NBER WORKING PAPER SERIES

THE CAUSAL EFFECTS OF AN INDUSTRIAL POLICY

Chiara Criscuolo
Ralf Martin
Henry Overman
John Van Reenen

Working Paper 17842
http://www.nber.org/papers/w17842

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2012

Helpful comments have come from seminar participants in Berkeley, Essex, HECER, Helsinki, LSE, Lausanne, NARSC, NBER, NIESR, Paris, Stanford and Stockholm. Financial support is from the British Academy and ESRC through the CEP and SERC. We would like to thank the Department of Business and Innovation for data access and Paul David, Fernando Galindo-Rueda, Pete Klenow, Enrico Moretti, Beatrice Parrish, Marjorie Roome, David Southworth and Alex Wilson for useful insights. The ONS Virtual Microdata Lab ensured access to ONS Data, Alberta Criscuolo helped with the EU legislation and Mehtap Polat provided excellent research assistance. Errors in use of these data are our own. This work contains statistical data from ONS which is Crown copyright and reproduced with the permission of the controller of HMSO and Queen's Printer for Scotland. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed a financial relationship of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w17842.ack

The Causal Effects of an Industrial Policy
Chiara Criscuolo, Ralf Martin, Henry Overman, and John Van Reenen
NBER Working Paper No. 17842
February 2012
JEL No. H25,L52,L53,O47

ABSTRACT

Business support policies designed to raise productivity and employment are common worldwide, but rigorous micro-econometric evaluation of their causal effects is rare. We exploit multiple changes in the area-specific eligibility criteria for a major program to support manufacturing jobs ("Regional Selective Assistance"). Area eligibility is governed by pan-European state aid rules which change every seven years and we use these rule changes to construct instrumental variables for program participation. We match two decades of UK panel data on the population of firms to all program participants. IV estimates find positive program treatment effect on employment, investment and net entry but not on TFP. OLS underestimates program effects because the policy targets underperforming plants and areas. The treatment effect is confined to smaller firms with no effect for larger firms (e.g. over 150 employees). We also find the policy raises area level manufacturing employment mainly through significantly reducing unemployment. The positive program effect is not due to substitution between plants in the same area or between eligible and ineligible areas nearby. We estimate that "cost per job" of the program was only $6,300 suggesting that in some respects investment subsidies can be cost effective.

Chiara Criscuolo
Centre for Economic Performance
London School of Economics
Houghton Street
London
WC2A 2AE
United Kingdom
c.criscuolo@lse.ac.uk

Ralf Martin
Imperial College Business School
London SW7 2AZ, UK
and Centre for Economic Performance, LSE
R.Martin@lse.ac.uk

Henry Overman
London School of Economics
Houghton Street
London WC2A 2AE UK
h.g.overman@lse.ac.uk

John Van Reenen
Department of Economics
London School of Economics
Centre for Economic Performance
Houghton Street
London WC2A 2AE
UNITED KINGDOM
and NBER
j.vanreenen@lse.ac.uk

INTRODUCTION

The Great Recession has brought industrial policy back into fashion. Huge subsidies have been granted by governments around the world to private firms most dramatically in financial services, but also in other sectors like autos. For example, the European Union (EU) countries spent €1.18 trillion on state aid in 2010, 9.6% of its GDP (European Commission, 2011). But business support policies are not new – most governments grant investment subsidies that claim to foster employment and productivity, particularly in disadvantaged areas. In 2010, €61bn was spent by EU countries on aid that was unrelated to the financial crisis. The US spends around $40-$50bn per annum on local development policies (Moretti, 2011). Despite the ubiquity and cost of such schemes, rigorous micro-econometric evaluations of the causal effect of these "industrial policies"[1] are rare.

The basic evaluation problem is that government programs might simply finance activities that firms would have undertaken in absence of the industrial policy. If this is the case, large amounts of taxpayer dollars could simply be wasted, even before we take into account the deadweight costs of taxation and other distortions induced by program design. The consensual view among economists is that industrial policy is a failure, but the econometric basis for this conclusion is hardly overwhelming. As Rodrik (2007) emphasises many of these policies are targeted on firms and industries that would be in difficulties in the absence of the program, so the coefficient on subsidy receipt in an OLS regression with (say) jobs as the dependent variable is likely to be heavily downwards biased.[2]

The main factor holding back credible evaluations of industrial policies is the absence of a clear identification strategy. We tackle this problem by exploiting a quasi-experiment that induced exogenous changes in the eligibility criteria governing the receipt of investment subsidies, which gave grants to firms for investment in economically disadvantaged areas of Britain ("Regional Selective Assistance" or RSA). Crucially for our identification strategy, there are strict rules governing the geographical areas that are eligible to receive aid from the British government determined by the European Union (EU). This is different from the US where the Federal government cannot prevent states from offering such business inducements (see Felix and Hines, 2011, for a discussion of US local incentives). These are common, formula-driven EU-wide rules that changed in 1993 and in 2000. We exploit the change in these "maps of

[1] "Industrial policy" means different things to different people. We are using it simply as a policy which directs investment subsidies to private sector firms. In our context, these subsidies are a government strategy to revitalize depressed geographical areas.

[2] For examples see Krueger and Tuncer (1982), Harrison (1994), Beason and Weinstein (1996) and Lawrence and Weinstein (2001).

assistance" to generate instrumental variables for the receipt of investment grants. This enables us to estimate the causal effect of the program on employment, investment, productivity and plant numbers (reflecting exit and entry). Our data set is constructed by linking very rich administrative panel data on the population of RSA program participants, and the population of British plants and firms for almost two decades.

We reach four substantive conclusions. First, there is a large and statistically significant average effect of treatment on the treated for employment and investment: a 10% investment subsidy causes about a 7% increase in employment with about half of this (3.6%) arising from incumbent firms growing (the intensive margin) and half due to greater net entry (the extensive margin). These effects are underestimated if endogeneity is ignored, as the participants in the scheme are firms and areas who would otherwise perform badly. Second, we find that the positive treatment effect is confined to smaller firms (e.g. with under 150 workers). We suggest that this is due to larger firms being more able to "game" the system and take the subsidy without changing their investment and employment levels, possibly combined with financial constraints for smaller firms. Third, from conducting the analysis at more aggregated area levels, we are also able to show positive program effects on area-wide manufacturing employment and large reductions in unemployment. We conclude that the positive micro effects are not simply due to substitution of jobs towards participants and away from non-participants in the same area, in neighboring (ineligible) areas or from non-manufacturing industries. The new jobs created appear to come from the pool of unemployed workers in the area, which is encouraging in terms of welfare (the cost per job is around $6,330). Finally, there appear to be no additional effects on productivity after controlling for investment effects and, since less productive plants receive more subsidies, this implies that the program lowers measured aggregate productivity because it increases the employment share of low productivity firms. This means less reallocation which many authors have found to be important in aggregate productivity (e.g. Olley and Pakes, 1996; Hsieh and Klenow, 2009).

Our paper is not the first to look at the impact of the RSA program. Most of the previous evaluation studies are based on "industrial survey" techniques where senior personnel 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 standard econometric techniques that are likely to underestimate the policy effect, these survey techniques are likely to over-estimate program impact (since firms receiving money are likely to be subject to positive response bias). A few other studies have used firm-level econometric techniques to evaluate the direct impact of RSA. For example, Devereux, Griffith and Simpson (2007) look at new investments by foreign-owned multinationals and UK-owned multi-plant groups using the largest RSA grant offers.[3] They find positive, but quantitatively tiny effects on multinational location decisions. This existing econometric evidence suggests small positive effects of RSA on employment, but very mixed effects on location, productivity, growth and survival. Relative to these existing studies our contribution is to use a policy experiment on the population of plants to identify causal effects.

Our paper also relates to a broader literature concerning evaluations of business support policies and place-based interventions. Several papers consider direct research subsidies to industrial R&D. Unlike the generally positive assessments on the effectiveness of indirect subsidies for R&D (e.g. Hall and Van Reenen, 2000, on R&D tax credits), the evidence on these direct subsidies is much more mixed (see the surveys in David et al, 2000, Klette et al, 2000, or Takalo et al, 2008). Two recent studies have used regression discontinuity design to assess causal effects. Bronzini and Iachini (2010) use a proposal's score by an independent committee as the running variable when analyzing the effects of receiving a R&D subsidy on Italian firms. Like us, they also identify a positive causal effect on investment, but only for small firms. In contrast, Jacob and Lefgren (2010) use a similar design for US National Institute of Health grants and identify a zero effect (this might be because marginally "just failed" applicants are likely to be able to access research funds from alternative sources).

Turning to place-based schemes, the most extensively researched policy are US Empowerment Zones which are neighborhoods receiving substantial Federal assistance in the form of tax breaks, job subsidies, etc. Busso, Gregory and Kline (2010) provide a recent evaluation using matched neighborhoods in other cities that also applied for the program as a control group and identify strong positive employment and wage effects, with only moderate deadweight losses. Neumark and Kolko (2010) provide a similar evaluation for California and survey previous papers with weaker identification strategies. Holmes (1998), Albouy (2009) and Wilson (2009) consider other place-based tax policies, while Wren and Taylor (1999), Bronzini and de Blasio (2006) and Martin et al (2011) provide evidence for regional policy in Europe. Gibbons, Overman and Sarvimaki (2011), and Einio and Overman (2011) discuss similar place based schemes in the UK, while Gobillon et al (2010) and Mayer et al (2011) provide estimates for France. In contrast to RSA, which targets specific firms within eligible areas, these schemes are generally not discretionary (subject to the firm meeting some basic requirements). In addition

[3] 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.

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.

Finally, there is a large literature on the impact of capital taxes and labor taxes which relate to our paper (for a recent review see Mirrlees, 2010). Unlike our RSA program, however, these general tax rules tend to be nation-wide rather than place specific, and general rather than at the discretion of an agency. Thus they are more likely to engender general equilibrium effects than the RSA policy which amounts to only around 0.1% of aggregate UK investment.

The rest of the paper is structured 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 and Section III describes the econometric modeling strategy. Section IV describes the data, Section V reports our results and Section VI provides some conclusions. In the Appendices we report more details on the RSA policy (Appendix A), the changes in EU rules (Appendix B), data details (Appendix C) and issues in aggregation (Appendix D).

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

I.A Overview

More extensive details of the RSA policy are given in Appendix A, but we describe the basics here. During our study period between 1986 and 2004, Regional Selective Assistance was the main business support scheme in the UK. From 1972 this provided discretionary grants to firms in disadvantaged areas characterized by low levels of per capita GDP and high unemployment ("Assisted Areas").[4] 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 a proportion of the project which was up to 35% in some years.

Because RSA had the potential to distort competition and trade it had to comply with European Union (EU) state aid legislation. This type of assistance is prohibited by European law, except in certain cases. In particular, Article 87 of the Treaty of Amsterdam allows for state aid in support of the EU's regional development objectives. The guidelines designate very deprived "Tier 1 Areas" (or "Development Areas") in which higher rates of investment subsidy can be offered, and somewhat less deprived "Tier 2 Areas" (or "Intermediate Areas") where lower

[4] In April 2004, in England, the RSA scheme was rebranded as the Selective Finance for Investment scheme and then Grant for Business Investment. It is still called RSA in Scotland and Wales.

subsidy rates were offered. There is an upper threshold called the Net Grant Equivalent (NGE)[5] which sets a maximum proportion of a firm's investment that can be subsidized by the government. These EU determined maximum subsidy rates differed over time and across areas.

Since the formula that determines which areas are eligible is 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 UK has latitude to decide the overall amount of the annual budget for RSA, it must conform to the EU rules when deciding which areas are eligible to receive RSA. Changes to area-level eligibility are therefore the key form of identification in our paper.

I.B Changes in eligibility over time

The map of the areas eligible for RSA changed in 1993 and 2000. There were also changes in the map of eligibility in 1984 which determines the starting position for the period under analysis (1986-2004) and there was also a change in 2006, after the end of our sample period. Figures 2-4 shows the map at three different points in time: 1986, 1993 and 2000 and illustrates the considerable changes in areas that gained or lost eligibility over time (see also Table A1).

Whether an area is eligible for any RSA is determined by a series of quantitative indicators of disadvantage which were changed over time but always included per capita GDP and unemployment (both relative to the EU average). The data used to determine which areas were eligible was from 1988 and before for the 1993 change, and 1998 and before for the 2000 change. Although the EU publishes which indicators it uses, it does not give the exact weight on the indicators which determine eligibility, but we can back out the implicit weights econometrically (see Appendix B). Regardless of the exact weights, the institutional set-up implies that an area can switch eligibility status because: (i) the indicators and weights on these indicators change over time; (ii) the average EU per capita GDP and unemployment rates change (e.g. when the formerly Communist states in Eastern Europe joined as new Members average EU GDP per capita fell); or (iii) the economic position of an area changes over time even for a fixed set of rules. The first two are clearly exogenous to area unobservables, but point (iii) raises endogeneity concerns that we address in several ways. First, recall that the information determining eligibility is lagged at least two years so it is likely to be weakly exogenous. However, unobservable area trends would still cause problems, so we also consider using only

[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 EU Official Journal C74/19 10.03.1998.

changes in the rules to construct instrumental variables for program participation and ignore all changes in area data (details are in Appendix B and Table 7). Furthermore, we show the robustness of the results to a full set of fixed effects interacted with time trends.

The assisted area map for RSA was redrawn in 1993 on the basis of 1988 guidelines using about 322 "Travel to Work Areas" (TTWA) as the underlying spatial units.[6] Assisted Areas fell into two categories: (a) Development Areas where the investment subsidy could be up to 30% Net Grant Equivalent and (b) Intermediate Areas where aid was limited to 20% Net Grant Equivalent. The European Commission introduced new guidelines in 1998 and the UK introduced new Assisted Area maps in 2000. For the most deprived "Tier 1" areas (Cornwall, Merseyside, South Yorkshire and West Wales) the maximum investment subsidy was now 35%. There were 65 scattered less deprived Tier 2 areas with four different levels of subsidy ranging from 30% to 10%. The 2000 map was based on electoral wards which are smaller areas than the TTWA used in 1993, and are similar in population size to US zip codes. Our eligibility instrument is therefore defined at the ward level and the econometrics allows for clustering the standard errors at this level, although we also show robustness to alternative ways of dealing with spatial autocorrelation such as clustering at higher levels (TTWA).

I.C Formal criteria for receipt of RSA investment subsidies

RSA was heavily targeted at the manufacturing sector – fewer than 10% of grant values went 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 based on the government's ability to assess the counterfactual situation of what would have happened in the absence of support. For example, a firm could reduce employment but claim that it would have reduced employment *by even more* without support. It is difficult for bureaucrats to accurately make such an assessment. The ability of a firm to "game" the system may be particularly easy for larger firms who can

[6] Travel to Work Areas is defined by the UK Census Bureau (Office for National Statistics). The criteria are (a) at least 75% of the resident, economically active population work in the area and (b) at least 75% of people working in the area also live in the area.

increase employment at subsidized plants at the expense of employment in unsubsidized plants which 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? Consider first the effects of a firm receiving RSA in a world with perfect capital markets. 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 optimal path of a firm's capital stock. If the firm is behaving optimally, then the change in after tax profits resulting from the 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 1 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 $(K_2 - K_1)(\rho_2 - \rho_1)$. 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 for its infra-marginal investments. Capital stays at the same level, 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

(MRPK< $\delta + \frac{r(1-\theta\tau)}{1-\tau}$, i.e. the value of ρ in equation (1) when ϕ = 0) 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 upward sloping when firms need external finance. This is illustrated in Panel B of Figure 1 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 the unconstrained firm.[7] This is illustrated in Panel B of Figure 1 (ΔK' > ΔK) and can also 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 effects 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

The previous sub-section focused on capital, but 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. What is the effect of

[7] Note that the program is not simply directed lending which will only have an effect on financially constrained firms (e.g. Banerjee and Duflo, 2008), but rather a directed subsidy which in general will also have effects on financially unconstrained firms too.

a proportionate change in the user cost of capital, ρ, on labor demand? 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 Hicks-Allen 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:

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

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.

