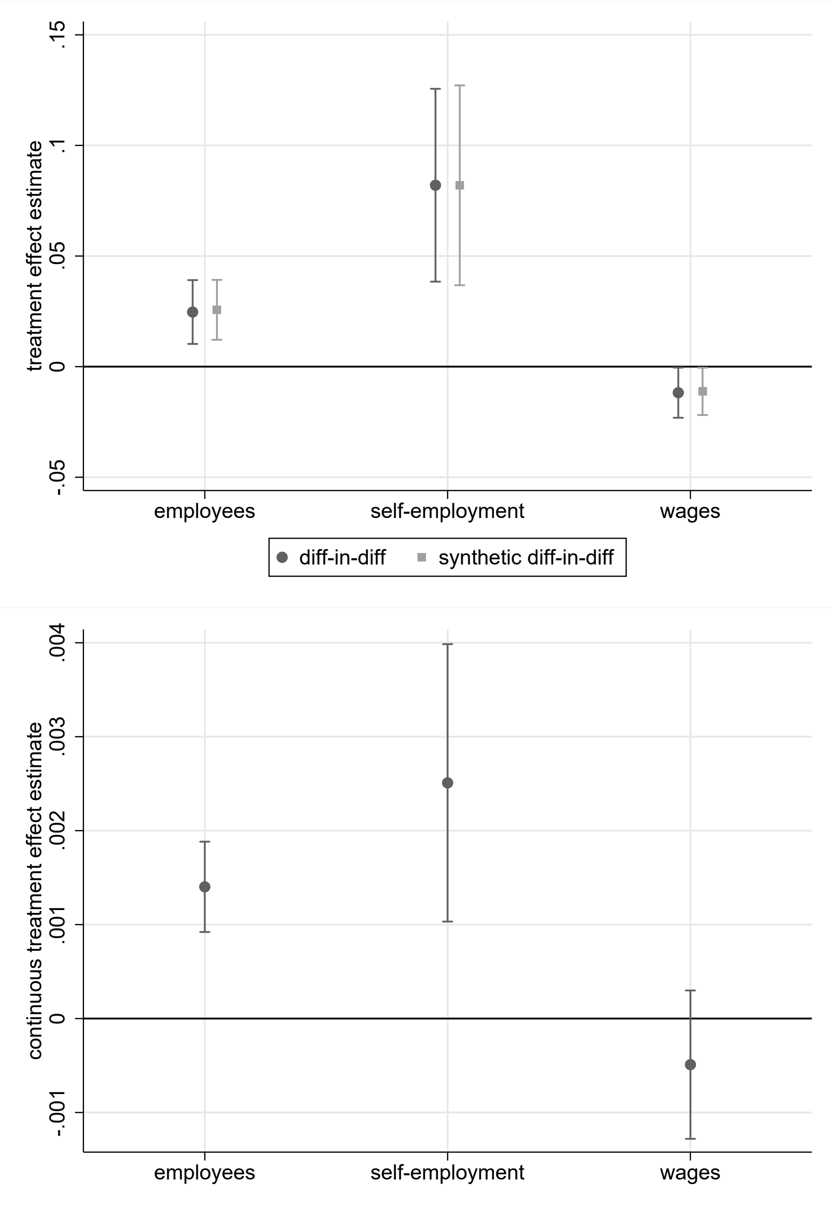

Figure 3: Point estimates and 95% confidence intervals for the simple and synthetic diff-in-diff models (upper panel) and the continuous treatment variable model (lower panel). All dependent variables have been log transformed.

the possibility of spatial spillovers, and the robustness of our results to interval censoring, missing data, confounding policies, gentrification, more complex trend specifications, and reductions in the control set.

6 Jobs, claimants and spillovers

Our results suggest that the NRF was a successful intervention into labor supply in deprived local labor markets in England. While its effects are relatively clear-cut for employment and wages, however, the effects on wider measures of job market performance are less clear.

First, as illustrated in the top panel of figure 6, there is no measurable effects of the Neighbourhood Renewal Fund on job counts, at least using our difference-in-differences methodology. Here, we are using job counts from the NOMIS 'Annual Business Inquiry' dataset,

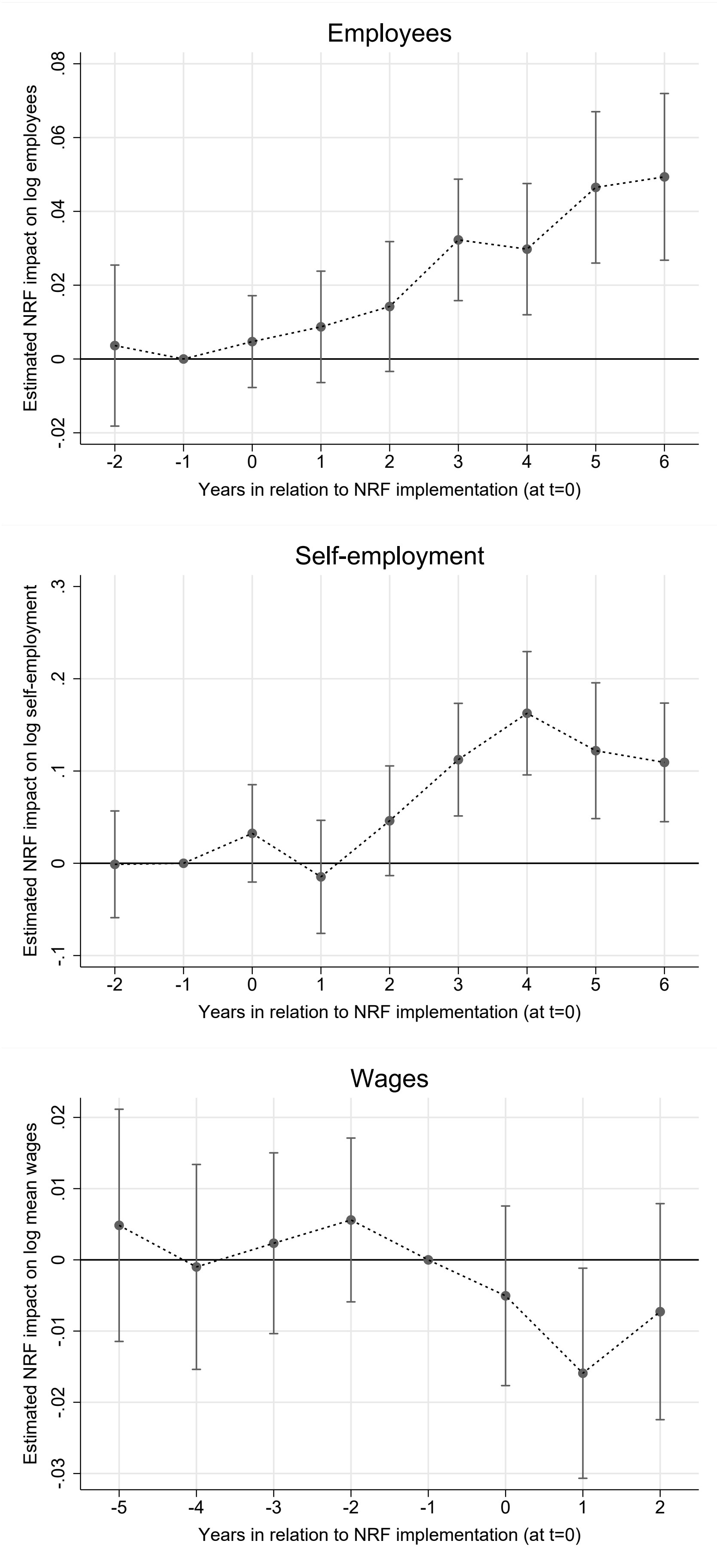

Figure 4: Event study plots from generalized difference-in-differences models.

