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Some causal effects of an industrial policy

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Abstract

We exploit changes in the area-specific eligibility criteria for a program to support jobs through investment subsidies. European rules determine whether an area is eligible for subsidies, and we construct instrumental variables for area eligibility based on parameters of these rule changes. Areas eligible for higher subsidies significantly increased jobs and reduced unemployment. A ten-percentage point increase in the maximum investment subsidy stimulates a 10% increase in manufacturing employment. This effect exists solely for small firms – large companies accept subsidies without increasing activity. There are positive effects on investment and employment for incumbent firms but not Total Factor Productivity.

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The Great Recession brought industrial policy back into fashion.[1] Governments around the world granted huge subsidies to private firms - most dramatically in financial services - but also in other sectors like autos. Business support policies are not new, however. Most governments offer subsidies that claim to protect jobs, reduce unemployment and foster productivity, particularly in disadvantaged geographical areas. For example, the US spends around $40 to $50bn per annum on local development policies (Moretti, 2011). Increasing geographical polarization has fostered social and political pressure for more place-specific policies. However, despite the ubiquity of such schemes, rigorous micro-econometric evaluations of the causal effects of these policies are rare. This is unfortunate given the mounting evidence on the persistent effect of negative economic shocks on local communities and the social and political implications of these pockets of disadvantage (e.g. Autor, Hansen and Dorn, 2016).

A major concern is that these programs might simply finance activities that firms would have undertaken anyway. The consensus among economists is that industrial policy usually fails, but the econometric evidence is surprisingly sparse. As Rodrik (2007) emphasizes, many of these policies are targeted on firms and industries that would be in difficulties even in the absence of the program, so naïve OLS estimates may miss any positive effects.[2]

We tackle the identification problem by exploiting a policy experiment that induced exogenous changes in the eligibility criteria governing whether plants in economically disadvantaged areas could receive investment subsidies from a major investment subsidy program in the UK. This program was called "Regional Selective Assistance" (RSA), but similar support programs exist in other European Union (EU) countries. Crucially for our identification strategy, there are rules governing the geographical areas that are eligible to receive aid from the UK government that are determined by the EU. This is different from the US where the Federal government cannot prevent states from offering such business inducements (e.g. Felix and Hines, 2013). We focus on a major policy change in the formula driven rules in the year 2000 because we have detailed administrative and institutional data before and after the change. Holding area characteristics fixed in the pre-policy change period we exploit only the change in the EU policy parameters - the "weights" given to the different observable factors (e.g. unemployment and per capita GDP) determining which geographical areas were defined to be more economically disadvantaged. This enables us to estimate the

[1] Here, we are using the term "industrial policy" in its broad sense, but our focus in the rest of the paper is on one important component of that policy that directs investment subsidies to private sector firms in an attempt to revitalize disadvantaged geographical areas.

[2] For examples see Krueger and Tuncer (1982), Beason and Weinstein (1996) and Lawrence and Weinstein (2001).

causal effect of the program on employment and unemployment (and also plant net entry, investment, and productivity). Our data set is constructed by linking rich administrative panel data on the population of UK establishments and the population of RSA program participants.

We reach four substantive conclusions. First, there is an economically large and statistically significant program effect - a 10 percentage point increase in an area's rate of maximum investment subsidy causes about a 10% increase in manufacturing employment and a 4% decrease in aggregate unemployment. These effects are underestimated if endogeneity is ignored, as the areas that become eligible for the program are those which, on average, experience negative shocks and whose establishments would otherwise perform badly, even in the absence of the policy. This conclusion is robust to many controls including other place-based policies such as EU Structural Funds (for which we also develop a rules based IV). Second, we show that these positive effects are not purely due to substitution of jobs towards eligible areas and away from neighboring (ineligible) areas. Third, we find that the positive treatment effect is confined to establishments in smaller firms (e.g. with under 50 workers). We suggest that this is due to larger firms being more able to "game" the system and take the subsidy without changing their level of economic activity. Finally, there appear to be no additional effects on productivity after controlling for the program's positive investment effects.

Our paper contributes to an emerging literature on the causal impact of place-based policies, see Kline and Moretti (2014a) for a survey and Kline and Moretti (2014b) on long-run effects on manufacturing jobs from the Tennessee Valley Authority policy. US Empowerment Zones - neighborhoods receiving substantial Federal assistance in the form of tax breaks and job subsidies - have been examined by Busso, Gregory and Kline (2013) who identify strong positive employment and wage effects, with only moderate deadweight losses.[3] Towards the end of the paper (subsection VI.D) we provide explicit comparisons of the size of our effects to those in the place-based policy literature and show that the larger magnitudes we find are likely to be rooted in methodological and program differences.

We also relate to a broader literature concerning evaluations of business support policies and place-based interventions (see Neumark and Simpson, 2014 for a review). Several papers

[3] Holmes (1998), Albouy (2009) and Wilson (2009) consider other place-based tax policies, while Wren and Taylor (1999), Bronzini and de Blasio (2006), Martin et al. (2011) and Becker et al. (2010, 2012a, b) provide evidence for regional policy in Europe. Gibbons, Overman and Sarvimaki (2011), and Einio and Overman (2015) discuss similar place based schemes in the UK, while Gobillon et al. (2012) and Mayer et al. (2017) provide estimates for France and Cerqua and Pellegrini (2014) for Italy. In contrast to RSA, which targets specific firms within eligible areas, these schemes are generally not discretionary (i.e. subject to the firm meeting some basic requirements). In addition to this substantive difference in the nature of the scheme, our paper is also unique in using exogenously imposed changes in area eligibility rules to identify the causal effects of the policy.

consider direct research subsidies to industrial R&D. Unlike the generally positive assessments of R&D tax credits (e.g. Fowkes et al., 2015), the evidence on these direct subsidies is mixed (e.g. the survey in Jaffe and Le, 2015).[4] Several recent studies have used Regression Discontinuity designs to assess the causal effects of direct grants. For example, both Howell (2017) on the US Small Business Innovation Research program and Bronzini and Iachini (2014) on Italian data use a proposal's application score by an independent committee as the running variable when analyzing the effects of receiving R&D subsidies. Interestingly, these studies are consistent with us in that they uncover much larger positive program effects on investment for small firms.

Our paper is not the first to look at the impact of the RSA program. Unfortunately, most of the previous evaluation studies are based on "industrial survey" techniques where senior managers at a sample of assisted firms are asked to give their subjective assessment of what the counterfactual situation would have been had they not received the grant (e.g. see National Audit Office, 2003, for a survey). In contrast to the OLS approaches discussed above that are likely to underestimate positive policy effects, these survey techniques typically over-estimate program impacts since firms receiving money are likely to exaggerate the scheme's benefits. Some other studies have also used firm-level econometric techniques to evaluate the direct impact of RSA.[5] Relative to existing studies our contribution is to exploit a policy rules change experiment on the population of plants to identify causal effects.

Finally, there is a large literature on the impact of capital and labor taxes (e.g. Mirrlees, 2010).[6] Unlike our RSA program, however, these general tax rules tend to be available nation-wide rather than place based, and automatic rather than at the discretion of an agency. They are also more likely to engender general equilibrium effects than the RSA policy that amounts to less than 0.1% of aggregate UK investment.

The structure of the paper is as follows. Section I describes the policy in more detail and outlines how eligibility changes over time. Section II sets out a simple theoretical framework to help interpret the results and Section III describes the econometric modelling strategy. Section

[4] See Takalo et al. (2013) or Einio (2014) for recent contributions.

[5] For example, Devereux et al. (2007) look at Greenfield investments by foreign-owned multinationals and UK-owned multi-plant groups using the largest RSA grant offers. They find positive, but quantitatively tiny effects on multinational location decisions. Hart et al. (2008) also focus on multinationals using a Heckman selection model. Jones and Wren (2004) and Harris and Robinson (2005) look at differences in survival between RSA recipients and non-recipients.

[6] A recent example is Zwick and Mahon (2017) who find substantial effects of temporary tax incentives on investment using shifts in accelerated depreciation. Interestingly, the results are especially large for smaller firms, which is broadly consistent with our findings as reported below.

IV describes the data, Section V reports our results at the area level and Section VI at the plant and firm level. Section VII provides conclusions. In the Online Appendices we report more details on the RSA policy (Appendix A), the changes in EU rules (Appendix B), data details (Appendix C), aggregation issues (Appendix D), other regional policies (Appendix E), further econometric results (Appendix F) and cost per job estimates (Appendix G).

I. INSTITUTIONAL FRAMEWORK: DESCRIPTION OF THE REGIONAL SELECTIVE ASSISTANCE (RSA) PROGRAM

I.A Overview

Regional Selective Assistance started in 1972 and from the early 1980s was the main business support scheme in the UK. The program provided discretionary grants to firms in disadvantaged areas characterized by low levels of per capita GDP and high unemployment ("Assisted Areas"). It was designed to "create and safeguard employment" in the manufacturing sector. Firms applied to the government with investment projects they wished to finance such as building a new plant or modernizing an existing one. If successful, the government financed up to 35% of the cost of an investment project.[7]

Because RSA had the potential to distort competition and trade, it had to comply with European Union state aid legislation. European law, except in certain cases, prohibits this type of assistance. In particular, Article 87(3) of the Treaty of Amsterdam allows for state aid only in support of the EU's regional development objectives. The guidelines designate very disadvantaged "Development (subsequently called Tier 1) Areas" in which higher rates of investment subsidy can be offered, and somewhat less disadvantaged "Intermediate (Tier 2) Areas" where lower subsidy rates were offered. There was an upper threshold to the investment subsidy called Net Grant Equivalent (NGE)[8], which sets a maximum proportion of a firm's investment that can be subsidized by the government. These EU determined maximum NGE rates differed over time and across geographical areas.

Since the formula that determines which areas were eligible (and at which NGE rate) was set about every seven years by the European Commission for the whole of the EU and not by the UK government, this mitigates concern of policy endogeneity. Although the overall budget for

[7] Although the structure of RSA is largely the same at time of writing, it has been rebranded several times after the end of our sample period (e.g. as the "Selective Finance for Investment Scheme" in 2004) so we refer to it in the past tense. In Wales and Scotland it retains the name of RSA.

[8] The Net Grant Equivalent (NGE) of aid is the benefit accruing to the recipient from the grant after payment of taxes on company profits. RSA grants must be entered in the accounts as income and are made subject to tax. Details for calculations of NGEs are available in EU Official Journal C74/19 10.03.1998.

RSA is determined by the UK and not the EU, the UK had to conform to EU rules when deciding which areas are eligible to receive RSA. Changes to area-level eligibility are driven by EU wide policy parameters and are therefore the key form of identification in our paper.

I.B Changes in eligibility over time

We focus on the change in the map of the areas eligible for RSA in 2000 using the period between 1997 and 2004, before and after the policy change. Although there have been changes in the area maps in 1984, 1993, 2000 and 2006 our access to program participation data does not extend beyond 2004 and we were unable to obtain precise information on the criteria for being an assisted area before 1993, so we cannot construct the rules change IV for the 1993 changes. Since there are also changes in the collection of total employment data before 1997 (see Appendix A), we mainly use 1997 as the first year (although we present OLS estimates of manufacturing employment in all years from 1986 onwards in a robustness exercise).

Figure 1 shows the maps of assistance in the pre-2000 period (left hand side) and post 2000 period (right hand side). There was considerable change in the areas that could receive assistance and the level of subsidy they were able to receive. Whether an area is eligible for any RSA is determined by a series of quantitative indicators of disadvantage which changed over time but always included per capita GDP and unemployment. For the 2000 change, the data used to determine which areas were eligible dated from 1998 and earlier. Although the EU publishes which indicators it uses, it does not give the exact policy parameters (the weights) on these indicators which determine eligibility, but we can estimate these parameters econometrically (see Section III).

This institutional set-up implies that an area can switch eligibility status for at least three reasons. First, there may be a change over time in the indicators used or the relative importance (weights) of each indicator. Second, changes in the average EU GDP per capita can push areas in or out of eligibility even if nothing changes in the area itself. For example, when the formerly Communist states in Eastern Europe joined the EU, average EU GDP per capita fell, meaning some poorer UK areas were no longer eligible for subsidies from the RSA program. Third, the economic position of an area changes over time even for a fixed set of rules. The first two reasons for eligibility changes are clearly exogenous to area unobservables, but the third is not. It helps that the information determining eligibility is pre-determined as it is lagged at least two (and up to ten) years before the policy change and therefore many years prior to current outcomes. However, there may be unobservable area trends that are correlated with eligibility status and outcomes. Areas which are in long-run decline are more likely to have falling employment and output and are therefore more likely to become eligible for the program, generating a downward bias on a difference-in-differences estimate of the program effect on jobs. Alternatively, there could be a temporary negative shock. This would increase the probability of an area becoming eligible, but it would also generate an upward bias on the treatment effect as the area mean reverts (an "Ashenfelter Dip" problem). To deal with endogeneity we focus on using only *changes* in the cross EU policy rules to construct instrumental variables for program participation and ignore all changes in area characteristics. As described more formally in Section III, we fix the area characteristics relevant for eligibility prior to the policy change and interact these with the changes in the EU wide policy parameters.

I.C Formal criteria for receipt of RSA investment subsidies

RSA was heavily targeted at the manufacturing sector – less than 10% of RSA spending was to non-manufacturing firms. The grants were discretionary, and firms could only receive grants if the supported project was undertaken in an "Assisted Area" and involved capital expenditure on property, plant or machinery. These were the most clearly verifiable aspects. In addition, the formal criteria stipulated that the project: (a) should be expected to lead to the creation of new employment or directly protect jobs of existing workers which would otherwise be lost and (b) would not have occurred in the absence of the government funding ("additionality"). Location, which forms the basis for our instrumental variable, is objective, clearly defined and enforceable. The other criteria are more subjective and are based on the government's ability to assess the counterfactual situation of what would have happened in the absence of government support. For example, a firm could cut jobs but claim that it would have reduced employment by even more without support. It is difficult for bureaucrats to make such an assessment of this claim with accuracy. The ability of a firm to "game" the system may be particularly high for larger firms who can increase employment at subsidized plants at the expense of employment in unsubsidized plants that did not receive RSA.

II. MODELLING THE EFFECTS OF AN INVESTMENT SUBSIDY

II.A Effects of the RSA policy on Capital investment

What are the likely effects of RSA on investment and employment in an eligible area? Initially we consider the effects of a firm receiving RSA in a world without financial frictions. The investment grant (ϕ) reduces the cost of capital facing the firm. To calculate the magnitude of this effect we can use the Hall-Jorgenson cost of capital framework (e.g. King, 1974). We consider the effects of a perturbation in the path of a firm's capital stock. If the firm is behaving optimally, then the change in after tax profits resulting from a one unit change in the capital stock will equal the unit cost of capital. Under RSA, depreciation allowances are granted on total investment, so we can write the cost of capital, ρ, as (e.g. Ruane, 1982):

$$\rho = \delta + \frac{r(1-\phi-\theta\tau)}{1-\tau} \tag{1}$$

where δ is the depreciation rate, τ is the statutory corporate tax rate, r is the interest rate and θ is the depreciation allowance. It is clear from equation (1) that the cost of capital is falling in the generosity of the investment grant ($\frac{\partial\rho}{\partial\phi} = -\frac{r}{1-\tau} < 0$). Panel A of Figure 2 illustrates the possible program effect by assuming that the level of the capital stock of a firm is determined from the intersection of capital demand (a downward sloping marginal revenue productivity of capital curve, MRPK) and a horizontal tax-adjusted user cost of capital (the supply of funds curve). Without any subsidy, the cost of capital is ρ_1 and a firm's capital stock is K_1. The RSA program reduces the effective cost of capital to ρ_2 and capital rises to K_2.

As discussed above, RSA attempts to target marginal investments. If only marginal capital projects obtain funding, the change in the capital stock is $\Delta K = K_2 - K_1$ at a taxpayer cost of $(K_2 - K_1)(\rho_1 - \rho_2)$. More realistically, the government has imperfect monitoring ability and so will achieve a lower increase in capital as some of the costs are diverted to funding infra-marginal investments that the firm would have made even in the absence of government intervention. The extreme case is where the government has zero monitoring ability and the firm simply accepts the subsidy without making any additional investment. The level of capital stays the same, but there is a direct transfer of funds from the taxpayer to shareholders. The firm will not voluntarily make investments that earn a rate of return below the outside market cost of capital[9] and can effectively lend out any excess subsidies at this market rate. It is likely that the government's monitoring problem is particularly severe for large firms which will typically be conducting many different types of investments, and an outside agency will have difficulty in assessing whether any grant is truly additional or not.

Now consider a world with imperfect capital markets such that we have a hierarchy of finance model (e.g. Bond and Van Reenen, 2007). Here a firm may be financially constrained if it must externally finance investment from debt or equity rather than relying on internal funds. In this case, the cost of capital/supply of funds curve is not horizontal as in Panel A, but becomes

[9] MRPK< $\delta + \frac{r(1-\theta\tau)}{1-\tau}$, i.e. the value of ρ in equation (1) when ϕ = 0.

upward sloping when firms need external finance. This is illustrated in Panel B of Figure 2 where we consider two firms indicated by different MRPK curves. A financially unconstrained firm has a schedule "MRPK (unconstrained)" which intersects the flat part of the supply of funds curve, and can finance all investments from internal funds. By contrast, a financially constrained firm has schedule "MRPK (constrained)" and has to rely in part on more expensive external funds. An identical subsidy will generate more investment from the financially constrained firm than from the unconstrained firm.[10] This is illustrated in Panel B of Figure 2 (ΔK' > ΔK) and can be seen from considering the cross partial derivative of equation (1): $\frac{\partial^2 \rho}{\partial \phi \partial r} = -\frac{1}{1-\tau}$ < 0. For firms facing an effective interest rate (r) higher than the risk free rate due to financing constraints, the marginal effect of a subsidy on the cost of capital is greater and so the effect on investment is larger. If small firms are more likely to be financially constrained, this is a second reason over and above lower monitoring difficulties why the program may have a larger treatment effect on small firms. As with the case of perfect financial markets, if the government cannot target marginal investments there will be zero effect on the financially unconstrained firms.

II.B Effects of the RSA policy on labor

One of the objectives of the program is to raise employment. Consider as a benchmark a constant returns to scale production function *F(K, L)* where K = capital and L = labor with perfect competition in all markets. The Marshallian conditions for derived demand are (e.g. see Hamermesh, 1990):

$$\eta_{L\rho} = s_K(\sigma - \eta)$$

Where $\eta_{L\rho} = \frac{\partial \ln L}{\partial \ln \rho}$ is the elasticity of labor with respect to the user cost of capital, σ = the elasticity of substitution between labor and capital, s_K = the share of capital in total costs and η is the (absolute) price elasticity of product demand. The sign of the effect will depend on whether the *scale* effect (determined by η) is larger than the *substitution* effect (determined by σ). The marginal effect of the investment subsidy is:

[10] Note that the program is not simply directed lending which will only have an effect on financially constrained firms (e.g. Banerjee and Duflo, 2014), but rather a directed subsidy which in general will also have effects on financially unconstrained firms.

$$\frac{\partial \ln L}{\partial \phi} = \frac{\partial \ln \rho}{\partial \phi} s_K(\sigma - \eta)$$

This shows that, in general, the subsidy could have a negative effect on employment, even if it increases capital. If $\sigma > \eta$ an increase in the investment subsidy will reduce labor. On the other hand, if $\sigma < \eta$ there is a positive effect on employment and the magnitude of this effect will be larger if capital is more important (high s_K). This is something we will examine empirically.

Finally, the formal rules of receiving RSA require that jobs must be created or safeguarded. In terms of the theory, this involves firms trying to convince government that they will not simply recycle funds (as discussed above) and that capital is a complement for labor (i.e. sigma is less than eta). Of course, the ability of the government to assess, monitor and enforce this might be doubted. Firms could still cut jobs but claim that employment would have fallen by even more in the absence of the subsidy

IIC. General Equilibrium effects

Total expenditure on RSA was about £164m per year in our sample period, which constitutes only a tiny fraction (0.065%) of total UK investment.[11] Consequently, although there may be general equilibrium effects on asset prices and wages (e.g. Glaeser and Gottlieb, 2008) these are unlikely to be large. Nevertheless, since there may be some equilibrium price effects in local areas we also examine the effect of the program on wages and population density. We find these effects to be insignificantly different from zero.

IID. Summary of Model

We take several predictions from the theory to the data. First, the investment subsidy should have positive effects on investment. Second, in the model the investment subsidy will have a positive effect on employment if scale effects are sufficiently large and the magnitude of any positive employment effect will be larger when the capital share is higher. Third, we may expect that the policy has a larger effect on small firms because: (i) big firms can more easily "game" the system by using RSA for investment they would have done anyhow; and (ii) smaller firms are more likely to be financially constrained. We find support for all of these predictions in the data.

[11] For example, RSA expenditure as 0.065% of total investment in 2004. Online Appendix Table A1 contains some descriptive statistics including the fact that total RSA grants were £149m in 2004 compared to £227bn spent in gross fixed capital formation (ONS, 2014).

III. ECONOMETRIC MODELLING STRATEGY

Our basic approach is to estimate the policy effects in a small-scale geographical area ("wards" that are similar in population size to a US zip code). We also present results at both a higher level of aggregation – travel to work areas (TTWAs) to assess spillovers (do jobs just get displaced from other areas?) and lower levels of aggregation (plant and firm-level) to assess issues around intensive vs. extensive margins of adjustment and heterogeneity of the treatment effects by firm size. There are 10,737 wards and 322 TTWAs in our dataset covering the whole of Great Britain (England, Wales and Scotland). Since the eligibility varies at the ward level and the number of wards is stable over time, the ward is a natural unit of observation to focus on. We write the relationship of interest as:

$$y_{r,t} = \lambda_1 NGE_{r,t} + \eta_r + \tau_t + v_{r,t} \quad (2)$$

Where $NGE_{r,t}$ (Net Grant Equivalent) is the key policy variable and is defined as the maximum investment subsidy available in ward area *r* in year *t* and ranges from zero to 35%. The main outcome, $y_{r,t}$, we examine is employment – a variable which is available at all levels of aggregation. However, we also examine unemployment and various other outcomes (e.g. investment, output, productivity and entry/exit).[12] Unemployment is useful to examine as we can assess whether increased employment is coming from drawing in people who were previously not working. The η_r is an area fixed effect, τ_t are time dummies and $v_{r,t}$ is an error term.

A concern with estimating equation (2) is that $NGE_{r,t}$ could be endogenous if areas are selected into the policy because they have experienced negative shocks. The wards which experienced a change in eligibility may have done so because of unobserved contemporaneous changes in the area that are correlated with our outcome variables. But, as discussed in Section I, eligibility and the level of maximum investment subsidy in an area also depend on EU-wide rules so changes in the parameters of these policy rules can be used to construct instrumental variables.

To examine this formally, denote eligibility in 2000 (and afterwards) as a discrete variable $S_{r,00}$ and similarly eligibility for the 1993-2000 period as $S_{r,93}$. So $S_{r,\tau} = 1$ if the area is eligible in period $\tau = \{93, 00\}$ and zero otherwise. The EU rules are explicit that eligibility in 2000 depends on a vector of area characteristics such as unemployment and per capita GDP relative to

[12] Unlike the US, the UK Office for National Statistics does not collect data on productivity, investment and wages at the plant level. The surveys are conducted at the firm ("reporting unit") level, including for multi-plant firms. This means that we cannot accurately calculate productivity measures at very detailed geographical level (e.g. ward).

the EU average. The EU also explicitly gives the period over which these data are dated which is from 1998 and earlier due to lags in data collection. Similarly, the policy states the (lagged) characteristics used to define eligibility in 1993 (which were dated 1991 and earlier). Some of the characteristics determining 1993 eligibility were the same as 2000 and some were not (see Online Appendix Table A2). We define the superset of all the area characteristics relevant in 1993 and 2000 as $X_{r,t}$. Therefore, the *propensity* of an area to be eligible in 2000 can be written as:

(3) $$S^*_{r,00} = \theta_{00} X_{r,00}$$

The characteristics ($X_{r,00}$) are area-specific but the policy parameters (θ_{00}) are EU wide. Similarly, propensity to be eligible in 1993 is:

(4) $$S^*_{r,93} = \theta_{93} X_{r,93}$$

Now consider the change in the propensity to be eligible:

(5)
$$S^*_{r,00} - S^*_{r,93} = \theta_{00} X_{r,00} - \theta_{93} X_{r,93} = (\theta_{00} - \theta_{93}) X_{r,93} + (X_{r,00} - X_{r,93})\theta_{93} + (\theta_{00} - \theta_{93})(X_{r,00} - X_{r,93})$$

The change in eligibility will depend on the changes in the policy parameters $(\theta_{00} - \theta_{93})$ and changes in area characteristics, $(X_{r,00} - X_{r,93})$. An obvious concern is that those areas that were declining may be more likely to become eligible for the policy and hence more likely to have worse outcomes. Consequently, we construct instrumental variables based solely on $\Delta \mathrm{z}_{r,t} \equiv (\theta_{00} - \theta_{93}) X_{r,93}$, the leading term in equation (5) instead of the actual change in eligibility which is a function of $(X_{r,00} - X_{r,93})$. These are “synthetic instrumental variables” in the spirit of Gruber and Saez (2002) that should be purged of any suspected bias as they are constructed based solely on the rule changes and not changes in area characteristics.

We present many tests of the validity of the IV strategy including running placebos on pre-policy periods (see in particular subsection V.D). Our preferred estimation technique is to estimate equation (2) by IV in long-differences, but then condition on all the lagged levels of variables in the vector $X_{r,93}$ so that the IV treatment effect is identified purely from the interaction terms. An alternative, less parametric, approach to identification would be to implement a fuzzy Regression Discontinuity Design (e.g. Dell, 2010) using the policy rule measures as running variables. We discuss our implementation of these RD approaches in subsection V.D and Appendix F. They produce qualitatively similar results to our preferred IV approach, but with less precise estimates. Essentially, the RD approach is harder to implement in our context because of the high dimensionality of the policy rules and measurement error in the running variable.