IIC. General Equilibrium effects

Total expenditure on RSA was about £163m per year in our sample period (see Table A2), which constitutes only 0.13% of total UK investment (e.g. RSA expenditure was £148m in 2004 compared to £113.8bn spent in aggregate investment). Consequently, although there may be general equilibrium effects on asset prices and wage (e.g. Glaeser and Gottlieb, 2009) these are unlikely to be large. Nevertheless, since there may be some equilibrium price effects in local areas we also examine the effect of program participation on wages (we find these effects to be insignificantly different from zero).

IID. Summary

We take several predictions from the theory to the data. First, the investment subsidy should have a non-negative effect on investment. Second, we may expect that the policy has a larger effect on small firms as: (i) big firms can more easily “game” the system by using RSA for investment they would have done anyhow; and (ii) because smaller firms are more likely to be financially constrained. Third, 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. We find support for all of these predictions in the data.

III. ECONOMETRIC MODELLING STRATEGY

III.A Basic Approach

Consider the equation:

$$y_{it} = \alpha RSA_{it} + \beta X_{it} + \eta_i + \tau_t + v_{it} \tag{2}$$

Where y_{it} is the outcome of interest for plant *i* at time *t* - we focus on ln(employment), but also analyze investment and productivity. Note that a plant is uniquely located in an area, *r*, and in a firm, *j*, but unless needed we suppress these sub-scripts for notational simplicity. RSA_{it} is the participation indicator which we initially assume to be binary. Because the objective of RSA is to increase employment, we are interested in the long run effect of treatment rather than the short-run effects that occur while the firm receives RSA. To capture this, we set $RSA_{it} = 1$ for all years from the first year the plant receives an RSA payment and $RSA_{it} = 0$ before they receive their first payment. RSA_{it} is always zero for plants that never receive *RSA*. X_{it} are covariates, the precise set of which depend on the outcome of interest; but we keep to a minimal set (such as age) to avoid the "bad control" problem. We decompose the error term into a plant-specific fixed effect, η_i, a time effect common across all plants,[8] τ_t, and v_{it}, a plant-specific time varying error term.

Our estimates will be inconsistent if there are unobserved transitory shocks v_{it} correlated with RSA_{it}. This is very likely to be the case as areas and firms who are facing difficulties are specifically targeted by the policy. In this case $E(RSA_{it} v_{it}) < 0$ and OLS will generally under-estimate α.[9] The natural instrumental variable, Z_{it}, for program participation, RSA_{it} is the level of maximum investment subsidy available in the area, NGE_{it}. The instrument exploits the fact that: (i) the EU determines the NGE; and (ii) only plants located in certain areas are eligible for RSA so many areas will have a zero value for the instrument. As discussed in Section I, although these areas are fixed at a given point in time the map of eligible areas changed twice in our sample period (in 1993 and 2000). The maximum investment subsidy also differs across eligible areas both at a point of time (between 10% and 35%) and also over time, so this gives a continuous

[8] The large number of observations makes year-by-sector fixed effects infeasible for the estimations using the population. We can use year-by-sector fixed effects for the smaller ARD sub-sample and show robust results.

[9] Working in the opposite direction is the fact that a second objective of RSA is to create new jobs, which may increase the likelihood of receiving a grant for firms who have experienced some positive shock, v_{it}.

element to the instrument. Since there are lags between applying for grants, receiving them and building the capital stock we lag the instrument by two periods in our baseline results, but we show robustness to changes in the exact timing of the instrument.

We estimate equation (2) by instrumental variables, but also consider the reduced form:

$$y_{it} = \pi_1 NGE_{it} + \pi_2 X_{it} + \tilde{\eta}_i + \tilde{\tau}_t + \tilde{v}_{it} \quad (3)$$

Under the covariance assumption $E(Z_{it}\tilde{v}_{it}) = 0$, the estimate of π_1 by OLS is the "intent to treat" effect, which is of interest in its own right.

When moving from theory to implementation, one complication arises over the unit of observation in the data. Data on investment, output and materials come from the firm-level rather than the plant-level.[10] For most firms the firm-level 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 and we also know the location of all plants within multi-plant firms. When we examine firm-level outcomes (such as investment) which are unavailable at the plant level we simply aggregate the relevant equation across all plants within a firm. For firms, equation (2) becomes:

$$y_{jt} = \alpha RSA_{jt} + \beta X_{jt} + \eta_j + \tau_t + v_{jt} \quad (4)$$

For example, y_{jt} is total firm employment, summing across all plants *i* in firm *j*, i.e. $y_{jt} = \sum_{i,i \in j} y_{it}$.

For the participation dummy we mainly use a binary indicator equal to one if any plant in the firm receives treatment, but we also consider alternatives such as the amount of RSA relative to the size of the firm. For the 20% of firms that are multi-plant, we construct the firm-level instrument as $Z_{jt} = \sum_{i,i \in j} w_i Z_{it}$ where w_i are plant weights. The weights themselves could introduce endogeneity bias. For example, the distribution of firm employment across plants could be affected by plant eligibility for *RSA.* To minimize endogeneity problems we use the location of the oldest plant in the firm (based on plant ages as recorded the *first* time we observe the firm in the data).[11] That is we set $w_i = 1$ for the oldest plant, zero otherwise. The location of the oldest plant is unlikely to be affected by current changes in the eligibility map.[12]

---

[10] We call this the firm level, *j*, but there could be many reporting units in one large firm.

[11] So even if the firm later shuts its oldest plant we still construct the instrument based on its location. This might weaken our instrument but ensures that it is not affected by relocation choices by multi-plant firms.

[12] Note that the interpretation of α subtly changes in the firm-level regression. Consider employment and assume that the number of plants is fixed. Assume that a firm initially has two plants in two ineligible areas and then one area becomes eligible for RSA following the EU rule change. In this case, the firm could substitute employees from the plant in the ineligible area to the plant in the eligible area without changing total firm employment. Analysis at the plant level in equation (2) would find a positive program effect, but analysis at the firm-level in equation (4)

III.B Heterogeneous Treatment Effects

If we relax the assumption that the response to RSA is the same across units we can re-write the plant-level equation of interest as:

$$y_{it} = \alpha_i RSA_{it} + \beta X_{it} + \eta_i + \tau_t + v_{it} \qquad (5)$$

where α_i is now the plant specific effect of treatment. In this case, the IV estimate of equation (2) can be considered the average effect of treatment on the treated (ATT).[13] Since RSA is only available to plants in eligible areas, no plants in the "control" areas receive treatment. Thus, the IV coefficient on RSA in equations (2) and (5) is both the ATT and the local average treatment effect. We also consider matching techniques using the propensity score to trim the sample of participants and controls to have common support.[14]

The discussion in Section II implied that treatment effects could be more pronounced for smaller firms, so one observable source of heterogeneous treatment effects we examine is size. We use firm employment, as a measure of size when splitting the sample, and to mitigate endogeneity biases we use the employment level in the initial period – the first year we first observe the firm in the data.

III.C Aggregation to the area level

We also examine the impact of RSA at higher levels of aggregation such as ward-level (similar to US zip codes) and travel to work areas (TTWAs). There are 10,675 wards and 322 TTWAs in the dataset. Analogously to equation (3), consider the reduced form at this level (abstracting away from time dummies and other covariates):

$$y_{rt} = \lambda_1 NGE_{rt} + \eta_r + v_{rt} \qquad (6)$$

The main reason for aggregating the data to higher levels is to examine whether there is any effect of the program on the extensive margin in addition to the intensive margin. When y_{rt} is area level employment it captures both extensive (plant entry and exit) and intensive margins (growth by incumbents). In addition, using the number of plants in the area as the dependent variable in equation (6) we can examine the extensive margin directly.

---

would find zero effect. That is, comparison across the two levels of aggregation helps identify whether this across-plant, within firm substitution is an important phenomenon (it turns out not to be perhaps because this would be an illegal use of the funds).

[13] For some examples see Angrist (2004), Imbens and Angrist (1994) and Heckman et al (1997).

[14] The combination of using program eligibility as an instrument combined with matching is proposed by Blundell et al (2004). Using the propensity score function we trim the sample to those firms that have a predicted propensity score larger than the 10th percentile of the propensity score distribution of treated firms, and lower than the 90th percentile of non-treated firms. We check the robustness of these results to more conservative thresholds. There are other ways to match including matching by area (looking at ineligible areas that are closer in observed characteristics to eligible areas), and matching within area by plant and firm observables.

Since the instrument is defined at the area level, this will deal with the issue that the plants that receive RSA may have negative spillover effects on plants who do not receive treatment. Within an area, plants who receive the RSA may expand at the expense of those who do not, but the reduced form and IV estimate of α will be robust to this potential cross-plant substitution. However, the instrument does not deal with cross-area substitution. Comparison of the policy effects at the ward level and the more aggregate TTWA examines cross-area substitution. When an area becomes eligible for RSA firms may relocate jobs from neighboring ineligible areas. For example, consider two contiguous wards, *r* and *r'*, in a single TTWA (the example is easily generalized to r = 1, 2... R contiguous wards). The ward employment regression can take the form:

$$y_{rt} = \lambda_1 NGE_{rt} - \chi NGE_{r't} + \eta_r + v_{rt}$$

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*. If we estimate the employment equation aggregating to the higher TTWA (subscript *a*) our equation becomes:

$$y_{at} = \mu NGE_{at} + \eta_a + v_{at} \tag{7}$$

Where NGE_{at} is the average NGE in the two wards weighted by the lagged ward-level employment levels; i.e. $NGE_{at} = w_{rt} NGE_{rt} + (1 - w_{rt}) NGE_{r't}$ where $w_{rt} = \left(\frac{L_{rt-1}}{L_{rt-1} + L_{r't-1}} \right)$. The coefficient on NGE in equation (7), μ, captures the effect of NGE net of any negative between-ward spillover effect. In Appendix D we show that $\mu \approx w_r \lambda_1 - (1 - w_r)\chi$. In the extreme case where the RSA simply causes shifting between areas (as Wilson, 2009 might suggest) the coefficient of NGE in equation (7) will be zero (μ =0) for two symmetric wards.

A final advantage of the area-level regressions is that we can examine other margins of substitution. For example, does the program raise the level of employment by creating more jobs for the unemployed? Does the program create more manufacturing jobs at the expense of less non-manufacturing employment (RSA is directed at manufacturing firms)? Policy makers may welcome an effect which shifts more activity into manufacturing, as these are often regarded as having higher value added and wages (e.g. Kline and Moretti, 2011). But it is clearly better in welfare terms if any growth in employment comes from reducing unemployment, rather than simply substituting jobs from one industry to another.

III.D Endogenous Eligibility?

One concern is that areas that gain eligibility are those who have worsening economic conditions, thus generating a bias on our instrument. Consider the first differenced equivalent of the reduced form equation (3), and ignoring time dummies for simplicity:

$$\Delta y_{it} = \pi_1 \Delta NGE_{it} + \pi_2 \Delta X_{it} + \Delta w_{it} + \Delta u_{it} \quad (8)$$

where we have decomposed the error term (Δv_{it}) of equation (2) into two components, Δw_{it} which is correlated with the eligibility changes and an idiosyncratic error, Δu_{it} which is not. Note, first, that since areas who are doing worse are more likely to become eligible, i.e. E($\Delta NGE_{it} \Delta w_{it}$) <0, this will lead to a *downwards* bias on the coefficient of interest, π_1, and make it harder to identify a positive policy effect.

Recall from the discussion in Section I, however, that the determination of area eligibility status depends on the EU rules which are set prior to the eligibility changes and implemented using historical data. For example, in drawing the 2000 map most of the indicators were based on data from 1998. As we lag the instrument by two periods, this means that the magnitude of this possible bias will depend upon the correlation between variables like unemployment rates (at least) four years ago, and current unobserved area-specific shocks. Furthermore, recall that the variation in the instrument is also driven by (i) changes in the indicators used to determine eligibility; (ii) changes in the weights on these indicators and (iii) changes in EU wide GDP per capita and unemployment rates (which changes as new members like Finland and Austria joined). These are all at the EU level and are independent of area-specific shocks in Britain.

In short, we think the magnitude of such endogenous eligibility biases are likely to be small, although we still consider some tests of this assumption. First, in some of the fixed effects specifications we also include plant-specific trends to proxy Δw_{it} which are likely to pick up any longer run decline in an area that are not reflected in the covariates. Second, we construct "theoretical" instruments based only on the rule changes, ignoring any changes in the economic conditions in the area in order to construct an IV purged of any potential correlation with Δw_{it}. This procedure is detailed in Appendix B, but essentially we use only pre-policy area characteristics and the changes of the weights given to these characteristics in the EU-wide state aid rules changes. This means that changes in the instruments are solely driven by the rules and independent of any trends in the area characteristics.

IV. DATA

IV.A Datasets

Details on data and matching are in Appendix C, but we summarize the most important factors 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).

SAMIS is the administrative database used to monitor RSA projects. It contains information on all program applications (almost 25,000) since RSA's inception 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 date, amount and payments.[15] We match program participants with data from the Interdepartmental Business Register (IDBR), which contains the population of all UK plants and firms. The IDBR includes addresses, industry and ownership/control structure as well as employment information. 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 which 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 77% of all the RSA applicants. The most common reason for non-matches is that the information on the SAMIS database of RSA participants 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 firm. All observable characteristics were balanced between the samples including application amounts, headquarter location, firm size and administrative location of agency analyzing the application (more details are available in Criscuolo et al, 2006).

A stratified random sample of firms is drawn from the population of plants in the IDBR data to form the ARD (Annual Respondents Database) which is a mandatory survey.[16] Information in the ARD is gathered at a higher level of aggregation called the "reporting unit" which we refer to as a firm. In about 80% of all cases a firm is a 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

[15] Around 90% of applications were granted. There is information on applications not granted and we considered using these as a control group, but legal restrictions prevent us from matching these projects into the administrative data.

[16] Stratification is based on industry affiliation, regional location and size. For details see Criscuolo, Haskel and Martin (2003).

contain the population of larger businesses covering around 90% of total UK manufacturing employment. From the ARD we obtain information on investment and productivity of firms.

IV.B Descriptive Statistics

Table A1 reports the number of areas, firms and plants whose RSA eligibility status changed when the eligibility maps were re-drawn in 1993 and 2000. For the purposes of the table, we record a change in eligibility status for any change in the Net Grant Eligibility rate. It is clear that there were a large number of changes. In 1993 1,893 wards (out of a population of over 10,000) changed status, with 1,034 enjoying an increase and 859 suffering a reduction. These changes in area eligibility affected 14,369 plants (12,505 firms) who saw their maximum subsidy increase, and 8,856 (7,361 firms) who saw a decrease in the subsidy rate they were eligible to. In 2000 the rules tightened so there were a larger number of losers. Thus, while the number of areas and plants who enjoyed an increase was about the same as 1993, 1,424 areas and 14,967 plants, a much larger number suffered a decrease: 2,624 areas and 35,953 plants.

