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The Labor Market Effects of Place-Based Policies: Evidence from England's Neighbourhood Renewal Fund

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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 eighty-eight 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, health care 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 and out-of-work benefit claimants. 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. Europe and the United States have implemented various forms of neighborhood renewal programs (along with enterprise zones, empowerment zones, and industrial cluster policies) since the end of the Second World War. A notable example is the National Strategy for Neighbourhood Renewal, which the New Labour Government introduced in the United Kingdom 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 eighty-eight 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 nonprofit) 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.

We largely follow the methodology of Alonso et al. (2019), by estimating the impact of the NRF on labor market outcomes using data on 345 local areas in England between 2000 and 2008. We use a difference-in-differences approach to model the impact of the NRF on employment and benefit claimant outcomes and a continuous treatment strategy to account for heterogeneous treatment intensity. Our major contributions are to provide the first evaluation of the effects of the NRF on worklessness since its official evaluation in 2010, the first evaluation of the effects of the NRF on employment and job creation, and the first evaluation which controls for geographic spillovers, differential trends, and confounding policies. Our findings suggest that the NRF had a significant impact on local employment and unemployment, implying 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 (SEU) was asked to produce a report on neighborhood problems during Labour's first year in office (Lupton & Power, 2005). While the SEU worked on this report, a variety of area-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 thirty-nine of around 4,000 small neighborhoods identified as deprived by the SEU (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 among local governments, public authorities, and civil society organizations were set up in eighty-eight 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 districts funded by the NRF are displayed in figure 1. These districts tended to be

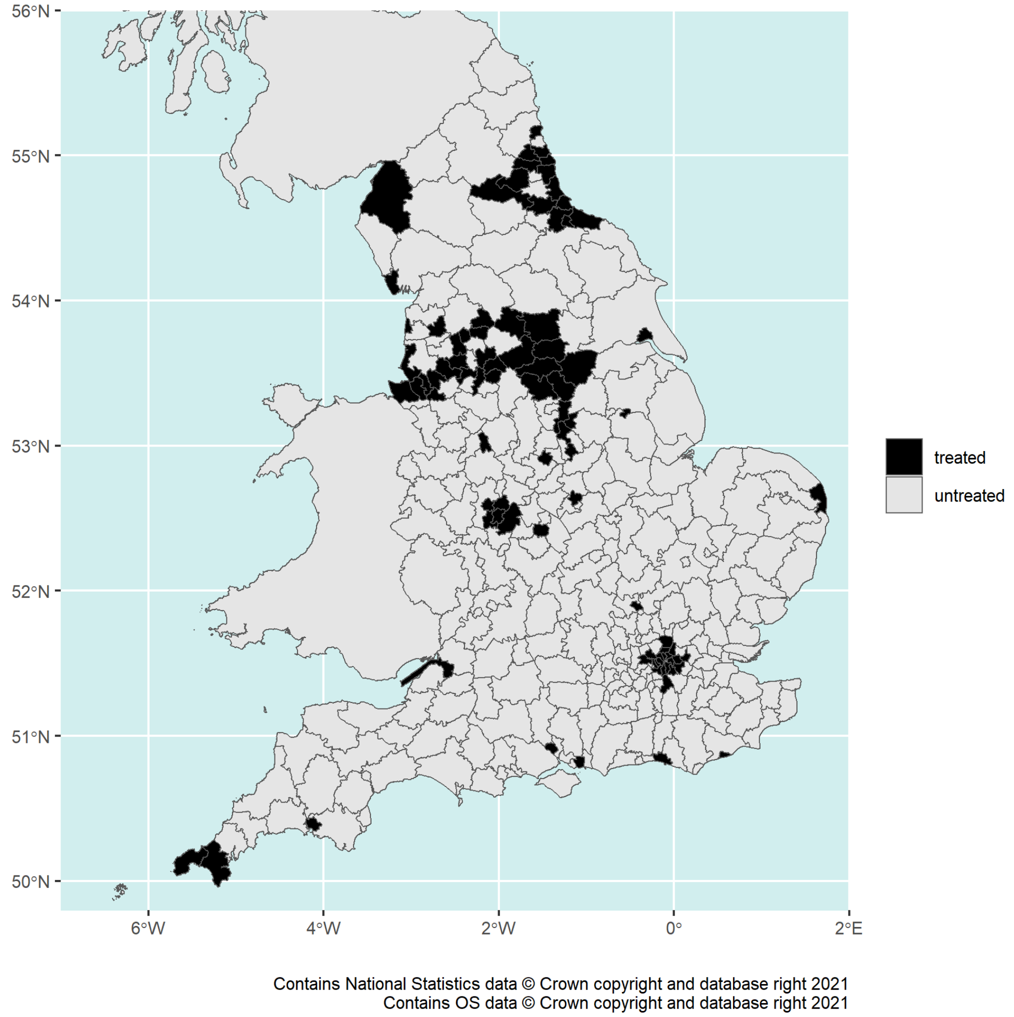

Figure 1: Map of England (with Wales and Scotland included for reference), with Neighbourhood Renewal Fund treatment areas highlighted in black.

in or around major cities, which is very different from the present government's focus on 'levelling up' towns and peripheral areas (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). Moreover, the index of multiple deprivation, commissioned by the Office of the Deputy Prime Minister, quite clearly identified inner-city areas as some of the most deprived in the country, and this is 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 child care support, and an additional £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).

The Working Neighbourhoods Fund replaced the NRF in 2008 (Dept. for Com. and Local Gov't, 2015). 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.