There are several practical issues in implementing this IV strategy (see Appendix B for more details). First, although the EU reveals what is in the X vector, it does not reveal the exact weights in θ that determine eligibility. That is, we know whether a particular element of X has a weight of zero, but not the exact value of the non-zero weights. Nevertheless, we can empirically recover the weights by estimating a regression equivalent of equations (3) and (4) from our data. With the estimated $\hat{\theta}_{\tau}$ we can assign changes to maximum subsidy rates (NGEs) to areas based on $(\hat{\theta}_{00}-\hat{\theta}_{93})X_{r,93}$ rather than any (potentially endogenous) changes in characteristics. Also, recall that $X_{r,93}$ is based on variables dated no later than 1991, so we are effectively using information from 1991 (and earlier) to construct instruments for the 1997-2004 period. The identification is from a non-linear interaction between these long pre-determined characteristics and the change in the policy parameters. Moreover, to allow for potential correlation between current variables and pre-1991 statistics we include $X_{r,93}$ as additional set of controls.

A second issue is that the maximum subsidy rate varies across the eligible areas (see Figure 1). For example, pre-2000 there were Tier 1 areas with an NGE of 30% and Tier 2 areas with an NGE of 20%. To deal with this, we estimate the policy parameters by performing ordered probit models[13] separately for 1993 (three grouped outcomes) and 2000 (six grouped outcomes[14]). We use the $X_{r,93}$ observables for both ordered probits. From the $\hat{\theta}_{\tau}$ we calculate the probability that an area will be in each subsidy regime in both pre and post 2000 periods. We then multiply these probabilities by the NGE in each regime to calculate an *expected* maximum subsidy level for an area based on pre-1993 characteristics and the policy parameters. This is the IV used which varies by area and across the policy change solely due to the policy parameters. Estimating by ordered probit also means that all probabilities are bounded between zero and one, which is not the case for OLS regression versions of (3) and (4).

[13] Nothing hinges on the particular distributional assumptions of the ordered probit. We have qualitatively similar results using ordered logits, OLS, etc. Appendix B discusses these alternatives.

[14] The six maximum subsidy rates after 2000 are 0 for ineligible areas, 10%, 15%, 20% and 30% for “Tier 2” areas and 35% for “Tier 1” areas. Pre-2000 the rates were zero, 20% and 30%.

Continuing our simplified discussion from above, we implement equation (2) by differencing out the area fixed effect:

(6) $$\Delta y_{r,t} = \lambda_1 \Delta NGE_{r,t} + \pi X_{r,93} + t_t + \Delta v_{r,t}$$

where $\Delta y_{r,t} \equiv y_{r,t} - y_{r,97}$, for t > 1997, as 1997 is the first year of our sample.[15] We identify equation (6) by 2SLS using $\Delta z_{r,t} \equiv (\theta_{00} - \theta_{93}) X_{r,93}$ as an instrument for $\Delta NGE_{r,t}$. We also present OLS, reduced forms and first stages. The dependent variables are estimated in natural logarithms, so we add one to the small number of observations where the outcome value is zero.[16]

Although the maximum investment subsidy rate (NGE) is an attractive treatment variable as it is the main EU-determined policy variable, an alternative specification to equation (6) is to use the subsidies actually paid out to firms in the area. The advantage of using the actual RSA subsidy is that it is more easily interpreted as "increasing the amount of dollar subsidies by 10% is associated with an increase in employment of y%". The disadvantage of using the RSA subsidy is that we do not know the exact timing of when the subsidies are paid after the first year of receipt, so we have to define RSA subsidy as the amount of subsidy (in thousands of pounds sterling) that an area receives on average per year. For these reasons, we present results using both NGE eligibility and RSA subsidies as treatment indicators.

The 2000 map of eligibility was based on Census wards. Our eligibility instrument is defined, therefore, at the ward level and in our baseline panel regressions our unit of analysis is at this level. As an extension, we also estimate our model at a higher level (TTWA) to investigate cross-area spillover effects and disaggregate to the plant/firm level to look at treatment heterogeneity. Similarly, in the baseline specification we cluster the standard errors at the ward level but show alternative treatments that allow for spatial autocorrelation (e.g. by clustering at the TTWA level or higher in subsection V.D). We discuss many additional econometric issues when we come to these results.

IV. DATA AND ESTIMATING POLICY RULES

[15] As discussed in Appendix A, we start our base period in 1997 because (i) unemployment statistics are only available on a spatially consistent basis from this year and (ii) the electronic business register (IDBR) began in 1994 and had some reliability issues issue in the first few years. We show robustness to starting in alternative years below.

[16] For example, when we are looking at employment, *L*, as an outcome the dependent variable is y = ln(1+*L*). 100 of our 10,737 areas have zero manufacturing employment in all years (0.9% of the sample) and 21 areas have no unemployed in all years (0.2% of the sample). Our results are robust to dropping all areas with zeros in any year or all wards which had a zero in any year. We also obtain near identical results using the Inverse Hyperbolic Sign transformation where we use $\ln[L+\sqrt{(1+L^2)}]$ as the dependent variable (see Card and Della Vigna, 2017).

IV.A Datasets

Details on the data are in Appendix C, but we summarize the most important features here. We combine administrative data on program participants with official business performance data from the UK Census Bureau (Office of National Statistics, ONS). Specifically, we match the Selective Assistance Management Information System (SAMIS) database, the Interdepartmental Business Register (IDBR) and the Annual Respondents Database (ARD).[17] The IDBR is the population business register containing every establishment's employment, location and industry. We use this to construct jobs by area, our primary dependent variable, distinguishing between manufacturing jobs (where RSA is targeted) and non-manufacturing jobs. We match in unemployment from the local areas labor market statistics through the ONS Nomis service.

SAMIS is the administrative database used to monitor RSA projects. It contains information on all program applications (almost 25,000) since the inception of RSA in 1972, and includes information on the name and address of the applicant, a project description, the amount applied for and the date of application. For successful applications, it provides the amount of subsidy and first date of payment.[18] We match program participants with data from the population in the IDBR that includes addresses, industry, ownership and employment. The lowest level of data is at the business site level. The lowest level of aggregation we consider are all business sites of a particular firm in a ward that we refer to as a "plant". This is because the unique business site identifier at the more disaggregated level is not always reliable.

We matched 82% of all the RSA applicants between 1997 and 2004. The most common reason for non-matches is that the information on the SAMIS database is inadequately detailed to form a reliable match to the IDBR. To check for selection we conducted a detailed comparison of the characteristics of projects and project participants of matched with non-matched firms. All observable characteristics were balanced between the samples including application amounts, headquarter location, firm size and administrative location of agency analyzing the application (see Criscuolo et al., 2006).

The ONS draws a stratified random sample of firms from the population of firms in the IDBR to form the ARD (Annual Respondents Database) from the Annual Business Inquiry (ABI) which is a mandatory survey. From the ARD we obtain information on investment, wages,

[17] The IDBR is the equivalent of the US Economic Census but has less data fields: it is a business register. The manufacturing part of the ARD is similar in structure to the US Annual Survey of Manufacturing.

[18] Around 90% of applications were granted. There is information on applications not granted and we considered using these as part of our empirical design, but legal restrictions prevent us from matching these projects into the administrative data.

and productivity of firms. For multi-plant firms, the ARD reports this information only at the aggregate firm level rather than the plant level available in the IDBR. Overall, 80% of firms are single-plant and located at a single mailing address. The ARD does not consist of the complete population of all UK manufacturing firms, since the sample is stratified with smaller businesses sampled randomly. However, it does contain the population of larger businesses, which cover 90% of total UK manufacturing employment.

IV.B Descriptive Statistics

Table 1 reports the number of wards broken down by the initial level of NGE 1993-1999 and the new post-policy change NGE after-2000. Before 2000 column (1) shows that 3,428 out of Britain's 10,737 areas were eligible for some investment subsidy (2,012 areas had a maximum subsidy rate of 20% and 1,416 areas had a maximum rate of 30%). After the policy change, row (1) shows that 486 areas (summing columns (3) through (7)) which were previously ineligible for any subsidy became eligible and 1,106 areas which used to be eligible became ineligible (summing 841 and 265 in column (2)). The total number of ineligible areas rose from 7,309 to 7,929. There were also a large number of areas that were eligible in both periods, but still switched their level of NGE. For example, row (3) shows that of the areas which, pre-2000, were eligible for up to a 30% subsidy rate, 388 became eligible for up to a 35% subsidy, while 717 saw their NGE fall to 20%, 30 to 15%, 16 to 10% and 265 to zero. Unsurprisingly, the majority of areas were ineligible for subsidies in all periods (6,823 areas out of a total of 10,737).

Aggregate expenditure on the program was about £164m per year over our sample period, and since 2001 has been generally declining over time. On average, 28% of all British wards are eligible for RSA accounting for 39% of manufacturing employment and 30% of manufacturing plants. Although, on average, only 3% of plants in eligible areas receive a new RSA grant in a given year, 18% of manufacturing employees have worked in a plant who received RSA at some point over our sample.

We report some more descriptive statistics in Table 2. Areas eligible for subsidies (Panel B) have higher unemployment and more manufacturing workers than other areas. For example, in the 1997-1999 period the average ward has 267 manufacturing workers (Panel A) compared to 351 in those areas eligible for RSA (Panel B). The average subsidy of a plant receiving a grant is just over £56,000 per year in the late 1990s and just under £36,000 in the 2000s (Panel B). In 2000-2004, an average plant has 20 employees (Panel C), although plants in eligible areas tend to be larger (27 employees). Panel D compares *firms* in eligible areas who receive subsidies with those who do not. Recipient firms are larger (87 vs. 31 workers), have 2.7% (= 0.042 - 0.015) lower TFP and 14% lower labor productivity (£38,600 vs. £44,900 value added per worker). As discussed in the Introduction, since the RSA program targets larger and less productive firms, naïve OLS analyses are likely to underestimate any potential positive effects.

IV.C Estimating the Policy Rules

The estimates used to construct the policy rule are presented in columns (1) and (2) of Table 3.[19] As discussed in Section III, these results are from ordered probit estimates of the area support levels (NGE). Note that three of the variables used as indicators in 2000 by policy-makers were not used in 1993 (the employment rate, the ILO unemployment rate and the share of manufacturing workers), so we drop these variables from the regressions in column (1), i.e. setting the coefficients on these variables to be zero. Similarly, there were two indicators that were used in 1993 but not in 2000 (the business start-up rate and the long-duration unemployment rate). Similarly, we set the coefficients on these variables to be zero in column (2).

Looking across Table 3, the signs are generally intuitive. Areas with higher GDP per capita, higher population density, more skilled workers, higher business start-up rates, lower structural unemployment rates, higher activity rates and higher employment rates are all significantly less likely to be eligible for higher investment subsidies. The only surprises are that the claimant count unemployment rate (in 1993) and the ILO unemployment rate (in 2000) take counter-intuitive negative signs. This seems to be due to collinearity among the many unemployment measures. In 1993 (2000) there are four (five) labor market indicators that are all highly correlated. We illustrate the collinearity issue by estimating similar regressions with fewer unemployment variables. For 1993, when we drop structural unemployment, the results in column (3) show that the coefficient on the current unemployment rate takes its expected positive sign. For the period from 2000 onwards, column (4) shows that the coefficient on ILO unemployment reduces in absolute size and is no longer significant when we drop the claimant count and structural unemployment rate.[20]

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[19] Online Appendix Table A2 has definitions and descriptive statistics.

[20] We checked that these collinearity issues are not spuriously driving our key findings. We constructed the rule change instruments dropping some of the potentially collinear variables and found that our results are robust. For example, using the estimates from the last two columns of Table 3 (instead of our baseline estimates using the first two columns) generated a coefficient (standard error) on the IV estimates in the employment equation of 0.653(0.322) compared to 0.953(0.260) in the baseline estimates of column (4) in Table 4, Panel A.

As discussed in Section III we use the results from columns (1) and (2) to construct our IV for the policy: the change in the predicted level of maximum investment subsidy based on pre-1993 area characteristics. The distribution of the level of the IV ($z_{r,t} \equiv \theta_{\tau} X_{r,93}$) is shown in Panel A of Online Appendix Figure A1, and the change (which we use as our IV, $\Delta z_{r,t} \equiv (\theta_{00} - \theta_{93}) X_{r,93}$) is in Panel B. There is a mass point close to zero in both levels and changes as most areas have a very low probability of being treated and this does not change over time (as in the actual data). However, the IV has positive mass over the entire support of the NGE distribution both in levels and in changes. Panel B shows an asymmetry with more areas predicted to lose eligibility than gain it, consistent with the actual changes in eligibility for investment subsidies reported in Table 1.

V. AREA LEVEL ANALYSIS

V.A Main Results

We turn first to the area level results, focusing mainly on those at the ward level. Recall that our identification strategy uses exogenous policy rule changes that determine which wards are "randomized in" to be eligible (or ineligible) for support.

Figure 3 shows changes in employment for areas whose support levels were predicted to *increase* because of the policy rule change in 2000 – i.e. a discrete version of our instrumental variable - compared to areas where support levels were predicted to *decrease*.[21] Since this is for manufacturing, a sector in long-run decline, both lines are on a downward trend, but there is no sign of significant differential trends prior to the 2000 policy change. The figure clearly suggests that manufacturing employment fell significantly less in areas where predicted eligibility for investment subsidies increased after 2000 compared to those areas where predicted eligibility fell.

Figure 4 reports the same results for unemployment. The 1997-2004 period was one of strong growth in the UK economy and unemployment was falling across the country. It is clear that there is a significantly faster fall in unemployment in the areas which where exogenously more likely to become eligible for investment subsidies after 2000 (dashed line). By 2004, these

[21] Recall that our instrument is derived from the change (due to rule changes) in predicted support levels. There are no areas where predicted support levels stay precisely constant because the probabilities are continuous.

areas enjoyed falls in unemployment about 7% higher than elsewhere. By contrast, prior to 2000 the falls in unemployment were statistically identical across the two groups of areas.[22]

Table 4 reports first area-level regression results. Panel A contains results for manufacturing employment. In column (1), we report regressions using the change in the area's maximum investment subsidy rate (NGE) as the main explanatory variable. There is a positive correlation with employment, but it is only significant at the 10 percent level and is small in magnitude: increasing the available investment subsidy by 10 percentage points is associated with a 1.2% increase in employment. Column (2) presents the reduced form using our policy rule instrumental variable constructed from exogenous changes in subsidy eligibility using the change in EU wide policy parameters. The coefficient on the IV is positive and significant as suggested by Figure 3. Column (3) reports the first stage regression with NGE changes as the outcome and shows that this is strongly predicted by our IV. The final column reports the IV results suggesting that the causal effect of RSA is over seven times as large as the OLS estimate of column (1). A 10 percentage point increase in the maximum investment subsidy (e.g. an increase in NGE from 0 to 0.1) leads to a 10% (= exp(0.0953x0.1) – 1)x100) increase in jobs. This OLS bias is consistent with what we would expect: a positive shock to an area decreases the probability of it becoming eligible for investment subsidies, so OLS underestimates the employment increasing effects of the policy.[23]

As noted above, RSA is focused on the manufacturing sector. Consequently, the increase in manufacturing employment in Panel A of Table 4 could come from decreases in jobs in non-manufacturing sectors. To assess this, we do two things. First, in Panel B we estimate identical specifications to Panel A, but instead use ln(unemployment) as a dependent variable to see if joblessness falls in eligible areas. Second, in Panel C we directly examine non-manufacturing employment. Panel B shows that unemployment falls significantly in areas that become eligible

[22] We also reproduced Figures 3 and 4 using the *actual* changes in areas eligible for RSA rather than the predicted changes. Consistent with our concern over endogeneity there is evidence of pre-trends in the expected direction using the actual changes. For example, areas that were ineligible for RSA, but became eligible after 2000 had larger average falls in employment than areas which did not change their eligibility status (or lost it).

[23] All results are robust to using the level instead of the logarithm of the dependent variable. For example, in levels the coefficient on NGE in the IV employment equation of column (4) of Panel A is 644.6 with a standard error of 112.9. This implies a ten percentage point increase in NGE increases the number of manufacturing jobs in a ward by 64 or 18% at the mean level of employment (351 in Panel B of Table 2). This larger effect is driven by outliers which are dampened by the log transformation. For example, if we winsorize the upper and lower 5% of the employment distribution and re-estimate in levels the coefficient on NGE becomes 303.9 with a standard error of 34.4, which for a 10 percentage point NGE increase implies a 9% rise in employment, much closer to our baseline results. Similarly, other transformations such as using the Inverse Hyperbolic Sign gave similar results. For example, the IV coefficient (standard error) was 0.968 (0.286) in a specification like column (4) of Panel A

for higher levels of investment subsidy. Just like manufacturing employment, the beneficial effects of the policy on unemployment is underestimated by the OLS estimates in column (1) compared to the IV estimates. A 10 percentage point increase in NGE causes a 4.2% fall in unemployment in column (4). By contrast, there appears to be no significant effect of NGE on non-manufacturing employment in Panel C. For example, the coefficient in column (4) is 0.177 with a standard error of 0.161 (compared to 0.953 for manufacturing). Consequently, NGE increases the share of manufacturing jobs as well as the total number of jobs in an area.[24]

Table 5 reports the same set of regressions as Table 4 but uses the amount of subsidy that an area receives on average per year as the main right hand side variable (rather than grant eligibility). Hence, we can interpret the estimated coefficient as the elasticity of the labor market outcome with respect to subsidy payments. We obtain qualitatively similar results to Table 4. For example, the final column suggests that a 10% increase in subsidy spending leads to a 2.9% increase in manufacturing jobs and a 1.3% fall in area unemployment and no effect on non-manufacturing jobs.[25]

V.B Other Policies

An important concern with our findings so far is whether there are other policies correlated with changes in RSA that could confound our results. For such a policy to bias the IV results, the omitted policy change would not only have to be effective in affecting jobs, but also be correlated with our rule change instrument (the interaction of the RSA policy parameters and the lagged area characteristics). To consider this issue we undertook a detailed investigation of all area-based policies we could find that changed in our sample period as documented in Appendix E. From this, we conclude that the only policy that causes material concerns are the EU "Structural Funds" (SF), which support infrastructure projects in roads and energy as well as initiatives for economic and social regeneration of urban areas.[26] As with RSA, the map of EU supported areas focused on disadvantaged areas and also changed in 2000. Fortunately, the areas that saw a change in their eligibility for Structural Funds are not all the same as those that saw a change in their eligibility for RSA. In fact, there is considerable variation in the areas that

[24] Using the share of manufacturing jobs as the dependent variable and estimating column (4) leads to a coefficient (standard error) of 0.137(0.033) on NGE. Using the ln(total number of jobs) has a coefficient (standard error) of 0.353(0.144) on NGE in this IV specification.

[25] Since some of the subsidies (and their effects) could persist for longer periods of time after an area becomes eligible we may be underestimating the longer-term effect as our dataset ends in 2004.

[26] The structural funds are the financial tools the EU uses to implement regional policy (see http://ec.europa.eu/regional_policy/en/funding/). Past evaluations report mixed results for the effect of structural funds. Recently Becker et al. (2010, 2012a,b) have a more positive assessment especially for regions with higher absorptive capacity (those that are richer and hence closer to the cut-off point for EU funding).

switched in and out of RSA and Structural Funds eligibility (see Online Appendix Table A3). Total SF spending is higher than RSA, although the direct SF grants to business are an order of magnitude smaller than RSA. For example in 1997 the total amount of RSA grants accepted was £158.3 million while the total amount of Structural Regional Development Funds was £621 million (House of Commons, 2000), only £15.6 million of this amount was paid as business grants (1997 Annual Report of the Industrial Development Act).

As with RSA, changes in Structural Fund eligibility are unlikely to be exogenous to local shocks as the Structural Funds are designed to provide support for declining areas. Consequently, we implement the same methodology used for RSA to develop an IV for Structural Funds based on the criteria that the EU used in determining whether an area is eligible for Structural Fund support. Despite considerable overlap with the variables used to determine RSA eligibility there are sufficient differences in the EU criteria to make this strategy viable. For example, local crime rates were a criterion for Structural Funds (but not RSA), and the start-up rate and activity rates were criteria for RSA (but not Structural Funds). We exploit these differences when estimating the Structural Fund policy rules.[27] From the estimated weights on the Structural Funds criteria and lagged characteristics we construct a policy rule change IV for EU Structural Funds and re-estimate our main specifications augmented to include these new variables.

Results accounting for SF are reported in Table 6 and should be compared to those reported in Table 4. Although our instruments are powerful in predicting eligibility for Structural Funds (see column (4)), the results are somewhat mixed on the policy itself. The coefficients generally suggest beneficial labor market effects of Structural Funds (except for the employment IV in column (5) of Panel A), but are significant only for unemployment (in the OLS and reduced form of columns (1) and (2)). More importantly, there remains a positive and significant effect of investment subsidies (NGE) in the IV regressions of column (5) and the reduced forms of column (2) for employment (and significant beneficial effects on unemployment) even after conditioning on Structural Funds. In our preferred IV specifications the coefficient on NGE rises from 0.953 to 0.999 for employment and changes from -0.414 to -0.409 for unemployment. Hence, although there is some evidence that Structural Funds may have some benefits, accounting for this policy does nothing to materially change our conclusions on the positive effects of the RSA program.

[27] See Online Appendix Table A4 (the analog to Table 3).

As noted above, we also considered a wide range of other place-based policies. We identified six other place-based policies that changed in our sample period: Employment Zones, Coalfields Regeneration Scheme, Regional Venture Capital Funds, Enterprise Grants, the New Deal for Communities and Devolution to Scotland and Wales. The details of each of these are discussed more in Appendix E. These policies do not have an explicit set of EU rules that we can use to construct the same instruments as RSA and Structural Funds. Therefore, to control for the effect of these policies we simply include a dummy variable which switches on when an area becomes eligible for the policy. Table 7 displays the results for employment reduced forms (Panel A) and IV regressions (Panel B) with specifications based on those of columns (2) and (4) of Table 4 Panel A.[28] We include each policy variable one by one in columns (1) through (6), and then all together in column (7). As is clear from the table, the effect of RSA is robust to the inclusion of all these other policy controls, remaining statistically significant with a very similar coefficient throughout (the coefficient in the reduced form is now 0.815 compared to 0.839 in the baseline Table 4 results and for IV is now 0.966 compared to 0.953 in the baseline). As for the other policies, some appear to have perverse negative and significant coefficients on jobs (e.g. New Deal for Communities and Devolution to Scotland and Wales) whereas others have generally positive coefficients (e.g. Regional Venture Capital Fund). Given that we do not have instruments for these policies, we should not read too much into the coefficients. Finally, column (8) also adds in Structural Funds to the specification of column (7), treated endogenously as in Table 6). The SF coefficient is significant in the reduced form of Panel A, but insignificant for the IV specification of Panel B.[29] More importantly for us, the RSA treatment effect remains significant.

The main message from both Tables 6 and 7 is that our estimates of the effects of RSA appear robust to a variety of ways of controlling for potentially confounding policies.

V.C Higher levels of aggregation (TTWA)

In this subsection we compare the policy effects at the ward level to the more aggregate "Travel to Work Area" (TTWA) level in order to examine spillover effects across areas.[30] When an area becomes eligible for investment subsidies firms may relocate jobs from neighboring ineligible

[28] Equivalent results for unemployment are contained in Online Appendix Table A5.

[29] The Structural Funds coefficient is significant for both specifications in Online Appendix Table A5 when we use unemployment as the dependent variable.

[30] A TTWA is similar to a US Commuting Zone. There is variation within a TTWA in ward eligibility. Post-2000, in a TTWA with at least one eligible ward only 31.5% of wards had positive NGE. Pre-2000 the number was 35%.

areas. For example, consider a ward, r and its neighbor r', in a single TTWA (the example is easily generalized to $r = 1, 2, \ldots, R$ contiguous wards). The ward employment regression (in long differences) can take the form:

$$\Delta y_{r,t} = \lambda_1 \Delta NGE_{r,t} - \chi \Delta NGE_{r',t} + \nu_{r,t}$$

Where the "spillover" coefficient χ reflects the fact that a neighboring area that becomes eligible for RSA may cause employment to relocate away from ward r. Below we estimate higher-level TTWA (subscript a) equations of the form:

(7) $$\Delta y_{a,t} = \mu \Delta NGE_{a,t} + v_{a,t}$$

Where $y_{a,t}$ is the log of TTWA employment and $NGE_{a,t}$ is the average NGE change in the two wards weighted by the lagged ward-level employment levels; i.e. $\Delta NGE_{a,t} = w_r \Delta NGE_{r,t} + (1 - w_r)\Delta NGE_{r\prime,t}$ where $w_r = \frac{L_{r,0}}{L_{r,0}+L_{r\prime,0}}$ is the share of employment in region r in the base year 0.[31] In Appendix D we show that if there are no spillovers (i.e. $\chi = 0$) we would expect to see that $\mu \approx \lambda_1$. If there are negative spillovers we would expect $\mu < \lambda_1$. In the extreme case where the program simply causes shifting between areas (as Wilson, 2009, suggests for R&D tax credits across American states) the coefficient of NGE in equation (7) will be zero (μ =0).

We replicate the results from Panels A and B of Table 4 at the TTWA level in Table 8. The qualitative results are similar and there is no evidence of the earlier results over-estimating the treatment effects. For example, the policy effect is 1.006 in the employment IV regressions in Panel A compared to 0.953 in the baseline results (and -0.806 for unemployment vs. -0.414 in the baseline). This is inconsistent with large negative spillover effects on neighboring areas. The unemployment results suggest that revitalizing one area may actually strengthen neighbors, although given the size of the standard errors, we should be cautious about concluding there are positive spillovers.[32]

V.D Other Area level Robustness Tests

We conducted a large number of other robustness tests, some of which we sketch here with details in Appendix F. First, our baseline regression results in Table 4 control for the levels of all variables in Table 3 that enter the policy rules (the $X_{r,93}$). We checked the robustness of the

[31] The weights are based on 1996 employment levels to mitigate endogeneity concerns.
[32] We are assuming that displacement is most likely to occur across neighboring areas. It is possible that displacement occurs from other areas of the UK, but it is likely that local displacement would be strongest.

results to using higher order polynomial functions of the $X_{r,93}$ (quadratic and interaction terms), dropping them completely or adding in the predicted probabilities from the ordered probits (see Online Appendix Table A6). The results were robust to these experiments.