Some basic information on the program is also contained in Table A2 showing the aggregate expenditure on the program and number of recipients per year (£163m over 1,300 plants annually implying that the average grant is £125,000). About 29% of all wards are eligible for RSA covering 39% of British manufacturing employment. Although only 6% of manufacturing workers are in plants receiving RSA, this accounts for 16% of employment in the eligible areas.

We report characteristics of RSA participants and non-participants in Table 1. We report the characteristics of treated plants *prior* to their receipt of RSA in order to partially control for endogenous response and focus more on baseline characteristics. Rows 1 and 2 show that plants and firms that receive RSA are significantly larger than non-participants. The average participating plant employs three times more workers than the average non-participant and the average firm is almost twice as big. These differences remain when comparing medians (column (4)). At the firm level we can also compare other measures of size using output and investment. According to both, participants are larger than non-participants. Finally in rows 5 and 6 we compare firms in terms of labor productivity (value added per employee) and Total Factor Productivity (TFP, see Appendix C). Participating firms are significantly *less* productive when they enter the program. These descriptive statistics are supportive of the view that the RSA program, like many industrial policies, is targeted at larger, less productive firms. Thus, simple

OLS analyses are likely to underestimate any potential positive effects of the policy as argued by Rodrik (2007).

V. RESULTS

V.A Main Results

We begin with results for the employment equations from the population of plants (Tables 2 and 3) and then move on to firms (Table 4). We then condition on the sub-sample of firms (the ARD) where we have output and capital data to investigate program effects on investment and productivity (Table 5). The next sub-section aggregates to a higher level across all plants in an area to investigate substitution effects looking at the number of jobs, firms and unemployment (Table 6). We also show results for various extensions and robustness checks in Table 7.

We start by reporting in Table 2 a basic employment equation at the plant level using the IDBR dataset of the manufacturing population. Panel A uses the full sample, Panel B uses a sub-sample of plants belonging to smaller firms (under 150 employees) and Panel C uses the sub-sample of plants belonging to larger firms (150 or more employees). The first four columns present results without plant fixed effects while the final four columns include fixed effects. We have just under 2.3 million observations and we cluster standard errors at the ward level (which is the geographical unit where the program eligibility status is determined - see section I).

In column (1), Panel A of Table 2 the RSA program participation dummy is positive and highly significant, which mainly reflects the fact that participants are larger than non-participants as shown in Table 1. Column (2) reports the reduced form where we regress ln(employment) on the policy instrument (NGE) which is the maximum investment subsidy available in the area. There is a strong positive association of employment with NGE. Column (3) reports the first stage of the IV estimates and shows, as expected, a strong effect of the policy instrument on program participation. The IV results are in column (4) which shows that the treatment effect is much larger than in the OLS estimates of column (1).

The last four columns of Panel A repeat the specifications of columns (1)-(4) but include a full set of plant fixed effects. In the OLS estimates of column (5) the coefficient on the program dummy remains significant at the 1% level, but is an order of magnitude smaller than column (1) consistent with the substantial selection bias towards larger plants. Columns (6) and (7) report the reduced form and first stage, with the policy instruments remaining positive and significant in both specifications. Finally in column (8), the IV results show a strongly significant positive RSA coefficient estimate about three times as large as the OLS estimates in column (5). The downward OLS bias is consistent with the view that RSA is awarded to plants that face negative shocks, exactly what one would expect from a policy aimed at under-performing firms in deprived areas. Ignoring these negative shocks leads to substantial underestimation of program impact.[17]

The magnitude of our preferred coefficient in column (8) of Panel A suggests that plants that receive RSA observe an increase in employment of 35.8 log points or 43%. Given the mean and median size of participating plants, this implies an increase of 34 employees (from 79) at the mean and of 2.6 (from 6) at the median. Note that the magnitude and significance of the effects are robust to reasonable change in assumptions over the timing of the instruments.[18] Since about 16% of employees in eligible areas are in plants receiving RSA, this would imply an area-wide effect employment of about 6%, which is consistent with some of the area-wide effects we estimate in the next sub-section. We also discuss the aggregate magnitudes of the program effects in more detail in sub-section V.C.

Panels B and C of Table 2 examine heterogeneity of the treatment effects by firm size. Panel B reports results for plants of smaller firms (less than 150 employees) whereas panel C reports results for larger firms. While we again find a significant and strong employment effect for plants of small firms (0.484 in the fixed effects IV specification of column (8)) we find an insignificant (and somewhat negative) effect for plants in large firms. This suggests that the overall effect in Panel A is entirely driven by smaller firms. Note that for large firms there *is* a positive and significant coefficient when we do not instrument (column (5), panel C). This suggests that large firms increase plant employment when a plant receives RSA, but that these plants would have increased employment even in the absence of the program. For both small and large firms the first stage regression coefficients are highly significant. This suggests that the absence of any effect for large firms is not because the instruments are weak for this sub-sample of larger firms.

A concern with these results is that there may be unobserved trends at the area or plant level that are correlated with the instrument. We examine this issue by running regressions in first differences and include plant fixed effects – i.e. plant-specific trends (see Table A3).[19] The

[17] Another possible explanation of why the OLS estimates are lower than the IV estimates is classical measurement error in the endogenous variable RSA. This could be true for at least two reasons in our case. Firstly there could be some degree of error when doing name-postcode matching, i.e. when we match the treatment information from the SAMIS database to the plant information from the business register. Secondly there might be some mismeasurement of the starting date of the treatment (see Appendix A).

[18] For example the coefficient (*standard error*) in column (8) is 0.358(*0.135*) using our baseline instrument dated t-2. If we use the NGE policy instrument at t-1 instead, the estimate increases to 0.415(*0.166*).

[19] It is computationally infeasible to add an additional 353,626 plant trends into the within group regressions.

program effects are largely unchanged which leads to the conclusion that omitting plant specific trends does not bias the results. For example, the differenced version of the pooled reduced form policy coefficient (*standard error*) of 0.086(*0.033*) from column (6) of Table 2 is 0.075(*0.029*) in Table A3. When we include a full set of plant-specific trends this effect falls to 0.068(*0.033*), which is a statistically insignificant change. The results are also robust to clustering the standard errors at a higher level.[20]

The cut-off value of 150 employees to define a "large" or "small" firm is somewhat arbitrary. Table 3 shows that the results are robust to alternative cut-offs. Panels A and B repeat the specifications of Table 2 using 100 employees (instead of 150) as the threshold, and Panels C and D use 50 employees. In Panel E we use continuous firm size (the log of firm employment in the initial year we observe the plant) instead of the discrete measures. The pattern in Table 2 is repeated: there are large and significant positive treatment effects for small firms but no significant effects for the larger firms. For example, Panel E shows that there is a significantly smaller treatment effects for large firms than smaller firms whether we look at IV or the reduced form.

Even if larger firms do not expand employment at treated plants it is still possible that their total employment increases through expanding employment in untreated plants (e.g. if receipt of RSA relaxes financial constraints at the firm level). To investigate this we repeat the specifications at the firm (rather than plant) level and report results in Table 4. The first four columns repeat the fixed effects specifications reported in Table 2, but aggregate the data across all plants in a firm. The last four columns use the sub-sample of firms in the ARD data where we observe a larger number of variables (such as capital and output). The qualitative findings for firms are very similar to those at the plant level: OLS estimates show significant and positive effects for the sample of firms of all sizes, but these results are only robust for small firms – the effect is insignificant for larger firms. The program effect for small firms in the ARD sub-sample of column (8) in Panel B is smaller than that of the population in column (4) because the ARD over-samples larger firms. Consequently, mean employment is larger in this subsample and since the treatment effect falls with size, it is unsurprising that the IV coefficient on the program dummy also falls when moving from the population to the ARD sub-sample.

A second possible explanation of the larger treatment effects for smaller firms is that they may receive relatively larger subsidies given their size: $10,000 of investment grant will clearly

[20] Recall that our baseline estimates cluster at 10,675 wards where policy eligibility is defined in 2000. If we instead cluster at the higher level of 332 Travel to Work Areas (TTWAs), the coefficient(*standard error*) in column (8) of Table 2 panel B is 0.484(*0.214*) which is still easily significant at conventional levels.

have a larger proportionate effect on a small firm than a large firm. This is unlikely to be the full explanation as the OLS coefficients on RSA are similar for large and small firms – it is the IV results that are different. Nevertheless, to investigate this issue more thoroughly we considered two alternative measures of treatment intensity. The first cumulates the amount of RSA received by the plant since birth and uses this instead of the binary participation dummy.[21] Our results remain qualitatively similar. In the preferred IV estimates (corresponding to column (8) of Table 2), the coefficient (*standard error*) on the treatment "elasticity" is 0.030 (*0.007*) for the pooled sample and 0.041(*0.008*) for the smaller firms. For plants in larger firms the coefficient is insignificant: -0.010 with a standard error of 0.016. Our second measure of treatment intensity uses the same idea but normalizes the amount received by lagged employment. Again we obtain similar results: the coefficient (*standard error*) on RSA for small firms is 0.053(*0.011*) whereas it is -0.020(*0.028*) for the large firms.

Table 5 again reports results for firms in the ARD sub-sample where we repeat the specifications from the earlier tables, but uses investment and productivity as dependent variables instead of employment. There is a positive treatment effect for investment in the pooled sample (Panel A column (4)), but the effect is significant only for smaller firms (compare column (4) of Panel B to Panel C). We observe positive and significant OLS coefficients for all samples in column (1) and a significant first stage in column (3). However, both in the reduced form and IV, there is only a significant and positive program effect for smaller firms. This is a similar pattern to employment, but note that the magnitude of the policy effect is much larger for investment than employment. The IV coefficient in column (4) of Panel B is 0.973 for investment in Table 5 compared to 0.277 for employment in Table 4. Similarly, the reduced form coefficient for investment in column (2) of Panel B is 0.500 compared to 0.142 for employment. This is consistent with the theory in Section II: the effects of the investment subsidy are likely to be stronger on capital (which is directly subsidized) rather than labor (which is only indirectly affected).

Results from production function specifications are reported in the last four columns of Table 5 (see also Appendix C). The dependent variable is labor productivity (gross output per worker), but since the regressions also include capital per worker and materials per worker the coefficients can be interpreted as the program effects on TFP.[22] The coefficients imply no effect

[21] We define this new continuous RSA measure as ln(1+ total subsidy) in order to avoid dropping zeros. Results available on request.

[22] This is a very simple way to estimate production functions and there are a host of alternatives (e.g. Ackerberg et al, 2007). We also looked at estimating TFP as a residual using the Solow approach of replacing the factor coefficients with factor shares in revenue (see Appendix C). The results were very similar to those reported here.

of the program on productivity in any sub-sample. We also repeated these specifications for average wages motivated by the idea that some of the benefits from subsidies may be captured by employees in the form of higher remuneration (e.g. Goolsbee, 1998). However, as with productivity we could find no significant positive effects of the RSA policy on wages: the coefficients were generally negative.[23]

The absence of productivity (and wage) effects may be disappointing to some policy-makers who emphasize the benefits of investment subsidies as a way of increasing TFP, but it is rather unsurprising from an economic perspective. The program offers investment subsidies; so while we would expect it to increase the capital stock per worker and perhaps employment, there is no strong reason to expect TFP to rise. As we discuss in the conclusions, the absence of a within firm investment effect, coupled with the fact that low productivity firms are being selected into the scheme, implies that the aggregate effect of the program is to lower measured productivity.

V.B Area Level Analysis

Employment growth at the extensive margin (net entry)

The plant and firm level regressions suggest that, at least for smaller firms, receiving RSA causes employment and investment to increase. These fixed effect regressions condition on a sample where we observe plants or firms in at least two years, so are subject to the concern that we are missing an effect of the program on entry and exit which could increase area employment on the extensive margin. Table 6 examines this issue by looking at area level employment and unemployment at higher levels of spatial aggregation.

Column (1) of Table 6 estimates the reduced form employment equation aggregating across all plants in a ward (the 10,000 or so areas where NGE eligibility is determined). Column (1) reveals a significant positive impact of the policy on employment at the ward level, especially for employment in smaller firms. The policy coefficient is larger than the equivalent columns at the plant level. For example, the coefficient is 0.086 at the plant level in Table 2 column (6) and 0.287 at the area level in Table 6 column (1). One explanation for this difference is that the policy may encourage the creation of new plants or reduce exit rates of existing plants. Column (2) uses the number of plants as the dependent variable and provides some evidence consistent with this hypothesis. There appears to be a significant and positive effect of the policy on the number of plants, which is particularly strong for smaller firms. Looking at the reduced form

[23] The OLS coefficients were significantly negative which is consistent with the idea that RSA is targeted on firms who were experiencing negative shocks. The IV coefficient (*standard error*) was -0.001(*0.107*) for the pooled sample, -0.080(*0.124*) for small firms and 0.090(*0.247*) for large firms. Results available on request.

coefficients on NGE, the sum of the effect on the number of plants (0.171) and the effect on incumbent employment (0.086) at 0.256, is close to the overall ward employment effect of 0.287. At the mean NGE (0.243) of eligible areas, this implies that the program increases employment by 7% (0.287*0.243).[24]

Do eligible areas gain at the expense of ineligible areas?

There is a concern that the OLS results of Table 2 overstate the treatment effects because when looking within an area plants that receive RSA may gain jobs at the expense of plants who do not receive subsidies. However, since the instrumental variable is defined at the area level (not the plant level), these spillover effects are controlled for in the IV specifications. In sub-section IIIC we discussed the analogous concern that we might overstate the benefits of the policy if the employment gains in eligible areas were due to a substitution of jobs away from ineligible areas. We can investigate this issue within broader local areas by aggregating across wards to a higher level.[25] In Britain there are 332 "travel to work areas" (TTWA) covering the 10,737 wards analyzed in the first two columns of Table 6. We repeat the employment and firm number regressions of the first two columns in columns (3) and (4), but this time aggregated to the TTWA level. Again, there does not appear to be evidence that the positive effects are diminished as we aggregate over the neighboring wards. The point estimates are actually slightly larger than in the equivalent columns in Panel A, although the differences are not significant. The only place where there is a stark difference is for numbers of plants in Panel C which focuses on larger firms. When looking at wards, there is a positive program effect for the number of plants in column (2), but this disappears when we look at the TTWA level in column (4). This is consistent with the idea that large firms move plants from neighboring ineligible wards into a ward that becomes eligible for RSA.

Do participants gain at the expense of other industries?

Like many industrial policies, RSA is targeted at manufacturing firms. This raises the possibility that there may be another form of substitution towards manufacturing and away from other sectors of the economy such as services. To investigate this we look at the effect of the policy on service employment. Unfortunately, service employment at the ward level (and general

[24] The figure of a 7% employment effect is consistent with the implication of the IV results. Since the ATT effect is 0.358 from column (8) of Table 3 Panel A and 16% of jobs are in plants receiving RSA in eligible wards (last column of Table A2), the implied aggregate effect within a ward is just under 6% (0.358*0.16=0.057).