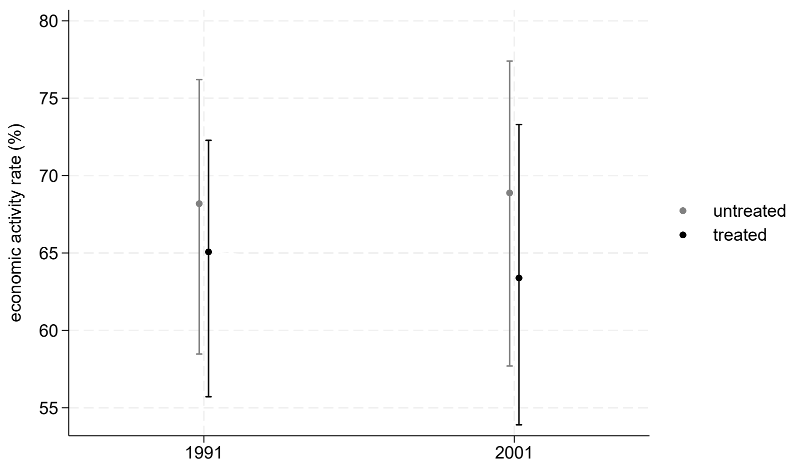

Figure 5: Economically active aged 16-74 as a % of district population aged 16-74, treated and untreated areas in 1991 and 2001 (using decadal census data). Markers show mean district; error bars show minimum and maximum.

which are defined on a workplace basis.[2] It is not obvious why job counts did not increase when employment did, but could be because people were finding jobs in small firms not covered by the Annual Business Inquiry, or because the number of people with more than one job declined as the number of employees increased. Alternatively, as employees are measured on a residence basis, while job counts are measured on a workplace basis, the newly employed residents of NRF treatment areas might have been finding jobs elsewhere.

Second, as discussed in appendix E, the overall populations of NRF areas were declining before 2002, and continued to decline after the policy was implemented. This implies that unemployment must have been declining in treated areas after 2002, but the existing claimant data suffer from pronounced pre-trends. To attempt to identify an approximate effect on total unemployment claimants, we resort to the 'credible' approach to parallel trends recently proposed in Rambachan & Roth (2023) in the lower panel of figure 6. Here, we are using headline claimant counts from the NOMIS 'Jobseeker's Allowance with rates and proportions' dataset, between 1998 and 2008, where the observations are as of March (i.e., the end of the fiscal year).

We explain the method used to compute the lower panel of figure 6 in appendix B. But essentially, the black points and error bars correspond to point estimates and 95% confidence intervals from a standard generalized difference-in-differences model, while the grey error bars correspond to 95% confidence intervals for identified sets of treatment effects which take pre-treatment differential trends into account. And because claimant counts were increasing in treated areas relative to non-treated areas prior to 2002, the results from a standard difference-in-differences model are biased towards zero. Thus, large parts of the 'credible' identified sets are larger in magnitude than the standard point estimates.

[2]These data are observed as of December. Given the fiscal year nature of the treatment data, we code the job count observations with a single year lag. So, for example, our 2002 observations for job counts correspond to the raw December 2001 observations, which is in the middle of fiscal year 2002. These run from 1999 to 2008.

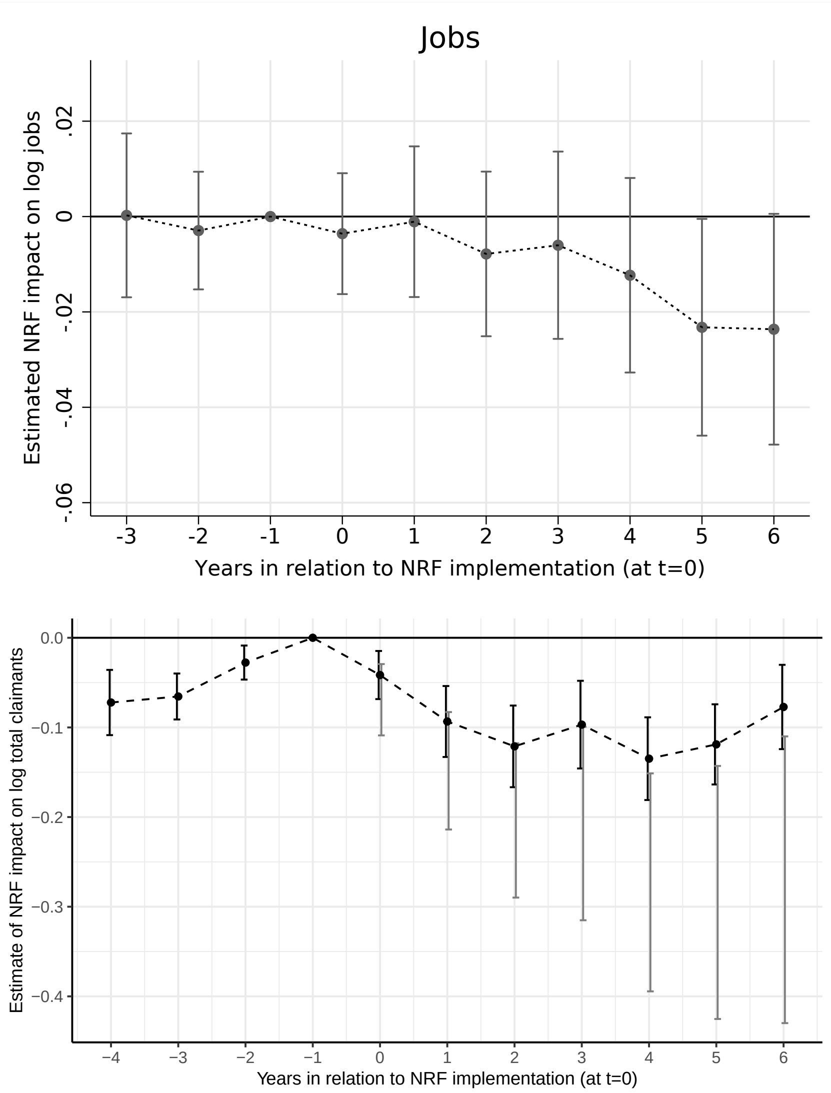

Figure 6: Event study plots from generalized difference-in-differences models in job count and claimant counts. The grey error bars in the lower panel summarize a sensitivity analysis to pre-trends, following Rambachan & Roth (2023).

The lower panel of figure 6 suggests that the Neighbourhood Renewal Fund reduced the claimant count in treated areas by somewhere between 20% and 40%, which suggests a reduction of somewhere between 1,000 and 2,000 out-of-work benefits claimants in the median treated area. This is broadly consistent with our results for employees and self-employment, although it should be borne in mind that these results rely on a calibrated sensitivity analysis and are, therefore, more uncertain than the results in section 5.