Second, although Figures 3 and 4 do not suggest any spurious differential pre-2000 trends we also ran placebo tests where we introduced "pseudo policies" of the same form as RSA in the pre-2000 period. These were always insignificant. For example, we estimated the employment reduced form on the 1995-2000 data but used the post-2000 policy instruments as if they were introduced in 1997 (see Online Appendix Table A7). The reduced form has a coefficient (standard error) of 0.162(0.163) compared to 0.839(0.228) in the main specification in column (2) of Table 4 Panel A.[33]

Third, we were concerned that we may have under-estimated the standard errors by clustering just at the ward level as there may be more spatial autocorrelation across areas as suggested by the fact that contiguous wards tend to have similar levels of NGE. Online Appendix F.1 discusses this in more detail, but in short we addressed this issue by clustering the standard errors at higher geographical levels such as (i) the TTWA (322 clusters); (ii) alternative clusters based on areas that had the same levels of NGE and shared, contiguous borders (102 clusters) or (iii) clusters based on areas that had the same levels of NGE and borders within 1km (80 clusters) and (iv) the NUTS2 regional level (34 clusters). Regardless of the approach, the coefficient on NGE remained significant in the employment regressions at the 5% level or greater in all specifications. The same was true when we used unemployment as the dependent variable with the sole exception of using the most conservative approach of clustering by the 34 NUTS2 areas.[34]

Fourth, we considered Regression Discontinuity designs (see Online Appendix F.2-F.4). In principle, since we know the variables underlying the rules, conditioning on polynomials of the rules should remove the correlation of NGE with unobservable influences on our outcomes. Implementing this design is empirically challenging in our context as we do not directly observe the running variable, the threshold is unknown and the variables underlying the policy rules are high dimensional (e.g. 8 indicators pre-2000 and 9 thereafter) and are likely measured with error.

[33] We use 1997 as the base year rather than 1995 as the unemployment series has a break in 1996. If we use 1995 as the base year for the employment regressions, our results are very similar. For example the coefficient (standard error) on NGE in the IV regression is 1.295(0.325).

[34] Another issue is that since the instruments are generated regressors (from Table 3), formally we should allow for this in the calculation of the variance-covariance matrix. Doing so, however, made very little difference to the results as shown, for example, in Online Appendix Table A8.

However, for one indicator, GDP per capita, we do know the cut-off for eligibility (75% of the EU average GDP per capita in the NUT2 region). We implement a RD Design using this threshold and find a significant effect of the cut-off on NGE as well as treatment effects that are larger than our main estimates, although very imprecisely estimated (see Online Appendix Table A9). For example, a ten percentage point increase in NGE causes an (insignificant) 19% increase in employment compared to 10% in our baseline.[35]

Finally, we conducted a large number of other robustness tests such as using a longer time period (from 1986 onwards instead of 1997; examining general equilibrium effects on factor prices -wages- and using matching estimators. Our results are robust to these tests.[36]

VI. MICRO-ANALYSIS AT FIRM AND PLANT LEVEL AND OVERALL MAGNITUDES

Having established that there appears to be a causal effect of increasing jobs (and reducing unemployment) in those areas that became eligible for higher rates of RSA subsidy, we now turn to the micro-economic impact of RSA at the plant and firm level.

VI.A Extensive vs. Intensive Margins: Number of Plants as an outcome

The area level employment effects could come from incumbents expanding (the intensive margin), higher net entry (the extensive margin of less exit or more entry) or a mixture of both. To address this we re-estimate the main specifications, but use the ln(number of manufacturing plants) as the dependent variable. Panel A of Table 9 reports the baseline results for the specifications of Table 4 where the treatment variable is NGE, Panel B has those for Table 5 (RSA subsidy amounts), Panel C has Table 6 (NGE and inclusion of Structural Funds) and Panel D has the analog of Table 8 (NGE at higher Travel to Work Areas). The policy does appear to have positive effects on the extensive margin, although the IV coefficients are insignificant in all panels except Panel A. We conclude from this table that the primary effect of the policy must be on the intensive margin, increasing jobs in incumbent firms, which we now turn to analyze explicitly.

[35] Another reason for the higher point estimates is the 75% of per capita GDP is also the threshold for receipt of Objective One structural Funds. Online Appendix F discusses various other RD Designs. For example, we also considered an alternative approach involving conditioning on polynomials of all the rules pre and post 2000. These produce significant and correctly signed coefficients on the policy variables that are larger in magnitude than the OLS estimates, but smaller than our preferred IV results (see Online Appendix Table A10).

[36] Details in in Online Appendix Table A11 and Online Appendix F5.

VI.B Heterogeneous policy effects by firm size

Table 10 presents ln(employment) regressions where the treatment variable continues to be the maximum investment subsidy available in the area where a plant is located (NGE). The IV results of column (4) of Panel A implies that an increase of NGE by 10 percentage points leads to a 4.7% increase in plant level employment. We also find a large difference between the OLS and IV coefficients that is consistent with strong selection effects at the plant level.

The discussion in Section II implied that the treatment effects could be more pronounced for smaller firms, so we examine size as one observable source of heterogeneous treatment effects. We use lagged *firm* employment as a measure of size when splitting the plant sample as credit constraints or the gaming of the system depends on the size of the firm, not the plant per se (e.g. a ten-worker factory owned by General Electric still benefits from GE's deep financial pockets). In addition, to mitigate endogeneity biases we measure size using the firm's employment level in 1996 - the year before the start of our estimation period (for firms born after 1996 we use size in the first year and drop this observation from the regressions).

We report plant level employment regressions separately for small firms (firm employment under 50) and large firms (over 50 employees) in Panels B and C respectively in Table 10. The first stages are strong for both types of firms with a near identical coefficient (0.68 vs. 0.64). However, the IV effect is positive and significant for plants in small firms but insignificant and around a sixth of the size for plants in big firms.[37] Similarly, there is a large and significant reduced form effect in column (2) for small firms but a small and insignificant effect for large firms.[38] This implies that plants that are part of small firms drove the aggregate area effect identified in the previous section.

There could be at least two different reasons for the heterogeneity of the policy effect by firm size. Firstly, although large firms are often based in areas that receive support – hence the highly significant first stage in column (3) – the size of their grants could be relatively less generous. An alternative story is that they are equally well supported, but the subsidies generate less jobs. We explored this by examining regressions of employment on actual RSA support.[39] Regardless of whether we use a dummy or a continuous treatment indicator there is a large and significant positive effect of receiving investment subsidies when estimated by IV for small

[37] In Online Appendix Table A12 we vary the exact definition of a "small" firm and show our results are robust to varying the exact size threshold.
[38] The effects are also significantly different at the 5% level for large firms vs. small firms (see Online Appendix Table A13 column (1)).
[39] Online Appendix Table A14 is the analog of Table 5.

firms, but not for large firms. Hence, these results reject the hypothesis that the absence of a large firm effect is because they simply obtain less subsidies, but it is rather that small firms create more jobs from the subsidies they receive compared to big firms.[40]

What could explain the different treatment effects between small and large firms? One possibility is that small firms might be (more) financially constrained than larger ones. With asymmetric information between borrower and lender, young firms will be at a disadvantage because credit markets will have less time to observe their performance. Recent evidence, however, stresses that although there is a correlation between youth and size, many small firms are not young (Haltiwanger et al., 2013). A simple test of the credit constraint hypothesis is to interact the treatment effects with firm age since younger firms are more likely to be subject to credit constraints. We ran IV employment regressions where we include interactions between NGE support level with both indicators capturing (i) whether the firm is small and (ii) whether the firm is young (using different definitions for young).[41] We instrument these treatment variables by including the equivalent interactions between the rule change instrument and the respective indicators. The interaction between the support level (NGE) and (small) size is always significant and positive whereas the interaction between NGE and being young is insignificant (and actually negative) and this finding is robust to the exact measure of being young. Since young firms respond *less* to the policy, the bigger program effect for small firms does not seem consistent with a simple financial constraints story.

An alternative explanation of these results is that large firms might have more scope to "game" the system; i.e. receive the subsidy without actually being constrained by the requirements of the program to create jobs. For instance, they might have more scope to pretend to create jobs while actually reducing employment in another location of the business.[42] Although we do not have direct evidence of this, this explanation is consistent with the pattern of results described above.

[40] An objection is that the relevant quantity is not the *elasticity* of employment with respect to a subsidies for large vs. small firms, but rather the marginal effect on the absolute number of jobs created with respect to a $1 increase in subsidy. Online Appendix Table A15 conducts this analysis and shows that a $1 of subsidy to a small firm still creates over eight times as many jobs as $1 of subsidy to a large firm according to our estimates.

[41] See Online Appendix Table A13.

[42] Recall from Section II that absent the requirement to create or safeguard jobs the RSA is effectively a subsidy to capital and might reduce the firm's choice of employment depending on the elasticity of substitution between labor and capital.

VI.C Firm level Results: Employment, Capital and Productivity

We report regressions at the *firm* level in Table 11, motivated by two considerations. First, it could be the case that the nationwide effect is zero if multi-plant firms are simply switching jobs within the firm across eligible and ineligible areas. Secondly, there are richer data at the firm level from official production surveys (the ARD) including output, capital and materials for a stratified random subsample of firms.

In the UK, data on investment, output and materials are reported at the firm level rather than the plant-level.[43] For most firms the firm and plant-level coincide - on average 80% of our observations are single plant firms. Employment, our main outcome of interest, is always available at the plant level in the IDBR data and we know the location of all plants within multi-plant firms. To examine firm-level outcomes (such as investment) which are unavailable at the plant level, we simply aggregate NGE across all plants using lagged plant employment shares within the firm as weights.

Panel A of Table 11 reports employment regressions at the firm level using the IDBR population. These are very similar to the plant level results, suggesting that within firm re-allocation across plants in response to the policy is not a major issue. In the other panels we use the ARD data that has information on other outcomes such as investment. In Panel B we report results for employment estimated using the ARD sub-sample and confirm our earlier finding of a positive causal impact on jobs. In Panel C we find larger impacts on capital investment than we did for employment consistent with the simple theory model in Section II. Panel D shows that there is also an impact on output. Finally, Panel E uses a Solow residual based TFP measure (for more details on the calculation see Online Appendix C) and finds no significant effect of the policy. We looked at a variety of other methods of calculating TFP, but in no case do we find a significant impact on productivity (see Online Appendix Table A16). There were also no significant program effects on wages.[44]

Motivated by the theory in Section II – suggesting that more capital-intensive firms are more responsive to the policy - we interacted the treatment effects with a dummy for whether the firm had a high level of capital costs in revenues prior to the 2000 policy change. Consistent with the model, firms where the capital share was high (big s_K) had stronger positive employment effects.

[43] We call this the firm level, *j*, but there could be many reporting units in one large firm.
[44] For example, when we replaced the dependent variable by wages in the reduced form of column (2) the coefficient on the policy rule IV was 0.287 with a standard error of 0.877. This is consistent with the absence of an area level wage effect of NGE (see Appendix F.5).

The interaction of the rule change IV and a dummy for high capital share firms had a coefficient (standard error) of 0.525(0.200) in the employment reduced form.[45]

VI.D Magnitudes

To consider the overall magnitude of the impact of RSA we consider what would have happened if, instead of re-drawing the map in 2000, the program had simply been abolished. Appendix G gives details of the calculations. We start with the IV coefficient from column (4) of Table 4 Panel A of 0.953 (indicating that a 10 percentage point NGE investment subsidy would increase area-level manufacturing employment by 10%) and consider the area by area change in NGE (to zero) given employment levels. This calculation suggests a loss of just under 156,000 jobs. The nominal average annual cost of RSA was about £164m. Using official estimates of administrative costs (17% of the aggregate grant value)[46] and a deadweight cost of taxation of 50%, this implies a total annual cost of £288m. This leads to a "cost per job" of £1,846 (=288/0.156), or $3,541 (at 2010 prices). If we took the more conservative OLS estimates from column (1) which has a treatment effect of 0.124, we get smaller job effects of just under 22,400 and the cost per job would be £12,857 (or $24,662). Since there do not appear to be large substitution effects from neighboring non-eligible areas, these do not need to be scaled down.

In Appendix G we provide figures for the limited number of studies that report cost per job for similar policies to those we examine here. Two methodological differences help partly explain our lower cost per job numbers. First, three area-based studies (Busso and Kline, 2008; Busso et al., 2010 and Freedman, 2012) do not use IV.[47] As noted already, we find much larger effects correcting for endogeneity using IV. Second, the three other studies only have estimates at the firm level. When we take into account that we find zero effects of RSA on large firms, we obtain a cost per job of $26,572, higher than the US figure in Brown and Earle (2017), but lower than the two Italian studies (Pellegrini and Muccigrosso, 2017, and Cerqua and Pellegrini, 2014). In addition to these methodological differences, the RSA program is different from the other studies in that it subsidises capital and not labor, and the government agency selects firms who

[45] See Online Appendix Table A17 (a generalization of column (2) in Table 11 Panel B).

[46] We use the administrative reports of the grants awarded averaging £164m and add to this the estimations from the National Audit Office (2003) that there were 10% spent in government administration costs for RSA, and an average 7% cost to firms in application and management costs. Note that our implied jobs effects are much larger than those found in the existing evaluations of the RSA policy surveyed National Audit Office (2003) and Wren (2005). We believe this is because no other study has exploited the exogenous changes in RSA eligibility to deal with the downward endogeneity bias.

[47] Cost per job figures for Busso and Kline (2008) are reported in Bartik (2010) and for Busso et al. (2010) in Glaeser and Gottlieb (2008).

can show evidence of job additionality rather than providing subsidies for all eligible firms that locate in supported areas.

The cost per job is, of course, far from a welfare calculation, as we are not factoring in other distortions such as the dampening effect on aggregate productivity of keeping open less productive firms and the usual static deadweight losses from capital subsidies. On the other hand, there are likely to be first-order benefits from the fact that RSA significantly reduces unemployment by reducing job losses in the manufacturing sector. So overall, these calculations suggest a more positive assessment of this selective place-based industrial policy than the existing literature.

VI.E "Big Push": Asymmetries of subsidy removal?

Recent work on place-based policies have emphasized that their long-run success depends on whether there are big dynamic effects (e.g. Kline and Moretti, 2014a). Is continued support needed in order to achieve lasting gains in employment or can a "big push" move an area into a new equilibrium were employment gains continue even after the subsidy has been removed? We can find no evidence for the big push hypothesis in our data for manufacturing employment or unemployment. For example, in one experiment, we defined a series of dummy variable for the NGE amount and length of time that an area had received RSA support and interacted these with our treatment effects, but there was no significant heterogeneity in this dimension.[48]

We also tried differentiating between areas that experienced an increase compared to a decrease in investment subsidies in 2000. The big push story suggests that areas losing subsidies should have less of a negative jobs effect than the positive effect of places gaining subsidies. We found that areas which lost subsidies had just as much of a negative effect (if not more) than areas which became eligible for subsidies.[49]

The absence of dynamic effects could be because the RSA policy is much less intense than the Tennessee Value Authority studied by Kline and Moretti (2014b) - it does not include

[48] For example, we created a dummy variable equal to one if an area received the maximum investment subsidy rate (NGE=30%) continuously between 1986 and 1999 (and zero otherwise) and interacted this with support level treatment. When included in the employment regression of column (4) of Table 4 (alongside the linear dummy), this interaction variable had an insignificant coefficient (standard error) of 0.121 (0.390). As an instrument for the interaction we use the interaction between rule change instrument and the 30% NGE indicator.

[49] For example, we ran our standard IV regressions of the form: $\Delta \ln y_{r,t} = \beta_1 \left[I\{\Delta NGE_{r,t} \leq 0\} \times \Delta NGE_{r,t} \right] + \beta_2 \left[(1 - I\{\Delta NGE_{r,t} \leq 0\}) \times \Delta NGE_{r,t} \right] + \alpha_t + \epsilon_{r,t}$ where, $I\{\Delta NGE_{r,t} \leq 0\}$ is an indicator variable equal to one if NGE falls in value. We instrumented these with the usual rule change instrument interacted with whether it increased or decreased. For both employment and unemployment as an outcome, areas which lost subsidies had significantly lower jobs (and higher unemployment). These coefficients where not significantly smaller than for the areas which gained subsidies.

infrastructure, for example. Nevertheless, our evidence does not seem supportive of the view that support of regions through this type of policies is likely to be transformational.

VII. CONCLUSIONS

There are surprisingly few micro-econometric analyses of the causal effects of industrial policies, despite their ubiquity across the world. In this paper, we have examined one business support policy – Regional Selective Assistance (RSA). We use exogenous changes in the eligibility of areas to receive investment subsidies driven by EU rule changes determining which areas were eligible for investment subsidies. When we correct for endogeneity we find evidence for a positive treatment effect on jobs in the eligible areas and on employment. We also find that the program effects are strong for smaller firms but effectively zero for larger firms. This is consistent with large firms being able to "game" the system and/or financial constraints being unimportant for these firms (although we do not find much evidence for this latter hypothesis). Interestingly, this stronger effect of business support policies on smaller firms is found in many other studies.[50] The fact that the treatment effect is confined to smaller firms strengthens arguments for restricting subsidies that go to larger enterprises, although one must be careful that this does not create strong disincentives for firms to grow (as they may forfeit such size related subsidies – see Garicano et al. 2016).

At the area level we also find that the program reduced unemployment and raised manufacturing employment mainly in the intensive margin (rather than the number of firms – the extensive margin). The positive effects on participants' employment was not due to equal and offsetting falls in employment in non-participants, non-eligible neighboring areas or sectors who were not covered by the scheme. Finally, we find no effects on (total factor) productivity. From a policy perspective, the fact that the subsidies were effective in raising employment and investment in these deprived areas at a modest "cost per job" should be regarded as a positive outcome. Although measured aggregate productivity falls as the RSA supported firms were on average less productive (creating a distortion through misallocation, as in Hsieh and Klenow, 2009, for example), this probably carries a modest welfare cost compared to the counterfactual where these employees enter unemployment (rather than being reallocated to firms that are more productive). Given the severe economic stress affecting some local communities with formerly

[50] For example, Howell (2017), Zwick and Mahon (2017) and Wallsten (2000) for the US, Gorg and Strobl (2007) for Ireland, Lach (2002) for Israel, Bronzini and Iachini (2014) for Italy, González et al. (2005) for Spain and Dechezlepretre et al. (2018) for the UK.

large manufacturing sectors (and the political implications of this), understanding the impact of the type of policy we have examined here is, in our view, very important.

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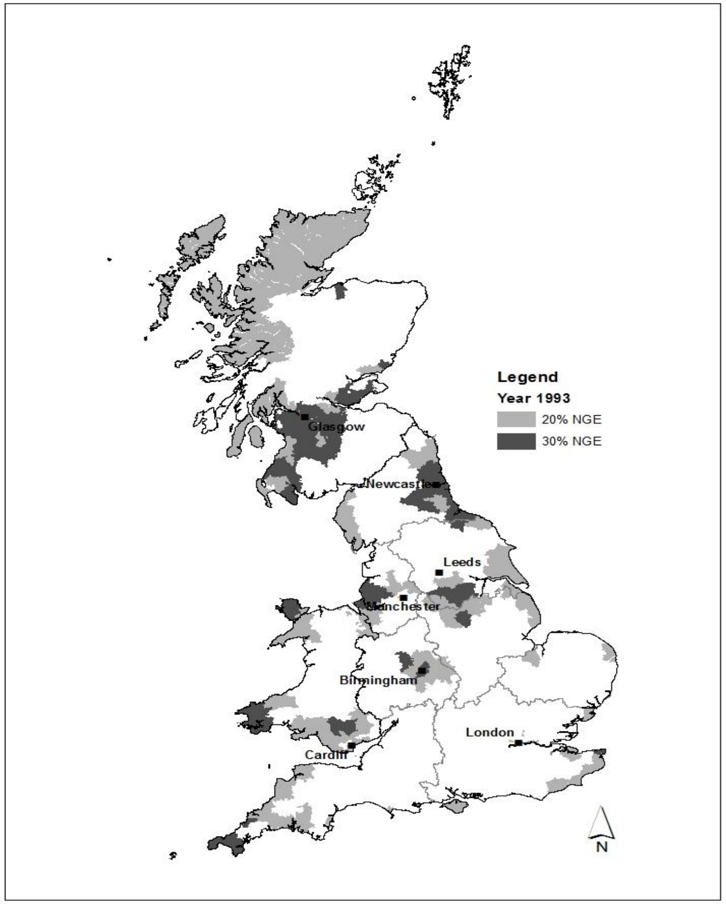
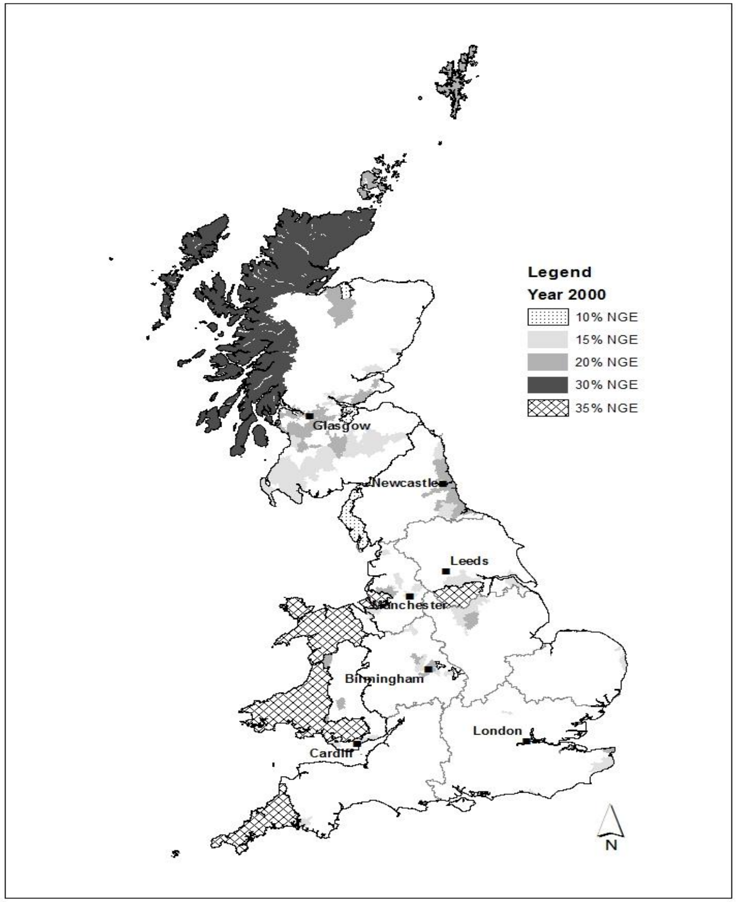
Figure 1: The Change in the level of maximum investment subsidy (NGE) between 1993 (left hand side) and 2000 (right hand side)

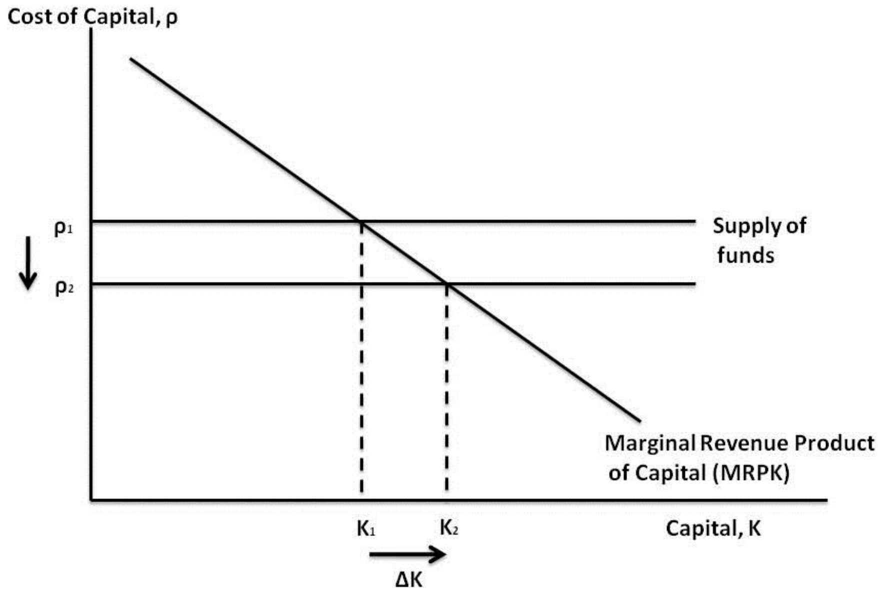




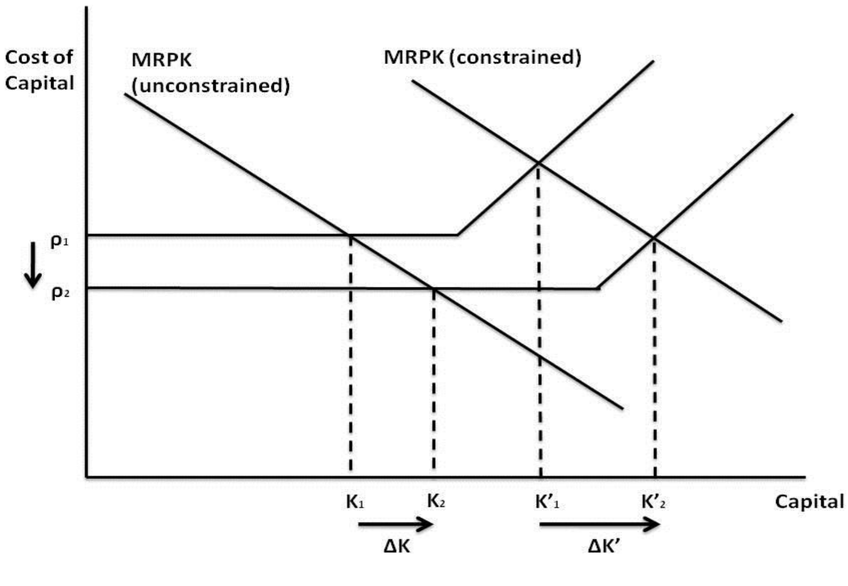
Notes: The shaded areas are those that are eligible for some Regional Selective Assistance. In the 1993-1999 period, the dark shaded areas are the very deprived areas eligible for an investment subsidy of up to 30% NGE (the maximum investment subsidy, Net Grant Equivalent). The light shaded areas are eligible for up to 20% NGE. After 2000 Tier 1 areas had 35% NGE and Tier 2 areas ranged between 10% and 30%.
Source: Department of Business, Innovation and Skills "Industrial Development Reports", various years.