3 Data

We use data on several different measures of labor market activity for the 345 local authority districts that existed in England between 2000 and 2008, eighty-eight of which received NRF funding (of which seven only received 'transitional funding'—see Social Exclusion Unit, 2001). Specifically, we use data on job counts, total employees, total self-employment, total claimants for out-of-work benefits, and claimants for out-of-work benefits across four separate age groups (17 and younger, 19 and younger, 24 and younger, and 25 and older).

All variables are observed by district, which are administrative units with an average population of around 140,000 during the sample period. The treatment indicator, treatment intensity, 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 job count data are from the NOMIS 'Annual Business Inquiry' dataset and are defined on a workplace basis. These data are observed as of December. Given the fiscal year

Table 1: Pre-treatment summary statistics

	Nonparticipants			Participants		
	Mean	25th pct	75th pct	Mean	25th pct	75th pct
Jobs	421.2	336.7	471.1	416.2	331.4	445.8
Employees	405.5	378.6	434.5	366.3	344.9	392
Self-employment	67.4	53.9	78.5	42.5	31.8	47.9
Claimants	11.1	7.2	14.2	25.5	20.5	29.5
Claimants (17 & under)	0.1	0	0.2	0.4	0.2	0.5
Claimants (19 & under)	1.1	0.6	1.4	2.8	2.2	3.4
Claimants (24 & under)	2.7	1.6	3.6	6.9	5.3	7.9
Claimants (25 & older)	8.3	5.5	10.6	18.6	14.3	21.9

Notes: Means, 25th and 75th percentiles. All variables are per 1,000 population as of 2001.

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.

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 observations.

Finally, the headline claimant counts are from the NOMIS 'Jobseeker's Allowance with rates and proportions' dataset, 1998-2008, and the claimant counts by age group are from the NOMIS 'Jobseeker's Allowance by age and duration' dataset, 1998-2008. We use claimant count observations as of March, which correspond to end-of-fiscal-year stocks. So, for example, our 2002 claimant count observations correspond to the end of the 2002 fiscal year. Table 1 reports summary statistics for our labor market variables in the year prior to NRF implementation.

4 Methods

We use two empirical specifications: a difference-in-differences model and a continuous treatment variable model. Our simplest difference-in-differences specification includes area and time fixed effects to estimate the impact of 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 jobs, employees, self-employed persons, or benefit claimants in year t, $t = 2000, ..., 2008$, and local area district i, $i = 1, ..., 345$. 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. Since 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. In addition, we report results from a generalized (or event-study) difference-in-differences model that captures lead and lag effects of the NRF. Lastly, we employ a continuous treatment variable approach:

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

In (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.

5 Results

The results from our simple difference-in-differences models are presented in table 2. If the parallel trends condition is met, our results suggest that there was a positive treatment effect of the NRF on treated areas. Specifically, while the treatment effect on local job counts was not statistically significant from zero over the study period, our results suggest that the NRF was associated with a 2.5 percent increase in employees, a 9.8 percent increase in self-employment, and a 5.6 percent decrease in claimants of out-of-work benefits. The latter implies a decrease of around 300 claimants in the median district, which is somewhat lower than the estimate of 750 in Dept. for Com. and Local Gov't (2010).

The results from our continuous treatment variable model are presented in table 3 and are consistent with the results in table 2. The coefficients indicate the effect of a £1 per capita increase in NRF spending per locality over the treatment period. Again, the estimated effect on local jobs is not statistically different from zero. On the other hand, £10 per capita of NRF funding is associated with a 1 percent increase in employees, a 4 percent increase in self-employment, and a 2 percent decrease in total out-of-work benefit claimants. The same amount of money is associated with a 10 percent decrease in claimants of those ages 17 and younger, a 4 percent decrease in claimants of those ages 19 and younger, a 2 percent

Table 2: Estimated difference-in-differences treatment effects using (1)

	Jobs	Employees	Self-Emp	Claimants	Claimants 17&under	Claimants 19&under	Claimants 24&under	Claimants 25&older
$\hat{\beta}$	-0.010	0.025	0.098	-0.056	-0.278	-0.104	-0.038	-0.071
	(0.226)	(0.001)	(0.000)	(0.003)	(0.000)	(0.000)	(0.039)	(0.001)
N	3520	3168	2706	3872	3545	3872	3872	3872
R^2	0.228	0.203	0.057	0.624	0.094	0.278	0.425	0.681

Table 3: Estimated continuous treatment variable effects using (2)

	Jobs	Employees	Self-Emp	Claimants	Claimants 17&under	Claimants 19&under	Claimants 24&under	Claimants 25&older
$\hat{\gamma}$	0.000	0.001	0.004	-0.002	-0.010	-0.004	-0.002	-0.002
	(0.766)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)
N	3168	2816	2354	3520	3238	3520	3520	3520
R^2	0.210	0.200	0.059	0.616	0.048	0.305	0.453	0.665

Note: p-values using standard errors clustered at district-level in parentheses in tables 2 and 3.

decrease in claimants of those ages 24 and younger, and a 2 percent decrease in claimants of those ages 25 and older.

There are two interesting takeaways from the results in tables 2 and 3. First, Cowen et al. (2008) observe that the levels of interest shown in self-employment within targeted groups—one of our larger effect sizes—were an unexpected benefit of the NRF (Cowen et al., 2008, pp.44). Second, while employees, self-employment, and claimants are measured on a residence basis, our jobs variable is measured on a workplace basis. In other words, our results suggest that residents of NRF treatment areas were more likely to be employed due to the program, but they were finding jobs in nontreatment areas. Both of these observations are consistent with the NRF primarily targeting the supply-side of local labor markets. In addition, the latter suggests that the possibility of spatial spillovers, which are plausible due to the small size of the treated areas, may well be a material issue.