Finally, one of the key identification assumptions that allows difference-in-differences estimates to be interpreted as causal treatment effects is the stable unit treatment value assumption. This assumption states that outcomes in one area are unaffected by the treatment assignment in other areas (Chagas et al., 2016). However, given the apparent absence of an effect on job counts, it is possible that the NRF was subject to spatial spillovers. In order to assess the extent of spatial spillovers across local authority districts, we report the results of a difference-in-differences model that includes a spatial lag, following Delgado &

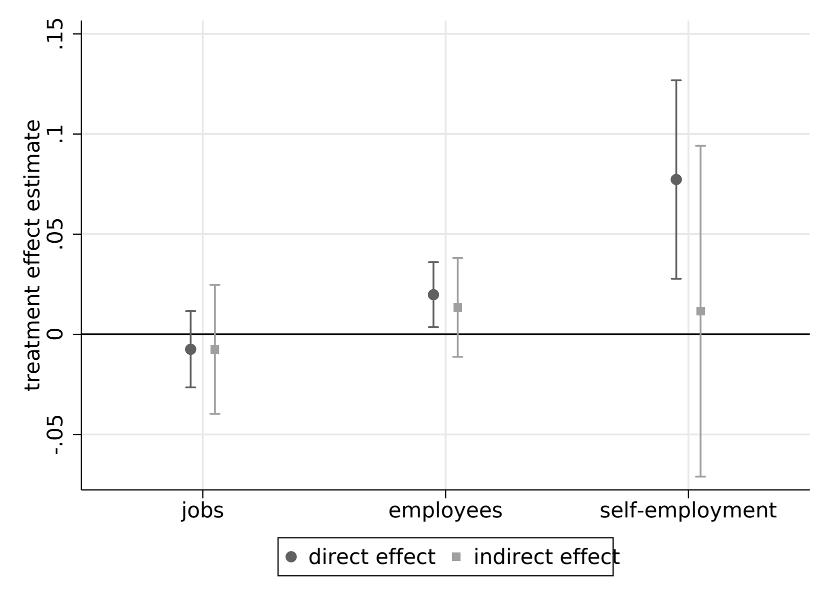

Figure 7: Point estimates and 95% confidence intervals for the direct effect ($\hat{\phi}$) and indirect effect ($\hat{\theta}$) estimates from the spatial difference-in-differences model.

Florax (2015). The following model is estimated:

$$y_{it} = \alpha_i + \delta_t + \phi D_{it} + \theta \sum_{j=1}^{352} w_{ij} D_{jt} + \epsilon_{it}, \tag{4}$$

in which w_{ij} is the (i, j)th element of the weighting matrix W that connects the 352 local authority districts in our sample, which is a row-normalized spatial contiguity matrix that indicates the degree to which each of the local authority districts share borders with treated areas. For example, if a district shares a border with 5 other districts and 3 of them are treated, then $w_{i.}$ will be equal to $3/5 = 0.6$. In this specification, ϕ captures the average direct treatment effect of the NRF, while θ captures the average indirect treatment effect on both treated and non-treated areas. The direct treatment effect in (4) differs from β in (1) in that the control group for the former is effectively restricted to those local authority districts which are both untreated and do not share a border with a treated area.

Figure 7 reports the results from the spatial difference-in-differences models for our job count and employment variables. Although statistically insignificant, all three indirect effect point estimates are positive and their confidence intervals are wide relative to those of the direct effects. Thus, we cannot rule out the possibility of large spillovers being present.

The apparent lack of spatial spillovers in job counts poses a conundrum, as our estimates suggest that the NRF increased the number of employees in treated areas, but did not increase the number of jobs. As discussed above, the likely alternatives are that the number of people with more than one job declined as the number of employees increased, or that people were finding jobs in small firms not covered by the Annual Business Inquiry.

Unfortunately, the ‘Local Area Labour Force Survey’ data on persons with second jobs suffers from severe missing data problems in the early 2000s, so we cannot test the first hypothesis directly. However, the number of people with second jobs in the UK as a whole declined by around 100,000 between 2002 and 2006 (see the ONS series YCBW), so this could be part of the story. On the other hand, the Annual Business Inquiry did not sample from employers not registered for PAYE (Calvert Jump, 2020), and smaller businesses were less likely to be sampled, so measurement error might also play a role.

7 Robustness checks

We provide a range of robustness checks in the online appendices C – G. The first, plotted in figure C1, takes into account a limited amount of interval censoring in the employment and job count data. Specifically, the employee and self-employee data are rounded to the nearest 100, while the job count data are rounded to the nearest 1000. Therefore, if x is the rounded observation, we compute lower and upper bounds $\{x_l, x_h\}$ for the employee and self-employee observations as $\{x - 50, x + 49\}$, and for the job count observations as $\{x - 500, x + 499\}$. The estimates in figure C1 are computed using a maximum likelihood approach described in, for example, Conroy (2005), and implemented as `intreg` in Stata, and are not materially different from those in figure 4 and the top panel of figure 6.

The second, plotted in figure C2, takes into account missing values in the self-employment data. The results in section 5 simply remove all local authorities with at least one year of missing data (as the synthetic difference-in-differences estimator requires a balanced panel). In contrast, the results plotted in figure C2 use multiple imputation to estimate the 462 missing self-employment observations between 2000 and 2007, using a regression model including employee numbers, population, benefit claimants, leads and lags of the treatment indicator, and area and year fixed effects. Again, the estimates are not materially affected.[3]

So far, we have not considered potentially confounding policies. The most obvious contender is the New Deal for Communities (NDC), which operated in many of the same areas as the Neighbourhood Renewal Fund, and also started disbursing funds in 2001 (Romero, 2009). Unfortunately, as there was only one local authority that received NDC funding without receiving NRF funding, it is not possible to completely disentangle the effects of the two policies. However, in appendix D we partially examine the robustness of our results by adding an extra treatment indicator for districts in receipt of both NRF and NDC funds. The NRF effect estimate on employees becomes insignificant in this model, while its sign remains positive, suggesting that employee growth in the treatment period was higher in local authority districts that received funding from both programs.

[3]There are also 13 missing wage observations, but because the remaining variables only have observations from 2000 we could only impute the missing wage observations between 2000 and 2004, which is not enough to produce reasonable estimates. In any case, 13 missing observations (out of $352 \times 8 = 2,816$ observations in total) is unlikely to be material.

Some scholars of New Labour's economic and social policies have pointed out that the effects of their regional policies cannot be neatly separated from broader regional factors. Lupton et al. (2013), for example, argue that the relative improvement in benefit claimant rates in NRF areas was, "partly to do with regional divergence and the varying fates of areas with different geography and economic bases rather than with programme interventions." While geography and industrial structure are either static or very slow-moving, and thus controlled for in our difference-in-differences models, one might be concerned that the NRF treatment areas happened to be developing or gentrifying, entirely by chance, at the start of the program. If these areas would have improved regardless of the receipt of NRF funds, then our estimates would be biased away from zero.

There are various pieces of evidence, however, that suggest that NRF treatment areas were not developing or gentrifying at the start of the program, which we summarise in appendix E. First, as displayed in figure E1, NRF areas were losing population by internal migration in the years immediately before NRF treatment. Figure E2 supports this observation, by displaying an approximation to the lead and lag effects of the NRF on district-level populations (the approximation is discussed in that appendix, while appendix F provides supplementary results using employment variables weighted by population). This exercise suggests that the population of NRF treatment areas was declining relative to non-treatment areas both before and after 2002, with no obvious treatment effect. Finally, figure E3 in appendix E reports estimates in which the districts in London are excluded from the sample, which is the most obvious candidate for gentrification in the early 2000s. The results are not materially affected, and thus we do not think that 'chance gentrification' is a convincing confounder (for more on internal migration in this period, see Lomax et al., 2014).