Figure 2: Effects of the RSA policy on capital

Panel A – Perfect Capital Markets

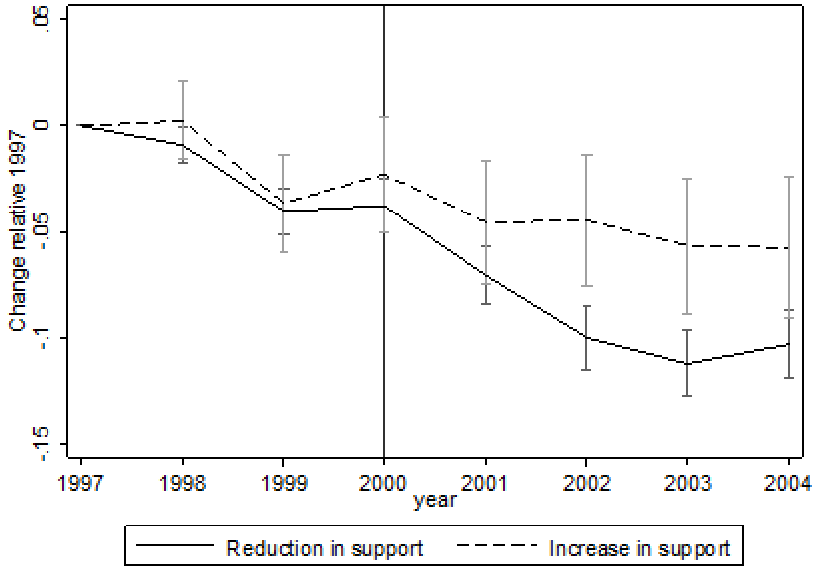


Panel B – Imperfect Capital Markets



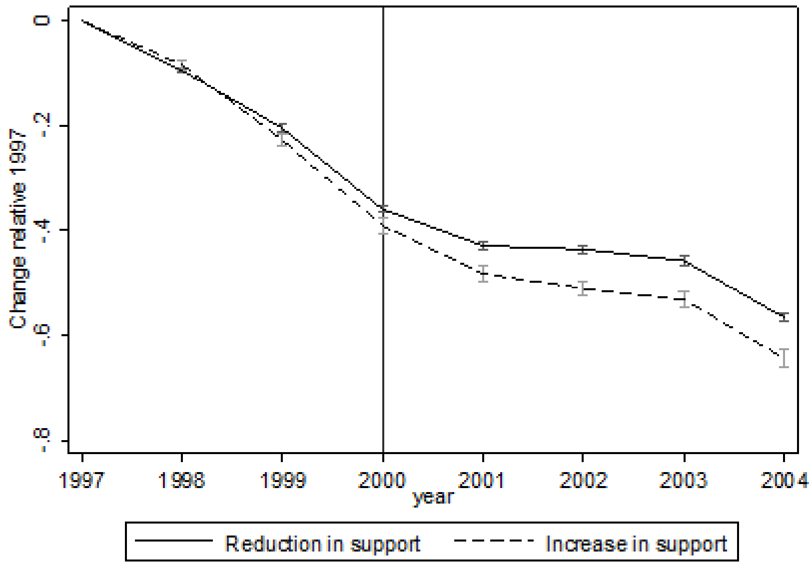
Notes: These figures examine the theoretical effect of the RSA policy reducing the cost of capital with perfect capital markets (Panel A) and imperfect capital markets (Panel B). For affected firms this is likely to raise capital, but the extent to which it does so will depend on a variety of factors such as whether a firm is financially constrained or more closely monitored (see text).

Figure 3: Changes in manufacturing employment in areas with increasing vs. decreasing support probability



Notes: Average changes relative to base year of 1997 in ln(employed) in a geographical area ("ward"). The dashed line shows average employment in wards that had an increase in support (as predicted by our policy rule IV). The solid line is average manufacturing employment in wards that had a decrease in support (as predicted by our policy rule IV). 95% confidence bands also shown. The vertical line in 2000 shows when the change in policy occurred.

Figure 4: Changes in unemployment in areas with increasing vs. decreasing support probability



Notes: Average changes relative to base year of 1997 in ln(number of unemployed) in a geographical area ("ward"). The dashed line shows average unemployment in wards that had an increase in support (as predicted by our policy rule IV). The solid line is average unemployment in wards that had a decrease in support (as predicted by our policy rule IV). 95% confidence bands also shown. Unemployment is measured by those claiming Unemployment Insurance (Job Seekers Allowance). The vertical line in 2000 shows when the change in policy occurred.

Table 1: Number of areas (wards) eligible for different maximum investment subsidies (NGE) pre and post policy change in 2000

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Rate after 2000					
	Rate in 1993-99	***Total***	***0%***	***10%***	***15%***	***20%***	***30%***	***35%***
(1)	***0%***	7,309	6,823	26	192	72	15	181
(2)	***20%***	2,012	841	102	539	33	118	379
(3)	***30%***	1,416	265	16	30	717	0	388
	Total	10,737	7,929	144	761	822	133	948

Notes: Table shows the numbers of wards in different regimes before and after the 2000 policy change. For example, it illustrates that of the 1,416 wards who were eligible for the maximum investment subsidy of 30% pre-2000 (column (1)), 388 became eligible for the maximum subsidy of 35% after 2000 (column (7)) and 265 lost their eligibility for subsidies completely (column (2)).

Table 2: Descriptive Statistics

	Years	Mean	Std. Dev.	Obs.	#Units
Panel A. All areas					
NGE (Maximum Investment Subsidy %)	97-99	0.1	0.1	32,211	10,737
	00-04	0.1	0.1	53,685	10,737
Average RSA Payment (£)	97-99	18,265.2	197,955	32,211	10,737
	00-04	9,837.1	134,036	53,685	10,737
Total Unemployment (claimant count)	97-99	113.5	150.6	32,211	10,737
	00-04	80.9	112.5	53,685	10,737
Manufacturing Employment	97-99	267.4	626.2	32,211	10,737
	00-04	233.0	535.9	53,685	10,737
Panel B. Areas Eligible for RSA subsidies					
NGE (Maximum Investment Subsidy %)	97-99	0.2413	0.0492	10,284	3,428
	00-04	0.2367	0.08896	14,040	2,808
Average RSA Payment (£)	97-99	56,132.2	346,827	10,284	3,428
	00-04	35,712.8	260,039	14,040	2,808
Total Unemployment (claimant count)	97-99	161.85	179.04	10,284	3,428
	00-04	123.85	147.95	14,040	2,808
Manufacturing Employment	97-99	350.96	823.91	10,284	3,428
	00-04	338.10	745.40	14,040	2,808
Panel C. Plant –Level					
Plant employment across all areas	97-99	21.2	102.30	406,615	167,415
	00-04	19.8	91.90	631,089	183,061
Plant employment across eligible areas	97-99	27.4	139.00	131,431	53,575
	00-04	26.8	125.85	176,902	50,926
Panel D. Characteristics of recipients and non-recipients in eligible areas (£), Firms					
Employment of recipients	97-04	87.2	323.4	16,413	4,550
Employment of non- recipients	97-04	31.3	199.6	188,899	39,308
Investment of recipients	97-04	1,717.0	105,686	3048	1,488
Investment of non- recipients	97-04	953.3	8,001	15,314	7,449
(Value added/ worker) - recipients	97-04	38.6	25.8	3048	1,488
(Value added/worker) non recipients	97-04	44.9	231.51	15,314	7,449
TFP of recipients (Indexed to industry × year average in logs)	97-04	-0.042	0.371	3,048	1,488
TFP of non- recipients (Indexed to industry × year average in logs)	97-04	-0.015	0.414	15,314	7,449

Notes: TFP is computed using a Solow residual "factor share" method and relative to an industry × year average (see Appendix C).

Table 3: Estimates of parameters on eligibility rule changes

	(1)	(2)	(3)	(4)
	Main specification		**Restricted variables**	
Year	**1993**	**2000**	**1993**	**2000**
Dependent Variable: level of NGE ordered variable				
GDP per capita	-0.022	-0.040	-0.034	-0.055
	(0.002)	(0.002)	(0.002)	(0.002)
Population density	-0.028	-0.034	-0.043	-0.015
	(0.002)	(0.002)	(0.002)	(0.002)
Share of high skilled workers	-0.584	-1.438	-0.904	-2.268
	(0.129)	(0.149)	(0.125)	(0.142)
Business Start-up rate	-2.414		-0.490	
	(0.240)		(0.183)	
Structural unemployment rate	83.251	32.681		
	(2.483)	(2.315)		
Activity rate	-1.147	-1.934	-1.235	-1.879
	(0.250)	(0.263)	(0.237)	(0.252)
Employment rate		-8.201		-11.259
		(0.462)		(0.444)
Current unemployment rate	-9.148	18.276	84.330	
(claimant count)	(3.240)	(3.565)	(1.846)	
ILO unemployment rate		-5.682		-0.122
		(0.824)		(0.760)
Long-duration unemployment	0.472		5.501	
Rate	(1.216)		(1.163)	
Share of manufacturing workers		-1.122		-1.870
		(0.202)		(0.196)
Observations (wards)	10,737	10,737	10,737	10,737
Cut-off 10%	0.000	-9.503	-0.579	-14.697
	(0.220)	(0.420)	(0.210)	(0.377)
Cut-off 15%				
	1.202	-9.426	0.478	-14.629
Cut-off 20%	(0.221)	(0.420)	(0.210)	(0.377)
Cut-off 30%		-8.938		-14.201
		(0.419)		(0.375)
Cut-off 35%				
		-8.272		-13.600
Log Likelihood	-5525.405	-6879.521	-6126.595	-7325.151

Notes: Coefficients (robust standard errors) from Ordered Probits of NGE maximum support categories for 1993 and 2000. Dependent variable in columns (1) and (3) takes values of 1 to 3 depending on the level of NGE (zero, 20% and 30%) and in columns (2) and (4) a value of 1 to 6 (zero, 10%, 15%, 20%, 30% and 35%). See Online Appendix Table A2 for variable definitions. "Restricted variables" drops (collinear) structural unemployment in columns (3) and (4) and claimant unemployment in column (4).

Table 4: Area Level regressions – Instrumenting Maximum Investment Subsidies (NGE) with Rule change

Method	(1) OLS	(2) Reduced Form	(3) First Stage	(4) IV
A. Dependent variable: ln(Manufacturing Employment)				
Maximum investment subsidy	0.124			0.953
NGE	(0.070)			(0.260)
Policy Rule Instrument		0.839	0.881	
		(0.228)	(0.033)	
B. Dependent variable: ln(Unemployment)				
Maximum investment subsidy	-0.137			-0.414
NGE	(0.024)			(0.078)
Policy Rule Instrument		-0.365	0.881	
		(0.069)	(0.033)	
C. Dependent variable: ln(Non-Manufacturing Employment)				
Maximum investment subsidy	0.006			0.177
NGE	(0.044)			(0.161)
Policy Rule Instrument		0.156	0.881	
		(0.141)	(0.033)	
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896

Notes: Standard errors (in parentheses below coefficients) are clustered at the area (ward) level. NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. All columns include a full set of linear (lagged) characteristics used to define eligibility in 1993 ($X_{r,93}$). The time period is 1997-2004. Policy Rule instrument is described in text. All variables are in differences relative to the base year of 1997.

Table 5: Area Level – Instrumenting amount of subsidy with Rule change

	(1)	(2)	(3)	(4)
Method	OLS	Reduced Form	First Stage	IV
A. Dependent variable: ln(Manufacturing Employment)				
ln(RSA subsidy)	0.012			0.288
	(0.002)			(0.134)
Policy Rule Instrument		0.839	2.909	
		(0.228)	(1.140)	
B. Dependent variable: ln(Unemployment)				
ln(RSA subsidy)	-0.002			-0.125
RSA	(0.001)			(0.053)
Policy Rule Instrument		-0.365	2.909	
		(0.069)	(1.140)	
C. Dependent variable: ln(Non-Manufacturing Employment)				
ln(RSA subsidy)	0.001			0.054
RSA	(0.002)			(0.052)
Policy Rule Instrument		0.156	2.909	
		(0.141)	(1.140)	
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896

Notes: Standard errors (in parentheses below coefficients) are clustered at the area (ward) level. RSA subsidy is the amount of subsidy (in thousands of pounds) that an area receives on average per year; i.e. for every area and for the post- and pre-2000 period we sum the subsidy amount and divide by the number of years in the period. All columns include a full set of linear (lagged) characteristics used to define eligibility in 1993 ($X_{r,93}$). The time period is 1997-2004. Policy Rule instrument is described in text. All variables are in differences relative to the base year of 1997.

Table 6: Area Level regressions accounting for Structural Funds (SF)

	(1)	(2)	(3)	(4)	(5)
Method	OLS	Reduced Form	First Stage NGE	First Stage SF	IV
A. Dependent variable: ln(Manufacturing Employment)					
Maximum investment	0.098				0.999
Subsidy, *NGE*	(0.081)				(0.328)
Structural Fund, *SF*	0.038				-0.029
	(0.037)				(0.079)
NGE IV		0.792	0.816	0.805	
		(0.224)	(0.034)	(0.067)	
Structural Fund IV		0.094	0.124	1.029	
		(0.059)	(0.011)	(0.026)	
B. Dependent variable: ln(Unemployment)					
Maximum investment	-0.099				-0.409
subsidy, *NGE*	(0.027)				(0.098)
Structural Fund	-0.061				-0.050
	(0.012)				(0.027)
NGE IV		-0.374	0.816	0.805	
		(0.066)	(0.034)	(0.067)	
Structural Fund IV		-0.103	0.124	1.029	
		(0.021)	(0.011)	(0.026)	
Number of areas (wards)	10,737	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896	85,896

Notes: Standard errors (in parentheses below coefficients) are clustered at the area (ward) level. NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. SF ("Structural Funds") is a dummy variable equal to 1 if an area is eligible for SF support. NGE IV is the policy rule change instrument we introduced before. Structural Funds IV is a rule change instrument that is computed in a similar way as NGE IV, except that rather than NGE eligibility we use SF eligibility (see text). All columns include a full set of linear (lagged) characteristics used to define eligibility in 1993. The time period is 1997-2004. All variables are in differences relative to the base year of 1997.

Table 7: Controlling for Other policies (in Employment Regressions)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable: Ln(Manufacturing Employment)								
A. Reduced Form								
Policy Rule IV	0.900	0.824	0.840	0.837	0.813	0.831	0.815	0.768
	(0.228)	(0.228)	(0.228)	(0.228)	(0.229)	(0.228)	(0.231)	(0.232)
Employment	-0.037						-0.024	-0.029
Zones	(0.025)						(0.025)	(0.025)
Coalfield		-0.052					-0.057	-0.058
Regeneration		(0.021)					(0.020)	(0.020)
Trust								
Regional Venture			0.031				0.023	0.026
Capital Funds			(0.018)				(0.018)	(0.018)
Enterprise Grants				-0.003			-0.009	-0.012
				(0.015)			(0.016)	(0.016)
New Deal for					-0.046		-0.057	-0.051
Communities					(0.023)		(0.023)	(0.023)
Devolution to						-0.029	-0.041	-0.055
Wales & Scotland						(0.020)	(0.021)	(0.022)
Structural Fund								0.134
IV								(0.063)
B. IV								
NGE	1.090	0.943	0.954	0.972	0.914	0.931	0.966	0.895
	(0.277)	(0.262)	(0.260)	(0.266)	(0.259)	(0.255)	(0.274)	(0.319)
Employment	-0.074						-0.048	-0.053
Zones	(0.028)						(0.027)	(0.028)
Coalfield		-0.03					-0.040	-0.040
Regeneration		(0.022)					(0.021)	(0.021)
Trust								
Regional Venture			0.044				0.032	0.034
Capital Funds			(0.018)				(0.018)	(0.018)
Enterprise Grants				0.033			0.018	0.019
				(0.019)			(0.018)	(0.018)
New Deal for					-0.059		-0.080	-0.077
Communities					(0.023)		(0.023)	(0.024)
Devolution to						-0.072	-0.075	-0.076
Wales & Scotland						(0.023)	(0.023)	(0.022)
Structural Fund								0.055
								(0.075)
Number of areas	10,737	10,737	10,737	10,737	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896	85,896	85,896	85,896	85,896

Notes: Standard errors (in parentheses below coefficients) are clustered at the area (ward) level. NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. The time period is 1997-2004. NGE Policy Rule IV is described in text. Panel A has a specification identical to column (2) in Panel A of Table 4 except additional policy variables have been included (see text). Panel B has a specification identical to column (4) in Panel A of Table 4 except additional policy variables have been included (see text). All variables are in differences relative to the base year of 1997.

Table 8: Higher level of aggregation - Travel to Work Area (TTWA)

	(1)	(2)	(3)	(4)
Method	OLS	Reduced Form	First Stage	IV
A. Dependent variable: ln(Manufacturing Employment)				
Maximum investment subsidy	0.538			1.006
NGE	(0.114)			(0.319)
Policy Rule Instrument		1.053	1.047	
		(0.362)	(0.222)	
B. Dependent variable: ln(Unemployment)				
Maximum investment subsidy	-0.263			-0.803
NGE	(0.062)			(0.265)
Policy Rule Instrument		-0.840	1.047	
		(0.249)	(0.222)	
Number of areas (TTWAs)	322	322	322	322
Observations	2,576	2,576	2,576	2,576

Notes: Standard errors (in parentheses below coefficients) are clustered at the TTWA. NGE ("Net Grant Equivalent") is the level of the employment weighted average maximum investment subsidy rate in the area. Standard errors below coefficients are clustered by area (TTWA level) in all columns. All columns include a full set of linear (lagged) characteristics used to define eligibility in 1993. The time period is 1997-2004. Policy Rule instrument is described in text. All variables are in differences relative to the base year of 1997.

Table 9: Number of Manufacturing Plants as an outcome

	(1)	(2)	(3)		(4)
Dependent Variable:	**Ln(Number of manufacturing Plants)**				
Method:	**OLS**	**Reduced Form**	**First Stage**		**IV**
A. Baseline					
Maximum investment subsidy	-0.025				0.209
NGE	(0.033)				(0.106)
Policy Rule Instrument		0.184	0.881		
		(0.094)	(0.033)		
B. RSA subsidy levels					
ln(RSA subsidy)	0.003				0.063
	(0.001)				(0.039)
Policy Rule Instrument		0.184	2.909		
		(0.094)	(1.140)		
Number of areas (wards)	10,737	10,737	10,737		10,737
Observations	85,896	85,896	85,896		85,896
C. Including Structural Funds in baseline			**First Stage**		
			NGE	**SF**	
Maximum investment	-0.062				0.097
Subsidy, *NGE*	(0.036)				(0.135)
Structural Fund	0.037				0.047
	(0.017)				(0.034)
NGE IV		0.117	0.816	0.805	
		(0.093)	(0.034)	(0.067)	
Structural Fund IV		0.060	0.124	1.029	
		(0.026)	(0.011)	(0.026)	
Number of areas (wards)	10,737	10,737	10,737		10,737
Observations	85,896	85,896	85,896		85,896
D. Travel to Work Area					
Max. investment subsidy,	-0.036				0.029
NGE IV	(0.053)				(0.126)
Policy Rule Instrument		0.030	1.047		
		(0.132)	(0.222)		
Number of areas (wards)	322	322	322		322
Observations	2,576	2,576	2,576		2,576

Notes: NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. The time period is 1997-2004. Policy Rule IV is described in text. Panel A has a specification identical to Panel A of Table 4 except the dependent variable is the ln(Number of manufacturing plants in area+1). Panel B corresponds to Table 5, Panel C to Table 6 and Panel D to Table 8. All variables are in differences relative to the base year of 1997.

Table 10: Plant Level employment regressions, splits by firm size

	(1)	(2)	(3)	(4)
Dependent variable:	**ln(Manufacturing Employment)**			
Method	**OLS**	**Reduced Form**	**First Stage**	**IV**
A. Pooled across all plants, 653,385 observations on 96,768 plants; 9,975 wards				
Maximum investment subsidy	0.011			0.463
NGE	(0.025)			(0.089)
Policy Rule Instrument		0.312	0.675	
		(0.058)	(0.040)	
B. Small (Plants in Firm with under 50 employees), 594,356 observations on 87,728 plants; 9,883 wards				
Maximum investment subsidy	0.006			0.441
NGE	(0.026)			(0.095)
Policy Rule Instrument		0.299	0.678	
		(0.063)	(0.040)	
C. Large (Plants in Firms with over 50 employees), 59,025 observations on 9,036 plants; 3,708 wards				
Maximum investment subsidy	0.027			0.070
NGE	(0.055)			(0.203)
Policy Rule Instrument		0.045	0.642	
		(0.130)	(0.050)	

Notes: NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. Standard errors below coefficients are clustered by area (ward level) in all columns. All columns include a full set of linear (lagged) characteristics used to define eligibility in 1993. The time period is 1997-2004. Policy Rule instrument is described in text. All variables are in differences relative to the base year of 1997.

Table 11: Firm Level – Effects on jobs, investment, output and TFP. Instrumenting maximum investment subsidy with Rule change

Method	(1) OLS	(2) Reduced Form	(3) First Stage	(4) IV
A. Dependent variable: ln(Employment),Full Sample (449,514 observations, 91,546 firms)				
NGE	0.039			0.670
	(0.024)			(0.078)
Policy Rule Instrument		0.493	0.735	
		(0.057)	(0.011)	
B. Dependent variable: ln(Employment), ARD sub-sample (45,511 observations, 21,389 firms)				
NGE	0.163			0.564
	(0.057)			(0.168)
Policy Rule Instrument		0.444	0.787	
		(0.132)	(0.032)	
C. Dependent variable: ln(Capital Investment), ARD sub-sample (45,511 observations, 21,389 firms)				
NGE	0.249			1.668
	(0.304)			(0.750)
Policy Rule Instrument		1.313	0.787	
		(0.588)	(0.032)	
D. Dependent variable: ln(Output), ARD sub-sample (45511 observations, 21389 firms)				
NGE	0.031			0.399
	(0.065)			(0.182)
Policy Rule Instrument		0.314	0.787	
		(0.143)	(0.032)	
E. Dependent variable: ln(TFP), ARD sub-sample (45,511 observations, 21,389 firms)				
NGE	-0.034			-0.071
	(0.043)			(0.099)
Policy Rule Instrument		-0.056	0.787	
		(0.078)	(0.032)	

Notes: Standard errors below coefficients are clustered by area (ward level) in all columns. Policy Rule instrument is described in text. The time period is 1997-2004. TFP is computed using a “factor share” method and relative to an industry × year average (see Appendix C). All columns include a full set of linear (lagged) characteristics used to define eligibility in 1993. All variables are in differences relative to the base year of 1997.

ONLINE APPENDIX TO[1]
"Some causal effects of an industrial policy" by Chiara Criscuolo, Ralf Martin, Henry G. Overman and John Van Reenen

APPENDIX A: MORE DETAILS OF THE RSA POLICY

During the period of our study, Regional Selective Assistance (RSA) was the main business support scheme in the UK.[2] Since the early 1970s, RSA provided discretionary investment grants to firms in disadvantaged regions typically characterized by relatively low levels of per capita GDP, high unemployment and general labor market weaknesses ("Assisted Areas").[3] It was designed to "create and safeguard employment". Assistance could be provided to establish a new business, to expand, modernize or rationalize an existing business, to set up research and development facilities or to move from development to production.

Because RSA had the potential to distort competition and trade between European countries, it had to comply with European Union (EU) legislation concerning state aid. Except in certain cases European law prohibits this type of assistance. Article 87(3) of the Treaty of Amsterdam (formerly Article 92(3) of the Treaty of Rome) allows for state aid in support of the EU's regional development objectives. The guidelines designate very deprived "Tier 1 Areas" (formerly, "Development Areas") in which higher rates of grant can be offered and somewhat less deprived "Tier 2 Areas" (formerly, "Intermediate Areas") where lower rates of investment subsidy were offered.[4] There is an upper threshold of support called maximum Net Grant Equivalent (NGE)[5] that essentially sets a maximum proportion of the firm's investment that can be subsidized by the government.

Since the main formulae that determine eligibility are decided periodically at the European level, and not at the Member State level, this mitigates concerns of endogeneity of policy decisions to a local area. In addition, although the UK government has latitude to decide the overall amount of the annual budget for RSA, it must stick to the EU rules when deciding which areas are eligible to receive RSA. Thus, changes to area-level eligibility are the key form of identification in our paper.

A.1 Changes in eligibility over time

The map of the areas eligible for RSA changes about once every seven years.[6] The maps were changed in 1984, 1993, 2000 and 2006. In the paper, we focus on the 2000 change because we could not (despite extensive investigation) discover the exact variables used in determining area eligibility in 1984 and previous years. Without this, we could not construct the rules change IV for the 1993 change, although we do show OLS results over the longer 1986-2004 period for manufacturing employment. There were changes in the way that the

[1] All notation is consistent within Appendices, but some Greek symbols are used to refer to different objects between Appendices.

[2] We discuss our choice of study period below. According to Harris and Robinson (2005), in 1998/9 RSA represented 19% of the UK's industrial policy spending.

[3] In April 2004, in England, the RSA scheme was rebranded as the "Selective Finance for Investment Scheme" and then "Grants for Business Investment". It is still called RSA in Scotland and Wales. Productivity became an official objective with the move from RSA to Selective Finance for Investment and remains an objective of Grant for Business Investment.