When claimants are disaggregated by age group, the NRF program is associated with a faster reduction for younger age groups. Specifically, the NRF is associated with a 27.8 percent decrease in claimants for those 17 and younger, a 10.4 percent decrease in claimants for those 19 and younger, a 3.8 percent decrease in claimants for those 24 and younger, and a 7.1 percent decrease in claimants for those 25 and older. To put these results into context, the median numbers of claimants in NRF treated areas in 2001 were 60, 533, 1,280, and 3,813 within those populations of 17 and under, 19 and under, 24 and under, and 25 and older, respectively. So in absolute terms, from these estimates, we would expect to see a reduction of just under 400 claimants in the median NRF district, which is broadly consistent with the results for total claimants provided above.

In comparison to the claimant numbers, the median number of employees in NRF treated areas in 2001 was 78,000, and the median number of self-employed workers was 10,000, so table 2 implies that the total increase in employment in the median NRF district was just under 3,000. This effect seems somewhat large compared to a median reduction of less than 400 claimants, suggesting that the parallel trends assumption might not be met in at least one of our models. To investigate this possibility, figures 2 and 3 present the results from generalized difference-in-differences models using the same dependent variables as in table 2, in which $t = 0$ corresponds to fiscal year 2002. Differences in data availability mean that the number of pre-treatment periods varies by dependent variable, but there are at least two pre-treatment periods in each case, and we constrain the effect on the first lead to equal zero. This means that the effect sizes in figures 2 and 3 are difference-in-differences relative to fiscal year 2001.

The parallel trends assumption appears to be satisfied for each dependent variable other than the claimants variables. With the exception of claimants 17 and under, numbers claiming out-of-work benefits appear to have been increasing in the NRF treatment areas relative to nontreatment areas prior to 2002. NRF treatment areas were selected based on their rank position in the Index of Multiple Deprivation, and thus the treatment areas were not selected *because* their local conditions were deteriorating. Nevertheless, the fact that the total numbers claiming out-of-work benefits appear to have been increasing in the NRF treatment areas relative to nontreatment areas prior to treatment suggests that our claimants results in table 2 are biased toward zero. This is consistent with the fact that our point estimates in table 2 suggest a considerably larger increase in employment than reduction in claimants of out-of-work benefits in treated areas.

While the results in tables 2 and 3 and figures 2 and 3 suggest a positive treatment effect of the NRF on labor market outcomes, they also suggest two sources of bias: spatial spillovers for each dependent variable and differences in pre-treatment trends for out-of-work benefits claimants. Thus, in the next section, we examine the robustness of our results using a spatial difference-in-differences model and a trend sensitivity analysis.

6 Robustness to spatial spillovers and differential trends

6.1 Robustness check 1: spatial spillovers

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 discussion above, it seems likely that the NRF was subject to spatial spillovers, and thus it is possible that the results in table 2 and figures 2 and 3 are biased. In order to assess the extent of spatial spillovers across local