Appendix G reports the robustness of our results to controlling for region-specific trends, as well as limiting the set of control units. Specifically, to provide further reassurance against the possibility of bias from spatial spillovers, we exclude all control units bordering treatment areas in figure G2 (we also provide a second map that details the different types of areas). Again, our main results are unaffected. And finally, it is of course possible that the regional effects of New Labour's national policies happened to be higher in NRF areas than elsewhere. One obvious possibility is the Jobcentre Plus roll-out, which also commenced in 2002 and is thought to have had positive effects on employment (Riley et al., 2011). It is worth bearing in mind, however, that one of the major goals of the National Strategy for Neighbourhood Renewal was an improvement in the delivery of nationwide policies within deprived areas (Lupton & Power, 2005). In fact, at least some of the NRF money was used in tandem with Jobcentre Plus offices in treatment areas (Cowen et al., 2008). If it is the case, therefore, that the regional effects of New Labour's national policies were higher in NRF areas than elsewhere, this would partly constitute a mechanism by which the NRF helped deprived areas, rather than a confounding factor.

8 Discussion

We provide evidence on the impact of Neighbourhood Renewal Fund designation on the labor markets of treated areas, estimating the effects on jobs, employment, and self-employment. We use data from various Office for National Statistics surveys from 1999 to 2008 to identify the program's impacts at the local authority district level. We compare outcomes in districts that received funds from the NRF to districts that were ineligible for the program, as in Alonso et al. (2019). Using a difference-in-differences model, we find no impact on the number of jobs in treated districts. However, we find positive and significant impacts on employment (2.5%) and self-employment (8.2%) of residents in treated districts over our six-year treatment period.

Our results suggest that the NRF increased employment in the median treated district by around 2,700 persons between 2002 and 2008, suggesting that the program's impact on local labor markets was somewhat more successful than contemporary evaluations suggested (Dept. for Com. and Local Gov't, 2010). This result is consistent with Alonso et al. (2019), who found that the NRF was associated with major reductions in violent crime and property crime, as improvements in an area's health, education, or crime profile should all contribute to improvements in labor supply. A simple value for money analysis in Appendix H on out-of-work benefits claimants indicates that the NRF provided good value for the funds targeted toward worklessness. Evaluations of the New Deal for Communities, a related New Labour program that appears to have interacted positively with the NRF, also suggest that this type of policy can improve local labor market outcomes (Romero, 2009).

It seems likely that those NRF interventions that targeted worklessness and education were most responsible for the improved employment outcomes of treated district residents. Bottom-up evidence from Dept. for Com. and Local Gov't (2010) suggests that NRF-funded worklessness interventions (e.g., advice, transitional employment schemes, and support for self-employment) were found to be effective at facilitating access to employment, training, and qualifications, while educational interventions (e.g., raising attainment, reducing exclusions, out of school activities, parental involvement, and basic skills) were effective at improving student attainment and gaining jobs for parents.

Overall, our study suggests that 'place-based people strategies' to improve local labor supply can be a successful strategy for improving labor market outcomes in deprived neighborhoods. This approach is most likely to work for areas like those funded by the NRF, which were mainly within or near major metropolitan areas. Stimulating labor demand is less important in these areas than it would be in more remote areas, or persistently depressed regions with poor transport links to employment opportunities. Nevertheless, strategies to improve local labor supply might serve as a complement to place-based policies that aim to stimulate local labor demand in such regions, of which Austin et al. (2018) provides one example. Crisp et al. (2014), in a comprehensive review of regeneration strategies, make the general point that any policy designed to create new jobs is likely to be more successful if programs are also implemented to help residents access those jobs. Our results suggest that New Labour's Neighbourhood Renewal Fund supplies a useful blueprint for this type of policy, and indeed any strategy that aims to improve local labor supply.

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Online Appendices

A Supplementary Data

Supplementary material related to this article can be found on Mendeley Data under the doi: 10.17632/j8ysdkmntt.2. (Scavette & Calvert Jump, 2024).

B Estimating the effect on unemployment claimants

To attempt to identify an approximate effect on total unemployment claimants, we resort to a 'credible' approach to parallel trends in the lower panel of figure 6. Here, we are using headline claimant counts from the NOMIS 'Jobseeker's Allowance with rates and proportions' dataset, between 1998 and 2008, where the observations are as of March (i.e., the end of the fiscal year).

This sensitivity analysis follows Rambachan & Roth (2023). Following their approach, we can decompose the 11-vector of generalized difference-in-differences point estimates for the effect of the NRF on out-of-work claimants (the black markers in the lower panel of figure 6) as follows:

$$\beta = \begin{bmatrix} \beta_{-4} \\ \beta_{-3} \\ \beta_{-2} \\ \beta_{-1} \\ \beta_0 \\ \beta_1 \\ \vdots \\ \beta_6 \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \\ 0 \\ 0 \\ \tau_0 \\ \tau_1 \\ \vdots \\ \tau_6 \end{bmatrix} + \begin{bmatrix} \delta_{-4} \\ \delta_{-3} \\ \delta_{-2} \\ 0 \\ \delta_0 \\ \delta_1 \\ \vdots \\ \delta_6 \end{bmatrix}, \tag{B.1}$$

in which β_t are the population parameters being estimated by the OLS estimators (i.e., the linear projection parameters), τ_t are the dynamic average treatment effects on the treated areas, and δ_t are the differences in trends between the treated and untreated areas that would have occurred absent treatment. We have already imposed the absence of treatment effects in the pre-treatment periods; the standard identification assumption in difference-in-differences models is the absence of differential trends in post-treatment periods, i.e., $\delta_t = 0 \; \forall \; t \geq 0$.

As we have done in this paper, it is common practice to argue that estimates of δ_t for $t < -1$ that are insignificantly different from zero constitute convincing evidence for the standard identification assumption. In cases like the total claimants panel of figure 6, the evidence is obviously unconvincing. In these cases, Rambachan & Roth (2023) suggest constructing a set of plausible values of δ_t for $t \geq 0$ based on the observed (significant) estimates of δ_t for $t < -1$, which yield an identified set of τ_t for $t \geq 0$ conditional on the assumed set of plausible differential trends. We follow their approach, using their `HonestDiD` package for `R`, by utilizing a smoothness restriction in which differential trends do not evolve too quickly compared to their past values. Denoting the set of plausible trends by Δ, we assume that,

$$\Delta = \{\delta : |(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})| \leq 0.02, \forall t\}, \tag{B.2}$$

i.e., we assume that the differential trends change by *at most* 2 percentage points in absolute value between consecutive periods. The assumed set Δ can be used to compute a confidence interval for the resulting identified set of treatment effects, using the results detailed in Rambachan & Roth (2023).

The results of this sensitivity analysis for log total claimants is plotted in figure 6, and has already been discussed in the main text. To recap, the original OLS point estimates and 95% confidence intervals are in black, while the 95% confidence intervals for the identified set of treatment effects are in grey. As is suggested by the pre-trend evolution, the bulk of the identified set of treatment effects is greater in magnitude than the OLS estimates, suggesting an average treatment effect on the treated areas of somewhere between 20% and 40%. In turn, the result suggests a reduction of somewhere between 1,000 and 2,000 out-of-work benefits claimants in the median treated area, which is broadly in line with the results on employment reported in section 5. Furthermore, this estimated reduction of between 1,000 and 2,000 claimants per treated district is larger than the estimate of around 750 in Dept. for Com. and Local Gov't (2010).