[25] It is beyond the scope of this paper to address the question of displacement at larger spatial scales (e.g. whether employment effects come at the expense of other firms in non-assisted areas in other parts of the country).

unemployment which we use in the next sub-section) is only available on a consistent basis since 1996. Consequently we first investigate whether the results are robust by re-estimating column (1) on the 1996-2004 sub-sample. We find that the results are robust in column (5) of Table 6, with a coefficient of 0.212 compared to 0.287 in the baseline. Column (6) then uses employment in the services sector as an outcome and finds that the effect of RSA is insignificant. This is reassuring since the policy is directed at manufacturing and would be unlikely to strongly affect services unless employers substituted manufacturing jobs for services jobs. This does not seem to be the case.

The Program appears to reduce unemployment

Since the RSA program appears to significantly increase area employment without reducing jobs in neighboring areas or in non-manufacturing industries, where is the extra employment coming from? One possibility is that unemployment falls, so column (7) contains regressions where the dependent variable is the number of people claiming unemployment insurance (called Job Seekers' Allowance) in the area. The results suggest that introducing the policy in an area significantly reduces the level of unemployment – a 10% NGE reduces unemployment by 6.9%. This is an important result as it suggests that the policy draws some unemployed workers into jobs.[26]

V.C Magnitudes

The most straightforward way to consider the overall magnitude of the RSA policy is to use the area-level reduced forms. The treatment effect from column (1) of Table 6 is 0.287 indicating that a 10% NGE investment subsidy would increase area employment by 2.9%. We estimate the effect of the average NGE on employment in every year 1986-2004. There were about 1.6m workers in manufacturing over the sample period and the mean level of the investment subsidy is 24%. In an average year we estimate about 111,000 extra jobs were created as a result of the policy (approximately 0.287*0.24*1.6). The nominal average annual cost of RSA was about £190m implying a total cost of £2,602 per job in 2010 prices. Using official estimates of administrative costs (17%)[27] and a deadweight cost of taxation of 30%, this

[26] Note that the unemployment results are robust if we re-estimate at the travel to work area-level (a coefficient of -0.676 with a standard error of 0.096).

[27] We use the administrative reports of the grants awarded averaging £162.9m 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

implies a cost per job of £4,871 (or $6,331). Since there do not appear to be large substitution effects from neighboring non-eligible areas or from other sectors like non-manufacturing (see Table 6), these do not need to be scaled down.[28]

What are the effects on aggregate unemployment? Over 1996-2004 the implied aggregate annual policy effect on jobs is 81,966 which is somewhat smaller than in pre-1996 period (because of (i) a coefficient of 0.212 instead of 0.287 and (ii) the shrinking size of the manufacturing sector). The equivalent fall in unemployment is 76,206 per year using the estimates in column (7) of Table 6. This is close to the implied aggregate employment effect which suggests that the bulk of the growth in employment is being drawn from the ranks of the unemployed.

The cost per job of $6,331 is higher than other labor market interventions in the welfare to work area, such as closer monitoring of unemployment insurance (e.g. Van Reenen, 2004, Black et al, 2003), but it is much cheaper than most other polices towards under-performing areas such as government created jobs or untargeted tax breaks. 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 the less productive firms. On the other hand, the government is also saving money from paying less out in unemployment benefits and other forms of welfare for workers who are drawn into employment. So overall, these calculations suggest a more positive assessment of a place-based industrial policy than the existing literature.

V.D Robustness and extensions

We have subjected our results to a battery of robustness tests and report some of them in Table 7. To keep the amount of information manageable we focus on the reduced form specifications for firms[29] and present results for the pooled sample in column (1), small firms in column (2) and for larger firms in column (3). The baseline results in Panel A are the same as those in column (2) of Table 4.

Changes in area eligibility were driven by rule changes every at the EU level as discussed in Section I. We argued that these were exogenous to contemporaneous shocks to employment, investment and TFP as they are based on area characteristics that are lagged at by least four years, and determined by many factors independent of the area (e.g. EU wide per capita GDP).

(2005). We believe this is because no other study has exploited the exogenous changes in RSA eligibility to deal with the downward endogeneity bias.

[28] 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.

[29] Table A4 shows we find virtually the same results when running the same regressions at the plant level.

Nevertheless, there may be some concern that declining areas in the past will continue to decline in the future and that this will make an area more likely to be selected into the program and thus more likely to have falls in jobs (note that this bias means we underestimate the treatment effect). In sub-section V.A we addressed this by including plant level trends. An alternative method of investigating this problem is to reconstruct the policy instruments based solely on changes in the EU wide weights (i.e. the weights associated with area GDP relative to the EU average rather than lagged changes in area GDP). This is detailed in Appendix B. Panel B of Table 7 shows the results of implementing this pure rules-based IV. The estimates show the same qualitative pattern as the baseline results and are, if anything, somewhat stronger.

A common concern in the treatment effects literature is that the control group may be poor comparisons for the treatment group. We implement a simple matching technique to deal with this using a propensity score method (Heckman et al, 1997). We run a probit of participation on a large number of lagged observables (labor productivity, employment, intermediate inputs, capital, age and a multinational dummy). We then choose only observations for the control group whose predicted probability of treatment lie between the 25th and 75th percentiles of the treatment group. We then re-estimated all our models on this sub-sample with common support. Note that the relevant comparison group is the ARD sample of column (6) Table 4 as we use some firm level covariates for the matching function. Panel C shows qualitatively similar results to Table 4 with coefficients generally more precisely estimated.

The EU invests in human capital, infrastructure and provides business support in deprived areas via the so-called “Structural Funds”. The maps which determine which areas are eligible for such funds are changed at similar times to the re-drawing of the maps for RSA (something which is not true for any other area-based policy interventions). This raises the concern that the effects we identify are not because of investment subsidies to firms but rather because of these Structural Fund Policies. Fortunately, although the areas eligible for the two policies are correlated they are far from identical. Both policy changes in 1993 and 2000 affected the maps of eligibility for RSA and EU Structural funds differently, thus allowing separate identification of the impacts of the two policies. Areas eligible for Structural Funds are determined by a different formula to the one determining areas eligible for RSA. Consequently we coded a dummy variable equal to one if an area was eligible for structural funds and zero otherwise. We entered this as an additional variable in the standard regressions and the results are contained in Panel D. Reassuringly the main results are very similar to those in the baseline of Panel A. Interestingly, structural funds appear to have no benefits at all for employment in the area, which suggests that these funds may be largely ineffectual in raising employment levels.

In the model of Section II, the marginal effect of the subsidy on employment was $\frac{\partial \ln L}{\partial \phi} = \frac{\partial \ln \rho}{\partial \phi} s_K(\sigma - \eta)$. A way to assess the plausibility of this model is to note that it implies that the effects of the policy will be larger in the sectors where capital intensity is greater (s_K high). Panel E of Table 7 investigates this. Consistent with the simple model outlined here we find that that there is a significant and positive interaction effect of the policy with capital intensity. In other words when the capital share is high the policy is more likely to lead to a positive effect on employment.

An alternative interpretation of the results is that they represent the government successfully reducing financial constraints for investment – this would be consistent with the finding that the treatment effect was significant only for small firms. Recent evidence, however, stresses that many small firms are not young (Haltiwanger et al, 2010) and therefore less likely to be subject to the information asymmetries thought to generate financial constraints. As a crude test of this idea we examined whether there was any evidence that there were larger treatment effects for younger firms. There were not[30] – it is really size rather than age which is the key element of heterogeneity which suggests that financing constraints are not the primary cause of the effects we identify.

VI. CONCLUSIONS

There are surprisingly few micro-econometric analyses of the causal effects of industrial policy, despite their ubiquity. In this paper we have examined one business support policy – Regional Selective Assistance (RSA). We use exogenous changes in the eligibility of businesses to receive support driven by policy changes at the EU level determining which areas were eligible for investment subsidies. When we correct for endogeneity we find evidence for a positive treatment effect on the treated in terms of employment and investment. Allowing heterogeneity in the effects of the policy we find that the program effects are strong for smaller firms but essentially 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. Interestingly, this stronger effect of business support policies on smaller firms is also found in Wallsten (2000) for the US, González et al (2005) for Spain, Lach (2002) for Israel, and Bronzini and Iachini

[30] For example, we re-estimated the baseline specification of column (1) of Table 7 Panel A, including an interaction of the NGE policy instrument with a dummy variable if the firm was younger than five years. The coefficient (*standard error*) on the interaction was -0.038 (*0.019*). This suggests that younger firms were, if anything, less sensitive to the policy intervention, the opposite of the financial constraints hypothesis. Results available on request.

(2010) for Italy and Gorg and Strobl (2007) for Ireland. The fact that the treatment effect is confined to smaller firms strengthens arguments for removing subsidies from larger enterprises.

At the area level we also find that the program reduced unemployment and raised employment both through the intensive and extensive (number of firms) 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. Rather, higher manufacturing employment seems to come from reducing the level of unemployment. 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" (around $6,331). Although, measured aggregate productivity falls as the RSA supported firms were on average less productive, this probably carries a modest welfare cost compared to the counterfactual where these employees enter unemployment (rather than be reallocated to more productive firms).

In terms of future work we intend to use the policy to obtain better identification of structural parameters in the production function. The coefficient on capital in firm or plant-level production functions is difficult to estimate due to selection and endogeneity problems.[31] Investment subsidies can be an external instrument that shifts the capital stock exogenously under the assumption that RSA does not have a direct effect on TFP (consistent with what we are finding here). A deeper question is whether these types of policies can *permanently* improve the position of disadvantaged areas through agglomeration effects. This requires investigating the longer-run dynamic effects of such policies after subsidies from an area are withdrawn, which is an avenue we are currently pursuing (see Kline and Moretti, 2011).

[31] See *inter alia* Olley and Pakes (1996), Ackerberg et al (2007) and Martin (2008).

REFERENCES

Ackerberg, Daniel, Kevin Caves and Garth Frazer (2007) "Structural Identification of Production Functions", mimeo UCLA.

Albouy, David (2009) "The Unequal Geographic Burden of Federal Taxation" *Journal of Political Economy*, 117(4):635-667.

Angrist, Joshua (2004) "Treatment effect heterogeneity in theory and practice" *Economic Journal*, 114(494), C52-C83.

Banerjee, Abhijit and Esther Duflo (2008) "Do firms want to borrow more? Testing credit constraints using a directed lending program" MIT mimeo.

Beason, Richard and David Weinstein (1996) "Growth, Economies of Scale and Targeting in Japan (1955-1990)", *Review of Economics and Statistics*, 78(2), 286-295.

Black, Dan, Jeffrey Smith, Mark Berger and Brett Noel (2003) "Is the Threat of Reemployment Services More Effective Than the Services Themselves? Evidence from Random Assignment in the UI System" *American Economic Review*, 93(4), 1313-1327.

Blundell, Richard, Monica Costa Dias, Costas Meghir and John Van Reenen (2004) "Evaluating the Employment Impact of a Mandatory Job Search Program" *Journal of the European Economic Association*, 2(4), 569-606.

Bond, Steve and John Van Reenen (2007) "Micro-econometric models of investment and employment" Chapter 65 in Heckman, James and Edward Leamer (eds) *Handbook of Econometrics Volume 6A* (2007) 4417-4498.

Bronzini, Raffello and Guido de Blasio (2006) "Evaluating the Impact of Investment Incentives: The Case of Italy's Law 488/1992" *Journal of Urban Economics* 60(2): 327-349.

Bronzini, Raffaello and Eleonora Iachini (2010) "Are incentives for R&D effective? Evidence from a regression discontinuity approach", mimeo Bank of Italy.

Busso, Mattias, Jesse Gregory and Patrick Cline (2010) "Do Local empowerment programs work? Evidence from the Federal Empowerment Zone program", NBER WP No. 16096.

Criscuolo, Chiara, Jonathan Haskel and Ralf Martin, (2003) "Building the evidence base for productivity policy using business data linking", *Economic Trends* 600.

Criscuolo, Chiara, Ralf Martin, Henry Overman and John Van Reenen (2006) "Longitudinal Micro Data Study of Selected BERR Business Support Programmes", BIS mimeo.

David, Paul, Bronwyn Hall and Andrew Toole (2000) "Is public R&D a complement or substitute for private R&D? A review of the econometric evidence" *Research Policy*, 29(4-5), 497-529.

Department of Business, Innovation and Skills (various years), *Industrial Development Reports*, London: HMSO.

Devereux, Michael, Rachel Griffith and Helen Simpson (2007) "Firm location decisions, regional grants and agglomeration externalities" *Journal of Public Economics*, 91(3-4), 413-435.

European Commission (2011) *State Aid Scorecard*, Brussels: European Commission, http://ec.europa.eu/competition/state_aid/studies_reports/studies_reports.html

Einio, Elias and Henry Overman (2011) "The Impact of the UK Local Enterprise Growth Initiative", mimeo LSE.

Felix, R. Alison and James Hines (2011) "Who offers tax-based development incentives?" NBER Discussion Paper 17466

Gibbons, Stephen, Henry Overman and Matti Sarvimaki (2011) "The impact of subsidizing commercial space in deprived neighbourhoods", mimeo LSE.

Glaeser, Edward and Joshua Gottlieb (2009) "The Economics of Place-Making Policies", Harvard Institute of Economic Research Discussion Paper No. 2166.

Gobillon, Laurent, Thierry Magnac and Harris Selod (2010) "Do Unemployed Workers Benefit from Enterprise Zones: the French experience", CEPR Working Paper 8084.

González, Xulia, Jordi Jamandreu, and Consuelo Pazó (2005) “Barriers to innovation and subsidy effectiveness”, *RAND Journal of Economics*, 36, 930-50.
Goolsbee, Austan, (1998) “Does Government R&D Policy Mainly Benefit Scientists and Engineers?” *American Economic Review*, 88(2), 298-302.
Gorg, Holger and Eric Strobl (2007) “The effect of R&D subsidies on private R&D” *Economica*, 74(294), 215-234.
Hall, Bronwyn and John Van Reenen (2000) “Fiscal Incentives for R&D: A New Review of the Evidence” *Research Policy*, 29, 449-469.
Haltiwanger, John, Ron Jarmin and Javier Miranda (2010) “Who Creates Jobs? Small vs. Large vs. Young” NBER Working Paper 16300
Hamermesh, Daniel (1990) *Labor Demand*, Princeton: Princeton University Press.
Harris, Richard and Chris Robinson (2005) “The Impact of Regional Selective Assistance on Sources of Productivity Growth: Plant Level Evidence from UK Manufacturing 1990-1998”, *Regional Studies*, 39(6), 751-765.
Hart, Mark, Nigel Driffield, Stephen Roper and Kevin Mole (2008) “Evaluation of Regional Selective Assistance (RSA) and its successor, Selective Finance for Investment in England (SFIE)” BERR Occasional Paper No. 2.
Harrison, Ann E. (1994) “An empirical Test of the Infant Industry Argument: Comment”, The *American Economic Review*, 84(4), 1090-1095.
Heckman, James, Hidehiko Ichimura and Petra Todd (1997), “Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Program”, *Review of Economic Studies*, 64, 605-654.
Holmes, Thomas (1998) “The Effects of State Policies on the Location of Industry: Evidence from State Borders,” *Journal of Political Economy* 106(4), 667-705.
Hsieh, Chang-Tai and Peter Klenow, 2009. ‘Misallocation and Manufacturing TFP in China and India’, *Quarterly Journal of Economics*, CXXIV (4)
Imbens, Guido W and Joshua D. Angrist (1994) “Identification and Estimation of Local Average Treatment Effects” *Econometrica*, 62(2), 467-75.
Irwin, Douglas and Peter Klenow (1996) “High-tech R&D subsidies: estimating the effects of Sematech” *Journal of International Economics*, 40, 323-44.
Jacob, Brian and Lars Lefgren (2010) “The impact of research grant funding on scientific productivity”, *Journal of Public Economics*, 95(9-10), 1168-1177.
Jones, Jonathan and Colin Wren (2004) “Inward Foreign Direct Investment and Employment: A Project-Based Analysis in North-East England” *Journal of Economic Geography* 4(5), 517-44.
King, Mervyn (1974) “Taxation and the cost of capital”, *Review of Economic Studies*, 41, 21-36
Klette, Tor, Jarle Møen and Zvi Griliches (2000) “Do subsidies to commercial R&D reduce market failures?” *Research Policy* 29(4-5) 471-496.
Kline, Pat and Enrico Moretti (2011) “Can Public Investment Shift Regional Growth Equilibria? One Hundred Years of Evidence from the Tennessee Valley Authority”, mimeo UC Berkeley.
Koopman, Jan-Gert (2011) “State aid priorities: Rescuing and restructuring banks and preventing subsidy races”, Presentation at CRA conference on Competition Policy, Brussels 7th December 2011
Krueger, Ann O. and Baran Tuncer (1982) “An empirical test of the Infant Industry Argument”, *American Economic Review* 72(5), 1142-1152.
Lach, Saul (2002) “Do R&D subsidies stimulate or displace private R&D? Evidence from Israel” *Journal of Industrial Economics*, 50, 369-90.