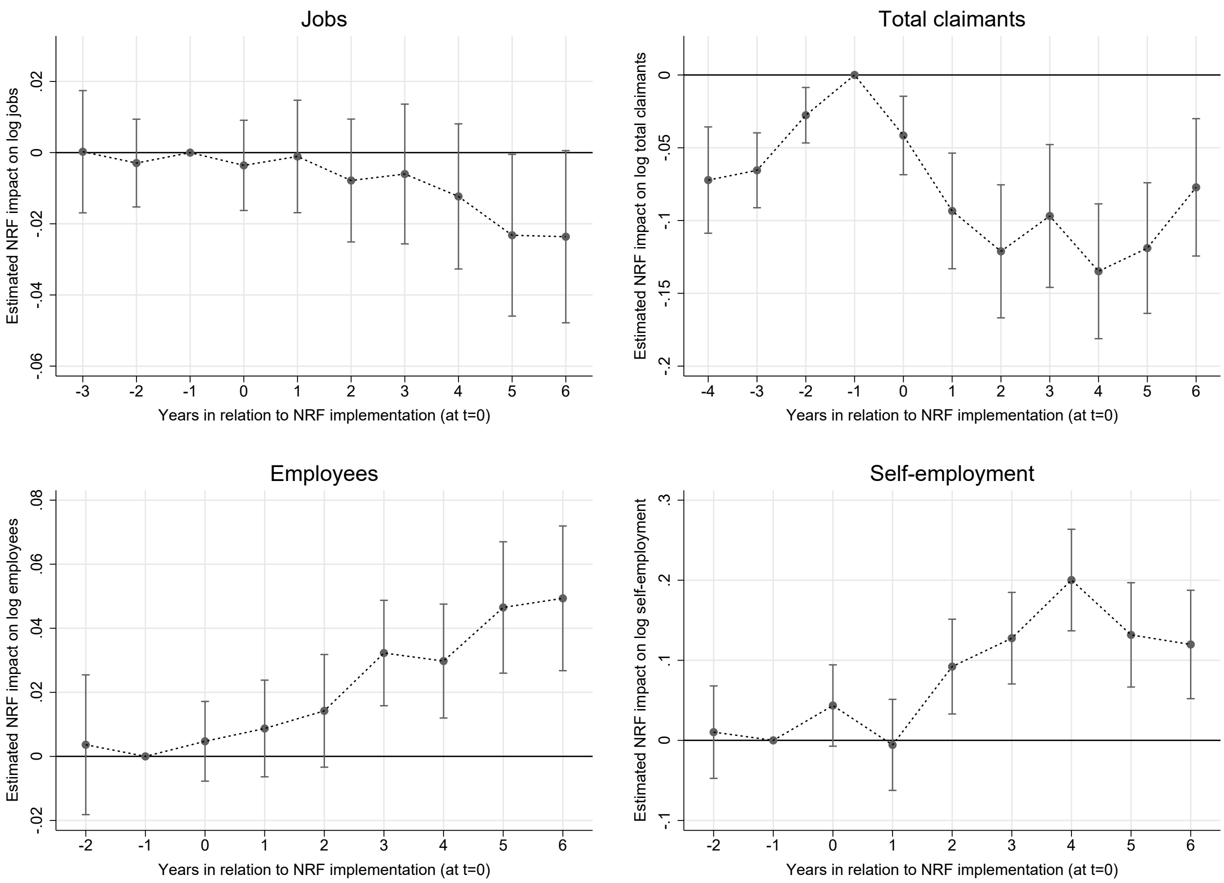

Figure 2: Lead and lag effects of the NRF on labor market variables, using model (1).

authority districts, we report the results of a difference-in-differences model that includes a spatial lag for treatment, following Delgado & Florax (2015). The following model is estimated:

$$y_{it} = \alpha_i + \delta_t + \phi D_{it} + \theta \sum_{j=1}^{345} w_{ij} D_{jt} + \epsilon_{it}, \tag{3}$$

in which w_{ij} is the (i, j)th element of the weighting matrix W that connects England's 345 local authority districts. The results in table 4 are estimated for a row-normalized spatial contiguity matrix that indicates the degree to which each of the local authority districts share a border with an NRF treated area. In other words, each $w_{i.}$ represents the degree to which a district's neighbors are treated.[1] 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 nontreated areas. The direct treatment effect in (3) 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.

Table 4 reports the results from the spatial difference-in-differences models for each of the

[1]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 $3/5 = 0.6$.

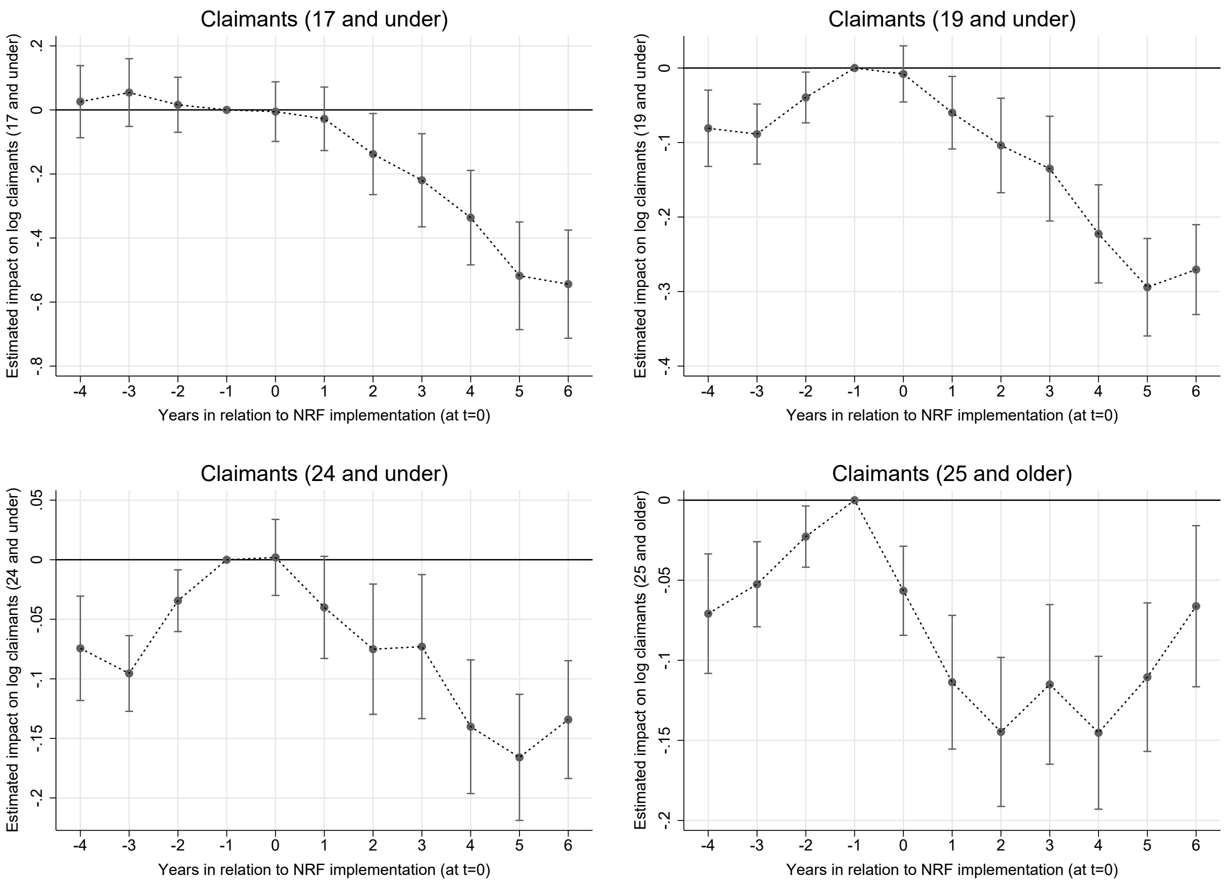

Figure 3: Lead and lag effects of the NRF on claimants by age group, using model (1).

eight dependent variables. The coefficients indicate that accounting for spillover effects, there are significant direct treatment effects of a 1.7 percent increase in employees, a 6.9 percent increase in self-employment, a 19.4 percent decrease in claimants for those 17 and younger, and a 4.9 percent decrease in claimants for those 19 and younger. There are no significant indirect treatment effects for neighboring local authority districts associated with jobs, employees, or self-employment. However, there are significant indirect treatment effects for each of the claimants variables: a 13.6 percent decrease for total out-of-work benefit claimants, a 22.5 percent decrease in claimants for those 17 and younger, a 17.3 percent decrease in claimants for those 19 and younger, an 11.4 percent decrease in claimants for those 24 and younger, and a 14.9 percent decrease in claimants for those 25 and older.