C Robustness to interval censoring and missing values

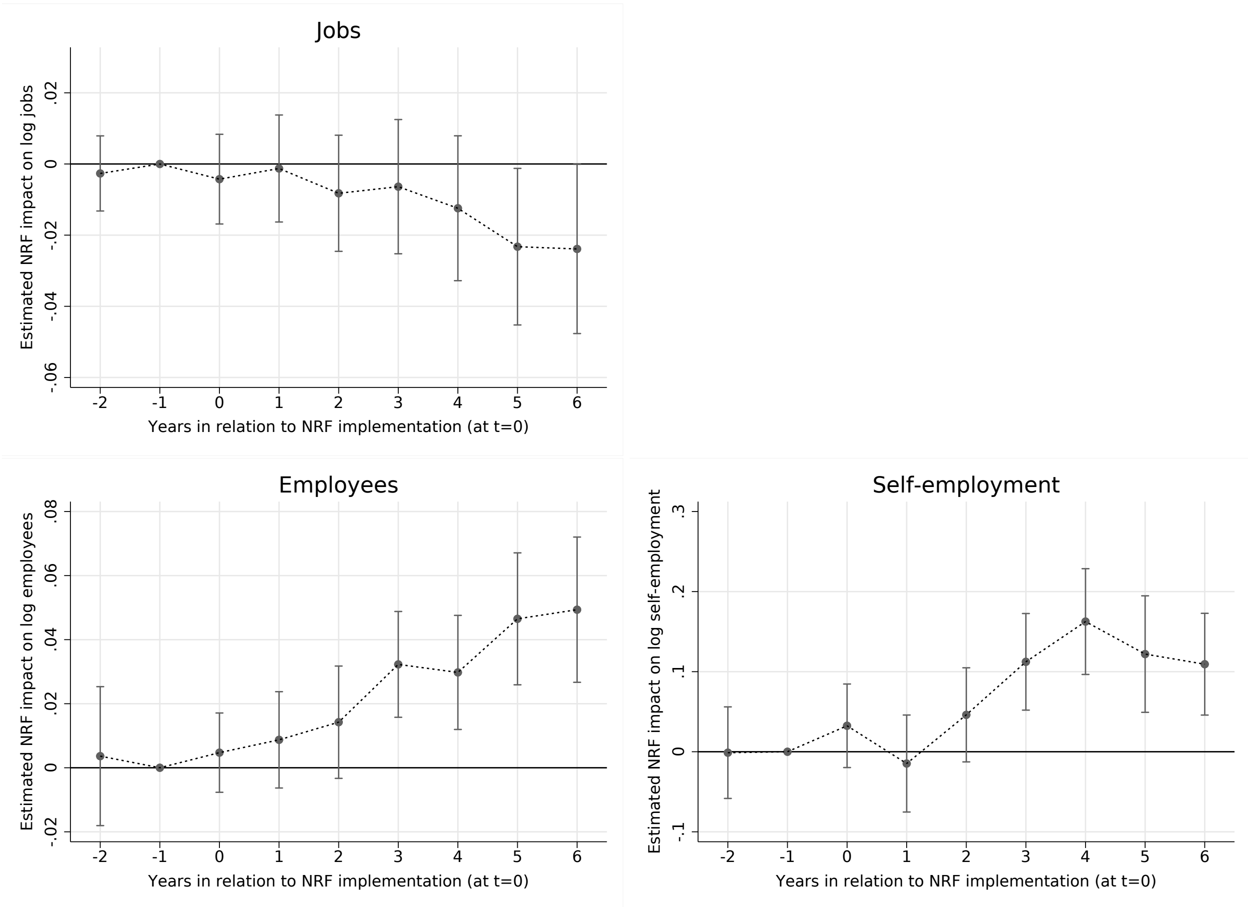

Figure C1: Lead and lag effects of NRF using interval censored regression (`intreg` in Stata).

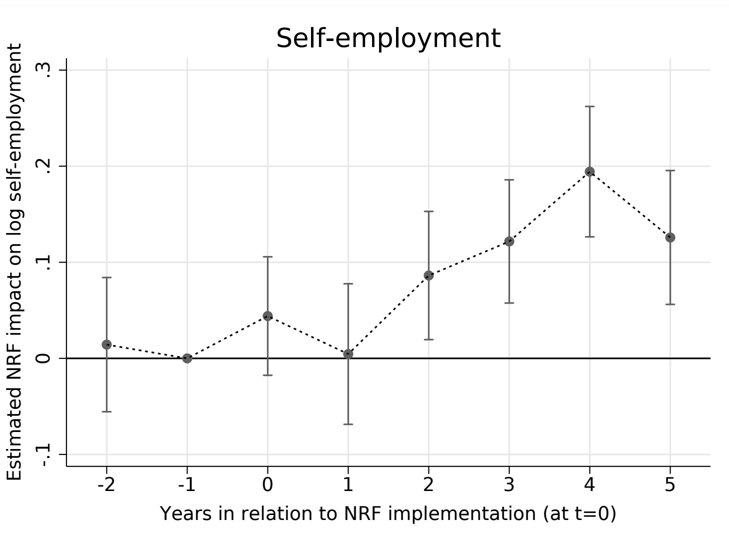

Figure C2: Lead and lag effects of NRF on self-employment using multiple imputation.

D Robustness to the New Deal for Communities

Thirty-seven out of the 88 local authority districts that received Neighbourhood Renewal Funds also received funding from the New Deal for Communities (NDC) program, which targeted small neighborhoods within local authority districts. In this section, we check the robustness of our main results by accounting for NRF areas that also received NDC funding. Equation D.1 accomplishes this by adding the dummy variable NDC_{it} equal to one for NRF treated areas that also received funding from the New Deal for Communities Program after 2001, and zero otherwise:

$$y_{it} = \alpha_i + \delta_t + \beta D_{it} + \rho NDC_{it} + \epsilon_{it}. \tag{D.1}$$

The results from this robustness check are reported in Table D1. The only coefficient that is affected substantially by including the NDC variable is the coefficient on employees. The ρ coefficient on employees suggests that some of the employment growth occurring during the treatment period occurred in NRF treated areas that also received funds from the NDC program. This is perhaps unsurprising, given that the double-treated areas from this specification encompass much of metropolitan London, Manchester, and Birmingham. Furthermore, since the treated areas from the NDC program are much smaller than the local authority districts targeted in the NRF, it is unlikely that the effects observed in the main section of the paper are the result of the NDC alone. However, it is helpful to note that both social spending programs likely had a positive compound effect on employment growth in the 37 areas that received funding from both NRF and NDC.

Table D1: Estimated difference-in-differences treatment effects using (D.1)

	Jobs	Employees	Self-Emp	Wages
D	-0.018	0.013	0.080	-0.015
	(0.103)	(0.162)	(0.008)	(0.032)
NDC	0.019	0.028	0.004	0.007
	(0.180)	(0.026)	(0.921)	(0.476)
N	3520	3168	1647	2760
R^2	0.229	0.205	0.114	0.839

p-values in parentheses

E Population estimates and results excluding London

As discussed in the main text, detailed population estimates are no longer available for English districts as they existed between 2000 and 2008, and while there are mid-year population estimates in the Alonso et al. (2019) replication files for these districts, the sample only starts in 2000. To produce approximate population figures, therefore, we utilise mid-year population estimates for the post-2009 (pre-2015) district boundaries from NOMIS. Out of the 88 NRF treatment districts, seven ceased to exist following the 2009 local government restructuring: Derwentside, Easington, Kerrier, Penwith, Sedgefield, Wansbeck, and Wear Valley. During this restructuring, Derwentside, Easington, Sedgefield and Wear Valley became part of the new Durham County, along with Durham City, Teesdale and Chester-le-Street. Kerrier and Penwith became part of the new Cornwall, along with Caradon, Carrick, North Cornwall and Restormel, while Wansbeck became part of the new Northumberland, along with Alnwick, Berwick-upon-Tweed, Blyth Valley, Castle Morpeth and Tynedale (see e.g., Calvert Jump, 2020, and the references therein for a discussion of local government restructuring in Britain).