Lawrence, Robert Z. and David E. Weinstein (2001) "Trade and Growth: Import Led or Export Led? Evidence from Japan and Korea" in Joseph E. Stiglitz and Shahid Yusuf eds., *Rethinking the East Asia Miracle*, Oxford: Oxford University Press.

Martin, Ralf (2008) "Productivity dispersion, competition and productivity measurement" Centre for Economic Performance Discussion Paper No. 0692.

Martin, Philippe, Thierry Mayer and Florian Mayneris (2011) "Public support to clusters: A firm level study of French Local Productive Systems" *Regional Science and Urban Economics*, 41(2), 108-123.

Mayer, Thierry, Francois Mayneris and L. Py (2011) "The Impact of Urban Enterprise Zones on Establishment Location Decisions - Evidence from French ZFUs". Mimeo Sciences-Po.

Mirrlees, James (2010) *Mirrlees Review of Taxation*, London: Institute for Fiscal Studies

Moretti, Enrico (2011) "Local Labor Markets" Chapter 14 in Orley Ashenfelter and David Card (eds) *Handbook of Labor Economics Volume 4B*, Amsterdam: North Holland

National Audit Office (2003) *Regional Grants in England*, 17th June, MHSO: London.

Neumark, David and Jed Kolko (2010) "Do Enterprise Zones Create Jobs? Evidence from California's Enterprise Zone Program", forthcoming *Journal of Urban Economics*.

Olley, Steve and Ariel Pakes (1996) "The dynamics of Productivity in the Telecommunications equipment industry", *Econometrica* 64 (6), 1263-1297.

Rodrik, Dani (2007) "One Economics, Many Recipes", Princeton: Princeton University Press

Ruane, Frances (1982) "Corporate Income Tax, Investment grants and the cost of capital", *Journal of Public Economics*, 17, 103-109.

Takalo, Tuomas, Tanja Tanayama and Otto Toivanen (2008) "Evaluating innovation policy: a structural treatment effect model of R&D subsidies", Research Discussion Papers 7/2008, Bank of Finland.

Van Reenen, John (2004) "Active Labour Market Policies and the British New Deal for Youth in Context" in Richard Blundell, David Card, and Richard Freeman (eds) *Seeking a Premier Economy*, Chicago: Chicago University Press.

Wallsten, Scott (2000) "The effects of government-industry R&D programs on private R&D: the case of the Small Business Innovation Research program" *RAND Journal of Economics*, 31, 82-100.

Wilson, Daniel. (2009) "Beggar thy Neighbor? The In-State, Out-of-State and Aggregate Effects of R&D Tax Credits" *Review of Economics and Statistics*, 91(2), 431-436.

Wren, Colin (2005) "Regional Grants: Are They Worth It?" *Fiscal Studies*, 26(2), 245-75.

Wren, Colin and J. Taylor (1999) "Industrial Restructuring and Regional Policy" *Oxford Economic Papers*, 51, 487-516.

<u>**Table 1: Summary statistics for RSA participants and non-participants**</u>

Variable			(1) mean	(2) Test of equality	(3) Standard deviation	(4) Median	(5) Observations
1.	Plant Employment	Non treated	22.25	***	118.92	2	3,193,504
		Treated	79.39		241.45	6	136,488
2.	Firm Employment	Non treated	253	***	737	111	145,389
		Treated	417		957	171	8,209
3.	Gross output (£)	Non treated	26,774	***	13,6448	6,622	136,524
		Treated	39,401		15,1614	10,256	7,247
4.	Gross Investment (£)	Non treated	1,082.76	***	8,471.20	147.70	145,382
		Treated	1,624.35		7,204.89	310.03	8,209
5.	Real Value added per worker (£)	Non treated	31.05	**	162.51	24.27	136,524
		Treated	26.32		23.51	22.38	7247
6.	Total Factor Productivity (TFP)	Non treated	0.02	***	0.33	0.01	134,755
		Treated	-0.03		0.29	-0.03	7,925

Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. Column (1) reports the mean of the variables separately for the treatment group (participants in RSA) in the period before treatment and the non-treatment group (for all periods). We also report the significance of a t-test of equality between the two groups in column (2). TFP is constructed as a residual relative to the four digit industry-time period mean using four digit industry factor shares as weights (where factors are intermediate inputs, labor and capital) – see Appendix C. Nominal variables are not deflated.

Table 2: Employment Regressions at the plant level

Method	(1) OLS	(2) Reduced Form	(3) First Stage	(4) IV	(5) OLS	(6) Reduced Form	(7) First Stage	(8) IV
Dependent variable: ln(plant Employment)								
A. Plants in all firms (2,258,571 observations over 10,675 clusters (Wards))								
RSA (Participant)	1.212***			3.016***	0.108***			0.358***
	(0.026)			(0.144)	(0.008)			(0.135)
NGE (investment subsidy)		1.088***	0.361***			0.086***	0.240***	
		(0.062)	(0.009)			(0.033)	(0.018)	
Fixed effects (353,626)	No	No	No	No	Yes	Yes	Yes	Yes
B. Plants in firms with less than 150 employees (2,151,881 observations over 10,668 clusters (Wards))								
RSA (Participant)	1.056***			2.736***	0.117***			0.484***
	(0.022)			(0.135)	(0.008)			(0.140)
NGE (investment subsidy)		0.922***	0.337***			0.115***	0.237***	
		(0.053)	(0.009)			(0.034)	(0.018)	
Fixed effects (339,767)	No	No	No	No	Yes	Yes	Yes	Yes
C. Plants in firms with 150 employees or more (106,690 observations over 4,443 clusters (Wards))								
RSA (Participant)	0.705***			0.414**	0.130***			-0.157
	(0.038)			(0.181)	(0.024)			(0.563)
NGE (investment subsidy)		0.306**	0.739***			-0.042	0.268***	
		(0.136)	(0.029)			(0.150)	(0.062)	
Fixed effects (13,859)	No	No	No	No	Yes	Yes	Yes	Yes

Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. Dependent variable is ln(employment) in all columns except columns (3) and (7) where the dependent variable is RSA. RSA equals unity if the firm owns at least one plant that has participated in RSA in the past and zero otherwise. NGE is the "Net Grant Equivalent", the maximum investment subsidy in the area where the plant is located. Eligibility for investment subsidies (NGE) is used as an instrumental variable in columns (4) and (8). All columns include a full set time dummies and controls for firm age. Standard errors below coefficients are clustered by area in all columns (the "ward" level - similar to a US zipcode). The time period is 1986-2004.

Table 3: Employment Regressions at the plant level – Alternative Size thresholds

Dependent variable: ln(Employment)

Method	(1) OLS	(2) Reduced Form	(3) First Stage	(4) IV
A. Plants in firms with less than 100 employees (2,117,695 observations over 10,677 clusters (Wards))				
RSA (Participant)	0.121***			0.472***
	(0.009)			(0.145)
NGE (investment subsidy)		0.110***	0.233***	
		(0.034)	(0.018)	
Fixed effects (335,550)	Yes	Yes	Yes	Yes
B. Plants in firms with more than 100 employees (140,876 observations over 4,995 clusters (Wards))				
RSA (Participant)	0.127***			0.150
	(0.020)			(0.403)
NGE (investment subsidy)		0.050	0.315***	
		(0.128)	(0.058)	
Fixed effects (18,076)	Yes	Yes	Yes	Yes
C. Plants in firms with less than 50 employees (2,039,908 observations over 10,665 clusters (Wards))				
RSA (Participant)	0.129***			0.557***
	(0.009)			(0.149)
NGE (investment subsidy)		0.127***	0.229***	
		(0.034)	(0.018)	
Fixed effects (326,095)	Yes	Yes	Yes	Yes
D. Plants in firms with more than 50 employees (218,663 observations over 5,877clusters (Wards))				
RSA (Participant)	0.129***			0.090
	(0.016)			(0.298)
NGE (investment subsidy)		0.030	0.316***	
		(0.094)	(0.047)	
Fixed effects (27,531)	Yes	Yes	Yes	Yes

E. Linear interactions with size (2,258,571 observations over 10,675 clusters (Wards))

Method	(1) OLS	(2) Reduced Form	(3) First Stage: RSA	(4) First Stage: RSA*Size	(5) IV
RSA (Participant)	0.167***				0.931***
	(0.009)				(0.168)
RSA * Size	0.002				-0.533***
	(0.005)				(0.132)
NGE (investment subsidy)		0.148***	0.229***	0.124***	
		(0.032)	(0.017)	(0.018)	
NGE*Size		-0.083***	-0.051***	0.245***	
		(0.025)	(0.009)	(0.033)	
F-Statistics of Excluded IV			240.757	199.425	
Fixed effects (335,550)	Yes	Yes	Yes	Yes	Yes

Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. Dependent variable is ln(employment) in all columns except column (3) Panels A-D and columns (3) and (4) Panel E (where the dependent variable is RSA). RSA equals unity if the firm owns at least 1 plant that has participated in RSA in the past and zero otherwise. NGE ("Net Grant Equivalent") is the eligibility for the maximum investment subsidy in the area where the plant is located. NGE is used as an instrumental variable in column (4) in Panels A-D and column (5) in Panel E. All columns include a full set of firm fixed effects, age controls and time dummies. Standard errors below coefficients are clustered by area (ward level) in all columns. The time period is 1986-2004. In Panel E "size" is the initial ln(firm employment) centered on the sample mean.

Table 4: Employment Regressions at firm level

	Dependent variable: ln(firm Employment)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Method	OLS	Reduced Form	First Stage	IV	OLS	Reduced Form	First Stage	IV
Sample	Population of all firms (IDBR)				ARD Sub-Sample of IDBR			
A. All Firms								
RSA	0.127***			0.403***	0.146***			0.180
	(0.008)			(0.140)	(0.012)			(0.157)
NGE		0.098***	0.244***			0.080	0.463***	
		(0.035)	(0.018)			(0.074)	(0.060)	
Observations	2,194,684	2,194,684	2,194,684	2,194,684	129,567	129,567	129,567	129,567
Fixed effects	333,463	333,463	333,463	333,463	24,407	24,407	24,407	24,407
Clusters	10,669	10,669	10,669	10,669	5,721	5,721	5,721	5,721
B. Firms with less than 150 employees								
RSA	0.133***			0.553***	0.156***			0.277*
	(0.008)			(0.150)	(0.015)			(0.165)
NGE		0.132***	0.238***			0.142*	0.515***	
		(0.036)	(0.018)			(0.085)	(0.067)	
Observations	2,131,047	2,131,047	2,131,047	2,131,047	87,748	87,748	87,748	87,748
Fixed effects	327,239	327,239	327,239	327,239	19,667	19,667	19,667	19,667
Clusters	10,666	10,666	10,666	10,666	5,313	5,313	5,313	5,313
C. Firms with 150 employees or more								
RSA	0.236***			-0.490	0.149***			-0.070
	(0.022)			(0.540)	(0.016)			(0.383)
NGE		-0.170	0.357***			-0.020	0.361***	
		(0.179)	(0.087)			(0.137)	(0.105)	
Observations	63,637	63,637	63,637	63,637	41,819	41,819	41,819	41,819
Fixed effects	6,224	6,224	6,224	6,224	4,740	4,740	4,740	4,740
Clusters	2,786	2,786	2,786	2,786	2,413	2,413	2,413	2,413

Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. Dependent variable is ln(employment) in all columns except columns (3) and (7) where the dependent variable is RSA. RSA equals unity if the firm owns at least one plant that has participated in RSA in the past and zero otherwise. NGE is the "Net Grant Equivalent", the maximum investment subsidy in the area where the plant is located. Eligibility for investment subsidies (NGE) is used as an instrumental variable in columns (4) and (8). All columns include a full set of firm fixed effects, age controls and time dummies. Columns (5)-(8) also include age and three digit sectoral trends Standard errors below coefficients are clustered by area in all columns (the "ward" level is similar to a US zipcode). The time period is 1986-2004.

Table 5: Investment (INV) and Gross Output (PROD) Production Functions at the firm level, ARD sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Method	OLS	Red. Form	First Stage	IV	OLS	Red. Form	First Stage	IV
Dependent variable	**Ln(INV)**	**Ln(INV)**	**RSA**	**Ln(INV)**	**Ln(PROD)**	**Ln(PROD)**	**RSA**	**Ln(PROD)**
A. All Firms								
RSA (Participant)	0.227***			0.621	0.000			0.009
	(0.030)			(0.426)	(0.004)			(0.057)
NGE (investment subsidy)		0.290	0.462***			0.004	0.434***	
		(0.198)	(0.060)			(0.024)	(0.059)	
Observations	129,584	129,584	129,584	129,584	85,488	85,488	85,488	85,488
Fixed effects	24,411	24,411	24,411	24,411	19,769	19,769	19,769	19,769
Clusters	5,721	5,721	5,721	5,721	5,282	5,282	5,282	5,282
B. Firms with less than 150 employees								
RSA (Participant)	0.222***			0.973*	0.004			0.026
	(0.040)			(0.501)	(0.005)			(0.067)
NGE (investment subsidy)		0.500*	0.514***			0.012	0.474***	
		(0.259)	(0.066)			(0.031)	(0.070)	
Observations	87,765	87,765	87,765	87,765	58,522	58,522	58,522	58,522
Fixed effects	19,671	19,671	19,671	19,671	15,519	15,519	15,519	15,519
Clusters	5,313	5,313	5,313	5,313	4,825	4,825	4,825	4,825
C. Firms with 150 employees or more								
RSA (Participant)	0.233***			-0.148	-0.008			-0.090
	(0.045)			(0.761)	(0.007)			(0.109)
NGE (investment subsidy)		-0.050	0.361***			-0.030	0.352***	
		(0.274)	(0.105)			(0.038)	(0.095)	
Observations	41,819	41,819	41,819	41,819	26,966	26,966	26,966	26,966
Fixed effects	4,740	4,740	4,740	4,740	4,250	4,250	4,250	4,250
Clusters	2,413	2,413	2,413	2,413	2,253	2,253	2,253	2,253

Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. The dependent variable in columns (1), (2) and (4) is INV, ln(gross investment). The dependent variable in columns (5), (6) and (8) is PROD ln(gross output per employee). In columns (4)-(8) all regressions include controls for ln(capital per employee), ln(intermediate inputs per employee), and ln(employment) so that the RSA and reduced form coefficients capture the effect on total factor productivity (TFP). See Appendix C for more details on production function estimates. RSA equals unity for all the periods in and after a firm has participated in the program and zero otherwise. NGE is Net Grant Equivalent (the maximum investment subsidy) at the area-level. Eligibility for investment subsidies used as an instrumental variable in columns (4) and (8). All columns include a full set of firm fixed effects, time dummies and three digit sectoral trends. Standard errors below coefficients are clustered by area in all columns (the ward level similar to a US zipcode). Time period is 1986-2004.