According to these results, there were significant direct and indirect treatment effects on claimants for the younger groups of claimants (17 and younger, 19 and younger), but only significant indirect treatment effects for the older groups and total claimants. Figures 4 and 5 present the lead and lag effects of the direct and indirect treatment effects for each of our labor market variables, which bring out the main results more clearly. There were no obvious spillover effects of the NRF on employment, and to the extent to which jobs increased, they increased in neighboring areas to treated areas. Again, this suggests that

Table 4: Estimated direct and indirect treatment effects using (3)

	Jobs	Employees	Self-Emp	Claimants	Claimants 17&under	Claimants 19&under	Claimants 24&under	Claimants 25&older
$\hat{\phi}$	-0.012	0.017	0.069	-0.009	-0.194	-0.049	-0.004	-0.019
	(0.221)	(0.042)	(0.010)	(0.709)	(0.001)	(0.055)	(0.875)	(0.456)
$\hat{\theta}$	0.007	0.012	0.043	-0.136	-0.225	-0.173	-0.114	-0.149
	(0.678)	(0.340)	(0.330)	(0.000)	(0.012)	(0.000)	(0.003)	(0.000)
N	3471	3119	2657	3823	3496	3823	3823	3823
R^2	0.230	0.198	0.051	0.629	0.091	0.289	0.433	0.684

Note: p-values using standard errors clustered at district-level in parentheses.

residents of NRF treated areas were finding jobs outside of their immediate neighborhoods. On the other hand, there appear to be significant spillover effects on claimants, which may be due to the fact that a large number of NRF treatment districts were very small districts in inner cities, in which the closest job centre to a claimant is not necessarily within their district. Again, there is evidence of pre-trends in the claimants plots, which we discuss in the next section.

6.2 Robustness check 2: sensitivity to differential trends

We have already noted that the evidence for pre-trends in our claimants variables suggest that our point estimates of the NRF treatment effects on out-of-work benefits claimants are likely to be biased toward zero. This is supported by the rather large increases in employment suggested by our models in log employees and self-employment, and the rather low absolute increases in out-of-work benefits claimants suggested by our log claimants models. To assess the magnitude of the pre-trend bias in our claimants results, we use the sensitivity analysis recently developed by Rambachan & Roth (2020).

Following Rambachan & Roth (2020), we can decompose the eleven-vector of point estimates in the total claimants panel of figure 2 as follows:

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

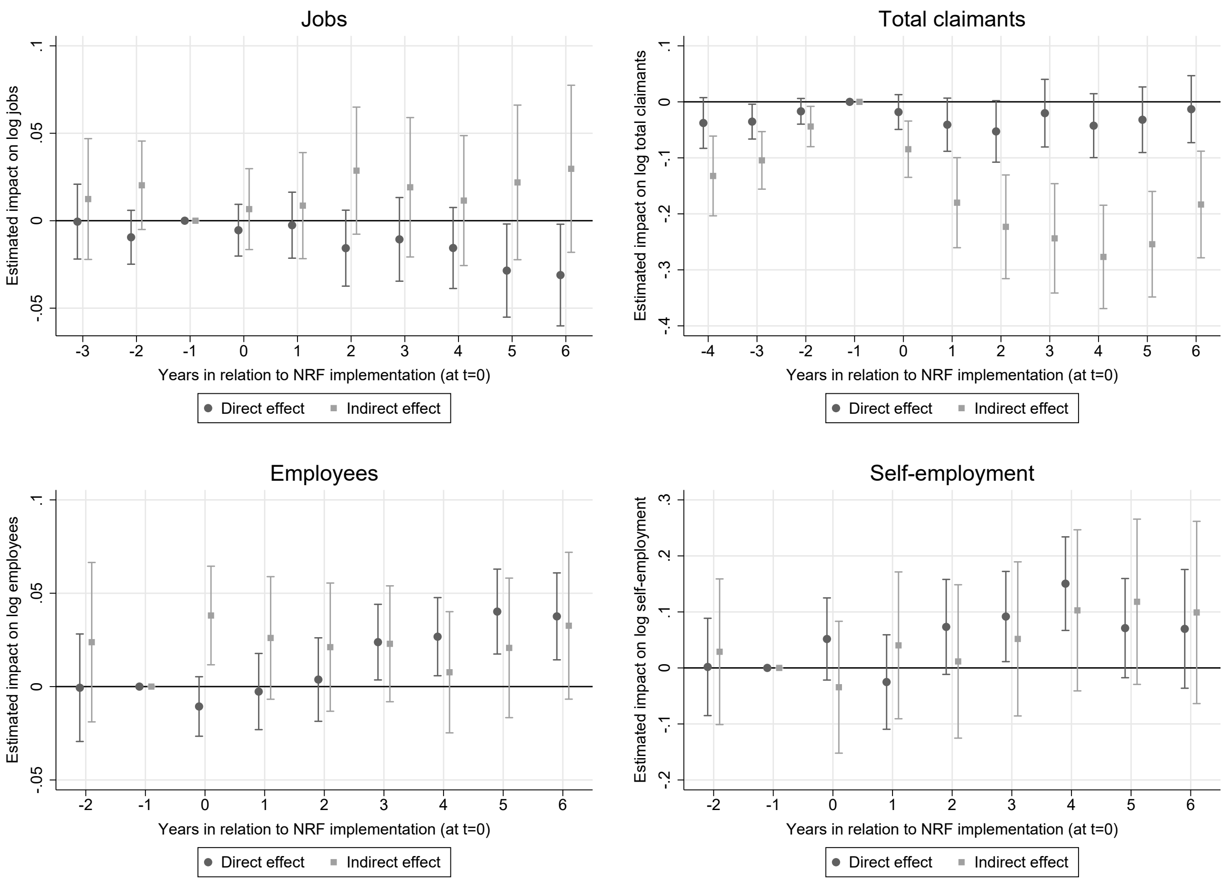

Figure 4: Dynamic direct and indirect effects of the NRF, using model (3).

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 (ibid., pp.7). 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 is convincing evidence for the standard identification assumption. In cases like the total claimants panel of figure 2, the evidence is obviously unconvincing. In these cases, Rambachan & Roth (2020) 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

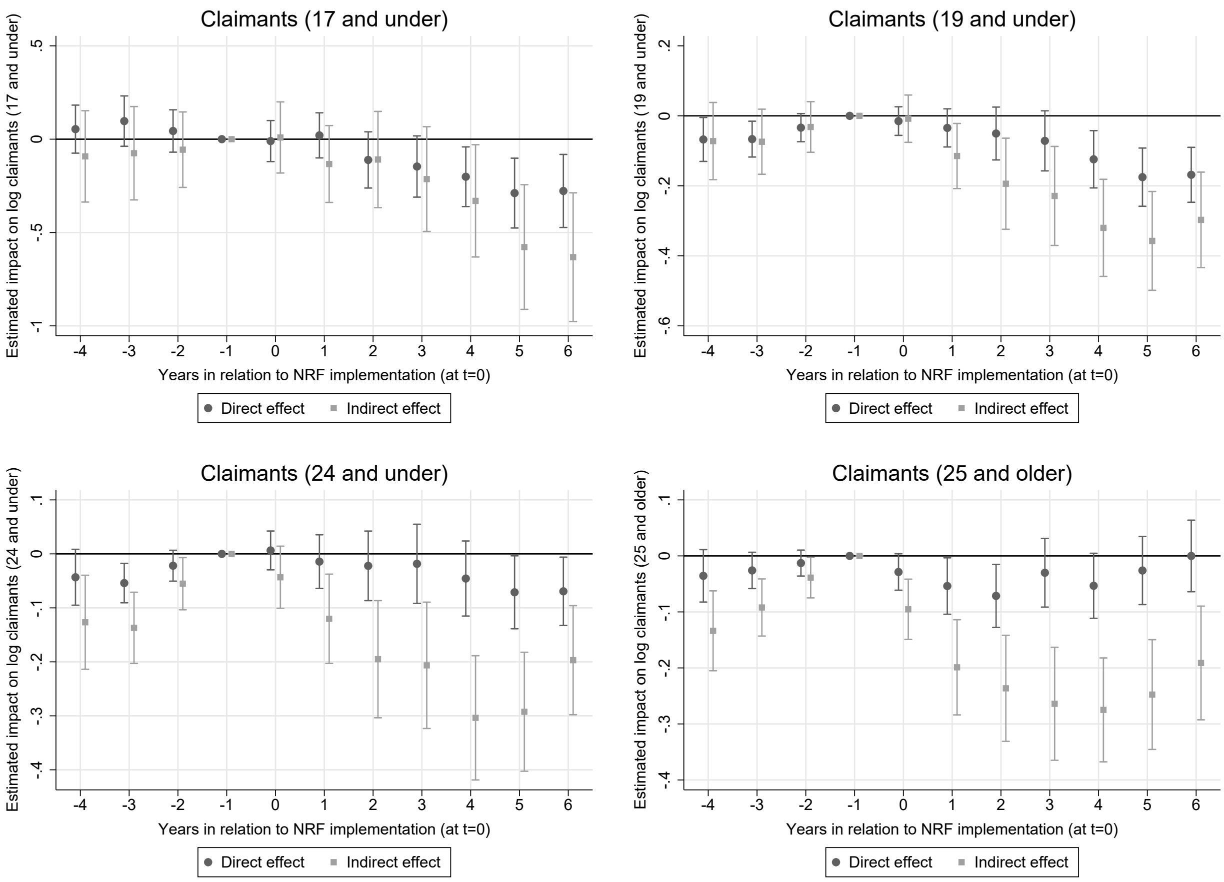

Figure 5: Dynamic direct and indirect effects of the NRF, using model (3).

compared to their past values. Denoting the set of plausible trends by Δ, we assume that,

$$\Delta = \{\delta : |(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})| \leq 0.02, \forall t\}, \tag{5}$$

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 (2020).