Given these changes, we code County Durham as treated and Cornwall and Northumberland as untreated in our population dataset at post-2009 (pre-2015) district boundaries. Running the generalized difference-in-differences model on these data produces figure E2, from which it is clear that the total populations of NRF treatment areas were declining relative to non-treatment areas both before and after 2002, with no obvious treatment effect. The positive employment effects discussed in the main body of text were not, therefore, simply because the populations of those areas started to increase in the early 2000s.

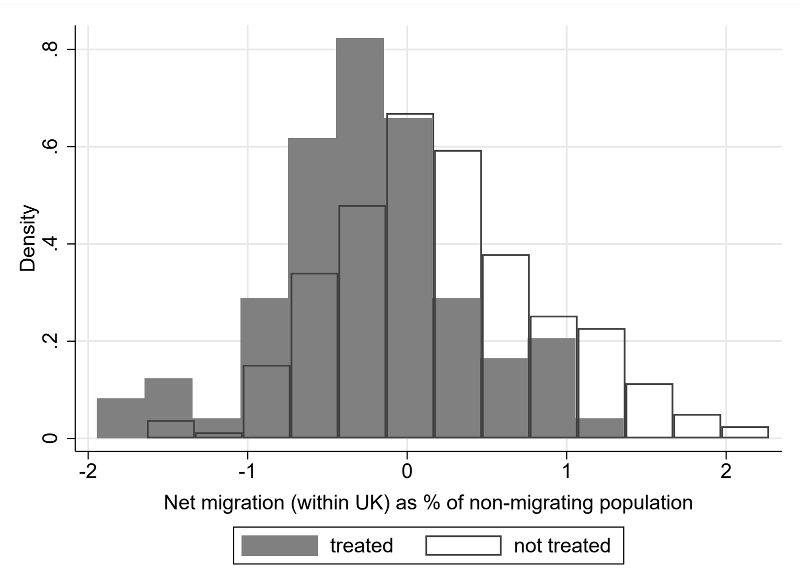

Figure E1: Net internal migration as a percentage of the non-migrating population by local authority district between 2000 and 2001, using 2001 census data.

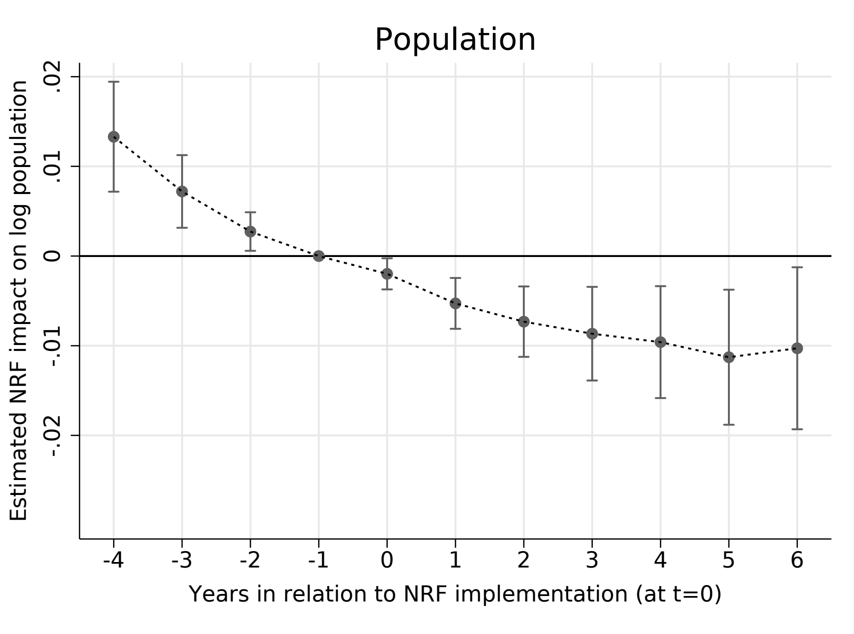

Figure E2: Lead and lag effects of NRF on district-level population.

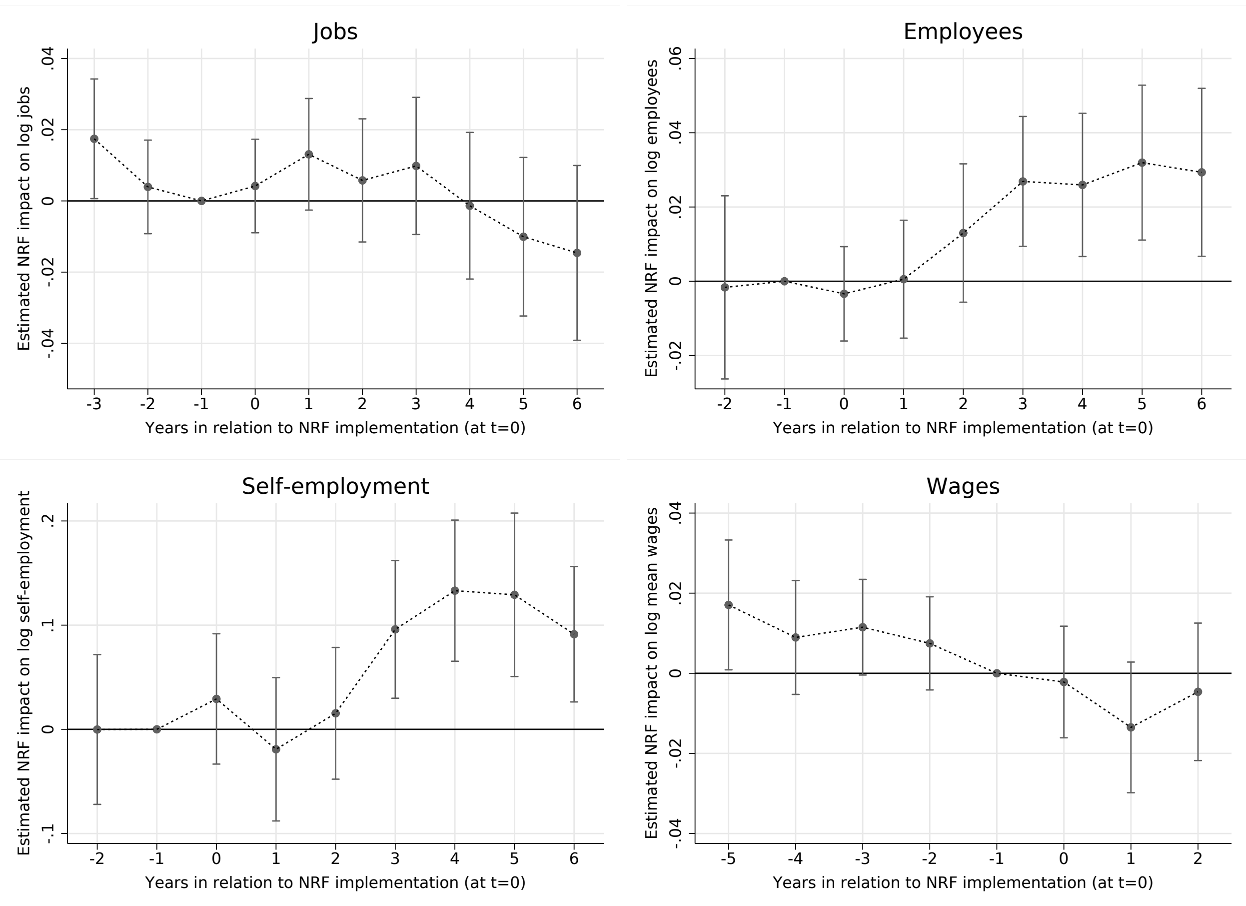

Figure E3: Lead and lag effects of the NRF on labor market variables, excluding the 32 districts in London.

