ISSN 2042-2695

CENTRE *for* ECONOMIC PERFORMANCE

CEP Discussion Paper No 1113

Revised January 2016
(Replaced January 2012 version)

The Causal Effects of an Industrial Policy

Chiara Criscuolo, Ralf Martin,
Henry G. Overman and John Van Reenen

THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE

E·S·R·C ECONOMIC & SOCIAL RESEARCH COUNCIL

Abstract
Business support policies designed to raise employment and productivity are ubiquitous around the world. We exploit changes in the area-specific eligibility criteria for a major program to support jobs through investment subsidies. Pan-European state aid rules determine whether a sub-national geographical area is eligible for subsidies, and we construct instrumental variables for area (and plant) eligibility based on the estimated parameters of these rule changes. We find areas eligible for business support create significantly more jobs (and reduce unemployment), and this is not due to job displacement between eligible and ineligible areas. An exogenous ten-percentage point increase in an area's maximum investment subsidy stimulates about a 7% increase in manufacturing employment. The treatment effect exists solely for small firms – large companies appear to "game" the system, accepting subsidies without increasing activity. There are positive effects on net entry of new plants, firm investment but no effects on Total Factor Productivity.

JEL Classifications: R11, H25, L52, L53, O25
Keywords: industrial policy, regional policy, employment, investment, productivity

This paper was produced as part of the Centre's Growth Programme. The Centre for Economic Performance is financed by the Economic and Social Research Council.

Acknowledgements
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 and grant ES/H010866/1. 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, and for useful insights. The ONS Virtual Microdata Lab ensured access to ONS Data, Alberta Criscuolo helped with the EU legislation and Mehtap Beyza Polat provided excellent research assistance. This work contains statistical data from ONS that 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 that may not exactly reproduce National Statistics aggregates. The opinions expressed and arguments employed herein are solely those of the authors and do not necessarily reflect the official views of the OECD or its member countries.

Chiara Criscuolo is a Senior Economist in the Structural Policy Division of the Science Technology and Industry Directorate at the OECD. Ralf Martin is an Assistant Professor of Economics, Imperial College Business School, London and Senior Research Associate with the Centre for Economic Performance, London School of Economics. Henry G. Overman is Director of the What Works Centre for Local Economic Growth and Professor of Economic Geography, London School of Economics. John Van Reenen is Director of the Centre for Economic Performance and Professor of Economics, London School of Economics.

Published by
Centre for Economic Performance
London School of Economics and Political Science
Houghton Street
London WC2A 2AE

INTRODUCTION

The Great Recession brought industrial policy back into fashion.[1] Governments around the world granted huge subsidies to private firms most dramatically in financial services, but also in other sectors like autos. Nevertheless, business support policies are not new – most governments offer business subsidies that claim to protect jobs, reduce unemployment and foster productivity, particularly in disadvantaged areas. For example, the US spends around $40-$50bn per annum on local development policies (Moretti, 2011). Despite the ubiquity of such schemes, rigorous micro-econometric evaluations of the causal effect of these policies are rare.

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

We tackle the identification problem by exploiting a policy experiment that induced exogenous changes in the eligibility criteria governing whether plants in economically disadvantaged areas could receive investment subsidies. This program was called "Regional Selective Assistance" (RSA) and exists in all European Union (EU) countries in one form or another. Crucially for our identification strategy, there are strict rules governing the geographical areas that are eligible to receive aid from the UK government that are determined by the EU. This is different from the US where the Federal government cannot prevent states from offering such business inducements (e.g. Felix and Hines, 2013). These are common, formula-driven EU-wide rules and we focus on a major policy change in the year 2000 because we have detailed administrative and institutional data before and after the change. Holding area characteristics fixed in the pre-policy change period we exploit only the change in the EU-wide policy parameters to the different observable factors determining which geographical areas were defined to be more economically disadvantaged (e.g. unemployment and per capita GDP). This enables us to estimate the causal effect of the program on unemployment, employment, plant net entry, investment, and productivity. Our data set is constructed by linking rich administrative panel data on the population of UK plants and the population of RSA program participants.