The results of this sensitivity analysis for log total claimants is plotted in figure 6. The original OLS point estimates and 95 percent confidence intervals are in black, while the 95 percent confidence interval 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 percent and 40 percent. 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 much more consistent with the results on employment reported in section 5. Unlike our point estimate from the simple difference-in-differences

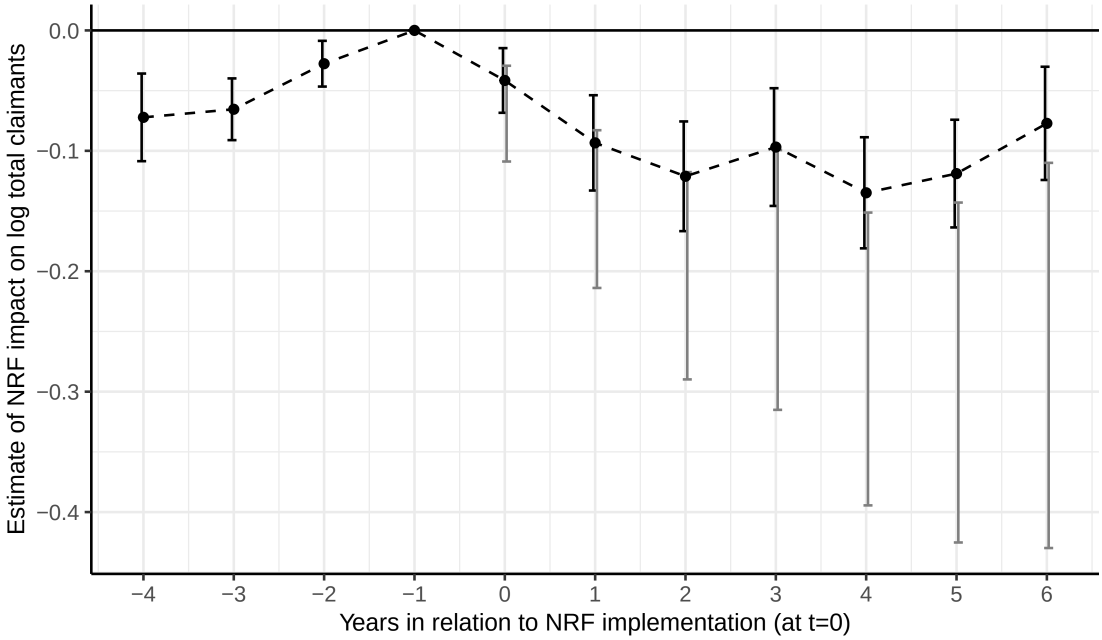

Figure 6: Sensitivity plot for total claimants.

model in table 2, an 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). Finally, the results of the same sensitivity exercise for claimants by age group is plotted in figure 7.

7 Discussion

Our examination of the Neighbourhood Renewal Fund suggests that the program increased employment in the median treated district by around 3,000 persons and reduced the number of out-of-work benefits claimants by at least 1,000 persons. The positive effects of the program appear to have been greater for younger people and involved at least some positive spillovers beyond the treated districts. These results are robust to controlling for differential trends and suggest 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). The results are also consistent with Alonso et al. (2019), who found that the Neighbourhood Renewal Fund 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.

While we have examined the robustness of our difference-in-differences results to deviations from the stable unit treatment value and parallel trend assumptions, there remains the possibility that we have not controlled for one or more confounding policies. One confounding policy is the New Deal for Communities, which operated in many of the same areas as the

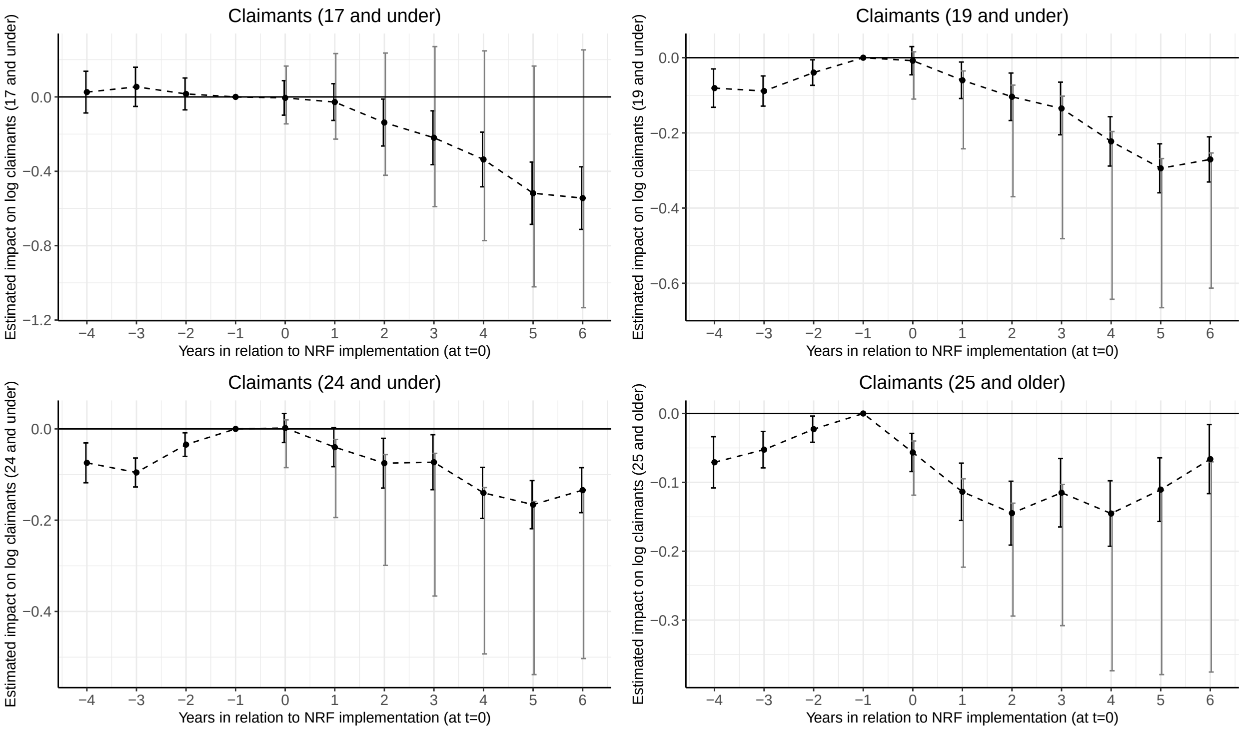

Figure 7: Sensitivity plots for claimants by age group.