[4] Article 87 of the Treaty of Amsterdam supersedes Article 93 of the Treaty of Rome which had previously governed State Aid. Article 87(3) of the Treaty of Amsterdam defines conditions where State aid may be compatible with EU laws. Article 87(3) (a) allows for "aid to promote the economic development of areas where the standard of living is abnormally low or where there is serious underemployment" [Tier 1 or Development Areas] and Article 87(3) (c) allows for: "aid to facilitate the development of economic activities or of certain economic areas, where such aid does not adversely affect trading conditions to an extent contrary to the common interest." [Tier 2 or Intermediate Areas] Additional restrictions apply to sectors with over-capacity: motor vehicles, synthetic fibres and yarns, iron and steel, coal, fishery and agricultural products.

[5] The Net Grant Equivalent (NGE) of aid is the benefit accruing to the recipient from the grant after payment of taxes on company profits. RSA grants must be entered in the accounts as income and are made subject to tax. Details for calculations of NGEs are available in the Commission's Official Journal C74/19 10.03.1998.

[6] Note that this happens in conjunction with the periodic revision of the Structural Funds, the EU's main policy for supporting economic development in less prosperous regions. Although the maps are different for RSA and Structural Funds, it is a potentially confounding influence that we consider carefully as discussed in the main text (subsection V.B)

SAMIS administrative data were gathered after 2004, so we end our sample period in 2004 and cannot easily use the 2006 change. We begin the regression analysis in 1997 for two reasons. First, unemployment data is unavailable on a consistent basis at the ward level before this year. Second, the electronic business register (the IDBR administrative data – see Online Appendix C and main text) was introduced in 1994 and the first few years have reliability concerns. The data is comprehensive since 1997. Nevertheless, our results are broadly robust to beginning the analysis in earlier years than 1997 (for example, see Online Appendix Tables A7 and A11).

The map of the eligible areas is determined by using a series of quantitative indicators. The level of GDP per capita, unemployment and population density are key indicators that have been used in all years. A series of additional indicators is also used, and the EU determines what these are and what years are used for their values – these are detailed in Online Appendix Table A2. The eligibility criteria are outlined in guidelines that are published before the implementation of the map (in our case 1998). The UK government will then gather quantitative information on indicators at the relevant area level and will propose a new map that has to be approved by the EU. The changes before and after 2000 is shown in Figure 1 and Criscuolo et al (2006) shows the map changes at other points in time.

(a) The 1993 change

The assisted area map for RSA was re-drawn in 1993 based on the 1988 guidelines using "Travel to Work Areas" as the underlying spatial units.[7] The Assisted Areas fell into two categories: (a) Development Areas (later called Tier 1) where aid could be granted up to a maximum of 30% NGE (Net Grant Equivalent - see above) and (b) Intermediate Areas (later called Tier 2) where aid was limited to 20% NGE. The new 1993 maps implied a net reduction in the number of assisted areas with Development Areas covering 17%, and Intermediate Areas covering 19%, of the total UK population.

(b) The change in 2000

The EU Commission introduced new guidelines for State Aid in 1998, and the UK responded to that with the introduction of a new Assisted Area map in 2000. The number of indicators rose from eight in 1993 to nine in 2000. The most disadvantaged areas were re-named "Tier 1" - Cornwall and the Isles of Scilly, Merseyside, South Yorkshire and West Wales and the Valleys. The maximum investment subsidy allowed in these areas was 35% NGE. "Tier 2" areas were more scattered and were constructed based on groups of electoral wards.[8] Within Tier 2 areas, the map identified four sub-tier areas eligible for different level of maximum NGE: 30%, 20%, 15% or 10%.

A.2 Formal criteria for receipt of RSA

During our study period (1997-2004), RSA targeted manufacturing sectors. The grants were discretionary, and firms could only apply if the supported project satisfied the following criteria. (a) *Location*: The project had to be undertaken in an Assisted Area. (b) *Investment*: It had to involve capital expenditure on property, plant or machinery; (c) *Jobs*: It should normally have been expected to lead to the creation of new employment or directly protect jobs of existing workers which would otherwise have been lost; (d) *Viability*: The project should be viable and should help the business become more competitive; (e) *Need*: The applicant had to demonstrate that assistance was necessary for the project to proceed as envisaged in terms of nature, scale, timing or location;[9] (f) *Prior Commitments*: As RSA could only be offered when the project could not proceed without it, the Department of Business (BIS) must have completed its appraisal and issued a formal offer of assistance before the applicant entered into any commitment to proceed with the project; (g) *Other Funding*: The greater part of the funding for the project should be met by the applicant or other sources in the private sector. Note that location, which forms the basis for our instrumental variables, is objective, clearly defined and enforceable.

The process for application was as follows. Firms completed an application form, in which they needed to prove additionality, to provide business plans, accounts and reasons for wanting the grant. They then submitted this to the local office of the Department of Business. During the period analyzed, the lag between submission and decision was normally between 35 and 60 days for standard grants, and 100 days or more for

[7] Travel to Work Areas (TTWA) are defined by the UK Office for National Statistics. The fundamental criterion is that, of the resident economically active population, at least 75% work in the area, and that of everyone working in the area; at least 75% live in the area.

[8] The data used for the boundaries come from the 1991 Census of Population. A detailed list of the assisted wards by local authority within regions and the NGEs to which they are eligible is available upon request.

[9] This may be to meet a funding gap, to reduce the risks associated with the project, or to influence the choice of location of a mobile project. It might also be to obtain parent company approval by meeting established investment criteria; or for some other acceptable reason. Each case is considered on its own merits.

grants above £2 million. The lag depended on the amount applied for, the time needed to ensure that all the criteria were met and on negotiations between the government agency and the firm. If the application was successful, the firm was paid the minimum necessary to get the project going. Additional payments started only after jobs were created/safeguarded and capital expenditure defrayed and were based on agreed targets. The payments were given in instalments – between two and seven and usually spread across more than one financial year. The government agency monitored the project with visits (normally one per year, but more frequently for risky projects).

APPENDIX B: THE ROLE OF CHANGES IN THE CRITERIA IN DETERMINING ELIGIBILITY FOR RSA

As noted in the main text, to deal with the issue that areas may be endogenously selected into being eligible for investment subsidies we use an instrument based on the probability that an area is assigned, based solely on the EU wide rule changes rather than changing area characteristics. There are two practical issues in implementing this IV. First, although the elements of the *X* vector determining eligibility for different subsidy levels are known, the exact policy parameters that determine eligibility are not. A second issue is that the maximum subsidy differs in the eligible areas according to the severity of disadvantage. For example, after 2000 an area could fall into several categories with a maximum support share of 10%, 15%, 20%, 30% or 35% percent. Before 2000, there were two maximum support categories: 20% and 30%.

We proceed by defining a latent variable $s^*_{r,\tau}$ for area r and the two time-periods τ which captures how the European Commission determines how disadvantaged an area is. The threshold cut-offs will determine which of the different maximum support level categories (NGEs) an area is to be placed in. In 2000 and after there are six bins (including zero) and before 2000, there were three bins. We keep to the same notation as in the main text in Section III, even though for simplicity there we discussed this issue in terms of a binary outcome, whereas now we are using the fact that we have multiple categories.

To construct instruments that are only driven by changes in the rules rather than changes in area conditions during the period we run two ordered probit regressions for the pre and post 2000 periods. Our vector of area characteristics $X_{r,93}$ includes all variables that were used by the EU for deciding about support status in the pre-2000 and post-2000 periods. However, we only estimate using values of the *X* variables dated prior to 1993 as used in 1993 rule change (in fact, given the lag structure used by the EU, the most recently date considered is 1991 – see Online Appendix Table A2). This is because using values dated after 1993 could potentially be endogenous (recall equation (5) in the main text). This makes the estimates of the policy rule less precise, but so long as there is sufficient power in the first stage then the instruments will be valid.

Formally, the model is:

$$s^*_{r,\tau} = \theta_\tau X_{r,93} + \varepsilon_{r,\tau} \quad \text{where } \tau = \{93,00\}$$

Where $s^*_{r,\tau}$ are the latent variables of "disadvantage" in area *r* at time τ; and there are threshold parameters, $\mu_{j(\tau),\tau}$ that will determine which subsidy regime *j* an area falls into. For example, in 1993, the ordered probit structure is that the observed $s_{r,93} = 0$ if $s^*_{r,93} \le \mu_{0,93}$, $s_{r,93} = 1$ if $\mu_{0,93} < s^*_{r,93} \le \mu_{1,93}$, and $s_{r,93} = 2$ if $s^*_{r,93} > \mu_{1,93}$. The observed bins correspond to different levels of maximum subsidy, $c_{j,\tau}$ where $j = 1, \ldots, J$ is an indicator for a bin. So in 1993 $c_{0,93} = 0, c_{1,93} = 0.2$ and $c_{2,93} = 0.3$. Denote the full parameter vector $\theta_\tau = \{\theta_{1,\tau}, \mu_{j,\tau}\}$,which are the "weights" and the "thresholds" respectively.

We report results from the estimation of the ordered probits in Table 3. The signs generally look broadly sensible (with the caveat that these are not marginal effects). For example, areas with higher GDP per person, lower labor force activity rates, lower population densities and higher long-duration unemployment are more likely to be high investment subsidy areas.

From these ordered probit estimates we obtain the predicted probabilities $\hat{P}_{j,r,\tau}$ of falling into each bin in each year for each area given their observables $X_{r,93}$ and the estimated parameters $\hat{\theta}_\tau$. We then create the predicted level of subsidy as:

$$z_{r,\tau} = \begin{cases} \sum_j c_{j,00} \hat{P}_{j,r,00} & if\ \tau = 2000 \\ \sum_j c_{j,93} \hat{P}_{j,r,93} & if\ \tau = 1993 \end{cases}$$

This specification has the advantage that we can interpret reduced form coefficients in a similar way as regressions of the actual support status (NGE). The IV we use in our baseline specifications is the change in this, $\Delta z_{r,\tau}$, the change in the predicted level of the maximum investment subsidy in the area. The distribution of the levels and changes of $z_{r,\tau}$ are in Figures A1 and A2.

We experimented with many other ways of constructing the IV to make sure that nothing hinges on modelling details and our results are robust. For example, in Online Appendix Table A18 we report our main results using instruments constructed from predictions of a linear probability model of the NGE values rather than the ordered probit of NGE categories. We also estimated models using ordered logit as well as simple logit and probit specifications on the binary event of a non-zero NGE value in an area.

APPENDIX C: MORE DETAILS ON DATA, MATCHING AND PRODUCTIVITY CALCULATION

C.1 The Datasets

We use administrative data on RSA program participants (SAMIS) with data from the Interdepartmental Business Register (IDBR), which contains both the names of the businesses and the identification numbers used by the Office for National Statistics (ONS) to conduct the Annual Business Inquiry (ABI).[10] The IDBR is a list of all businesses in the UK, their addresses, type of activity and ownership structure. The list is compiled using a combination of tax records, accounting information (every UK firm must lodge some information at Companies House). The smallest unit in the IDBR is a site that contains name, address and information on the number of employees and industry. We also know the enterprise (firm) that owns the site and whether this is part of a larger group ("enterprise group"). Investigation showed that some of the most micro-units (the sites identifiers) are not reliable over time; we grouped all sites of a firm in a Ward into a single "local unit" which we refer to as a "plant" in the text.

A stratified random sample of enterprises is drawn every year from the IDBR to form the sampling frame for the ABI (Annual Business Inquiry), the mandatory annual survey of UK businesses. Data from the ABI is made available to researchers in the form of the ARD (Annual Respondents Database), which provides information on output, investment, intermediate inputs, employment, wages, etc.[11] The ARD is similar to the US Annual Survey of Manufacturing (ASM) with the caveat it covers all sectors (not just manufacturing) and is at a higher level of aggregation than the plant-level ASM. Not only is the ARD a sub-sample of the population IDBR, but the information is reported at a more aggregated level across the entire firm ("reporting unit"), rather than at the plant ("local unit") level. For example, a firm with two 10 workers plants in two different wards will have only total employment reported in the ARD (20 workers), whereas the IDBR will identify both local units. Note that in about 80% of all cases a firm is single plant and located entirely at a single address.

The upshot is that whereas employment can be matched exactly to an area, so we can analyze at whatever level we like (e.g. plant, firm or ward); the analysis of investment and productivity for a representative sample can only be accurately conducted at the firm level, and not a lower level. Note that the ARD contains the population of larger businesses (those over 100 or 250 employees depending on the exact year) and accounts for around 90% of total UK manufacturing employment.

C2. Matching Datasets

Since the performance data comes from sources unrelated to program participation, several problems arise in matching. The Department of Business uses name and postcodes from its administrative SAMIS data to match a list of participants and applicants to the population IDBR. This matching may occur at the plant-level or the firm level. Often a firm will apply for funding; so that we cannot know for sure whether a plant has benefitted from RSA receipt (although for the 80% of single-firm plants there is never an ambiguity). Thus, one measure of program participation is simply whether a plant was in a firm that received any RSA (which we can always

[10] The IDBR was introduced between 1994 and 1995. Previously, that sampling was based on a Business Register maintained by the Office of National Statistics.

[11] Stratification is broadly based on industry affiliation, regional location and size. For details, see Criscuolo et al. (2003).

define precisely). For a small number of cases, the same SAMIS identifier could match to multiple IDBR firms. In these cases we aggregated the IDBR firms together, but we checked the results were robust to dropping these few cases (they were). The ARD is a strict sub-set of the IDBR, so the issues discussed above apply in the same way to this dataset.

The SAMIS database has information on 54,322 program applications and whether the application was successful. Applicant numbers declined in the 2000s as the total budget for RSA fell. Using name, postcode and CRN numbers, the information in BIS files was linked to the IDBR over the whole period. The matching rate was 82% over the sample period (1997-2004).

There is a variety of reasons for non-matches. The most common reason is that the information on the SAMIS database of RSA participants is inadequately detailed to form a reliable match to the IDBR. It is also possible that the IDBR misses some of the smaller and shorter-lived firms who receive RSA. To check biases arising from matching we conducted a detailed comparison of the characteristics of projects and project participants of firms that BIS matched with IDBR relative to all the projects in the SAMIS database. The analysis shows that the set of "IDBR matches" do not significantly differ from the rest of the projects in the database on observed characteristics, and this is the case for both unsuccessful and successful applications. The variables we considered in the regression were application amounts; headquarter location, a dichotomous variable that is one if the application was handled by the London office of BIS, foreign owned, and a BIS code that seeks to identify "internationally mobile" jobs. More details are available from the authors and in Criscuolo et al (2006).

The area level average subsidy rates used in Table 5 are generated from aggregating up all subsidies granted to plants in ward in the two periods (1997-1999 and 2000-2004) and then dividing by the number of years in each sub-period.

C.3 Firm Size Definition

In some of the analysis, we split by firm size (e.g. Table 10). To mitigate endogeneity concerns we use firm size as measured by employment in a base period, for which we choose 1996, the year before our estimation period. For all plants belonging to a firm who were not alive in 1996 we use the year of birth to determine the size class and exclude data from the first year in our regressions of employment. We also experimented with dropping post-1996 entrants, which led to very similar results.

C.4 TFP (Total Factor Productivity) measures

There are numerous ways to obtain a TFP measure, a subject of ongoing debate in the literature (see inter alia Olley and Pakes, 1996 and Ackerberg et al, 2015). The results in Panel E of Table 11 are based on a simple "factor share" method and relative to an industry by year average. We define $TFP_{it} = \tau_{it} - \bar{\tau}_{I(i)t}$ where $\tau_{it} = r_{it} - \bar{S}_{MI(i)t} m_{it} - \bar{S}_{LI(i)t} l_{it} - \left(1 - \bar{S}_{MI(i)t} - \bar{S}_{LI(i)t}\right) k_{it}$. In this expression r_{it} is ln(firm revenue) for firm i in period t, m_{it} is ln(materials), l_{it} is ln(employment) and k_{it} is ln(capital). $\bar{S}_{MI(i)t}$ is the share of materials in revenues in the four-digit industry and $\bar{S}_{LI(i)t}$ is the share of labor costs in revenues at the industry level. $\bar{\tau}_{I(i)t}$ is the average value for τ_{it} in year t in the four digit industry.

We also considered alternative ways of computing TFP (see Online Appendix Table A16). Firstly, we consider a "regression-based" method where we use ln(revenues) as the dependent variables and include on the right-hand side in addition to treatment controls ln(labor), ln(materials) and ln(capital). Secondly, we consider a more structural production function estimation approach as proposed in Martin (2012) which takes into account firm specific variation in market power when computing TFP. This requires running the following (first stage) regression: $\Xi_{it} = \beta_k k_{it} + \rho(\Xi_{it-1} - \beta_k k_{it-1}) + \nu_{it}$ where $\Xi_{it} = \frac{r_{it} - s_{Mit}(m_{it} - k_{it}) - s_{Lit}(l_{it} - k_{it})}{s_{Mit}}$ and S_{Mit}, S_{Lit} are the variable factor shares at the firm level. From this we can estimate a productivity index as $TFPMUOMEGA = \frac{\Xi_{it} - \hat{\beta}_k k_{it}}{\hat{\beta}_k}$.

APPENDIX D: AGGREGATING ACROSS SPATIAL UNITS

We consider the aggregation from lower (wards) to higher levels area (Travel to Work Areas) as discussed in subsection V.C. For simplicity consider the set-up of a single Travel to Work Area (TTWA, denoted a, consisting of two wards r and r' and consider two periods t = 0 and t = 1. It is straightforward to generalize this to multiple-ward TTWAs (we do this in the empirical application). Suppose we know that as a consequence of the program in period 1, ward r experiences a change of employment of α_r log points whereas ward r' experiences a change of $\alpha_{r'}$ log points; i.e. $\ln L_{r,1} - \ln L_{r,0} = \alpha_r$ and similarly for ward r'.

We are interested in what will be the effect of the policy on total employment at the higher TTWA level. We can write TTWA employment as the sum of the two wards: $L_{a,t} = L_{r,t} + L_{r',t}$. Hence the logarithmic change in employment is:

$$\ln L_{a,1} - \ln L_{a,0} = \ln[e^{\alpha_r} w_r + e^{\alpha_{r'}}(1 - w_r)] \tag{D1}$$

where $w_r = \frac{L_{r,0}}{L_{r,0}+L_{r',0}}$ is the share of employment in Ward 1 in period 0. Re-write equation (D1) as:

$$\ln[e^{\alpha_r} w_r + e^{\alpha_{r'}}(1 - w_r)] = \alpha_{r'} + \ln[(e^{\alpha_r - \alpha_{r'}} - 1)w_r + 1] = \nu_1 + \alpha_{r'} + (e^{\alpha_r - \alpha_{r'}} - 1)w_r$$

Where ν_1 is an approximation error that is small for values of $(e^{\alpha_r - \alpha_{r'}} - 1)w_r$ close to zero. Similarly note that $(e^{\alpha_r - \alpha_{r'}} - 1) = \nu_2 + \ln[(e^{\alpha_r - \alpha_{r'}} - 1) + 1] = \nu_2 + \alpha_r - \alpha_{r'}$ for $(e^{\alpha_r - \alpha_{r'}} - 1)$ close to zero and where ν_2 is another approximation error.[12] Consequently, we can write the change in TTWA employment as:

$$\ln L_{a,1} - \ln L_{a,1} \approx \alpha_{r'} + (\alpha_r - \alpha_{r'})w_r = w_r\alpha_r + (1 - w_r)\alpha_{r'} \tag{D2}$$

In other words: the percentage TTWA level change is approximately the percentage change in each ward weighed with the employment share of each ward.

This allows us to examine the case of negative spillovers as well. Suppose region r experiences an increase in support $\Delta NGE_r > 0$ but there is no change in ward r'. This leads to a positive effect of $\alpha_r = \beta\Delta NGE_r$ in region r at the expense of a possible negative spillover of $\alpha_r = -\chi\Delta NGE_r$ in region r'. For the aggregate TTWA we would consequently expect the effect on employment to be:

$$\alpha_a = w_r\, \lambda_1 \Delta NGE_r - (1 - w_r)\chi\Delta NGE_r$$

Indeed, in the case where the policy simply shifts jobs from one ward region to the other we would expect

$$\chi = \lambda_1 \frac{w_r}{1 - w_r}$$

i.e. if r' is smaller than r, χ would be bigger than β. On the other hand, if we assume that there are no spillovers equation (D2) becomes

$$\ln L_{a,1} - \ln L_{a,0} \approx \alpha_a = \lambda_1 \Delta NGE_a \tag{D3}$$

where $NGE_a = \sum_r w_r NGE_r$. This implies that if we regress (changes) in TTWA ln(employment) on the employment weighted share of area level NGE changes we would expect to recover comparable impact estimates as we did when running ward level regressions on (changes) in NGE. By contrast, if there are negative spillovers we expect a coefficient smaller than λ_1 when running a regression as implied in equation (D3). We would also expect some bias towards zero because of the approximation error implied in equation (D3). In our empirical estimates at the TTWA level in Table 8, we find treatment effects that look (if anything) larger than the ward level β estimates in Table 4. This leads us to the conclusion that negative spillovers are not a major issue of concern in our application.

APPENDIX E: OTHER PLACE-BASED POLICIES

Our identification strategy uses exogenous policy rule changes that determine which wards are "randomized in" to be eligible (or ineligible) for RSA support. The exogenous policy rule change that we use stems from the change in the UK assisted area map drawn up to comply with revised EU regulation. One potential threat to

[12] Note that the two errors go in opposite directions with the first one overestimating and the second one underestimating the true figure. The second error is also likely larger so that on net we are underestimating the true figure. Simulations of the errors suggest that these are under 5%.

identification is the existence of other regional policies that use geographical areas to determine eligibility and experience similar changes in eligibility at around the same time as the rules for RSA eligibility change. If such policies exist, then they may cause us to over-estimate the effect of RSA eligibility if these other policies positively affect RSA-eligible areas. In this Appendix, we consider a wide range of place-based policies and discuss whether they raise concerns and, if so, how we address these in the paper. Broadly, there appears to be only one policy – the Regional Development Fund aspects of EU Structural Funds – that is potentially problematic as it has both cross-area variation and rules that changed at the same time as RSA.[13]

E.I EU Structural Funds (SF)

The change in the Assisted Areas map for RSA in 2000 coincides with several changes to the EU "Structural Funds (SF)." SF are important instruments for delivering EU regional policy mainly through infrastructure spending. Total SF spending is higher than RSA, although the direct SF grants to business are an order of magnitude smaller than RSA. For example, in 1997 the total amount of RSA grants accepted was £158.3 million while the total amount of SF Regional Development was £621 million (House of Commons, 2000), only £15.6 million of this were Funds for business grants (1997 Annual Report of the Industrial Development Act).

Our data cover two program periods 1997-1999 and 2000-2004. In the earlier period, the EU Structural Funds were organized around "objectives." Broadly, only Objectives 1 and 2 really matter for us.[14] Objective 1 is targeted at the poorest regions. Objective 2 regions are less poor but suffer from high unemployment and/or have high shares of employment in declining industries. Objective 1 accounts for about 70% of all SF spending, whereas Objective 2 accounts for only 11%.

The rules for eligibility for Objective 1 were very similar in both periods - a region must have a GDP per capita that is below 75% of the EU average.[15] Objective 1 is defined on the NUTS2 geographical areas whereas Objective 2 is defined on smaller units. [16] A number of criteria were used to determine eligibility for Objective 2 that were similar to RSA such as the unemployment rate, the percentage share of manufacturing jobs; falls in employment and the fraction of skilled workers. The reference year for which these were taken were sometimes different from RSA, however.[17] One factor determining eligibility for Objective 2 SF that did not determine RSA were local crime rates, and we include these variables (robberies, burglaries and drug crimes) when predicting which areas were eligible for SF.

Since the maps for SF and RSA eligibility change at the same time and both are aimed at disadvantaged areas, a concern is that the RSA effect may be confounded by the effects of SF. We can observe the maps of eligibility for SF and RSA and, in fact there are many differences. There are several reasons for these differences. First, the exact weights given to different variables in the policy rule are not the same for RSA and SF. Second, the "reference year" used to define the variables is different. Third, the level of aggregation used to determine eligibility also differs. Fourth, and perhaps most importantly, the variables that enter the policy rules for RSA and SF are not all the same. Crime rates enter the policy rule for SF but not RSA. Similarly, although the structural unemployment, the activity rate, the long-term unemployment rate and the start-up rate of new businesses affect whether an area is eligible for RSA at various points of time, they are never in the list of variables that determine SF eligibility.

For example, GDP per capita is a key component for eligibility to Objective 1 SF support and the highest investment subsidy rates of RSA (i.e. a "Development Area" or Tier 1 area). Indeed, the maps for eligibility are identical 2000-2006 (NUTS2 areas of Cornwall and the Isles of Scilly; Merseyside; South Yorkshire; West Wales and the Welsh Valleys). However, in the 1993-99 period the two maps differ significantly. The RSA Development Areas comprise 123 Travel-To-Work Areas (TTWAs) or parts of such areas. In addition, the only

[13] The bulk of EU transfers to the UK are towards agriculture via the Common Agricultural Policy. Structural Funds also include an Agricultural Guidance Fund and a Social Fund (that does not have an explicit regional component). Since these are not very relevant for a place-based industrial policy like RSA, we simply refer to the Regional Development Fund aspect of SF as "Structural Funds" for brevity in what follows.

[14] Objectives 3 and 4 were not spatially targeted at particular types of region so are not a threat to identification of RSA. Objective 5 was subsumed into Objective 2 after 2000.

[15] Calculations are based on three-year averages: 1989-1991 for the early period and 1994-1996 for the post-2000 period.

[16] To give a better idea of the size of these territorial units consider that in the UK there are 37 NUTS 2, each covering between 800,000 and 3,000,000 inhabitants and 133 NUTS 3, each covering between 150,000 and 800,000 inhabitants. The equivalent in the US could be municipalities or city/county/authorities. Note that the geography used for eligibility to RSA are "wards" (NUTS 5) with an average population of about 6,600 people.

[17] For example, for manufacturing share the reference year was 1975 for the 1993-1999 period and 1985 for the 2000-2006 period.

two regions eligible to Objective 1 support over this period were Merseyside and the Highlands and Islands.[18] This is mainly because of a different level of aggregation used to determine RSA compared to SF.