We reach four substantive conclusions. First, there is an economically large and statistically significant program effect - a 10 percentage point increase in an area's rate of investment subsidy

[1] We us the term "Industrial policy" simply as a program that directs investment subsidies to private sector firms. In our context, these subsidies are a government attempt to revitalize disadvantaged geographical areas.
[2] For examples see Krueger and Tuncer (1982), Beason and Weinstein (1996) and Lawrence and Weinstein (2001).

causes about a 3% decrease in aggregate unemployment, an 8% increase in manufacturing employment and 2% increase in the number of plants. These effects are underestimated if endogeneity is ignored, as the areas that become eligible for the program are those which, on average, experience negative shocks and whose firms would otherwise perform badly, even in the absence of the policy. Second, we show that the positive micro effects are not simply due to substitution of jobs towards program participants and away from non-participants in the same area, or in neighboring (ineligible) areas. The new jobs created appear to come from the pool of unemployed workers living in the area, which is encouraging in terms of welfare. Third, we find that the positive treatment effect is confined to the establishments in smaller firms (e.g. with under 50 workers). We suggest that this is due to larger firms being more able to "game" the system and take the subsidy without changing their behavior. Finally, there appear to be no additional effects on productivity after controlling for the program's positive investment effects. Since larger and less productive plants are more likely to receive subsidies, this implies that the program lowers measured aggregate productivity because it increases the employment share of low productivity firms (e.g. Hsieh and Klenow, 2009).

Our paper contributes to an emerging literature on the causal impact of place-based policies - see Kline and Moretti (2014a) for a survey and Kline and Moretti (2014b) on long-run effects on manufacturing jobs from the Tennessee Valley Authority policy. US Empowerment Zones - neighborhoods receiving substantial Federal assistance in the form of tax breaks and job subsidies - have been examined by Busso, Gregory and Kline (2013) who identify strong positive employment and wage effects, with only moderate deadweight losses.[3]

We also relate to a broader literature concerning evaluations of business support policies and place-based interventions (see Neumark and Simpson, 2014 for a review). Several papers consider direct research subsidies to industrial R&D. Unlike the generally positive assessments on R&D tax credits (e.g. Fowkes et al, 2015), the evidence on these direct subsidies is mixed (e.g. the survey in Jaffe and Le, 2015).[4] Several recent studies have used regression discontinuity design to assess the causal effects of direct grants. For example, both Howell (2015) on US SBIR and Bronzini and

[3] Holmes (1998), Albouy (2009) and Wilson (2009) consider other place-based tax policies, while Wren and Taylor (1999), Bronzini and de Blasio (2006), Martin et al (2011), Becker et al (2010, 2012a, b) provide evidence for regional policy in Europe. Gibbons, Overman and Sarvimaki (2011), and Einio and Overman (2015) discuss similar place based schemes in the UK, while Gobillon et al (2012) and Mayer et al (2013) provide estimates for France and Cerqua and Pellegrini (2014) for Italy. In contrast to RSA, which targets specific firms within eligible areas, these schemes are generally not discretionary (subject to the firm meeting some basic requirements). In addition to this substantive difference in the nature of the scheme, our paper is also unique in using exogenously imposed changes in area eligibility rules to identify the causal effects of the policy.

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

Iachini (2014) on Italian data use a proposal's application score by an independent committee as the running variable when analyzing the effects of receiving R&D subsidies. Interestingly, these studies are consistent with us in that they uncover much larger positive program effects on investment for small firms.[5]

Our paper is not the first to look at the impact of the RSA program. Unfortunately, most of the previous evaluation studies are based on "industrial survey" techniques where senior managers at a sample of assisted firms are asked to give their subjective assessment of what the counterfactual situation would have been had they not received the grant (e.g. see National Audit Office, 2003, for a survey). In contrast to OLS approaches 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 exaggerate the scheme's benefits. Some other studies have also used firm-level econometric techniques to evaluate the direct impact of RSA.[6] Relative to existing studies our contribution is to exploit a policy experiment on the population of plants to identify causal effects.

Finally, there is a large literature on the impact of capital and labor taxes (e.g. Mirrlees, 2010). 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. They are more likely to engender general equilibrium effects than the RSA policy that amounts to only around 0.1% of aggregate UK investment.

The structure of the paper is as follows. Section I describes the policy in more detail and outlines how eligibility changes over time. Section II sets out a simple theoretical framework and Section III describes the econometric modelling strategy. Section IV describes the data, Section V reports our results and Section VI provides some conclusions. In the online Appendices we report more details on the RSA policy (Appendix A), the changes in EU rules (Appendix B), data details (Appendix C) and issues in aggregation (Appendix D).