Neighbourhood Renewal Fund, and also started disbursing funds in 2001 (Romero, 2009). In appendix A, therefore, we examine the robustness of our results to this policy by adding an extra treatment indicator for districts in receipt of New Deal for Communities (NDC) funds. Perhaps because the NDC operated at a much finer spatial scale than the Neighbourhood Renewal Fund, our results are robust to this exercise. Only the employees model is strongly affected by controlling for the NDC program, which suggests that much of the employment growth in the treatment period occurred in local authority districts that received funding from both programs.

Other 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, observe that the relative improvement in 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." However, confounding variables related to geography or industrial structure are either static or very slow-moving, which is exactly what difference-in-differences models are designed to control for. One might also be concerned with the NRF treating areas that, entirely by chance, happened to be developing or gentrifying 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. However, as displayed in figure 8, NRF areas were losing population by internal migration in the two years prior to NRF treatment, so we do not think that this is a convincing source of bias (and see, e.g., Lomax et al., 2014).

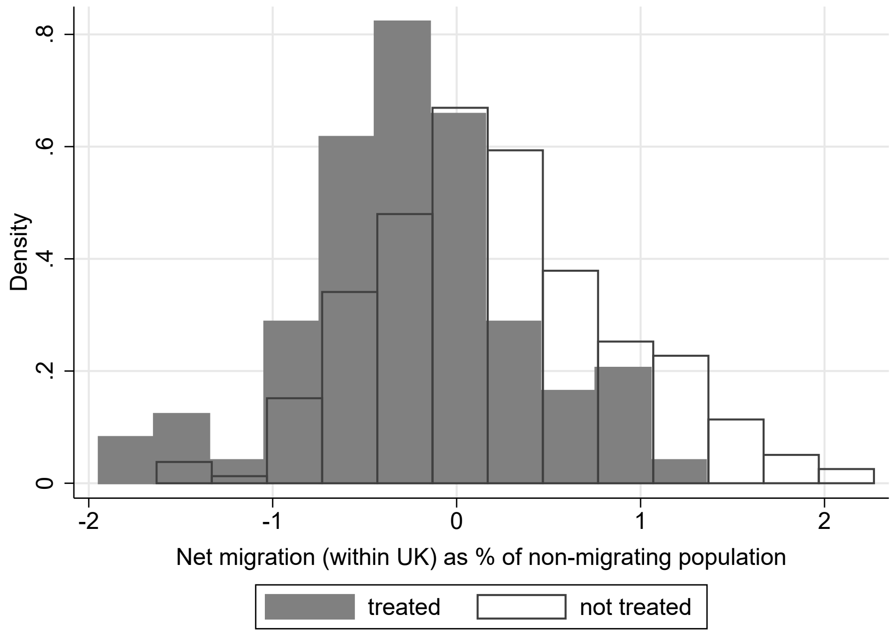

Figure 8: Net internal migration as a percentage of the non-migrating population by local authority district between 2000 and 2001, using 2001 census data.

A more significant issue is whether our estimates are biased away from zero because the regional effects of New Labour's national policies happened to be higher in NRF areas than elsewhere. It is worth bearing in mind 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). 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 pure confounding factor.

Our results suggest 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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Appendix

A Robustness to the New Deal for Communities

Thirty-seven out of the eighty-eight 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 estimates from Table 2 by accounting for NRF areas that also received NDC funding. Equation 6 accomplishes this by adding the dummy variable NDC_{it} equals one for NRF treated areas that also received funding from the New Deal for Communities Program after 2001 and zero otherwise:

$$y_{it} = \alpha_i + \delta_t + \beta D_{it} + \rho NDC_{it} + \epsilon_{it}. \tag{6}$$

The results from the robustness check are reported in Table A1. The only coefficient that was affected substantially by including the NDC variable is the coefficient on employees. The ρ coefficient on employees suggests that much 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 NDC alone. However, it is helpful to note that both social spending programs likely had a positive compound effect on employment growth in the thirty-seven areas that received funding from both NRF and NDC.

Table A1: Estimated difference-in-differences treatment effects using (6)

	Jobs	Employees	Self-Emp	Claimants	Claimants 17&under	Claimants 19&under	Claimants 24&under	Claimants 25&older
D	-0.018	0.013	0.083	-0.083	-0.259	-0.116	-0.056	-0.105
	(0.103)	(0.162)	(0.004)	(0.000)	(0.000)	(0.000)	(0.015)	(0.000)
NDC	0.019	0.028	0.030	0.063	-0.046	0.028	0.041	0.080
	(0.180)	(0.026)	(0.393)	(0.027)	(0.623)	(0.332)	(0.127)	(0.010)
N	3520	3168	2706	3872	3545	3872	3872	3872
R^2	0.229	0.205	0.057	0.626	0.094	0.278	0.426	0.682

p-values in parentheses