F Results using population denominators

This exercise involves re-estimating the generalized difference-in-differences model using employment indicators divided by total district-level population. As discussed in the main text, we only have population figures at the correct geography from the Alonso et al. (2019) replication files between 2000 and 2007, and these are not working age population figures. Nevertheless, the results displayed in figure F1 are broadly in line with our main results, despite the fact that the population figures themselves suffer from pre-trends, as discussed in appendix E. Given this last observation, we consider the results reported in the main body of text to be more reliable.

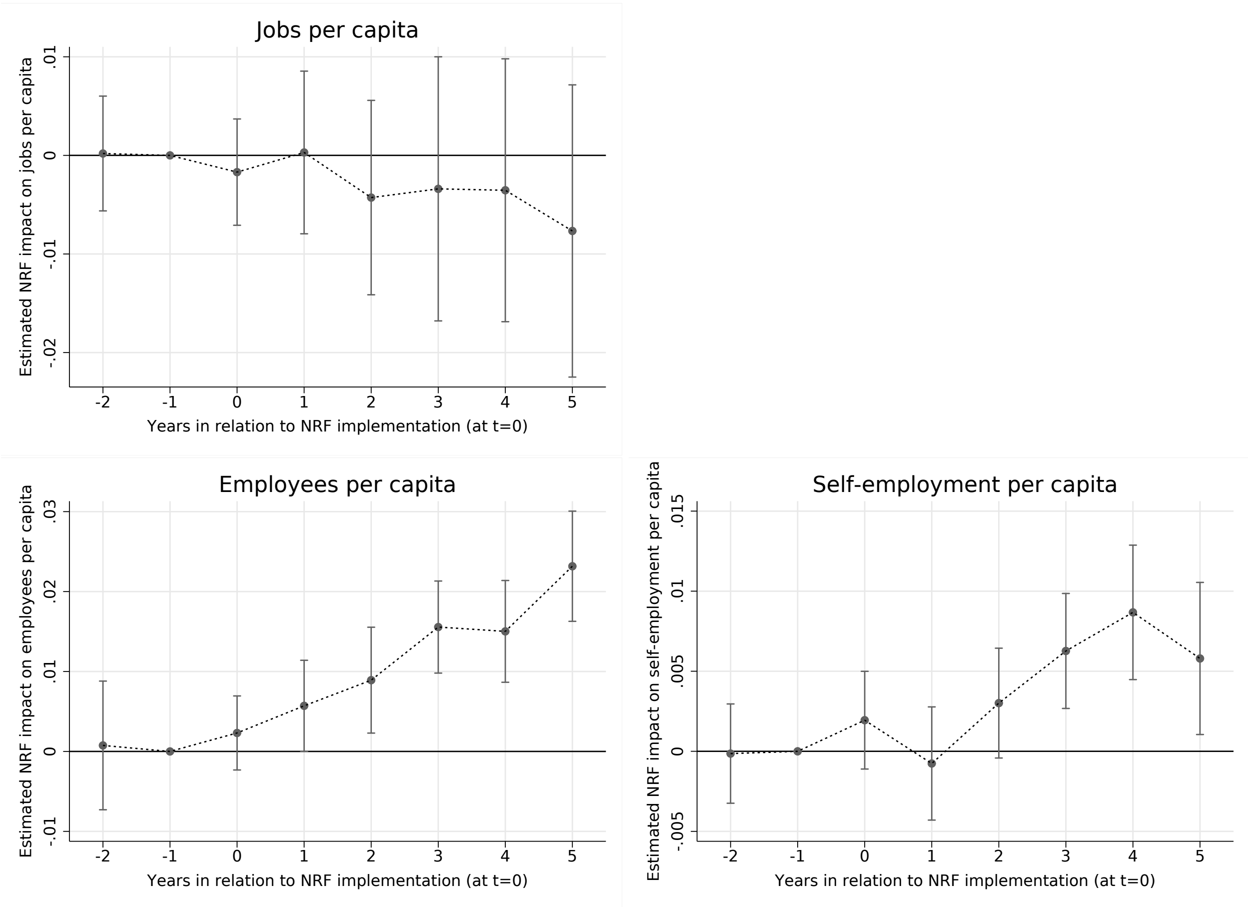

Figure F1: Lead and lag effects of the NRF on labor market variables divided by population.

G Robustness to regional trends and excluding contiguous areas

Our final robustness exercise involves assessing the robustness of our results to region-specific trends and the exclusion of contiguous non-treated areas. Figure G1 displays the results of the basic difference-in-differences model in (1) in which the year dummies δ_t are replaced by a set of region-specific year dummies (where the English regions are the South West, the South East, London, the West Midlands, the East Midlands, the East of England, the North West, Yorkshire and Humberside, and the North East).

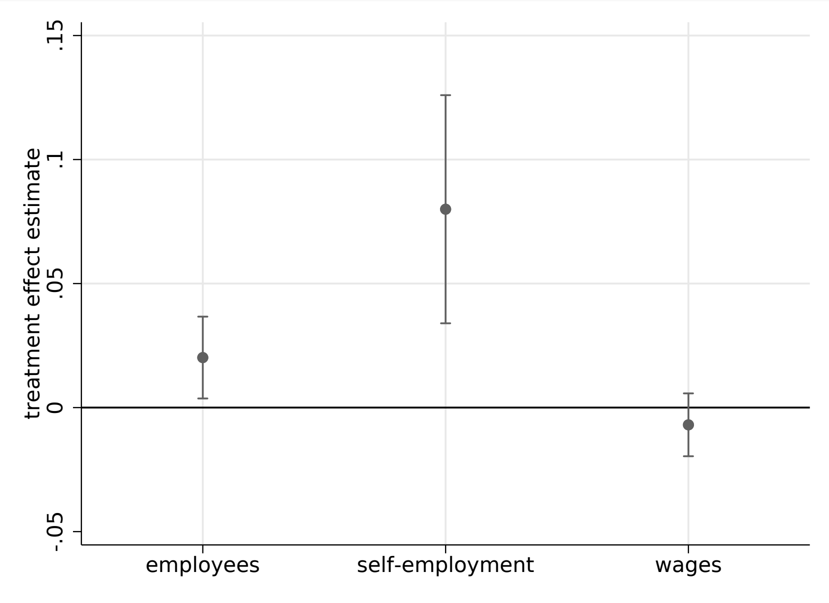

Figure G1: Point estimates and 95% confidence intervals for the simple diff-in-diff models with a shared national trend replaced with region-specific trends.