[5] By contrast, Jacob and Lefgren (2010) use a regression discontinuity design for US National Institute of Health grants and find no effect on academic productivity.

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

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

I.A Overview

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

Because RSA had the potential to distort competition and trade, it had to comply with European Union state aid legislation. European law, except in certain cases, prohibits this type of assistance. In particular, Article 87 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" in which higher rates of investment subsidy can be offered, and somewhat less deprived "Tier 2 Areas" where lower subsidy rates were offered. There is an upper threshold called the Net Grant Equivalent (NGE)[7], 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 geographical 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 national 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 driven by EU wide policy parameters and are therefore the key form of identification in our paper.

I.B Changes in eligibility over time

We focus on the change in the map of the areas eligible for RSA in 2000 using the period between 1997 and 2004, before and after the policy change. We also compare the results with those for longer periods. There have been changes in the area maps in 1984, 1993, 2000 and 2006. Our administrative data access does not extend beyond 2004 and we were unable to obtain precise

[7] 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.

information on the criteria for being an assisted area before 1993, so we cannot construct the rules change IV for the 1993 changes (although we show OLS estimates in this earlier period).

Figure 1 shows the maps of assistance in the pre-2000 period (left hand side) and post 2000 period (right hand side). There was considerable change in the areas that could receive assistance and the level of subsidy they were able to receive. Whether an area is eligible for any RSA is determined by a series of quantitative indicators of disadvantage which were changed over time but always included per capita GDP and unemployment (both relative to the EU average).[8] For the 2000 change, the data used to determine which areas were eligible dated from 1998 and before. Although the EU publishes which indicators it uses, it does not give the exact policy parameters ("weights") on these indicators which determine eligibility, but we can estimate these parameters econometrically (see sub-section IIIA).

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

[8] See Table A3 for a full list of variables, definitions and some descriptive statistics on these variables.

I.C Formal criteria for receipt of RSA investment subsidies

RSA was heavily targeted at the manufacturing sector – fewer than 10% of grants by value 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 more subjective and are based on the government's ability to assess the counterfactual situation of what would have happened in the absence of government support. For example, a firm could cut jobs but claim that it would have reduced employment *by even more* without support. It is difficult for bureaucrats to make such an assessment of this claim with any accuracy. The ability of a firm to "game" the system may be particularly high for larger firms who can increase employment at subsidized plants at the expense of employment in unsubsidized plants that did not receive RSA.

II. MODELLING THE EFFECTS OF AN INVESTMENT SUBSIDY

II.A Effects of the RSA policy on Capital investment

What are the likely effects of RSA on investment and employment in an eligible area? 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 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 2 illustrates the possible program effect by assuming that the level of the capital stock of a firm is determined from the intersection of capital demand (a downward sloping marginal revenue productivity of capital curve, MRPK) and a horizontal tax-adjusted user cost of capital (the supply of funds curve). Without any subsidy, the cost of capital is ρ_1 and a firm's capital stock is K_1. The RSA program reduces the effective cost of capital to ρ_2 and capital rises to K_2.

As discussed above, RSA attempts to target marginal investments. If only marginal capital projects obtain funding, the change in the capital stock is $\Delta K = K_2 - K_1$ at a taxpayer cost of $(K_2 - K_1)(\rho_1 - \rho_2)$. More realistically, the government has imperfect monitoring ability and so will achieve a lower increase in capital as some of the costs are diverted to funding infra-marginal investments that the firm would have made even in the absence of government intervention. The extreme case is where the government has zero monitoring ability and the firm simply accepts the subsidy for its infra-marginal investments. The level of 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[9] and can effectively lend out any excess subsidies at this market rate. It is likely that the government's monitoring problem is particularly severe for large firms which will typically be conducting many different types of investments, and an outside agency will have difficulty in assessing whether any grant is truly additional or not.