Online Appendix Table A3 presents the degree of overlap in eligibility for RSA and SF over time to illustrate the amounts of non-overlap. Row 1 shows that out of 10,737 wards, 2,424 (22.6%) were eligible for both SF and RSA over the 1993-99 period. Of these, 1,743 (71.9%) continued to be eligible for both policies after 2000. 681 wards (28.1%) lost eligibility for RSA but maintained eligibility for SF (none of these wards lost eligibility for SF or lost eligibility for both types of support). Similarly, rows 2 and 3 look at changes in eligibility over time of wards that pre-2000 were eligible for only one type of support (RSA in row 2 and SF in row 3). The last row shows that most wards (6,602 or 61.5% of the population) were ineligible for both policies pre-2000. Of these 3% subsequently became eligible for both types of support; and 2% for RSA only with the majority (95%) remaining ineligible for both.

Since there may be unobservables that determine whether an area becomes eligible for SF this can create endogeneity issues. We can exploit the same identification strategies we use for RSA for SF to deal with this problem. Although some of the criteria determining SF are the same as RSA, many are different. For example, crime variables affect whether an area is eligible for SF, they do not appear in the criteria determining RSA eligibility. Similarly, structural unemployment, the activity rate, the long-term unemployment rate and the start-up rate of new businesses affect whether an area is eligible for RSA, they are not in the list of variables that determine SF eligibility. Hence, analogously to Table 3 we estimate a model where the dependent variable is whether an area is eligible for SF in Online Appendix Table A4 separately for the earlier period (1993) and later period (2000). The coefficients generally look sensibly signed: areas with lower GDP per capita, less population density, more manufacturing and worse job markets are more likely to be eligible for SF. Additionally, five of the six crime coefficients suggest that places with more crime are significantly more likely to be eligible for structural funds (the only exception is drug crime in 1993).

Analogously to our strategy for RSA, we use the estimates in Online Appendix Table A4 to build up a "SF rules change IV" and enter this alongside our standard specifications in Table 6. We show there that although there is a little evidence of beneficial effects of structural funds on unemployment in the reduced forms, the SF treatment variable is not significant at the 5% level in the IV specifications for either employment or unemployment. More importantly for our purposes, the effect of the RSA policy is robust to inclusion of the SF variable (see discussion in subsection V.B in main text).

E.2 Enterprise Grant (EG) Scheme

Another change that happened in 2000 was a revision in the way Regional Selective Assistance was administered to small and medium sized enterprises (SMEs) and for smaller projects. These smaller grants were renamed as "Enterprise Grants" (EG). In England and Scotland, EG's began in January 2000. They were a simplified scheme for SMEs in RSA eligible areas. The scheme replaced small-scale RSA grants and provided funding up to a maximum of 15% of investment.

In England (but not Scotland) EGs also became available in "Tier 3" areas (see Figure 4 in http://www.tandfonline.com/doi/pdf/10.1080/00343400123609). These Tier 3 areas were outside those eligible for assistance under RSA. Small firms (under 50 employees) could receive up to 15% investment subsidies and medium sized firms (between 50 and 249 employees) could receive up to 7.5% in Tier 3. In Wales, EGs were not introduced until 2002 and then were available throughout the country.

The aggregate spending on EGs was low compared to RSA. For example, in Scotland in 2001 only £3m was spent on EGs, under 3% of the total RSA budget.

Following our strategy for other area-based policies (see next subsection) we can include a dummy variable equal to one when an area becomes eligible for EGs. Although these are generally the same as RSA, the introduction of Tier 3 in England in 2000 and the delayed introduction until 2002 in Wales, enables us to separately identify their effect.[19]

This does, however, raise the concern that the larger effect of RSA on plants belonging to small firms could be due to EGs. The results in Online Appendix Table A14 cast doubt on this. Here we use the actual subsidy amounts (effectively RSA plus EG) and do not find that the results are due to smaller firms receiving relatively large grants. However, a data issue is that the Scottish and Welsh (after 2002) subsidies exclude EG,

[18] For example, while Cornwall was not eligible to Objective 1 status; TTWAs such as Penzance and St. Ives or Newquay were Development Areas. Similarly, no part of Wales was eligible to Objective 1 aside from part of Blaenau Gwent and Abergavenny; Thanet and South Pembrokeshire are Development Areas.

[19] Note that we have access to the subsidy amounts of EG in England, but not in Scotland or Wales. Since EGs were effectively part of the RSA treatment before and after 2000 we consider the reduced form estimates a reflection of the "RSA and EG bundle." However, since we showed in Table 7 that EG had no effect on the RSA policy effect; that the EG coefficient itself is small and considering also the aggregate amount spend on EG was also relatively small, it is reasonable to assume that our overall estimates are due to RSA.

so could be generating this effect. To check this, we allowed the coefficient on the RSA treatment effect to be different in England from in the rest of the sample (Wales and Scotland). If the result were driven by measurement error in the subsidy amount, we would expect that the coefficient should be significantly different. We found that the interaction terms were not significant (-0.402 with a standard error of 0.625) suggesting that this is not a first order issue.

E.3 Other Area Policies

Online Appendix Table A19 considers many regional and active labor market policies that operated during our estimation period. The Table provides information on the timing of the policy and basic information on area eligibility. The clear majority of policies (10 out of 14) are purely national in nature and do not have specific local area eligibility.[20] Thus, the time dummies will control for them.

Apart from Structural Funds and Enterprise Grants discussed above, there are five other potential policies with a geographical area component: Employment Zones, Coalfields Regeneration Trust, New Deal for Communities and Regional Venture Capital Funds.

(a) *Employment Zones* were designated areas of high long-term unemployment where a package of policies was delivered aimed at improving the chances of those on long-term unemployment insurance getting back into work. The Job Center assessed whether extra training, job subsidies, more intense work search, etc. were needed and delivered these with the threat of benefit sanctions. These started in April 2000 and we code the 15 designated areas with a dummy equal 1 after 2000 and zero otherwise. The areas are: Birmingham, Brent, Brighton and Hove, Doncaster, Glasgow, Haringey, Liverpool and Sefton, Merthyr Tydfil (including Caerphilly and Blaenau Gwent), Middlesbrough (including Redcar and Cleveland), Newham, North West Wales (Conwy, Denbighshire, Anglesey, Wrexham, Caernarfonshire and Merionethshire), Nottingham, Plymouth, Southwark and Tower Hamlets.

(b) *The Coalfields Regeneration Trust* (http://www.coalfields-regen.org.uk/) contains a set of initiatives designed to support areas historically dependent on Coalfields. This includes help on skills, setting up new businesses and finding new jobs. This program began in 1999, so the affected areas were coded to be 1 from 2000 onwards and zero otherwise. The coal-field districts were: Allerdale, Alnwick, Amber Valley, Ashfield, Bassetlaw Barnsley, Blaenau Gwent, Blyth Valley, Bolsover, Broxtowe, Caerphilly, Cannock Chase, Canterbury, Castle Morpeth, Chesterfield, Chester-le-Street, Clackmannanshire, Copeland, Derwentside, Doncaster, Dover, Durham, East Ayrshire, Easington ,Erewash, Fife, Forest of Dean, Gedling, Hinckley and Bosworth, Kirklees, Knowsley, Leeds, Lichfield, Mansfield, Melton, Merthyr Tydfil, Midlothian, Moorlands, North Lanarkshire, North Warwickshire, North-East Derbyshire, Neath PT, Newark and Sherwood, Newcastle-under-Lyme, North Tyneside, Nottingham, Nuneaton and Bedworth, NW Leicestershire, Rhondda CT, Rotherham, Rushcliffe, South Derbyshire, South Lanarkshire, Salford, Sedgefield, Selby, Sheffield, South Staffordshire, South Tyneside, St Helens, Staffordshire, Stoke-on-Trent, Sunderland, Tamworth, Torfaen, Wakefield, Wansbeck, Wear Valley and Wigan.

(c) *The New Deal for Communities* was targeted at the most deprived areas of England. These were usually very small localities, generally on public housing projects, suffering from low employment, high crime and health problems. Local public services across different agencies (welfare benefits, housing, health and social care) tried to offer "joined up" interventions. The program started in 1998 in 17 areas (ending in 2008), and then another 22 were added in 1999 (ending in 2011). As usual, we have a dummy that turns on in these years for the relevant areas. The communities targeted in round 1 (1998) include:

Local authority Area	*wards/estates/communities*
Birmingham	Kings Norton
Bradford	Little Horton, Marshfield and West Bowling
Brighton	East Brighton
Bristol	Barton Hill
Hackney	Shoreditch
Hull	Preston Road
Leicester	Braunstone
Liverpool	Kensington
Manchester	Beswick and Openshaw

[20] Some of the policies have small local area pilot schemes. See, for example, Blundell et al (2004) on the New Deal for Young People or Koenig et al (2018) on Job Centre Plus.

Middlesbrough	West Middlesbrough
Newcastle Upon Tyne	Arthur's Hill, Cruddas Park, Rye Hill and Elswick
Newham	West Ham and Plaistow
Norwich	North Earlham, Larkman and Marlpit
Nottingham	Radford and Hyson Green
Sandwell	Greets Green
Southwark	Aylesbury Estate)
Tower Hamlets	Ocean Estate

In Round 2 (1998) the following communities were targeted:

Local authority Area	*wards/estates/communities*
Birmingham	Aston
Brent	South Kilburn
Coventry	Wood End, Henley Green and Manor Farm
Derby	Derwent
Doncaster	Central Doncaster
Hammersmith and Fulham	North Fulham
Haringey	Seven Sisters
Hartlepool	West Central Hartlepool
Islington	Finsbury
Knowsley	Huyton
Lambeth	Clapham Park
Lewisham	New Cross Gate
Luton	Marsh Farm
Oldham	Hathershaw and Fitton Hill
Plymouth	Devonport
Rochdale	Heywood
Salford	Charlestown and Lower Kersal
Sheffield	Burngreave
Southampton	Thornhill
Sunderland	East End and Hendon
Walsall	Blakenhall
Wolverhampton	All Saints and Blakenhall Community Development

(d) *Regional Venture Capital Funds* was national from 2003 but affected two regions (West Midlands and East of England) from 2002. The program provided small-scale equity (under £500,000) to firms with "growth potential". We included a dummy which switches on for these two regions in 2002 (the national program is in the time dummies).

(e) *Devolution to Scotland and Wales.* Following successful Referenda, in 1999 greater powers were delegated from central government in London to Wales (Government of Wales Act 1998) and Scotland (Scotland Act 1998). Although the budget allocated to RSA and the administration of the scheme was (partially) decentralized, the EU driven determination of eligible and ineligible areas was not changed, so the identification scheme we are using is unaffected by these changes. Nevertheless, we included a dummy for Wales and Scotland in 1999 and thereafter to control for any effects.[21]

E.4 Summary on "Other Policies"

Table 7 (where the dependent variable is manufacturing employment) and Online Appendix Table A5 (where the dependent variable is unemployment) in the main text shows that our RSA effects are robust to all these "other policy" controls (including adding in Structural Funds).

[21] Note that in 1999 there were also greater powers to the nine Regional Development Agencies that covered contiguous areas in England (Statutory Instrument 1999/672). Any effect from this would be captured by the time dummies in the regression with post 1999 Scotland and Wales controls.

APPENDIX F: FURTHER RESULTS

F.1 Spatial Clustering of the Standard Errors

Our main results rely on clustering the standard errors at the ward-level because this is the level where NGE eligibility is determined. In the language of Abadie et al (2017) our experimental design delivers quasi-random variation at the ward-level, so this is ex ante the appropriate level for clustering. The data underlying the policy variables are at a mixture of levels of aggregation (some at the wide NUTS2 level, but others as low as the ward level).

If treatment eligibility was determined solely by factors at a geographically higher level than the ward level our approach could underestimate the correct standard errors. In this section, we explore several more conservative clustering approaches. These include:

1. Clusters based on TTWAs (Travel to Work Area)
2. Clusters based on contiguous neighboring wards receiving identical support levels.
3. Clusters based on "close" neighboring wards receiving identical support levels
4. NUTS-2 level clustering

Online Appendix Table A20 reports versions of our main results in Table 4 with errors clustered at these different levels. Irrespective of the precise level of clustering we find that our treatment effects are significant at 5% level or greater for the employment regressions. For unemployment, we lose significance at conventional levels only when clustering at the extremely conservative NUTS-2 level (34 clusters).

In Panel A of Online Appendix Table A20 we simply cluster at the level of the TTWA (322 clusters), the least conservative approach in the Table (but more conservative than in Table 4 where clustering is at the ward level). In Panel D we cluster at the level of the NUTS2 region (34 clusters), the most conservative approach. We also investigated more sophisticated approaches where we created clusters of areas that were "close" to each other and had the same level of NGE support in both the pre and post 2000 period. We tried several alternatives, two of which we report in the paper. The first of these defined "close" as contiguous wards – i.e. we aggregated all wards having a shared boundary and the same level of NGE support in the two periods. We use NUTS 2 boundaries to define clusters for wards that receive no support. This gave us 102 clusters in total: 70 clusters of wards with positive NGE and another 32 clusters with zero NGE (i.e. no support). Our second approach defined "close" as being within 1km of another ward (rather than strictly contiguous) with the same NGE. Once again, we used NUTS 2 boundaries to define clusters for wards that receive no support. This gave us 80 clusters in total: 48 clusters of wards with positive NGE and another 32 with zero NGE. Both approaches are illustrated in Online Appendix Figure A3. Regression results are reported in Panels B and C of Online Appendix Table A20.

In short, our core results appear broadly robust to various ways of dealing with spatial autocorrelation.

F.2 Relationship of Regression Discontinuity Designs to our baseline IV approach

We explore the impact of different levels of support (NGE) on various area *r* level outcomes at time *t*. Recall our basic model is:

$$y_{r,t} = \lambda_1 \, NGE_{r,t} + \epsilon_{r,t}$$

where $\epsilon_{r,t} = \eta_r + \nu_{r,t}$. To deal with area fixed effects that are potentially correlated with treatment our basic approach involves identifying λ_1 from differences

$$\Delta y_{r,t} = \lambda_1 \Delta NGE_{r,t} + \Delta \nu_{r,t} \tag{F1}$$

Where $\Delta y_{r,t} = y_{r,t} - y_{r,1997}$ and there was a change in NGE after 2000.

While differencing eliminates the fixed effect there is concern that differential trends in the outcome variables could affect treatment so that $E\{\Delta \nu_{r,t} | \Delta NGE_{r,t}\} \neq 0$. This could be because an area that does more poorly in the period leading up to 2000 would be more likely considered in need of support so that

$E\{\Delta\nu_{r,t}|\Delta NGE_{r,t}\} < 0$. Indeed, the mechanism that leads to this is that the European Commission deems areas as disadvantaged and therefore in need of support based on a set of area level characteristics at certain points in time. This rule can be described as:

$$NGE_{it} = \begin{cases} f_{93}(X_{r,93}) \text{ if } t < 2000 \\ f_{00}(X_{r,00}) \text{if } t \geq 2000 \end{cases} \tag{F2}$$

where X is a vector of area characteristics, i.e. support levels of NGE are a mapping of local area characteristics. For the period between 1993 and 2000 the area characteristics are based on some point in time before 1993, for the period from 2000 onwards NGE is based on area characteristics at some point between 1993 and 2000 (see Online Appendix Table A2). These include weightings of the different characteristics (including a weight of zero for some characteristics in some periods) as well as a variety of thresholds.

As consequence of this,

$$E\{\nu_{r,t}|X_{r,93}, NGE_{r,t}\} = E\{\nu_{r,t}|X_{r,93}\} \text{ for } t < 2000 \tag{F3a}$$

and

$$E\{\nu_{r,t}|X_{r,00}, NGE_{r,t}\} = E\{\nu_{r,t}|X_{r,00}\} \text{ for } t \geq 2000 \tag{F3b}$$

i.e. NGE is correlated with the error term only because it is in part driven by $X_{r,00}$ and $X_{r,93}$.

This in turn implies that if we could observe $E\{\nu_{r,t}|X_{r,p}\}$ – where period $p \in \{93{,}00\}$ – we could obtain an unbiased estimate of λ from a regression of

$$\Delta y_{r,t} = \lambda_1 \Delta NGE_{r,t} + E\{\Delta\nu_{r,t}|X_{r,00}, X_{r,93}\} + \Delta\xi_{r,t}$$

where $\Delta\xi_{rt} = \Delta\nu_{r,t} - E\{\Delta\nu_{r,t}|X_{r,00}, X_{r,93}\}$

Of course, we have no way of observing $E\{\Delta\nu_{r,t}|X_{r,00}, X_{r,93}\}$, directly but since it is driven entirely by observables we can use a non-parametric approach to estimate it; i.e. we can run a regression of

$$\Delta y_{r,t} = \lambda_1 \Delta NGE_{r,t} + \phi(X_{r,00}, X_{r,93}) + \Delta\xi_{r,t} \tag{F4}$$

where $\phi(X_{r,00}, X_{r,93})$ is approximated by a series expansion or similar non-parametric method. Note that λ_1 and $\phi(X_{r,00}, X_{r,93})$ are separately identifiable because ΔNGE is determined by both X variables *and* EU rules that change over time. This is very similar to a regression discontinuity approach where we control for the (unknown) running variable $\phi(X_{r,00}, X_{r,93})$.

We provide results using this approach in Online Appendix Table A10 for both employment and unemployment. The estimates are significant and larger in absolute magnitude than the OLS estimates in Table 4 column (1). However, they are *smaller* than our preferred IV results in column (4) of Table 4.

One reason for this difference could be measurement error in the running variable in equation (F4). The measurement error could be simply that the variables we use are not exactly the ones used to determine eligibility of an area, because for example at the time the information might have come from an older vintage of data than the ones that we are using.

As is well-known even classical measurement error can create serious biases in RD Designs. This is because continuous measurement error smooths over the discontinuity (see Battistin et al, 2009). In many non-RD design approaches like matching, estimators do converge at a standard rate to a biased value with classical measurement error (e.g. Battistin and Cheshire, 2014) and will be negligible for sufficiently small variance of the measurement error. By contrast, Davezies and Le Barbanchon (2017) show that in RD Designs even a small amount of classical measurement error results in inconsistency of the usual estimator. They show that this seems to be important not just in theory, but also in their Monte Carlo evidence and empirical application. The standard methods to deal with measurement error in the running variable are not applicable to our context where we know neither the true value of the running variable (even for a subset of the data) nor the cut-off (e.g. Battistin et al, 2009; Porter and Yu, 2015).

The advantage of the IV strategy we pursue in the main part of the paper is that the instrument may contain classical measurement error, but it will not cause bias so long as the instruments are not weak. And we showed the strength of instruments through standard techniques such as F-statistic in the first stage.

F.3 A RD Design in Levels?

Although we have described a potential RD Design in terms of differences in equation (F4), one could also imagine an RD Design using levels of the variables as in equation (F2). Consider the model in the period before the 2000 policy change (the same issues arise for post-2000):

$$y_{r,t} = \lambda_1 NGE_{r,t} + f\big(X_{r,93}\big) + v_{r,t}$$

Moment condition (F3a) implies that we can consistently identify λ under the usual RD assumptions. If we considered NGE as a discrete dummy (eligibility vs. non-eligibility), then the RD is considering areas "just above" the surface $f\big(X_{r,93}\big)$ where an area becomes eligible to areas "just below" the surface. The problem however is that we observe neither the running variable nor the cut-off. But we do observe all the elements of the running variable $X_{r,93}$. Thus, one might think the cut-off for the surface could be identified empirically from the data.

Unfortunately, the complexity of the rules (plus potentially measurement error in the X's) did not enable us to do this in a convincing way. The basic issue is that there are many indicators underlying the running variable (8 before 2000 and 9 afterwards) and these could be combined in a huge number of non-linear combinations. In addition, there are multiple NGE levels, so we are not just looking along the eligibility/non-ineligibility boundary, but also at different levels of NGE. The only aspect of the rules where we could identify a clear difference was by using ex ante information for the cut-off for GDP per capita (see below). Here we were able to implement an RD design in levels, and although the results are qualitatively similar to our main results they are noisy.

We also considered applying spatial discontinuity methods as first used in the paper by Dell (2010). Here the running variable is a function of geography (such as latitude and longitude). Unlike our context, however, the cut-off is known (it's when you cross the boundary) and the number of dimensions underlying the running variable is smaller (two compared to 8 or 9).

F.4 An RD Design in levels using a known cut-off for one of the elements of RSA rules

While we know which area level statistics have been used to determine if an area is eligible for treatment, we do not know the exact threshold(s) that were used to classify areas. One exception is the GDP per capita relative to the EU wide average criteria. Only areas with a relative per capita GDP of below 75% are eligible for the maximum amount of support ("Tier 1" status).

We consider using this threshold to generate a fuzzy Regression Discontinuity (RD) design. Note that in our main results we exploit many other criteria for eligibility that are based on the ward level. The 75% threshold, by contrast is based on the NUTS2 level of aggregation. There are over 10,000 wards but only 34 NUTS2 levels in Great Britain, which severely reduces the variation in the source of identification. Furthermore, only four NUTS2 areas are below the 75% over the 1997 to 2004 period.

This caveat notwithstanding Online Appendix Table A9 details the RD results. We estimate $lnY = \beta_1 D + \beta_2(R-75) + \beta_3\,[D \times (R-75)] + \epsilon_{RF}$ where R is the running variable (i.e. GDP per capital relative to EU average of the NUTS 2 region), D is a dummy variable equal to 1 if an area's running variable is below 75% and ϵ_{RF} is the error term. Similarly, our IV estimates are $lnY = \beta_{1,NGE} NGE + \beta_{2,NGE}(R-75) + \beta_{3,NGE}\,[D * (R-75)] + \epsilon_{NGE}$ where we instrument NGE by D.

In column (1) of Online Appendix Table A9 the dependent variable is simply the value of NGE. The coefficient on the threshold in this "first stage" is positive and significant, suggesting a 9 percentage point increase in NGE from crossing the threshold. This is consistent with an increased level of NGE from 18% to 27% when crossing the threshold on average. In column (2) we present the reduced form for employment and in column (3) we present the IV result. In both cases the effect of the policy appears to be positive. Similarly, both reduced form and IV for unemployment (columns (4) and (5)) suggest that the policy reduces unemployment. Using the IV estimates a 10 percentage point in NGE causes a 19% increase in employment and a 14% reduction in unemployment. These are larger than our main estimates in the text.

Although these implied effects are larger than our baseline estimates, they are very imprecisely estimated: neither is significant at conventional levels. This is unsurprising given the fuzziness of the design: the corresponding F-statistic on the first stage is only 7.3. The fuzziness of the RD design can also be seen in Online Appendix Figure A2. The break at the threshold is hard to see clearly due to the small number of observations. It is most visible for the NGE first stage but is much noisier for the labor market outcomes.

We also looked at empirically identifying cut-offs for all other variables in Online Appendix A2 that made up the policy rules as well as combinations of them. Although sometimes thresholds could be seen in the data for NGE and these policy variables (like GDP per capita), they were quite noisy. When using these thresholds in a RD Design like that for GDP per capita, we generally found that the point estimates suggested improved labor market outcomes, but with statistically insignificant effects (like Online Appendix Table A9).

Hence, although it is reassuring that the RD design delivers point estimates that are not very different from our main results, they are imprecise. Our baseline IV approach that use the other criteria does help us obtain more precision in the results albeit at the expense of a more parametric specification.

F.5 Other General Robustness tests: GE effects; longer time-period and matching

We have conducted many other robustness tests, especially on the core baseline results of Table 4.

First, we look to see if there are general equilibrium effects on wages by using average ln(wages) as an outcome variable in equivalent specifications to Tables 4 and 6. As argued in subsection II.C it is unlikely that there are substantive GE effects from the RSA policy given the magnitude of the sums spent and the nature of the policy. As expected, we never found significant effects (e.g. in the equivalent of column (4), Panel A of Table 4 NGE has a coefficient of 0.287 with a standard error of 0.877). Although the RSA policy also has no significant effect on wages in Table 6, we *did* find evidence of some equilibrium effects of SF. In the equivalent of column (5) of Table 6 SF has a coefficient (standard error) of 0.933(0.333). So, there may be some impact of this wider policy, even though we have shown it does not confound the RSA impact we identify.

Second, we estimated the regressions over a longer time-period (from 1986 to 2004) which includes the policy change in 1993 as well as the one we use in 2000. Unfortunately, detailed information on the construction of the policy rules for the period before 1993 is not available so we cannot construct the rule change instruments. Hence, we only run regressions of outcome variables on *actual* NGE support levels.[22] As noted above, data on unemployment and non-manufacturing employment is not available on a consistent basis pre-1997 (and even the manufacturing series has some concerns in these earlier years). Nevertheless, putting these concerns aside, we find coefficients implying that an increase of support intensity by 10 percentage points leads to a growth of 2.8% more jobs (see Online Appendix Table A11, column (2)). This is somewhat higher, but not significantly different from the results in column (1) which just uses the 1997-2004 period.

Third, we examined trimming the sample on a common support; i.e. we exclude observations that fall into the extremes of the distribution of employment and unemployment across wards. We successively drop larger bands from the edges of the distribution (1%, 5%, and 10%). None of this has much effect on the estimates (see last six columns of Online Appendix Table A11).

APPENDIX G: RSA COSTS PER JOB AND A COMPARISON WITH OTHER ESTIMATES IN THE LITERATURE

G.1 Calculating additional jobs in our study

We work out the cost per job by looking at the reduction in jobs that would arise if, instead of redrawing the map in 2000, the government had abolished the policy, i.e. had set NGE to zero in all areas. Note that while our model is specified using logs of employment we cannot use the approximation that the resulting estimates represent percentage changes because this only holds for relatively small changes. However, in our case we have changes in NGE which can be up to 35%. Hence, we calculate counterfactual employment levels when support is withdrawn in an area *r* as:

$$\ln EMP_r^{CF} = \ln EMP_r - \hat{\lambda}_1 NGE_r$$

Where EMP_r^{CF} is the counterfactual employment level in the absence of the policy, EMP_r is the current level of employment and β is the estimated coefficient on NGE. Consequently, the reduction in jobs in area *r* becomes

[22] Another limitation is that there is no consistent series of ward level unemployment for the period before 1996 so we just focus on employment.

$$\Delta EMP_r^{CF} \equiv \quad EMP_r^{CF} - \quad EMP_r \quad = \left(\frac{1}{\exp(\hat{\beta} NGE_r)} - 1\right) EMP_r$$

We calculate this counterfactual employment for each area using the area average level of employment from 2000-2004 (to smooth out any yearly variation) and the area level of NGE pre-2000.