Table 6: Employment and Number of Plants at the Area Level, Reduced Forms

Dependent Variable	(1) ln(Employ-ment)	(2) ln(#Plants)	(3) ln(Employ-ment)	(4) ln(#Plants)	(5) ln(Employ-ment)	(6) ln(Service Employment)	(7) ln(Unemploy-ment)
Level of aggregation	**Wards**	**Wards**	**TTWA**	**TTWA**	**Wards**	**Wards**	**Wards**
Years	**1986-2004**	**1986-2004**	**1986-2004**	**1986-2004**	**1996-2004**	**1996-2004**	**1996-2004**
A. Aggregating over all plants							
NGE	0.287**	0.171***	0.355***	0.248***	0.212*	0.090	-0.687***
	(0.118)	(0.049)	(0.133)	(0.083)	(0.114)	(0.061)	(0.043)
Observations	177,794	177,794	6,001	6,001	84,362	73,829	83,912
#Fixed effects & clusters	10,737	10,737	322	322	10,737	10,737	10,737
B. Aggregating over plants in firms with less than 150 employees							
NGE	0.495***	0.195***	0.512***	0.275***			
	(0.111)	(0.051)	(0.146)	-0.091			
Observations	177,794	177,794	6,001	6,001			
#Fixed effects & clusters	10,737	10,737	322	322			
C. Aggregating over plants in Firms with 150 employees or more							
NGE	0.211	0.078**	-0.178	-0.023			
	(0.171)	(0.039)	(0.298)	(0.153)			
Observations	177,794	177,794	6,001	6,001			
#Fixed effects & clusters	10,737	10,737	322	322			

Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. ln(Employment) is the (log of) the total employees in manufacturing in the area + 1. "ln(#Plants)" = the log of 1+ the total number of manufacturing plants in the area. Ln(Unemployment) is the log of the total number of people who are claiming unemployment insurance in the area. Ln(Service employment) is the log of the total number of workers in service firms. These specifications are all equivalent to the reduced form specifications. The data in these regressions is aggregated up to the area level – either 10,737 wards or 322 Travel to Work Areas (TTWA). To make sure our results are not driven by outliers, we drop observations with area level employment growth rates higher than the 99th percentile of the employment growth rate distribution. RSA equals unity for all the periods in and after a plant has participated in the program and zero otherwise. NGE is Net Grant Equivalent (the maximum investment subsidy) at the area-level. All columns include a full set of area fixed effects and time dummies. Standard errors below coefficients are clustered by area in all columns (the ward level in the ward level equations and the TTWA level in the TTWA equations).

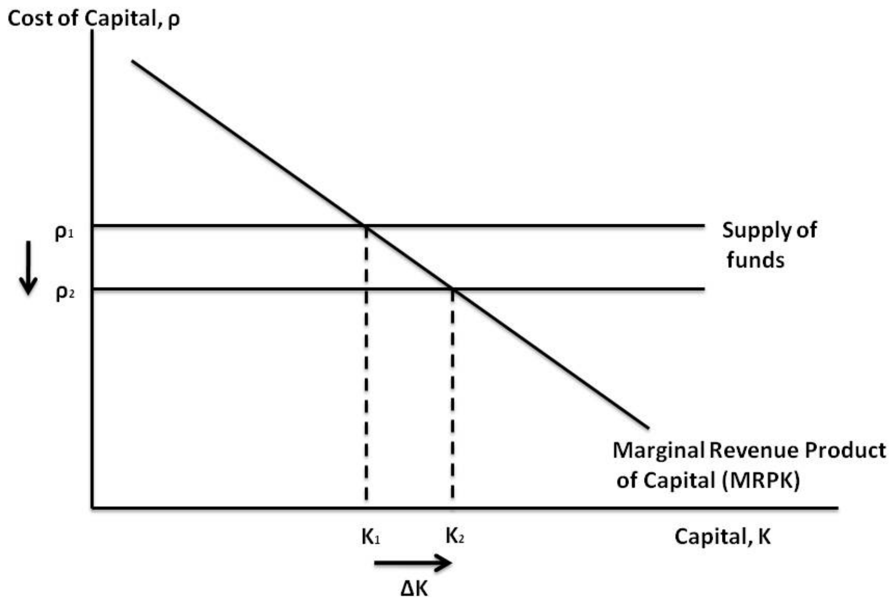
Table 7: Robustness Tests on firm level employment, Reduced Forms

	(1) All	(2) Firms with less than 150 employees	(3) Firms with 150 employees or more
Dependent variable: ln(Firm Employment)			
A. Baseline			
NGE	0.098***	0.132***	-0.170
	(0.035)	(0.036)	(0.179)
Observations	2,194,684	2,131,047	63,637
Fixed effects	333,463	327,239	6,224
Clusters	10,669	10,666	2,786
B. Instruments only based on rule changes			
NGE	0.149***	0.152***	-0.113
	(0.056)	(0.056)	(0.319)
Observations	668,284	650,342	17,942
Fixed effects	123,685	120,512	3,173
Clusters	10,247	10,229	1,932
C. Common support sample			
NGE	0.292**	0.282**	0.090
	(0.121)	(0.143)	(0.177)
Observations	55,318	32,613	22,705
Fixed effects	9,825	7,067	2,758
Clusters	3,617	3,034	1,651
D. Controlling for EU Structural Funds			
NGE	0.111***	0.146***	-0.140
	(0.036)	(0.037)	(0.179)
EU Structural Funds	-0.010	-0.010	-0.040*
	(0.006)	(0.006)	(0.021)
Observations	2,194,684	2,131,047	63,637
Fixed effects	333,463	327,239	6,224
Clusters	10,669	10,666	2,786
E. Interactions with sectoral capital intensity			
NGE	0.051	0.045	-0.060
	(0.041)	(0.042)	(0.199)
NGE X Sectoral Capital Intensity	0.110*	0.204***	-0.210
	(0.059)	(0.058)	(0.291)
Observations	2,193,917	2,232,554	63,611
Fixed effects	333,254	327,033	6,221
Clusters	10,669	10,666	2,785

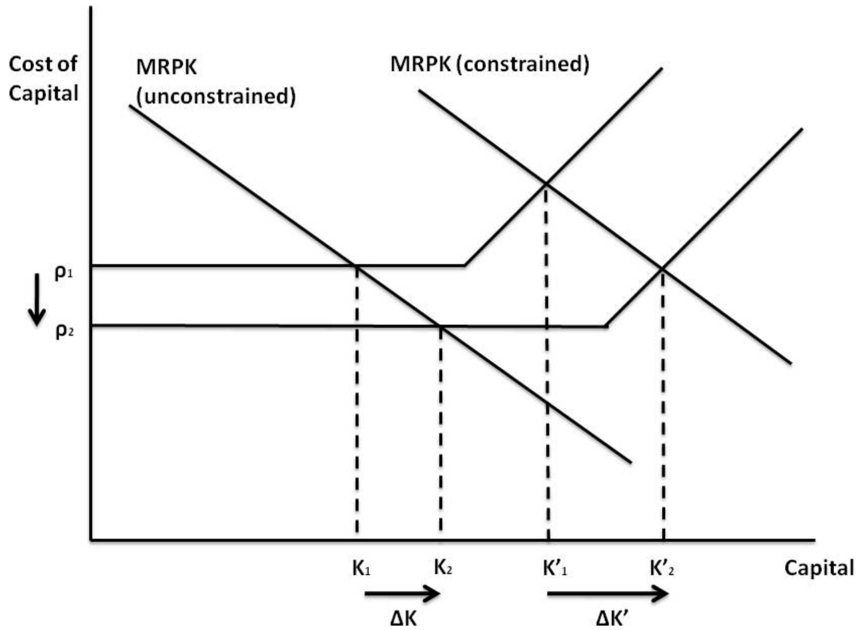
Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. These specifications are all based on those in Table 3 column (6) which is reproduced in Panel A as the baseline column. Panel B constructs the instrument using the rule changes only (see text and Appendix B). Panel C uses a matching technique based on the propensity score to obtain a "common support". We run a probit of RSA participation on a range of lagged observables (see text) and then use only observations that fall between the 25th and 75th percentile of the propensity score distribution. Panel D include a dummy variable indicating whether the ward was also eligible to receive EU Structural Funds. Panel E includes capital intensity in the three digit industry as additional regressor and interacted with the policy instrument, NGE. Time period is 1986-2004.

Figure 1: Effects of the RSA policy on capital

Panel A – Perfect Capital Markets

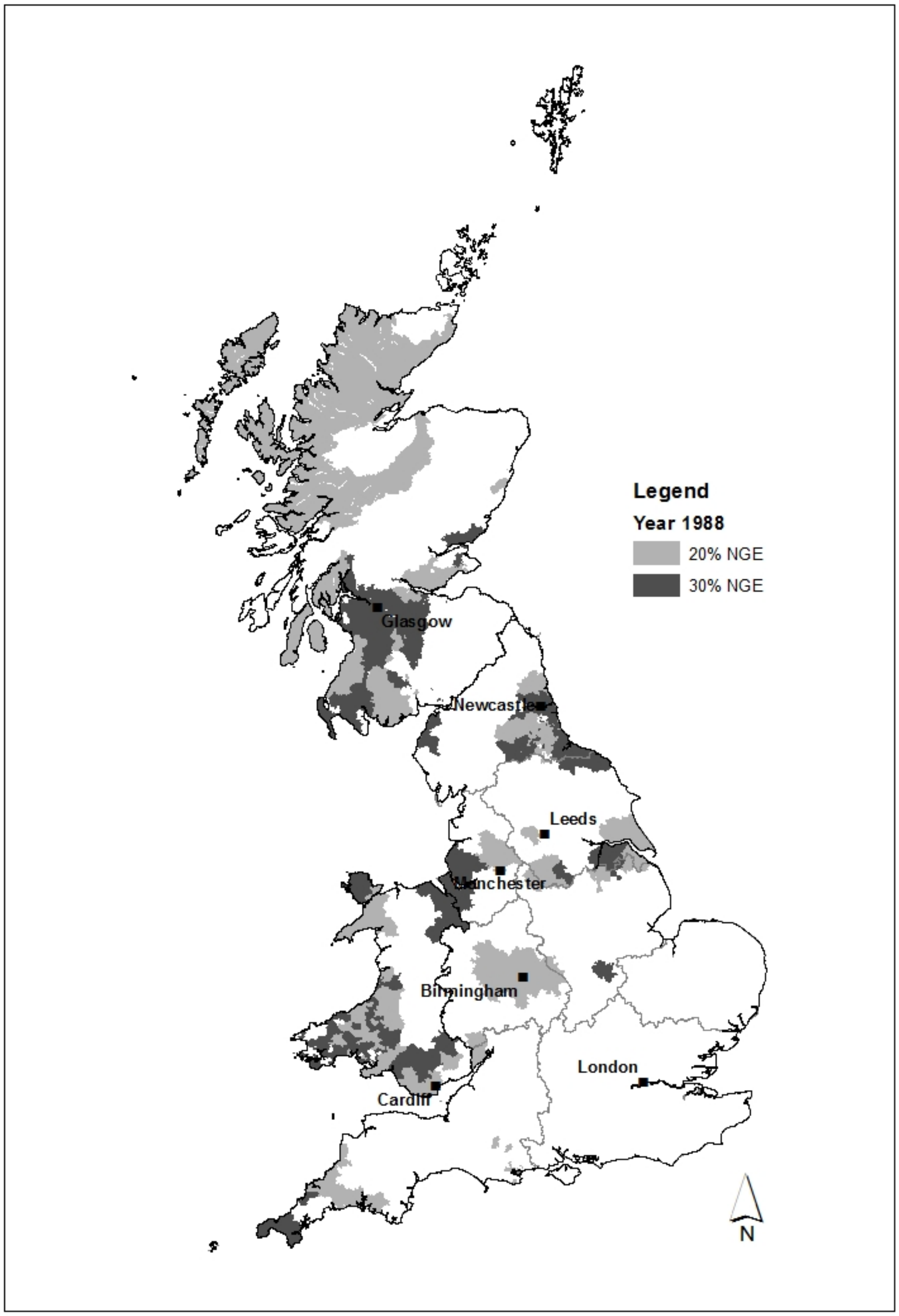


Panel B – Imperfect Capital Markets



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 2: Assisted Areas Map 1986- August 1st 1993

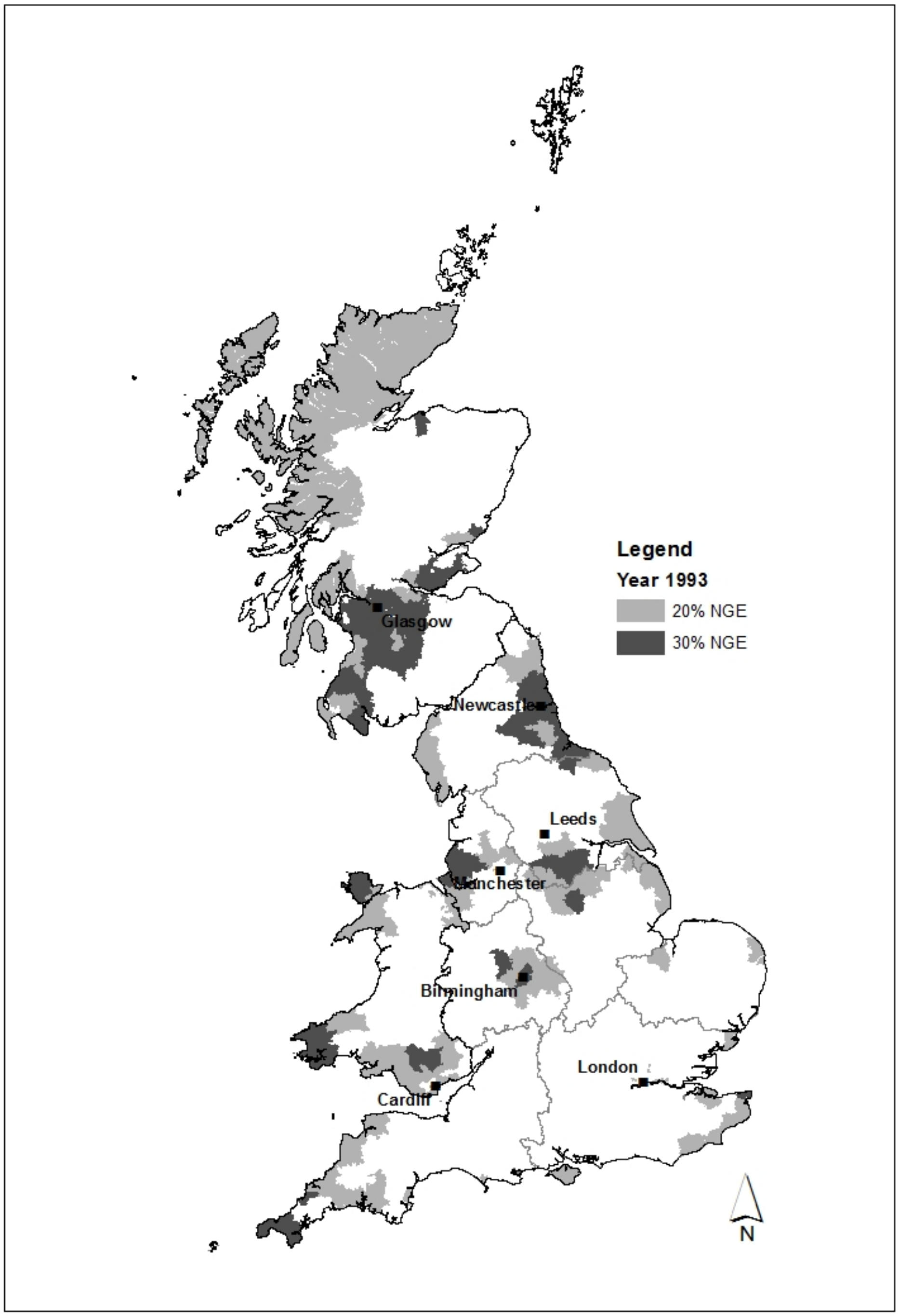


Notes: The shaded areas are those which are eligible for some Regional Selective Assistance. The dark shaded areas are the very deprived areas eligible for an investment subsidy of up to 30% NGE (Net Grant Equivalent, the maximum investment subsidy). The light shaded areas are eligible for up to 20% NGE.