Now consider a world with imperfect capital markets such that we have a hierarchy of finance model (e.g. Bond and Van Reenen, 2007). Here a firm may be financially constrained if it must externally finance investment from debt or equity rather than relying on internal funds. In this case, the cost of capital/supply of funds curve is not horizontal as in Panel A, but becomes upward sloping when firms need external finance. This is illustrated in Panel B of Figure 2 where we consider two firms indicated by different MRPK curves. A financially unconstrained firm has a schedule "MRPK (unconstrained)" which intersects the flat part of the supply of funds curve, and can finance all investments from internal funds. By contrast, a financially constrained firm has schedule "MRPK (constrained)" and has to rely in part on more expensive external funds. An identical subsidy will generate more investment from the financially constrained firm than from the unconstrained firm.[10] This is illustrated in Panel B of Figure 2 ($\Delta K' > \Delta K$) and can be seen

---

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

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

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 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 £164m per year in our sample period (see Table A2), which constitutes only a tiny fraction (0.065%) of total UK investment.[11] Consequently, although there may be general equilibrium effects on asset prices and wages (e.g. Glaeser and Gottlieb, 2008) these are unlikely to be large. Nevertheless, since there may be some equilibrium price effects in local areas we also examine the effect of 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 positive effects 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 and Identification Strategy

Our basic approach is to estimate the policy effects at a small-scale geographical area level. We also present results at a higher level – the travel to work areas (TTWAs) and lower levels of aggregation (plant and firm-level) which will help us assess some of the mechanisms around spillover effects (do jobs just get displaced from other areas?) and intensive vs. extensive margins of adjustment (i.e. incumbent expansion vs. net entry). There are 10,737 wards and 322 TTWAs in our dataset covering the whole of Great Britain (England, Wales and Scotland). Since the eligibility varies at the ward level and the number of wards is stable over time the ward is a natural unit of observation to focus on. We write the relationship of interest as:

$$y_{rt} = \lambda_1 NGE_{rt} + \eta_r + \tau_t + v_{rt} \tag{2}$$

Where NGE_{rt} is the key policy variable and is defined as the maximum investment subsidy available in ward area r in year t and ranges from zero to 35%. The main outcome, y_{rt}, we examine is employment – a variable which is available at all levels of aggregation. However, we

[11] For example, RSA expenditure as 0.065% of total investment in 2004. Table A2 shows RSA grants in aggregate of £148.5m compared to £227bn spent in gross fixed capital formation (ONS, 2014).

also examine unemployment and entry/exit at the area level.[12] Unemployment is useful to examine as we can assess whether increased employment is coming from other sectors (like services) or by drawing in people who were previously not working. The η_r is an area fixed effect, τ_t are time dummies and v_{rt} is an error term.

A concern with estimating equation (2) is that NGE_{rt} could be endogenous if areas are selected into the policy because they have experienced negative shocks. As discussed in Section I, the level of maximum investment subsidy in an area depends on cross-EU rules. The areas which experienced a change in eligibility, as detailed in Table A1, may have done so because of unobserved contemporaneous changes in the area that are correlated with our outcome variables.

To see this formally, denote eligibility in 2000 (and afterwards) as a discrete variable $S_{r,00}$ and similarly eligibility for the 1993-2000 period is $S_{r,93}$. So $S_{r,\tau} = 1$ if the area is eligible in period $\tau = (93,00)$ and zero otherwise. The EU rules are explicit that eligibility in 2000 depends on a vector of area characteristics such as unemployment and per capita GDP relative to the EU average. The EU also explicitly gives the period over which these data are dated which is from 1998 and earlier due to lags in data collection. Similarly, the policy states the (lagged) characteristics used to define eligibility in 1993 (which were dated 1991 and earlier). Some of the characteristics determining 1993 eligibility were the same as 2000 and some were not. We define the superset of all the area characteristics relevant in 1993 and 2000 as $X_{r,\tau}$. Therefore, the *propensity* of an area to be in 2000 can be written as:

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

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

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

Now consider the change in the propensity to be eligible:

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

The change in eligibility will depend on the changes in the policy parameters $(\theta_{00} - \theta_{93})$ and changes in area characteristics, $(X_{r,00} - X_{r,93})$. An obvious concern is that those areas that were

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

declining may be more likely to become eligible for the policy and hence more likely to have worse outcomes. Consequently, we construct instrumental variables based solely on $(\theta_{00} - \theta_{93})X_{r,93}$, the leading term in equation (5) instead of the actual change in eligibility which is a function of $(X_{r,00} - X_{r,93})$. These are "synthetic instrumental variables" in the spirit of Gruber and Saez (2002) that should be purged of any suspected bias as they are constructed based solely on the rule changes and not changes in area characteristics. Note that since our main estimates are "within groups" where we estimate in levels with a full set of area dummies rather than first differences, the IV for NGE in equation (2) is $z_{r,t} = (\theta_{00} - \theta_{93})X_{r,93}$ if $t \geq 2000$ and zero otherwise.