In the main text, we do this calculation using the area level IV coefficient of 0.953. Using this coefficient we estimate that 156,000 jobs would have been lost if the policy had been abolished in 2000. With costs of £288 million, calculated as reported in the text, this gives us a cost per job of £1,846. Taking the more conservative OLS coefficients of 0.124 we get smaller job effects of just under 22,400 and a correspondingly higher cost per job of £12,857, again as reported in the text.

For the purposes of comparing to other studies, it is also useful to have an estimate from the firm level regressions. As the effect on large firms is insignificant from zero, we use the IV coefficient for small firms of 0.441 and calculate as before, but now using area level employment in supported small firms as the basis for the area level calculation. This gives an estimate of 20,790 jobs and a cost per job of £13,852.

G.2 Comparing the magnitude of our effects with other Place-Based Policies

We provide information on several other cost-per job estimates that have been published for similar area-based policies. To identify these studies, we started with the What Works Centre for Local Economic Growth (2016a, b) reviews which report results from a systematic review of the evaluation evidence. Systematic search terms were developed and applied on multiple platforms covering published research, working papers and government reports (e.g. EconPapers, Google Scholar, Gov.co.uk and OECD.org). The results were sifted based on relevance (area-based policy evaluations, OECD focus, and English language reports) and robustness of method according to the Maryland Scientific Methods Scale (Sherman et al. 1998). The What Works Centre uses a methodological cut-off point which requires a before and after comparison for treated and a suitable control group.[23] The review reports that the initial search found more than 2,100 policy evaluations and that sifting left 58 evaluations that met this minimum criterion. From these, we took the three studies that provided cost per job estimates.

We supplemented these three studies with additional cost per job estimates from more recently published evaluations of area-based policies identified using additional searches on Google Scholar. These additional searches mainly focused on identifying evaluations of area-based policies (although we also included one study that provided loans rather than grants to small businesses) published in the leading peer reviewed journals and other journals in the relevant field (which we judged to be “Urban Economics” given the nature of the intervention).

Ultimately, we found six cost-per job estimates for area-based policies that are reported in Online Appendix Table A21 (these are drawn from eight separate papers and we include our own estimates in this paper for comparison). We report the name of the program (column (1)), the country where the program was implemented (column (2)), a brief description of the intervention (column (3)), the econometric methodology (column (4)), the unit of analysis (column (5)), the cost per job estimate (column (6)) in 2010 US$, and the source articles (column (7)). We converted to US dollars using the original currency to dollar exchange rate in the price base year for reported costs. For example, if program costs were originally reported in £2003, then the £-$ exchange rate for 2003 would have been used. Historical yearly average exchange rates were taken from www.ofx.com. Finally, we adjusted to 2010 constant prices using a consumer price index for the US taken from the World Bank at https://data.worldbank.org/.

This exercise also identified several studies which provided less direct estimates of the cost per job – either modelling these from evaluations for other outputs (e.g. productivity), undertaking calculations using additional ad-hoc assumptions (e.g. imposing additionality or multiplier assumptions), or reporting ex-ante appraisal estimates. Figures for these studies are available on request. The range of costs is similar to those reported in Online Appendix Table A21.

Our own cost per job estimates of RSA in row 1 of Online Appendix Table A21 of $3,541 looks much lower than those reported in the other studies. Two methodological differences help explain our lower cost per job numbers. First, two of the three area-based studies reported in rows 4 and 5 use OLS rather than IV. If we use our OLS coefficients we derive a cost per job of $24,662 (reported in row 2), which is within the range of these two studies ($18,295 and $63,100).

Second, results for the three firm-based studies (rows 7 through 9 of Online Appendix Table A21) should be based on cost per job estimates derived using coefficients from our firm-level regressions. Here, we

[23] In practice, this means any study that uses an identification strategy based on a minimum Conditional Independence Assumption on observables such as propensity score matching or regression.

need to account for the fact that the effect of RSA on large firms is zero. Doing this by using data on small firms and the coefficient from the small firm only regressions gives a cost per job of $26,572 (row 3). This is higher than the US figure (row 7), but lower than the two Italian studies (rows 8 and 9). This suggest that ignoring (in our case positive) area level multipliers over-estimates the cost per job. It is also important to restrict job calculations to those firms for whom estimated effects are positive (in our case, small firms).

The comparisons in Online Appendix Table A21 highlight two ways in which our cost per job estimates improve on existing studies. But it is also important to note that aspects of RSA policy design also help explain some of the differences that persist even after making these methodological adjustments. RSA is selective and targeted at manufacturing firms who can provide evidence that they require support and that they do not serve just the local geographical market. By contrast, most of the area-based policies in Online Appendix Table A19 are non-selective providing support to all firms within the target area regardless of whether they can provide evidence that the subsidies are likely to create additional jobs. To the extent that RSA procedures better identify firms generating additionality we would expect less deadweight for RSA than for these other non-selective schemes (see the model discussion in Section II in the main text).

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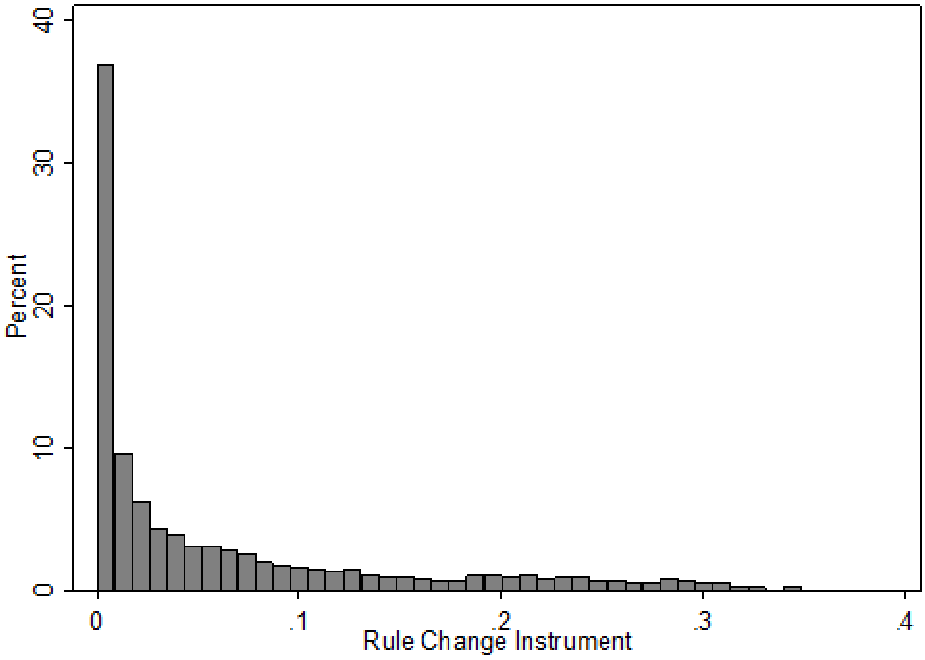
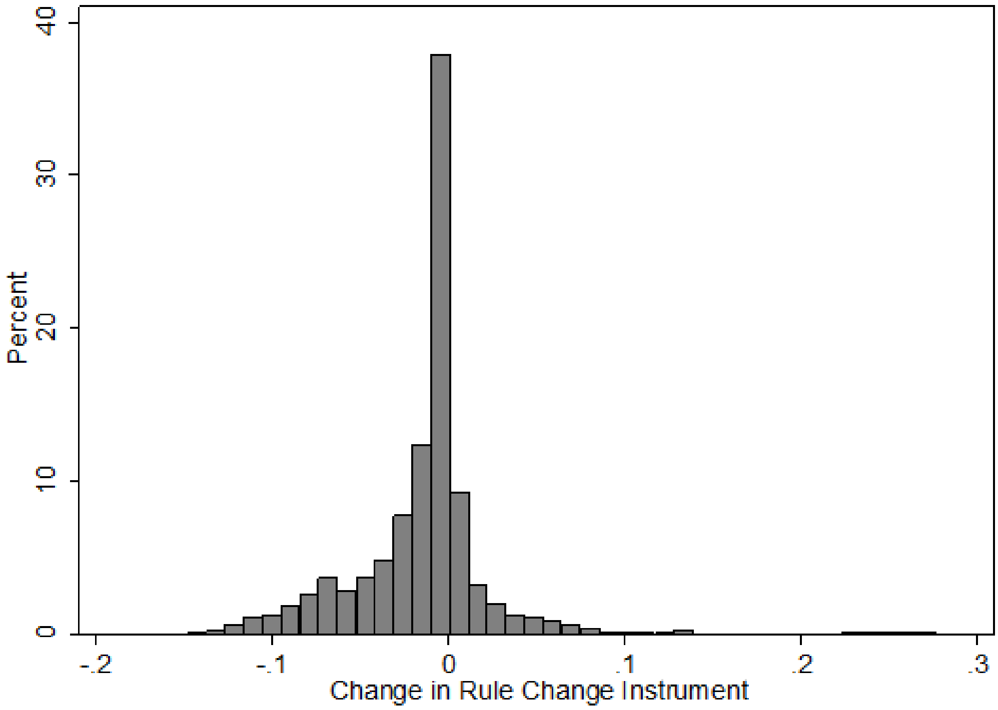
Figure A1: Distribution of the level of the policy rule instrumental variable

Panel A: Level of the instrument

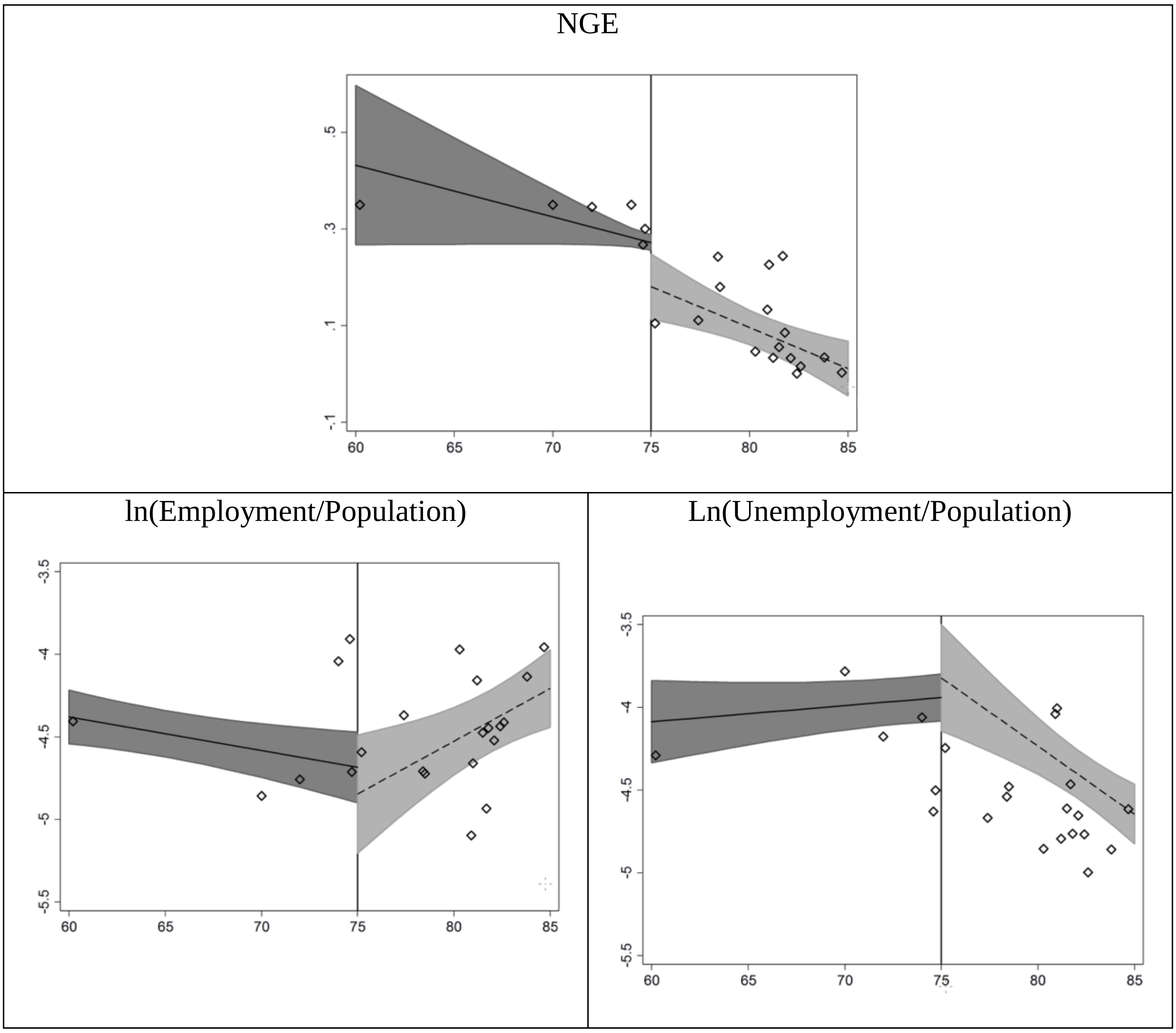


Panel B: Change in the value of the instrumental variable



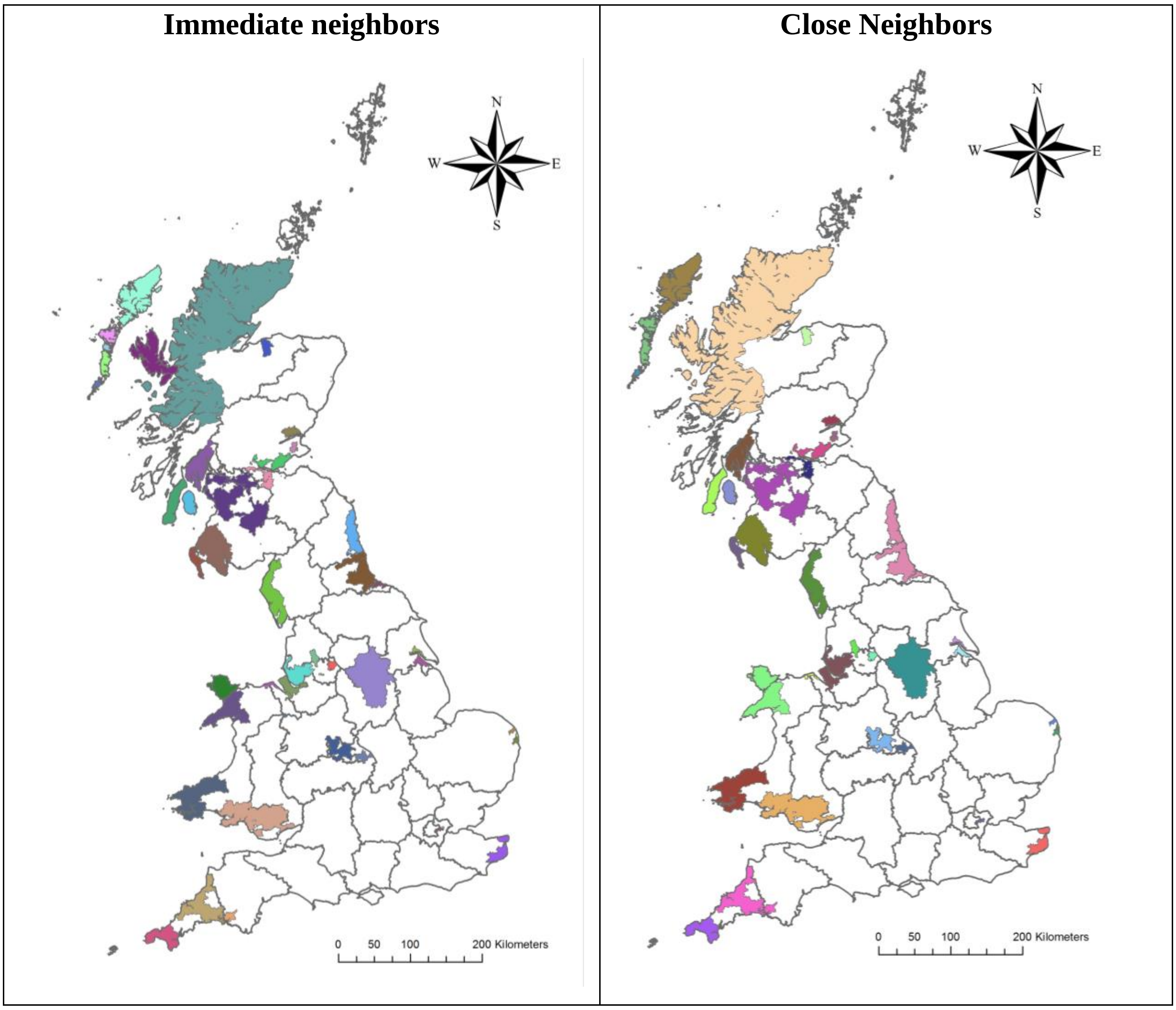
Notes: Histograms of the policy rule instrument based on 10,737 wards. Panel B is $\Delta z_{r,t}$ which is the actual IV used and Panel A is $z_{r,t}$ constructed from the expected probability of being in each subsidy regime multiplied by the level of subsidy in that regime. It is constructed form the ordered probits in Table 3 from which we can calculate the probability that an area falls into a subsidy regime in all years and the actual level of NGE. See Appendix B for further details.

Figure A2: Regression discontinuity at 75% GDP per capita relative to EU average threshold



Notes: The running variable on the horizontal axis is GDP per capita of the NUTS2 area relative to the EU wide average level. The diamonds show the mean value (across wards) of the dependent variable at a particular value of the running variable. Shaded areas are 95% confidence intervals.

Figure A3: Alternative approaches to spatial clustering



Notes: The Figure illustrates our two spatial clustering approaches based on similar support levels in neighboring clusters. The colored areas show common clusters based on the same support level in both the pre and post 2000 period for neighboring areas (wards). Non-treated wards are grouped based on NUTS2 areas shown in white. On the left, neighboring is defined as two wards having a common contiguous border leading to 70 treated and 32 non-treated clusters. On the right, neighboring it is defined as being no further than 1km away leading to 48 treated and 32 non-treated clusters. The corresponding regression results are reported in Online Appendix Table A20 Panels B and C, respectively.

Table A1: Descriptive statistics across areas (Wards), Manufacturing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Aggregate expenditure on RSA (£m)	**Average NGE (maximum investment subsidy) rate in eligible wards**	**Eligible Wards (as % of all Wards)**	**Jobs in eligible areas (millions)**	**Plants in Eligible Areas**	**Jobs in eligible areas (as % of all jobs)**	**% plants in eligible areas**	**% plants in eligible areas receiving support**
1997	158.27	0.241	31.9	1.230	44,755	42.1	32.4	4.4
1998	115.32	0.241	31.9	1.211	43,575	41.9	32.3	3.1
1999	91.76	0.241	31.9	1.168	43,101	41.7	32.3	2.8
2000	185.68	0.237	26.1	1.041	36,557	38.6	28.0	3.4
2001	219.69	0.237	26.1	1.002	35,837	37.9	28.0	3.1
2002	192.71	0.237	26.1	0.939	35,274	37.6	28.0	3.0
2003	197.26	0.237	26.1	0.900	34,797	37.7	28.1	3.1
2004	148.58	0.237	26.1	0.866	34,437	37.9	28.0	3.0
Average	163.66	0.238	28.3	1.045	38,542	39.4	29.6	3.1

Notes: Column (1) is total expenditure on RSA. Column (2) is the average NGE across eligible wards. Column (3) is the share of wards that are eligible for RSA. Column (4) are the number of jobs in eligible areas. Column (5) is the number of plants in eligible areas. Column (6) reports the jobs in eligible areas as a fraction of all jobs. Column (7) reports the plants in eligible areas as a fraction of all plants. Column (8) is the fraction of plants in eligible areas that receive support. All data refer to manufacturing sector.

Source: Industrial Development Reports (various years) and authors' calculation using the IDBR, ARD and SAMIS matched data.

Table A2 - Variables that define Rules for eligibility

Variable	**Definition**	**Timing of data used by EU for eligibility**	**Source**	**Used in which years for rules**
GDP per capita	Value added in the area per person (NUTS2)	1991 (for 1993); 1994-96 average (for 2000)	Eurostat	1993 and 2000
Population Density	Number of inhabitants per square km (district)	1981 (for 1993) 1991 (for 2000)	Census	1993 and 2000
Share of high Skilled workers	Share of working residents aged over 16 in high skilled (SOC Groups 1 to 3) occupation (ward)	1991 (for both)	Census	1993 and 2000
Start-Up rate	Annualized net percentage rate of growth of company VAT registrations (except retail and agriculture); i.e. total registrations minus de-registrations (district)	1987-1991	ONS Business Register	1993
Structural Unemployment rate	Average annual unemployment (based on ILO definition) rate 5-year average (district)	1986-90 (for 1993); 1992-96 (for 2000)	ONS	1993 and 2000
Activity rate	Fraction of working age population who are economically active. For men: 16-64; for women: 16-59. (ward)	1991 (for both)	Census	1993 and 2000
Employment Rate	Residents in employment divided by population of working age (district)	1992	Labor Force Survey	2000
Current Unemployment rate (Claimant Count)	Average monthly unemployment rate over year. Based on residents claiming unemployment insurance divided by labor force (district)	1991 (for 1993); 1998 (for 2000)	ONS	1993 and 2000
ILO Unemployment Rate	Proportion of residential labor force who are "ILO" unemployed (district)	1992	Labor Force Survey	2000
Long-duration Unemployment rate	Number claiming unemployed insurance for more than a year as a fraction of the labor force (ward)	1991	Census	1993
Share of manufacturing workers	Number of manufacturing employee jobs divided by total jobs (ward)	1991	Census	2000

Notes: These are the definitions of variables used by the EU to determine whether an area is eligible for RSA and if so, at what level of support. The definitions column also gives the level of aggregation that the data is defined at (in parentheses). ILO unemployed are defined as individuals who are (i) without a job, want a job, have actively sought work in the last four weeks and are available to start work in the next two weeks, or (ii) are out of work, have found a job and are waiting to start it in the next two weeks. People who are not claimants can appear among ILO unemployed if they are not entitled to unemployment related benefits. Similarly, unemployment claimants may not appear in the LFS measure of unemployment if they state that they are not seeking, or are not available, to start work. The average district in our data contains 25 wards and the average NUT2 contains 15 districts.
Source: Official Journal of the European Communities (1998), OJ C 74, 10.3; and OJ C 88/C 212/02, 12.8.1988; Department of Trade and Industry (1999) "The UK Government's proposals for new Objective 2 areas" Official letter SG(2000) D/ 106293; Department of Trade and Industry (1993), "Review of the assisted areas of Great Britain. Background document on the new assisted areas map."

Table A3: Changes in area eligibility for Structural Funds (SF) and RSA before and after 2000

	(1)	(2)	(3)	(4)	(5)
	Total	**Eligible for RSA and eligible for SF in 2000 onwards**	**Eligible for RSA and not eligible for SF 2000 onwards**	**Non-eligible for RSA and eligible for SF 2000 onwards**	**Non-eligible for RSA and non-eligible for SF 2000 onwards**
1. Eligible for RSA and eligible for SF in 1993-99	2,424 (22.58% of total wards)	1,743	0	681	0
% of row	*100%*	*71.91%*	*0.00%*	*28.09%*	*0.00%*
2. Eligible for RSA and not eligible for SF in 1993-99	1,004 (9.35% of total wards)	384	195	0	425
% of row	*100%*	*38.25%*	*19.42%*	*0.00%*	*42.33%*
3. Non-eligible for RSA and eligible for SF in 1993-99	703 (6.55% of total wards)	175	0	528	0
% of row	*100%*	*24.89%*	*0.00%*	*75.11%*	*0.00%*
4. Non-eligible for RSA and non-eligible for SF in 1993-99	6,606 (61.53% of total wards)	177	134	0	6,295
% of row	*100%*	*2.68%*	*2.03%*	*0.00%*	*95.29%*
Total	**10,737**	**2,479**	**329**	**1,209**	**6,720**

Notes: This is the transition matrix showing numbers of wards before and after the policy change in 2000 (for RSA and SF). For example, column (1) of the first row details that there were 2,424 areas eligible for both RSA and SF pre-2000 (22.58% of total wards as noted in parentheses below the figure). The next 4 rows show how these rows transitioned into different RSA and SF regimes. For example, column (2) shows 1,743 (*71.91*%) remained eligible for both RSA and SF after 2000 whereas column (4) shows 681 (*28.09%*) lost access to RSA but not SF.

Table A4: Estimates of parameters on eligibility rule changes for Structural Fund (SF) IV

Year	1993	2000
Dependent Variable: Eligibility for Structural Funds		
GDP per capita	-0.046	-0.057
	(0.002)	(0.002)
Population density	-0.029	-0.046
	(0.002)	(0.003)
Share of high skilled workers	-0.501	-0.406
	(0.149)	(0.155)
Employment rate	-2.955	-6.703
	(0.376)	(0.524)
Current unemployment rate	33.835	52.296
(claimant count)	(2.479)	(2.600)
ILO unemployment rate	4.274	0.162
	(0.793)	(0.912)
Share of manufacturing workers	3.028	2.127
	(0.215)	(0.218)
Robberies	263.371	265.231
	(19.781)	(21.382)
Drug Crimes	-87.770	12.598
	(6.250)	(2.013)
Burglaries	35.245	7.093
	(0.046)	(0.057)
Log Likelihood	-4,261.914	-3,900.468
Number of areas (wards)	10,737	10,737

Notes: denotes significance at the 1% level, 5% level and 10% level. The table reports the regressions we perform to derive instruments for structural fund eligibility of an area. For that we regress in column (1) a dummy indicating structural fund eligibility pre-2000 on various area level statistics evaluated before 1993. The second column performs the same regression on a dummy indicating SF eligibility post-2000 (with the same control variables). Standard errors (in parentheses below coefficients) are clustered at the area (ward) level.