Source: Department of Business.

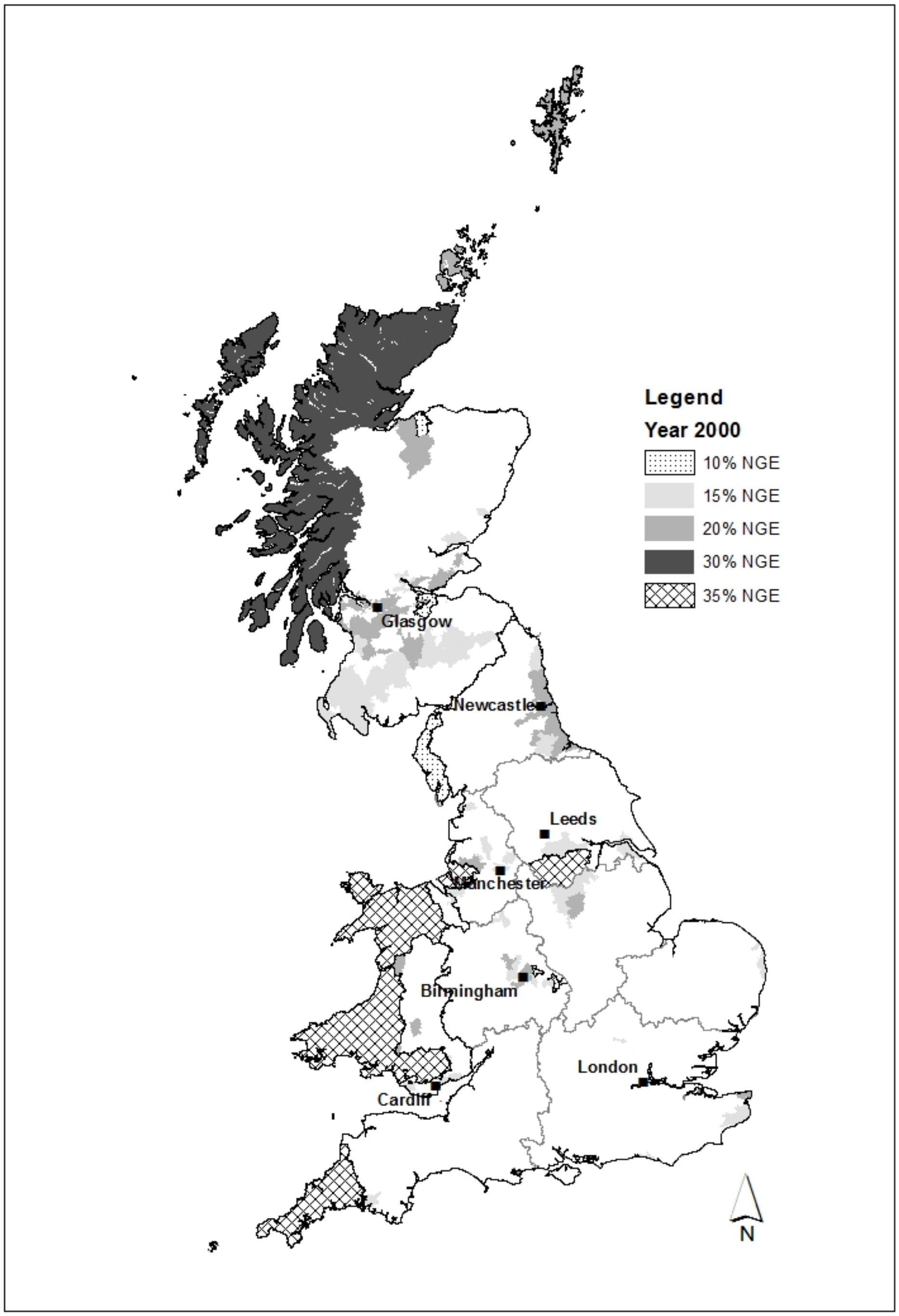
Figure 3:

Assisted Areas Map August 1st 1993 to January 1st 2000



Notes: The shaded areas are those which are eligible for some Regional Selective Assistance. The dark shaded areas are the very deprived areas eligible for an investment subsidy of up to 30% NGE (Net Grant Equivalent, the maximum investment subsidy). The light shaded areas are eligible for up to 20% NGE.
Source: Department of Business.

Figure 4: Assisted Areas with detailed NGE rates after January 1st 2000



Notes: This shows all the different levels of NGE (Net Grant Equivalent, the maximum investment subsidy) by area. Tier 1 areas had 35% NGE and Tier 2 areas ranged between 10% and 30%.

Source: Department of Business

Table A1: Changes in eligibility for RSA at times of EU rules changes

		(1)	(2)	(3)	(4)
Unit of Observation	**Year**	**Total Number of Units s**	**Units of Observations which changed their eligibility to receive RSA**	**Increase in eligibility for subsidies**	**Decrease in eligibility for subsidies**
Areas (wards)	1993	10,737	1,893	1,034	859
	2000	10,737	4,048	1,424	2,624
Plants	1993	146,420	23,225	14,369	8,856
	2000	163,796	50,920	14,967	35,953
Firms	1993	125,444	19,866	12,505	7,361
	2000	148,598	45,692	13,520	32,172

Notes: The first rows report the number of areas (wards). Note that because of a small number of changes in the definition of wards over time these are slightly below the number of wards in the regression tables. Column (2) indicates how many areas changed their eligibility status and this is divided into increases (column (3)) and decreases (column (4)). The other rows report the equivalent statistics for plants and firms. The eligibility maps changed in 1993 and 2000. For example, in 1993 there were 23,225 changes in support status at the plant level. 62% of these changes (14,369/23,225) were increases in eligibility rates (either from zero to a positive number or a lower to a higher subsidy rate).

Table A2: Descriptive statistics across areas (Wards), Manufacturing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Aggregate expenditure on RSA (£m)	**Number of plants receiving RSA**	**Average NGE rate in eligible wards**	**Eligible Wards (as % of all Wards)**	**%jobs in eligible areas**	**% Plants receiving RSA (eligible wards)**	**% total jobs - for plants receiving RSA (in all wards)**	**% total jobs – RSA plants (in eligible wards)**
1986	120.498	924	0.248	0.29	0.37	0.225	0.05	0.12
1987	214.851	1,175	0.248	0.29	0.38	0.316	0.06	0.15
1988	234.437	1,758	0.248	0.29	0.37	0.468	0.06	0.17
1989	190.620	1,607	0.248	0.29	0.38	0.391	0.06	0.15
1990	154.754	1,545	0.248	0.29	0.39	0.296	0.06	0.16
1991	136.414	1,214	0.248	0.29	0.39	0.348	0.06	0.17
1992	115.184	1,032	0.248	0.29	0.39	0.314	0.07	0.18
1993	172.326	1,108	0.248	0.29	0.4	0.338	0.08	0.20
1994	154.570	1,396	0.241	0.29	0.39	0.345	0.08	0.19
1995	134.957	1,590	0.241	0.29	0.38	0.391	0.07	0.20
1996	157.001	1,765	0.241	0.32	0.41	0.432	0.08	0.20
1997	158.279	1,529	0.241	0.32	0.41	0.432	0.09	0.21
1998	115.332	1,332	0.241	0.32	0.41	0.361	0.08	0.18
1999	91.763	1,210	0.241	0.32	0.41	0.327	0.07	0.17
2000	185.684	1,226	0.237	0.32	0.41	0.304	0.06	0.16
2001	219.694	1,126	0.237	0.32	0.4	0.235	0.06	0.15
2002	192.713	1,069	0.237	0.26	0.37	0.161	0.04	0.11
2003	197.266	1,078	0.237	0.26	0.37	0.094	0.03	0.08
2004	148.583	1,028	0.237	0.26	0.37	0.066	0.03	0.08
Total	162.891	1,300	0.243	0.29	0.39	0.296	0.06	0.16

Notes: Column (1) is total expenditure on RSA and column (2) is the number of RSA recipients (both from the population). Column (3) is the average NGE across the eligible wards. Column (4) is the share of total wards which are eligible for RSA and column (5) is the share of employment in these wards. Column (6) is the proportion of plants receiving RSA in eligible areas. We calculate the total employment in plants receiving RSA and estimate this as a proportion of total jobs in all wards (in column (7)) and as a proportion of jobs in eligible wards (in column (8)).

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

Table A3: Employment Regressions in differences at the local unit level, checking for plant-specific trends

	(1) All Plants	(2) Plants in small firms	(3) Plants in large firms
	No plant specific trends		
NGE	0.075***	0.091***	0.001
	(0.029)	(0.029)	(0.090)
Observations	1,631,753	1,542,644	89,109
Plants	243,724	233,424	10,300
Clusters	10,573	10,563	3,954
	Plant specific trends		
NGE	0.068**	0.074**	0.058
	(0.033)	(0.034)	(0.098)
Observations	1,631,753	1,542,644	89,109
Plants	243,724	233,424	10,300
Clusters	10,573	10,563	3,954

Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. The table reports regressions in differences. The regressions reported in the second panel include a full set of plant fixed effects thus controlling for plant specific trends. The dependent variable is ln(employment). NGE is the “Net Grant Equivalent”, the maximum investment subsidy in the area where the plant is located. The time period is 1986-2004.

Table A4: Robustness Tests on plant-level employment, Reduced Forms

	(1) All	(2) Firms with less than 150 employees	(3) Firms with 150 employees or more
Dependent variable: ln(Firm Employment)			
A. Baseline			
NGE	0.086***	0.115***	-0.040
	(0.033)	(0.034)	(0.150)
Observations	2,258,571	2,151,881	106,690
Fixed effects	353,626	339,767	13,859
Clusters	10,675	10,668	4,443
B. Instruments only based on rule changes			
NGE	0.120**	0.156***	-0.303*
	(0.051)	(0.053)	(0.183)
Observations	639,604	615,280	24,324
Fixed effects	122,260	117,574	4,686
Clusters	10,225	10,202	2,566
D. Controlling for EU Structural Funds			
NGE	0.098***	0.128***	0.007
	(0.034)	(0.034)	(0.152)
EU Structural Funds	-0.009	-0.009	-0.051**
	(0.006)	(0.006)	(0.021)
Observations	2,258,571	2,151,881	106,690
Fixed effects	353,626	339,767	138,59
Clusters	10,675	10,668	4,443
E. Interactions with sectoral capital intensity			
NGE	0.048	0.038	0.075
	(0.039)	(0.040)	(0.191)
NGE X Sectoral Capital Intensity	0.089	0.183***	-0.223
	(0.055)	(0.057)	(0.295)
Observations	2,257,800	2,151,142	106,658
Fixed effects	353,413	339,559	13,854
Clusters	10,675	10,668	4,443

Notes: *** denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level. These specifications are all based on those in Table 2 column (6) which is reproduced in Panel A as the baseline column. Panel B constructs the instrument using the rule changes only (see text and Appendix B). Panel D include a dummy variable indicating whether the ward was also eligible to receive EU Structural Funds. Panel E includes capital intensity in the three digit industry as additional regressor and interacted with the policy instrument, NGE. The time period is 1986-2004.

APPENDIX A: MORE DETAILS OF THE RSA POLICY

Introduction

During the period of our study (1986 to 2004) Regional Selective Assistance (RSA) was the main business support scheme in the UK.[32] Since the early 1970s it provided discretionary grants to firms in disadvantaged regions typically characterized by relatively low levels of per capita GDP and high unemployment ("Assisted Areas").[33] 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. In general, this type of assistance is prohibited by European law except in certain cases. In particular, Article 87 of the Treaty of Amsterdam 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.[34] There is an upper threshold of support called the Net Grant Equivalent (NGE)[35] which essentially sets a maximum proportion of the firm's investment that can be subsidized by the government.

Since the main formulae which determine eligibility are decided periodically at the European level, and not at the UK level, this mitigates concern of endogeneity of policy decisions to a local area. And although the UK has latitude to decide the overall amount of the annual budget for RSA, they 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.

Changes in eligibility over time

The map of the areas changed periodically before 1993 and about seven years since then. 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 (a potentially confounding influence that we check for in Table 7). During our sample period, areas eligible for RSA changed twice: first in 1993 and then again in 2000. There were also changes in 1984, before our sample period begins and in 2006, after our sample period ends.

The map of the eligible areas is proposed by the UK, but needs to be approved by the EU in accordance with the EU regional guidelines and in respect of Article 87 of the Amsterdam Treaty. Decisions on area eligibility are based on a two stage procedure. The first stage identifies areas that have relatively low GDP or high unemployment when compared to their national average. The thresholds for deciding what low per capita GDP and high unemployment is are, in turn, based on the position of the country relative to the EU average. For the second stage, a series of additional indicators is used to reassess eligibility for areas that were not covered by the first stage. Thus, when the map is redrawn there are several sources of exogenous (to the area) variation: changes in UK per capita GDP and unemployment; changes in EU per capita GDP and unemployment; and changes in the EU rules over the set of other indicators used and the weights given to these indicators in determining eligibility.

[32] 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.

[33] In April 2004, in England, the RSA scheme was rebranded as the Selective Finance for Investment scheme and then Grant 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.

[34] 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.

[35] 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.

The eligibility criteria are outlined in guidelines which are published before the implementation of the map (in our case 1988 and 1998). The UK government will then gather quantitative information on indicators at the relevant area level and will propose a new map. Figures 2-4 shows the map at three different points in time: 1984-1993, 1993- 2000 and 2000-2006. Below we discuss the changes in 1993 and 2000.