We present many tests of the validity of the IV strategy including running placebos on pre-policy periods. One tough test we implement is to estimate the reduced form of equation (2) in long-differences, but then condition on all the lagged levels of variables in the vector $X_{r,93}$ so the IV is identified purely from the interaction terms. We will show that our results are robust even to this experiment.

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

A second issue is that the maximum subsidy varies across the eligible areas (see Figure 1). For example, pre-2000 there were "Tier 1" areas with an NGE of 30% and "Tier 2" areas with an NGE of 20%. To deal with this, we estimate the policy parameters by performing ordered probit models[13] separately for 1993 (three grouped outcomes) and 2000 (six grouped outcomes[14]). We

[13] Nothing hinges on the particular distributional assumptions of the ordered probit. We have qualitatively similar results using ordered logits, OLS, etc. Appendix B discusses these alternatives.
[14] The six maximum subsidy rates after 2000 are 0 for ineligible areas, 10%, 15%, 20% and 30% for "Tier 2" areas and 35% for "Tier 1" areas.

use the $X_{r,93}$ observables for both ordered probits. From the $\hat{\theta}_{\tau}$ we calculate the probability that an area will be in each subsidy regime in both pre and post 2000 periods. We then multiply these probabilities by the NGE in each regime to calculate an *expected* subsidy level for an area based on pre-1993 characteristics and the policy parameters. This is the IV used which varies by area and across the policy change solely due to the policy parameters. Estimating by ordered probit also means that all probabilities are bounded between zero and one, which is not the case for OLS regression versions of (3) and (4).

Although the maximum investment subsidy (NGE) is an attractive treatment variable as it is the main EU policy variable, an alternative specification to equation (2) is to use the subsidy actually paid out to a firm. The advantage of using the actual subsidy is that it that it is more easily interpreted as "increasing the amount of dollar subsidies by 10% is associated with an increase of employment of y%". The disadvantage of using RSA is that we do not know the exact timing of when the subsidies are paid after the first year of receipt. For these reasons, we present both NGE eligibility and RSA participation as treatment indicators in different tables of the empirical results.

The 2000 map was based on electoral "wards" that are similar in population size to a US zip code. Our eligibility instrument is defined, therefore, at the ward level and we cluster standard errors at this level or higher. As an extension, we also estimate at a higher level (TTWA) to investigate cross-area spillover effects and disaggregate to the plant/firm level to look at treatment heterogeneity. We discuss econometric issues when we come to these results.

IV. DATA AND ESTIMATING POLICY RULES

IV.A Datasets

Details on data 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).[15]

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 amount of subsidy and

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

first date of payment.[16] We match program participants with data from the population IDBR that includes addresses, industry, ownership and employment. The lowest level of data is at the business site level. The lowest level of aggregation we consider are all business sites of a particular firm in a ward that we refer to as a "plant". This is because the unique business site identifier at the more disaggregated level is not always reliable.

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

The ONS draws a stratified random sample of firms from the population of plants in the IDBR to form the ARD (Annual Respondents Database) which is a mandatory survey. Information in the ARD is gathered at a higher level of aggregation than a plant called the "reporting unit" which we refer to as a "firm". Overall, 80% of firms are single-plant and located in a single mailing address. The ARD does not consist of the complete population of all UK manufacturing firms, since the sample is stratified with smaller businesses sampled randomly. However, it does contain the population of larger businesses, which cover 90% of total UK manufacturing employment. From the ARD we obtain information on investment, wages, and productivity of firms.

IV.B Descriptive Statistics

Table A1 reports the number of areas 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 maximum investment subsidy rate. In 2000, 4,048 wards (out of 10,737) changed their maximum subsidy rate. Of these switchers, 1,424 areas and 14,967 plants had an increase and 2,624 areas and 35,953 plants had a decrease.[17]

Aggregate expenditure on the program was about £164m per year over our sample period, and since 2001 has been generally declining over time (see Table A2). On average, 28% of all

[16] 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.