Table A5: Controlling for Other policies – Unemployment as an outcome

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable: Ln(Unemployment)								
A. Reduced Form								
Rule change IV	-0.336	-0.367	-0.363	-0.399	-0.350	-0.346	-0.292	-0.253
	(0.069)	(0.069)	(0.069)	(0.069)	(0.069)	(0.068)	(0.069)	(0.069)
Employment Zones	-0.017						-0.032	-0.028
	(0.008)						(0.008)	(0.008)
Coalfield Regeneration Trust		-0.009					0.004	0.004
		(0.006)					(0.006)	(0.006)
Regional Venture Capital Funds			0.036				0.041	0.039
			(0.008)				(0.008)	(0.008)
Enterprise Grants				-0.066			-0.053	-0.050
				(0.006)			(0.007)	(0.007)
New Deal for Communities					0.025		0.050	0.045
					(0.007)		(0.007)	(0.007)
Devolution to Wales and Scotland						0.068	0.062	0.073
						(0.007)	(0.007)	(0.007)
Structural Fund IV								-0.108
								(0.023)
B. IV								
NGE	-0.407	-0.420	-0.413	-0.463	-0.394	-0.388	-0.346	-0.234
	(0.084)	(0.079)	(0.078)	(0.081)	(0.078)	(0.076)	(0.082)	(0.096)
Employment Zones	-0.004						-0.024	-0.016
	(0.009)						(0.009)	(0.009)
Coalfield Regeneration Trust		-0.019					-0.002	-0.001
		(0.006)					(0.006)	(0.006)
Regional Venture Capital Funds			0.031				0.038	0.036
			(0.008)				(0.008)	(0.008)
Enterprise Grants				-0.083			-0.062	-0.064
				(0.007)			(0.007)	(0.007)
New Deal for Communities					0.031		0.058	0.054
					(0.007)		(0.007)	(0.007)
Devolution to Wales and Scotland						0.086	0.074	0.077
						(0.007)	(0.007)	(0.007)
Structural Fund								-0.087
								(0.026)
Number of areas (wards)	10,737	10,737	10,737	10,737	10,737	10,737	10,737	
Observations	85,896	85,896	85,896	85,896	85,896	85,896	85,896	

Notes: denotes significance at the 1% level, 5% level and 10% level. This is the same specification as Table 7 except with ln(unemployment) instead of ln(manufacturing employment) as the dependent variable. Standard errors (in parentheses below coefficients) are clustered at the area (ward) level. NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. The time-period is 1997-2004. Rule Change IV is described in text. Panel A has a specification identical to column (2) in Panel A of Table 4 except additional policy variables have been included (see text). Panel B has a specification identical to column (4) in Panel A of Table 4 except additional policy variables have been included (see text).

Table A6: Alternative ways of controlling for initial conditions in area regressions

Method	(1) OLS	(2) Reduced Form	(3) First Stage	(4) IV
A. Dependent variable: ln(Manufacturing Employment); No initial lagged values				
Maximum investment subsidy	0.218			1.140
NGE	(0.071)			(0.244)
Policy Rule Instrument		1.007	0.883	
		(0.215)	(0.032)	
B. Dependent variable: ln(Unemployment); No initial lagged values				
Maximum investment subsidy	-0.226			-0.583
NGE	(0.025)			(0.076)
Policy Rule Instrument		-0.515	0.883	
		(0.067)	(0.032)	
C. Dependent variable: ln(Manufacturing Employment); Second order polynomial in all lagged characteristics				
Maximum investment subsidy	0.095			1.110
NGE	(0.071)			(0.247)
Policy Rule Instrument		0.930	0.837	
		(0.204)	(0.034)	
D. Dependent variable: ln(Unemployment); Second order polynomial in all lagged characteristics				
Maximum investment subsidy	-0.139			-0.671
NGE	(0.024)			(0.079)
Policy Rule Instrument		-0.562	0.837	
		(0.064)	(0.034)	
E. Dependent variable: ln(Manufacturing Employment); Including predicted probabilities				
Maximum investment subsidy	0.139			0.887
NGE	(0.070)			(0.256)
Policy Rule Instrument		0.786	0.886	
		(0.227)	(0.033)	
F. Dependent variable: ln(Unemployment); Including predicted probabilities				
Maximum investment subsidy	-0.127			-0.334
NGE	(0.024)			(0.078)
Policy Rule Instrument		-0.296	0.886	
		(0.069)	(0.033)	
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896

Notes: denotes significance at the 1% level, 5% level and 10% level. Standard errors (in parentheses below coefficients) are clustered at the area (ward) level. NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. The time-period is 1997-2004. These specifications are the same as Table 4 except instead of including lagged linear controls used to define eligibility in 1993 ($X_{r,93}$), Panels A and B exclude them, while Panels C and D include both linear controls and a second order polynomial in these terms (all cross products and quadratic terms). Panels E and F include the predicted probabilities of receiving a particular level of support as additional controls. These are derived from the multinomial regression of support level state on long lagged area level statistics, which we use to construct our instruments. (Table 3 columns (1) and (2)).

Table A7: Area Level regressions – Placebo regressions 1995-1999

	(1)	(2)
Dependent Variable: ln(employment)		
Specification	Baseline: 2000	Placebo: 1997
Years	1997-2004	1995-1999
Policy Rule Instrument	0.839	0.162
	(0.228)	(0.163)
Number of areas (wards)	10,737	10,737
Observations	85,896	53,685

Notes: denotes significance at the 1% level, 5% level and 10% level. Column (1) is the same specification as the reduced form employment equation of Table 4 Panel A using the baseline in 2000 when actual policy rule change took place. The placebo in column (2) uses employment changes as if the policy change happened in 1997 in column (2).

Table A8: Area level - Bootstrapped standard errors to account for generated regressors

Dependent Variable:	**Ln(Employment)**	**Ln(Unemployment)**
Policy Rule Instrument	0.839	-0.365
	(0.215)	(0.072)
Number of areas (wards)	10,737	10,737
Observations	85,896	85,896

Notes: denotes significance at the 1% level, 5% level and 10% level. The standard errors in most of our tables ignore the fact that our policy rule instrument emerges after regressing support status on various area level statistics taken into account by EU rules; i.e. as reported in Table 3. Here we provide bootstrapped results (clustered at the ward level, 200 replications). This shows standard errors very similar to those simpler ones found in column (2) of Table 4.

Table A9: Regression Discontinuity Design approach using only GDP per capita policy variable

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	**NGE**	**ln(Employment)**		**Ln(Unemployment)**	
	First Stage	**Reduced Form**	**IV**	**Reduced Form**	**IV**
D (Threshold)	0.091	0.161		-0.119	
	(0.034)	(0.188)		(0.161)	
NGE			1.7665		-1.299
			(2.339)		(1.962)
Running variable	-0.017	0.064	0.094	-0.082	-0.104
	(0.005)	(0.021)	(0.060)	(0.018)	(0.05)
Running	0.006	-0.084	-0.095	0.092	0.100
Variable Threshold	(0.006)	(0.024)	(0.037)	(0.020)	(0.032)
Observations	27,562	27,562	27,562	27,562	27,562
Wards	4,079	4,079	4,079	4,079	4,079
# of NUTS2 clusters	14	14	14	14	14

Notes: denotes significance at the 1% level, 5% level and 10% level. Coefficients are from OLS regressions, with standard errors below clustered by NUTS2. An observation is a ward-year. All regressions include ln(population in the ward). Bandwidth is (wards in) NUTS2 areas between 60% to 95% GDP per capita of EU average. See text for exact specifications. The dependent variables in columns (2) through (4) are normalized on area population.

Table A10: Alternative RD Design

	(1)	(2)
Dependent Variable:	Employment	Unemployment
Maximum investment subsidy (NGE)	0.160	-0.210
	(0.070)	(0.024)
Area statistics defining support eligibility pre-2000	Yes	Yes
Area statistics defining support eligibility post-2000	Yes	Yes
Number of areas (wards)	10,737	10,737
Observations	85,896	85,896

Notes: denotes significance at the 1% level, 5% level and 10% level. Coefficients are from OLS regressions, with standard errors below clustered by ward. NGE (“Net Grant Equivalent”) is the level of the maximum investment subsidy in the area. All columns include a full set of linear (lagged) characteristics used to define eligibility in the pre and post-2000 period. The time-period is 1997-2004. Dependent variables are in differences of relative to the base year of 1997.

Table A11: Robustness of Ward Level regressions – Long time horizon (1986-2004) and Common Support

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Var:	**ln(Employment)**		**Ln(Employment)**			**Ln(Unemployment)**		
Years	1997-2004	1986-2004	1997-2004	1997-2004	1997-2004	1997-2004	1997-2004	1997-2004
Trimming:	None	None	1%	5%	10%	1%	5%	10%
NGE	0.169	0.280						
	(0.057)	(0.071)						
Rules Change IV			0.895	0.856	0.958	-0.419	-0.322	-0.313
			(0.234)	(0.269)	(0.303)	(0.068)	(0.073)	(0.079)
# of areas(wards)	10,737	10,737	10,322	8,755	6,985	10,322	8,755	6,985
Observations	85,896	204,003	82,576	70,040	55,880	82,576	70,040	55,880

Notes: denotes significance at the 1% level, 5% level and 10% level. Each cell is from a different regression and each of the eight panels is a different sample and measure of the treatment effect. Columns (1) and (2) are by OLS (as we do not know pre-1993 policy rules to construct IVs) and include a full set of area fixed effects and time dummies (so within groups rather than the standard long-differences regressions as it is unclear which year to use as baseline). Standard errors below coefficients are clustered by area (ward level) in all columns. The time-period is 1986-2004 in columns (1) and (2) and 1997-2004 in the other columns. NGE is maximum investment grant subsidy. Columns (3)-(8) trim the sample to get a closer common support and re-run the reduced form of Table 3 Panel A column (2). "1%" trims the sample at the lowest and top percentiles, "2%" trims from 2nd to 98th percentile, etc.

Table A12: Alternative Firm Size cut-offs, Plant-level employment regressions

	(1) OLS	(2) Reduced Form	(3) First Stage	(4) IV
Panel A. Small Firm employment less than 40 in 1996 (583,259 observations; 86,109 firms; 9,874 areas)				
NGE	-0.002			0.429
	(0.027)			(0.096)
Policy Rule Instrument		0.292	0.680	
		(0.064)	(0.040)	
Panel B. Large Firm employment greater than 40 in 1996 (70,126 observations; 10,659 firms; 4,008 areas)				
NGE	0.065			0.200
	(0.052)			(0.191)
Policy Rule instrument		0.126	0.629	
		(0.120)	(0.052)	
Panel C. Small Firm employment less than 60 in 1996 (601,976 observations; 88,837 firms 9,893 areas)				
NGE	0.004			0.431
	(0.026)			(0.094)
Policy Rule instrument		0.292	0.679	
		(0.062)	(0.040)	
Panel D. Large Firm employment greater than 60 in 1996 (51,409 observations; 7,931 firms; 3,466 areas)				
NGE	0.055			0.174
	(0.059)			(0.221)
Policy Rule Instrument		0.11	0.632	
		(0.140)	(0.052)	

Notes: denotes significance at the 1% level, 5% level and 10% level. Standard errors (in parentheses below coefficients) are clustered at the area (ward) level. These are all plant-level regressions splitting the samples by firm size in 1996 (or the year the plant enters the sample). Each cell is from a different regression. All columns include a full set of area fixed effects time dummies. Standard errors below coefficients are clustered by area (ward level) in all columns. The time-period is 1997-2004. Policy Rule instrument is described in text.

Table A13: Do small firms respond to treatment more because they are younger?

	(1)	(2)	(3)	(4)	(5)
Dependent variable: ln(employment)					
Young = Alive for no more than:		**3 years**	**4 years**	**5 years**	**6 years**
NGE	0.034	0.043	0.067	0.047	0.071
	(0.184)	(0.184)	(0.185)	(0.186)	(0.187)
NGE × Small Firm	0.407	0.408	0.430	0.429	0.442
	(0.200)	(0.201)	(0.201)	(0.203)	(0.202)
NGE × Young firms		-0.187	-0.396	-0.108	-0.162
		(0.315)	(0.225)	(0.170)	(0.160)
Observations	653,385	653,385	653,385	653,385	653,385
Firms	96,768	96,768	96,768	96,768	96,768
Number of Areas (wards)	9,975	9,975	9,975	9,975	9,975

Notes: denotes significance at the 1% level, 5% level and 10% level. These are specifications equivalent to Table 10 Panel A column (4) except that we include additional interactions as specified. Column (1) includes an interaction with "small" - defined as firms with less than 50 employees (as in Table 10) only. Columns (2) to (5) are based on different definitions of a "young" firm. Column (2) defines young to be a firm that is one year old or younger; in column (3) young = 2 years old or less, etc. All treatment variables are instrumented using the equivalent interactions between the policy rule instrument and the respective indicators for "small" and "young".

Table A14: Is absence of policy effect on plants in large firms because they receive less subsidies?

Method	(1) OLS	(2) First Stage	(3) IV
Sample: Pooled across all plants, 653,385 observations on 96,768 plants, 9,975 wards			
A. Pooled, Dummy for subsidy receipt			
Receiving any subsidy?	-0.004		1.658
RSA>0	(0.011)		(0.464)
Policy Rule Instrument		0.188	
		(0.040)	
B. Pooled, subsidy amount			
ln(subsidy)	0.001		0.276
RSA	(0.001)		(0.112)
Policy Rule Instrument		1.132	
		(0.418)	
Sample: Small (Plants in Firm with under 50 employees) 594,356 observations on 87,728 plants, 9,880 wards			
C. Small, Dummy for subsidy receipt			
Receiving any subsidy?	-0.02		1.891
RSA>0	(0.013)		(0.607)
Policy Rule Instrument		0.158	
		(0.037)	
D. Small, subsidy amount			
Ln(subsidy)	0.000		0.329
RSA	(0.001)		(0.160)
Policy Rule Instrument		0.908	
		(0.401)	
Sample: Large (Plants in Firm with over 50 employees), 59,029 observations on 9,040 plants, 3,708 wards			
E. Large, Dummy for subsidy receipt			
Receiving any subsidy?	0.022		0.120
RSA>0	(0.018)		(0.345)
Policy Rule Instrument		0.375	
		(0.139)	
F. Large, subsidy amount			
ln(subsidy)	0.002		0.019
RSA	(0.002)		(0.055)
Policy Rule Instrument		2.384	
		(1.569)	

Notes: denotes significance at the 1% level, 5% level and 10% level. Each cell is from a different regression and each of the six panels is a different sample and measure of the treatment effect. Standard errors below coefficients are clustered by area (ward level) in all columns. The time-period is 1997-2004. Policy Rule instrument is described in text. “Receiving any subsidy” (RSA>0) is a dummy switched on when the firm begins receiving an investment subsidy and ln(subsidy), RSA, is the log of (1+the amount of subsidy received). All columns include a full set of long memory area statistics. All variables are in differences of ln(1+Y) relative to the base year 1997, where Y is the raw value of the variable.

Table A15: RSA Impact in large vs small firms

	(1) **Average Subsidy amount (£1,000s)**	(2) **Average number of employees**	(3) **Elasticity between employment and subsidies**	(4) **Marginal Impact of subsidy**
Small Firm (under 50)	29.45	18	0.329	0.201
Large Firm (over 50)	107.19	211	0.012	0.024

Notes: The Table calculates the marginal effect of £1,000 of subsidy on the number of jobs, split by large and small firms. Column (1) is the average subsidy received and column (2) is the average plant size from our data 1997-99. In column (3) we report the elasticity between jobs and subsidies received (γ) as estimated in column (3) of Online Appendix Table A14 for small firms (Panel D) and large firms (Panel F). Since $\gamma = \frac{\partial \ln \mathrm{L}}{\partial \ln \phi}$ where L = employment and ϕ = subsidy, the marginal effect of a \$ of subsidy on the number of jobs is: $\frac{\partial \mathrm{L}}{\partial \phi} = \gamma \frac{L}{\phi}$. This is given in column (4). It shows that the marginal impact of subsidies on jobs is over eight times (= 0.201/0.024) bigger in plants belonging to small firms rather than large firms.

Table A16: Alternative ways of measuring firm-level productivity

	(1)	(2)	(3)
Method of measuring TFP:	**Factor Share**	**Regression**	**MU OMEGA**
Policy Rule instrument	-0.034	-0.017	0.336
	(0.043)	(0.065)	(3.206)
Observations	45,511	45,511	18,999
Firms	21,389	21,389	9,139

Notes: denotes significance at the 1% level, 5% level and 10% level. These are reduced form specifications corresponding to column (2) of Panel E in Table 11. **“**Factor Share” method in column (1) reproduces the results reported in Panel E of Table 11 for reference; i.e. TFP is computed using a “factor share” method and relative to an industry by year average “Regression” method in column (2) includes (the log of) labor, materials and capital as additional control variables in a specification where the dependent variable is ln(revenue). “MU OMEGA” in column (3) implements the structural production function framework proposed in Martin (2012) which takes into account firm specific variation in market power when computing TFP. The exact method of construction is in the final subsection of Appendix C.

Table A17: Firm level regressions with capital intensity interactions

Dependent variable: ln(employment)		
Policy Rule Instrument	0.519	0.247
	(0.112)	(0.137)
Policy Rule Instrument × High Capital Intensity		0.525
		(0.200)
Observations	72,902	72,902
Firms	12,242	12,242

Notes: denotes significance at the 1% level, 5% level and 10% level. These are specifications equivalent to column (2) of Table 11 Panel B. Standard errors (in parentheses below coefficients) are clustered at the area (ward) level. Capital intensity is firm level average capital to labor ratio before 2000. "High capital intensity" is a dummy equal to one if capital intensity is above the sample median and zero otherwise. Sample is smaller than in other firm level results because firms without valid observations for pre-2000 capital intensity are excluded.

Table A18: Instrumenting NGE with Policy Rule Instrument (but using linear probability model instead of ordered probit for Table 3)

	(1)	(2)	(3)	(4)
Method	OLS	Reduced Form	First Stage	IV
A. Dependent variable: ln(Employment)				
Maximum investment subsidy	0.124			1.509
NGE	(0.070)			(0.255)
Policy Rule Instrument		1.266	0.839	
		(0.208)	(0.036)	
B. Dependent variable: ln(Unemployment)				
Maximum investment subsidy	-0.137			-0.767
NGE	(0.024)			(0.099)
Policy Rule Instrument		-0.644	0.839	
		(0.079)	(0.036)	
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896

Notes: denotes significance at the 1% level, 5% level and 10% level. NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. All columns include a full set of area fixed effects time dummies. Standard errors below coefficients are clustered by area (ward level) in all columns. The time-period is 1997-2004. This Table corresponds to Table 4, however, we use a slightly different version of the policy rule instrument. Instead of the ordered probit reported in Table 3, the instrument here is based on a binary Probit of the event "NGE>0".

Table A19: Other policies (introduced between 1997 and 2004)

Policy	Aim	When Introduced?	Area Eligibility
New Deal for the Long Term Unemployed	Helping long-term unemployed (over 25 years old) into work. Mandatory work search, training or wage subsidy.	July 1999	National
Employment Zones (EZ)	To improve the employability of the long-term unemployed through skill acquisition, fast-track job services and removal of restrictions to getting jobs.	April 2000	In 15 disadvantaged areas EZ provision replaced New Deal for Long-Term Unemployed.
Coalfields Regeneration Trust	To support areas historically dependent on Coalfields.	1999	Coalfields
New Deal for Communities	To tackle multiple deprivation in the poorest areas.	1998-2008	17 areas 1998-2010; 22 areas 1999-2011 (10 in London and others throughout England). 9,900 people per area on average
New Deal for 18 - 24 year old unemployed people	To help young unemployed people find work. Mandatory work search, training or wage subsidy.	July 1999	National
New deal for Lone Parents	To encourage lone parents into work.	April 1998	National
New Deal for Partners of Unemployed People	To give unemployed partners of unemployed access to employment programs.	April 1999	National
New Deal for Disabled People	Helping people off disability benefit and into work	July 2001	National

Job Centre Plus	Merged services of working age welfare and unemployment benefits. Increased IT spending and incentives for public sector workers. Aim of getting more benefit claimants into work.	April 2002	National
Phoenix Fund	To encourage entrepreneurship in disadvantaged areas.	1999	National
Enterprise Fund	To give entrepreneurs access to finance by creating a £180m fund for debt and equity finance to SMEs with growth potential (e.g. UK High Technology growth fund specialized in fund of fund investments in venture capital).	December 1998	National
Regional Venture Capital Funds	Provision of small scale equity (under £500,000) to firms with growth potential.	West Midlands and East of England from 2002	All England from 2003
Grant for research and development	To provide grants for investigating innovative ideas and knowledge transfer.	1999	National
Single Regeneration Budget	To support local initiatives to make a contribution towards the area regeneration.	1994-2002	National

Notes: Details of different policies that could potentially confound the effects of RSA.

Table A20: Alternative approaches to spatial correlation

Panel A: Clusters based on Travel to Work Areas (TTWA); 322 clusters

Method	OLS	Reduced Form	First Stage	IV
Dependent variable: ln(Manufacturing Employment)				
Maximum investment subsidy	0.124			0.953
NGE	(0.080)			(0.279)
Policy Rule Instrument		0.839	0.881	
		(0.238)	(0.133)	
Dependent variable: ln(Unemployment)				
Maximum investment subsidy	-0.137			-0.414
NGE	(0.052)			(0.198)
Policy Rule Instrument		-0.365	0.881	
		(0.180)	(0.133)	
Number of Clusters	322	322	322	322
Panel B: Clusters based on immediate neighbors; 102 clusters				
Dependent variable: ln(Manufacturing Employment)				
Maximum investment subsidy	0.124			0.953
NGE	(0.082)			(0.330)
Policy Rule Instrument		0.839	0.881	
		(0.259)	(0.140)	
Dependent variable: ln(Unemployment)				
Maximum investment subsidy	-0.137			-0.414
NGE	(0.053)			(0.237)
Policy Rule Instrument		-0.365	0.881	
		(0.197)	(0.140)	
Number of Clusters	102	102	102	102
Panel C: Clusters based on close neighbors; 80 clusters				
Dependent variable: ln(Manufacturing Employment)				
Maximum investment subsidy	0.124			0.953
NGE	(0.081)			(0.343)
Policy Rule Instrument		0.839	0.881	
		(0.275)	(0.145)	
Dependent variable: ln(Unemployment)				
Maximum investment subsidy	-0.137			-0.414
NGE	(0.053)			(0.239)
Policy Rule Instrument		-0.365	0.881	
		(0.198)	(0.145)	
Number of Clusters	80	80	80	80
Panel D: Clusters based on NUTS2 regions; 34 clusters				
Dependent variable: ln(Manufacturing Employment)				
Maximum investment subsidy	0.124			0.953
NGE	(0.087)			(0.458)
Policy Rule Instrument		0.839	0.881	
		(0.367)	(0.188)	
Dependent variable: ln(Unemployment)				
Maximum investment subsidy	-0.137			-0.414
NGE	(0.058)			(0.278)
Policy Rule Instrument		-0.365	0.881	
		(0.236)	(0.188)	
Number of Clusters	34	34	34	34

Notes: These are identical regressions to Table 4 (85,896 observations) except we allow the standard errors to be clustered at a higher level than the ward. Panel A clusters the standard errors at the Travel to Work Area (TTWA) level. Panel B clusters at the "neighboring ward" level defined to be wards that are (i) directly adjacent (ii) that receive the same level of support pre and post-2000. Wards not receiving any support are clustered at the NUTS2 level. Panel C defines neighboring wards more broadly to be (i) within 1km of each other and (ii) receiving the same level of support pre and post 2000. Wards not receiving any support are again clustered at the NUTS2 level. Panel D is the most conservative simply clustering at the NUTS2 level.

Table A21: Cost per job estimates in RSA compared to others in the literature

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
#	**Program**	**Country**	**Program Description**	**Method**	**Unit**	**Cost per job (2010 USD)**	**Source(s)**
1	Regional Selective Assistance	UK	Investment subsidies to businesses in disadvantaged areas.	IV	Area (wards)	3,541	This paper
2	Regional Selective Assistance	UK	Investment subsidies to businesses in disadvantaged areas.	DD	Area (wards)	24,662	This paper
3	Regional Selective Assistance	UK	Investment subsidies to businesses in disadvantaged areas.	IV	Small Firms	26,572	This paper
4	Empowerment Zones	US	Grants, hiring credits and other benefits for businesses in distressed urban areas.	DD	Area (tract)	18,295	Bartik (2010), Busso et al. (2010)
5	Empowerment Zones	US	Grants, hiring credits and other benefits for businesses in distressed urban areas.	DD	Area (tract)	63,100	Glaeser and Gottlieb (2008), Busso and Kline (2008)
6	New Markets Tax Credit	US	Subsidised capital investment in low-income neighborhoods.	RDD	Area (tract)	50,820	Freedman (2012)
7	Small Business Administration loans	US	Guaranteed and partially-guaranteed loans up to $5.5m for small businesses.	IV	Firm	22,781	Brown and Earle (2017)
8	Law 488/91	Italy	Capital subsidies to businesses in least-developed regions.	RDD	Firm	42,638	Pellegrini and Muccigrosso (2017)
9	Law 488/91	Italy	Capital subsidies to businesses in least-developed regions.	RDD	Firm	68,409	Cerqua and Pellegrini (2014)

Notes: Cost per job estimates have been converted from original units to US$ using yearly average exchange rates for the year that costs were reported for and then deflated to 2010 using a US consumer price index from the World Bank. Midpoints are taken where cost per job is reported as a range. In cases where base year is not stated, the last year of reported expenditures is taken. In the methods column: IV is instrumental variables, DD is differences-in-differences and RDD is regression discontinuity design. If more than one source is cited, the first source provides the cost per job estimate based on job effects that are cited in the second source.