(a) The 1993 change

The assisted area map for RSA was redrawn in 1993 on the basis of the 1988 guidelines using "Travel to Work Areas" as the underlying spatial units.[36] The 1993 rules used a different set of indicators to those used in drawing the 1984 map although some items (such as per capita GDP and unemployment) remained core indicators. The indicators included: persistently high unemployment, the proportion of long-term unemployed, participation rates, growth/decline in local industries, demographic changes, major firm closures, geographical distance from major markets, population density and urban problems. The exact weights given to the different indicators in determining eligibility are not stated, but we can econometrically estimate the rules (see Appendix B). The Assisted Areas fell into two categories: (a) Development Areas where aid could be granted up to a maximum of 30% NGE (Net Grant Equivalent - see above) and (b) Intermediate Areas 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 was restricted from those in 1988. The main criteria to decide eligibility were still per capita GDP, labor market performance and the share of manufacturing. Tier 1 (most deprived) areas were: 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. These 65 zones were constructed on the basis of groups of electoral wards.[37] We discuss below how this change in decision rules affects an area's eligibility independently of an area's economic conditions. Within Tier 2 Areas the map identified four sub-tier areas eligible for different level of maximum NGE. The level of aid intensities proposed for these areas varied according to the seriousness and intensity of the problems in each region, relative to other EU countries.

For the most disadvantaged sub-tier areas, that were geographically distant and sparsely populated, a maximum subsidy rate of 30% NGE was allowed.[38] The maximum NGE level for relatively less deprived areas was 10%.[39] However, if those (less deprived) areas adjoined a Tier 1 area they had a 20% ceiling. The rest of the eligible areas aid ceilings were either an NGE of 20% or 15% (with the decision as to which applies made by referring to current conditions as well as the NGE in the 1993 map).

Formal criteria for receipt of RSA

During our study period (1986-2004), 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;[40] (f) *Prior Commitments*: As RSA could only be offered when the project could not proceed without it, BIS must have completed its appraisal and issued a formal offer of assistance before the applicant entered into

[36] Travels to Work Areas are defined by the UK Census Bureau (Office for National Statistics). The fundamental criterion is that, of the resident economically active population, at least 75% actually work in the area, and also, that of everyone working in the area; at least 75% actually live in the area.
[37] The data used for the zone 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.
[38] These areas have a population density of less than 12.5 inhabitants per square kilometre and are mainly the Highlands of Scotland (1.2% of assisted areas population were in these areas).
[39] These are areas with a higher GDP per capita and lower unemployment rate than the Community average (covering 4.2% of assisted areas population).
[40] 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.

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 BIS office. The lag between receipt and decision depended on the amount applied for. During the period analysed, the lag was normally between 35 and 60 days, and 100 days or more for grants above £2 million. The lag depended on the time needed to ensure that all of the criteria were met and on negotiations between the government agency and the firm on the terms of assistance. 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 installments – 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 discussed in Section I, the level of subsidy that plants in an area are eligible for depends on the EU rules. We argue that changes in the rules generate exogenous changes in the level of maximum investment subsidy (NGE) that we use to construct our policy variables. A concern is that the areas which experienced a change in eligibility, as detailed in Table A1, did so because of unobserved contemporaneous changes in the area that are correlated with our outcome variables. This would invalidate the IV strategy.

To see this formally, consider the change in eligibility in 2000. Denote eligibility in 2000 as a discrete variable Z_{00}. For the year 2000 (and afterwards) area eligible depends on a vector of its characteristics, X, and the weights attached to these characteristics (denoted by the vector θ). As noted above (e.g. Appendix A) the data used to determine eligibility was from 1998 (and before) and the weights were also set at or before 1998. Thus eligibility in 2000 is defined as:

$$Z_{r00} = \theta_{98} X_{r98} \quad \text{(B1)}$$

The characteristics are area specific but the weights are European wide. Between 1993 and 2000 an area was eligible for RSA depending on characteristics in 1988 and a different set of weights that were set at or before 1988. We can therefore write eligibility in 1993 as:

$$Z_{r93} = \theta_{88} X_{r88} \quad \text{(B2)}$$

The vector X is the superset of all variables that were used in any year (2000 and 1993) to determine eligibility. When a characteristic ceases to be included as a determinant it is given a weight of zero in the θ_t vector. Now consider what determines the change in eligibility status between 2000 and 1999 (or equivalently 1st January 2000 and 1st January 1993):

$$Z_{r00} - Z_{r93} = \theta_{98} X_{r98} - \theta_{88} X_{r88} = (\theta_{98} - \theta_{88}) X_{r88} + (X_{r98} - X_{r88})\theta_{88} + (\theta_{98} - \theta_{88})(X_{r98} - X_{r88}) \quad \text{(B3)}$$

The change in eligibility will depend on the changes in the weights $\theta_{98} - \theta_{88}$ and changes in area characteristics, $(X_{r98} - X_{r88})$. We have argued that because changes in area characteristics are unlikely to be correlated with current shocks to outcome variables because: (1) they are determined in 1998 at least two years prior to our outcome variables; (2) X contains a large component of factors unrelated to any area-specific shocks (such as average per capita GDP in the European Union that changed when the EU expanded); and (3) unobserved trends would be captured by plant-specific trends which we found did not change the results in sub-section V.A. However, one could be concerned that areas which were declining may be more likely to be selected into RSA and more likely to have employment falls. Although this would generally cause a downward bias to the coefficient in RSA we can investigate how important these are by using *only* the change in weights and the level of area characteristics a decade earlier to determine eligibility status. In other words we use only $(\theta_{98} - \theta_{88}) X_{r88}$, the leading term in equation (B3) as an IV instead of the actual change in eligibility. These are “theoretical instrumental variable” that should be purged of any suspected bias as they are constructed based solely on the rule changes.

Implementation of this idea is complicated by the fact that, although the EU reveals what is in the *X* vector, it does not reveal the exact weights in θ that determine eligibility. We know whether a particular element of *X* has a weight of zero, but not the non-zero weights. Nevertheless, we can empirically estimate these weights by estimating a regression equivalent of equations (B1) and (B2). With the estimated $\hat{\theta}$ we can assign changes to maximum subsidy rates to (NGEs) areas based on $(\hat{\theta}_{98} - \hat{\theta}_{88})X_{r88}$ rather than any (potentially endogenous) changes in characteristics $(X_{r98} - X_{r88})$.

We detail the results of this experiment detailed in Panel B of Table 7 to show that our results are stable. The *X*'s include GDP per capita, unemployment levels, the proportion of long-term unemployed, persistently high unemployment, participation rates, share and change of manufacturing employment, average plant age, the proportion of firm closures, geographical distance from major markets and population density. Results from regressions of equation (B1) and (B2) are available on request.

APPENDIX C: MORE DETAILS ON DATA, MATCHING TO RSA AND PRODUCTIVITY ESTIMATION

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), the UK Census Bureau to conduct the Annual Business Inquiry (ABI)[41]. The IDBR is a list of all businesses in the UK, their addresses, type of activity and ownership/control structure. The list is compiled using a combination of tax records, accounting information (every UK firm has to lodge some information at Companies House). The smallest unit in the IDBR is a site which 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.[42] The ARD is the UK equivalent of the US Longitudinal Respondents Database. Not only is the ARD a sub-sample of the population IBRD, but the information is reported at a more aggregated level across the entire firm ("reporting unit"), rather than at the plant ("local unit"). For example, a firm with two 10 worker 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 the population 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.

Matching Datasets

Since the performance data comes from sources unrelated to program participation, several problems arise in matching. The Department of Business (BIS) 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 particular plant has benefited from RSA receipt (although for the 80% of single-firm plants there is never an ambiguity). Thus, our primary measure of program participation is whether a plant was in a firm that received any RSA (which we can always 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

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

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

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, from 1972-2004, whether or not the application has been successful. Applicant numbers declined in the 2000s as the total budget for RSA fell.[43] Using name, postcode and CRN numbers, the information in BIS files was linked to the IDBR over the whole period. The matching rate was 77% over the sample period (1986-2004) and improves over time. This improvement reflects the fact that the IDBR was overhauled in 1994 to be closer to the true population – many of the exiters were not kept up to date in the earlier period. Given the lower match rate in earlier years and fewer observations in the last few years, we focus only on data between 1986 through 2004 in the econometric analysis, and show robustness to estimation solely on post 1996 information (see Table 6).

There are 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 out on 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 which 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).

Given some of the matching issues, using a single binary indicator of whether a plant was in a firm that ever received RSA in the past as a coarse, but more robust indicator of participation. The accurately defined policy IVs which contain no matching issues are more robust in this regard, and the larger IV coefficients may also reflect this measurement error problem.

As an alternative measure of RSA treatment intensity to the simple dummy we used the actual amount of RSA subsidy paid out to a firm (normalized on employment). The results were very similar to the ATT effect estimated in the main tables (see text). This is on the "Payment RSA database" that supplements the information in SAMIS with the date and amount of all payments. The data is less reliable in 1986-1987, so we also tested the robustness of these results to just estimating on the period from 1988 onwards.

TFP (Total Factor Productivity) measures

There are numerous ways to obtain a TFP measure, a subject of ongoing debate in the economic literature (see inter alia Olley and Pakes, 1996 and Ackerberg et al, 2007. The results in Table 5 are based on a simple production function approach. We assume that the production function, Q = AF(K,L,M) can be approximated by Cobb-Douglas and write this in the form

$$\ln Q_{it} = \ln A_{it} + \beta_L \ln L_{it} + \beta_K \ln K_{it} + \beta_M \ln M_{it} \tag{C1}$$

where A is TFP, Q is output, L is employment, K is capital and M is intermediate inputs. This can be re-written as:

$$\ln\left(\frac{Q_{it}}{L_{it}}\right) = \ln A_{it} + \beta_K \ln\left(\frac{K_{it}}{L_{it}}\right) + \beta_M \ln\left(\frac{M_{it}}{L_{it}}\right) + (\beta_L + \beta_K + \beta_M - 1)\ln L_{it} \tag{C2}$$

If the RSA program increases TFP (i.e. shifts up A) then we obtain the regression we estimate in Table 5 columns (5) - (8):

[43] This fall in total RSA numbers and funds was in large part because the "Enterprise Grant" element was spun off in 2000. The areas eligible for Enterprise grants were wider than RSA eligible areas and the levels of funding were smaller (under £75,000 and confined to "innovative" projects). If we include enterprise grants in the analysis the treatment effects are similar to the ones in the main paper. For example, replicating the baseline plant-level IV specification of column (8) of Table 2, Panel B we obtain a coefficient(*standard error*) of 0.502(*0.138*) compared to 0.484(*0.140*).

$$\ln\left(\frac{Q_{it}}{L_{it}}\right) = \lambda RSA_{it} + \beta_K \ln\left(\frac{K_{it}}{L_{it}}\right) + \beta_M \ln\left(\frac{M_{it}}{L_{it}}\right) + (\beta_L + \beta_K + \beta_M - 1)\ln L_{it} \tag{C3}$$

We cannot empirically reject λ=0 so generally impose constant returns to scale. Note that in these regressions we also control for fixed effects, time dummies and industry trends. In this regression output and materials are deflated by two digit industry output and input prices. Capital is built from the gross investment flows using a perpetual inventory method and allowing for differential depreciation rates across the three main asset classes (equipment, structures and vehicles).

We considered alternative ways to estimate TFP. First, we followed Solow and replaced the production parameters (β_K , β_M , β_L) with their factors shares in total revenue in the four digit industry. This allows the coefficients to vary by industry, but assumes that factor and product markets are competitive. This calculation underlies the means presented in Table 1. Second, we estimated equation (C1) separately industry by industry and calculated TFP as a residual based on this series of regressions. We then used TFP as the dependent variable in equation (C2) and did not condition on the other factor inputs. We obtained similar results from these different methods, so feel confident that the qualitative findings are robust.

APPENDIX D: AGGREGATING ACROSS SPATIAL UNITS

We consider the aggregation from lower (wards) to higher levels area (Travel to Work Areas) as discussed in sub-section IIIC. For simplicity consider the setup of a single Travel to Work Area (TTWA, denoted *a*) consisting of two wards r1 and r2 and also assume that we are dealing only with two periods t = 0 and t = 1. It is straightforward to generalize this to multiple wards, TTWAs and time periods (we do this in the empirical application). Suppose we know that as a consequence of the RSA program in period 1, ward r1 experiences a change of employment of α_{r1} log points whereas ward r2 experiences a change of α_{r2} log points; i.e. $\ln L_{r1,1} - \ln L_{r1,0} = \alpha_{r1}$.

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: $L_{a,t} = L_{r1,t} + L_{r2,t}$. Hence the logarithmic change in employment is:

$$\ln L_{a,1} - \ln L_{a,0} = \ln\left[e^{\alpha_{r1}} s_{r1,0} + e^{\alpha_{r2}}\left(1 - s_{r1,0}\right)\right] \tag{D1}$$

where $s_{r1,0} = \dfrac{L_{r1,0}}{L_{r1,0} + L_{r2,0}}$ is the share of employment in Ward 1 in period 0. Re-write equation (D1) as:

$$\ln\left[e^{\alpha_{r1}} s_{r1,0} + e^{\alpha_{r2}}\left(1 - s_{r1,0}\right)\right] = \alpha_{r2} + \ln\left[\left(e^{\alpha_{r1}-\alpha_{r2}} - 1\right)s_{r1,0} + 1\right] = \nu_1 + \alpha_{r2} + \left(e^{\alpha_{r1}-\alpha_{r2}} - 1\right)s_{r1,0}$$

where ν_1 is an approximation error that is small for values of $\left(e^{\alpha_{r1}-\alpha_{r2}} - 1\right)s_{r1,0}$ close to zero.

Similarly note that $\left(e^{\alpha_{r1}-\alpha_{r2}} - 1\right) = \nu_2 + \ln\left[\left(e^{\alpha_{r1}-\alpha_{r2}} - 1\right) + 1\right] = \nu_2 + \alpha_{r1} - \alpha_{r2}$ for $\left(e^{\alpha_{r1}-\alpha_{r2}} - 1\right)$ close to zero and where ν_2 is another approximation error[44].

Consequently, we can write the change in TTWA employment as:

$$\ln L_{a,1} - \ln L_{a,0} \approx \alpha_{r2} + \left(\alpha_{r1} - \alpha_{r2}\right)s_{r1,0} = s_{r1,0}\alpha_{r1} + \left(1 - s_{r1,0}\right)\alpha_{r2} \tag{D2}$$

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

[44] 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%.

This allows us to examine the case of negative spillovers as well. Suppose the policy leads to a positive effect of λ in region 1 at the expense of a negative spillover of χ in region 2 (as is assumed in equation (7) in the main text). For the aggregate TTWA we would consequently expect the effect on employment to be:

$$s_{r1,0}\lambda - \left(1 - s_{r1,0}\right)\chi$$

Also note that if we assume that the treatment effect of different levels of NGE can be modeled linearly as

$$\alpha_r = \beta \; NGE_r$$

then equation (D2) implies that running a TTWA level regression with ward-level employment weighted average NGE as treatment variable should lead to comparable magnitudes of estimates of the TTWA level impact $\beta_a = \beta_a NGE_a$ where $NGE_a = \sum_r s_r NGE_r$. Considering that the approximation error leads to an underestimate we certainly can rule out negative spillover effects if we find that the TTWA level effects are equal or larger than the ward level effects.