Figure G2 excludes non-treated districts bordering treated districts, where the excluded districts are highlighted in grey in figure G3.

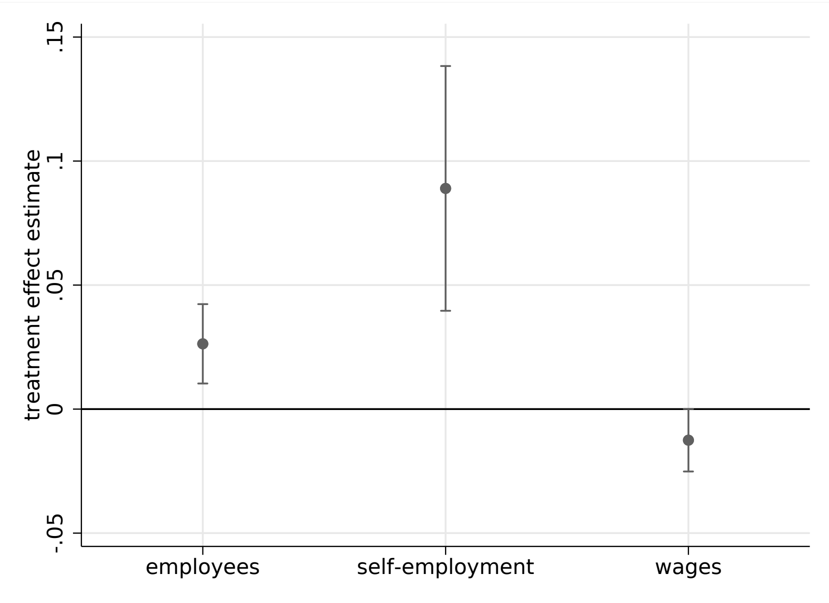

Figure G2: Point estimates and 95% confidence intervals for the simple diff-in-diff models with non-treated contiguous units removed from the control group (i.e., regions highlighted in grey in figure G3).

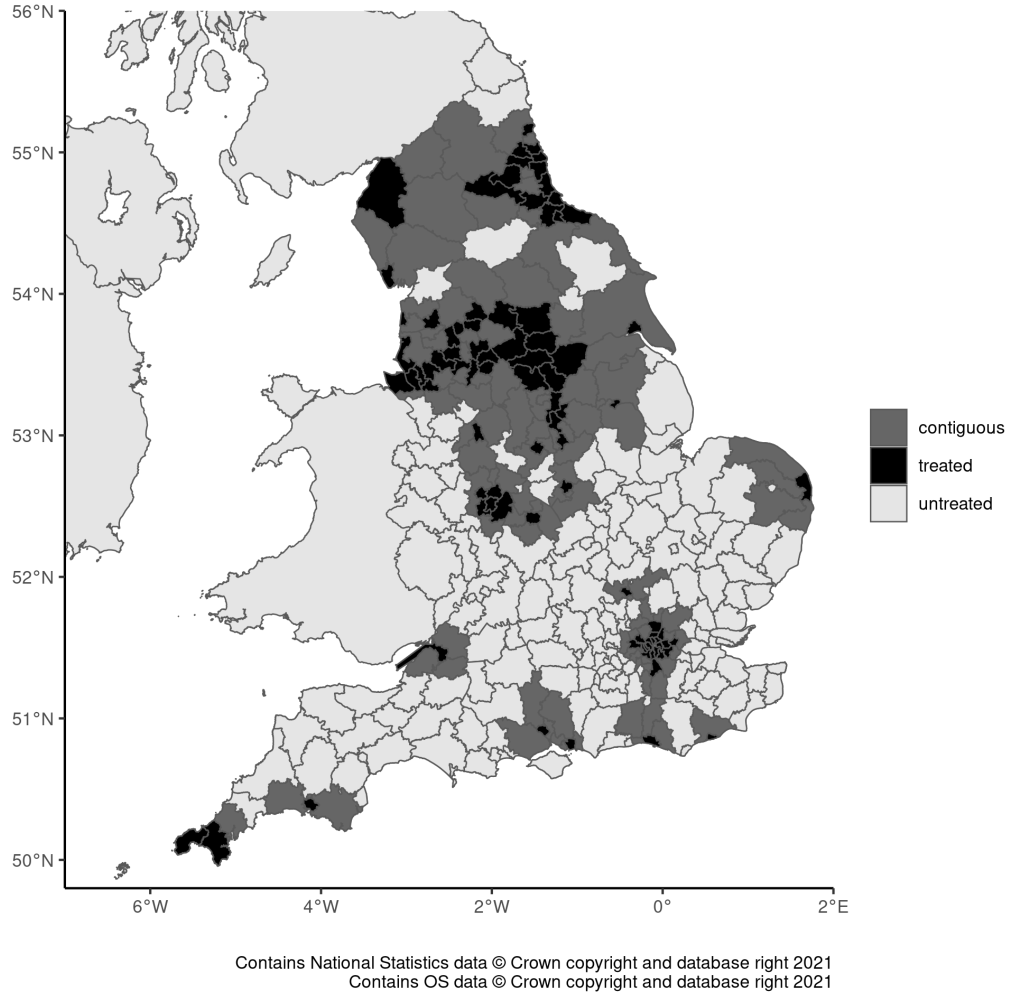

Figure G3: Map of England (with surrounding geography included for reference), with Neighbourhood Renewal Fund treatment areas highlighted in black and areas contiguous to treated areas (i.e., non-treated areas that receive a positive spatial weight in the spatial regressions) highlighted in grey.

Our main results are robust to this exercise, providing further reassurance that spatial spillovers are not biasing our results. Moreover, as displayed in figure G3, one can see that the bulk of the areas that are not either treated or contiguous to treated areas are below the North-South divide, which traditionally bisects the Severn and either the Humber estuary or the Wash, i.e., Lincolnshire, see Green (1988). In other words, not only did the NRF increase employment in treated areas relatively to the average non-treated area, it also increased employment in treated areas relative to the traditionally prosperous areas below the North-South divide (and outside of the pockets of deprivation within the South, the majority of which were treated by the NRF).

H Estimating the Policy's Value for Money

In this section, we outline a simple value for money analysis of the NRF's worklessness expenditures. Using conservative benefit estimates from our model, we find that the NRF provided good value for money.

On the cost side, Dept. for Com. and Local Gov't (2010) estimates that 13 percent of the core £2.4 billion NRF expenditures (£312 million) was devoted to worklessness. Additionally, £35 of other public funds were spent for every £100 of NRF expenditures (£109 million). Therefore, £421 million of government funds were devoted to worklesness programs through the NRF.

On the benefit side, we estimate that the NRF reduced the number of out-of-work benefits claimants by between 1,000 and 2,000 persons per treated district. Given that there were 88 NRF areas, this implies that the policy collectively reduced worklessness by at least 88,000 claimants.

Using the conservative benefit estimate of 88,000 claimants and the £421 million program cost, we calculate a cost per claimant reduction of no more than £4,784. Dept. for Com. and Local Gov't (2010) suggests an annual net benefit to the exchequer of a claimant reduction of £8,000-£9,000 per person. Therefore, our results indicate that the value of the reduction in claimants likely far exceeded (approximately doubled) the NRF's costs targeted at worklessness. This is consistent with Hollenbeck (2008), which finds that U.S. state workforce training grants resulted in jobs being created or retained at a public cost of less than $10,000 per job.