[17] By way of comparison, in 1993 only 1,893 wards changed status, with 1,034 enjoying an increase and 859 suffering a reduction. These changes in area eligibility affected 14,369 plants who saw their maximum subsidy increase, and 8,856 plants who had a decrease in their maximum subsidy rate.

British wards are eligible for RSA accounting for 39% of manufacturing employment and 30% of manufacturing plants. Although, on average, only 3% of plants in eligible areas receive a new RSA grant in a given year, 18% of manufacturing employees have worked in a plant who received RSA at some point over our sample.

We report some descriptive statistics in Table 1. Areas eligible for subsidies have more unemployment and a higher number of manufacturing workers than other areas. For example, in the 1997-1999 period the average ward had 267 manufacturing workers (Panel A) compared to 351 in those areas eligible for RSA (Panel B). The average subsidy of a plant receiving a grant was just over £56,000 per year in the 1990s and just under £36,000 in the 2000s (Panel B). In 2000-2004, an average plant has 20 employees (Panel C), although plants in eligible areas tend to be larger (27 employees). Panel D compares *firms* in eligible areas who receive subsidies with those who do not. Recipient firms are larger (87 vs. 31 workers), have 2.7% (= 0.042 - 0.015) lower TFP and 14% lower labor productivity (£38,600 vs. £44,900 value added per worker). Since the RSA program, like many industrial policies, targets larger and less productive firms, naïve OLS analyses are likely to underestimate any potential positive effects (Rodrik, 2007). Below we examine differences between plants belonging to larger firms (e.g. more than 50 employees) compared to smaller plants (e.g. 50 employees or less) and show that treatment effects are larger for the smaller firms.

IV.C Estimating the Policy Rules

The estimates used to construct the policy rule are presented in columns (1) and (2) of Table 2 (Table A3 has definitions and descriptive statistics). Note that three of the variables used as indicators in 2000 by policy-makers were not used in 1993 (the employment rate, the ILO unemployment rate and the share of manufacturing workers), so we set the coefficients on these variables to be zero. Similarly, there were five indicators that were used in 1993 but not in 2000: the share of high skilled workers, the start-up rate, the structural unemployment rate, the activity rate and the long-run unemployment rate. Again, we set the coefficients on these variables to be zero.

Looking across Table 2, the signs are generally intuitive. Areas with lower GDP per capita, low population density, fewer skilled workers, low start-up rates, high structural unemployment rates, low activity rates and low employment rates are all significantly more likely to be eligible for higher subsidies. The only surprises are that the claimant count unemployment rate (in 1993) and the ILO unemployment rate (in 2000) take counter-intuitive negative signs. This seems to be due to co-linearity among the many unemployment measures. In 1993 (2000) there are four (three) labor market indicators that are all highly correlated. We illustrate the co-linearity issue by estimating similar regressions with fewer unemployment variables. For 1993, when we drop structural unemployment, the results in column (3) show that the coefficient on the current unemployment rate takes its expected positive sign. For the period from 2000 onwards, column (4) shows that the coefficient on ILO unemployment also takes the expected positive sign when we drop the claimant count unemployment rate.[18]

As discussed in sub-section IIIA we use the results from columns (1) and (2) to construct our IV for the policy: the predicted level of maximum investment subsidy based on pre-1993 area characteristics. The distribution of the level of the IV is shown in Panel A of Figure A1, and the change is in Panel B. There is a mass point close to zero in both levels and changes as most areas have a very low probability of being treated and this does not change over time (as in the actual data). However, the IV has positive mass over the entire support of the NGE distribution both in levels and in changes. Panel B shows an asymmetry with more areas predicted to lose eligibility than gain it, consistent with the actual change in eligibility reported in Table A1.

V. RESULTS

V.A Main Area Level (Ward) Results

We first examine the area level results. Recall that our identification strategy uses exogenous policy rule changes that determine which wards are “randomized in” to be eligible (or ineligible) for support. We begin with unemployment counts because this is an overall measure of local performance, before moving to employment, which is the immediate target of the RSA policy. Figure 3 shows changes in unemployment for areas whose support levels were predicted to *increase* because of the policy rule change in 2000 – i.e. a discrete version of our instrumental variable - compared to areas where predicted support levels *decreased*.[19] The 1997-2004 period was one of strong growth in the UK economy and unemployment was falling across the country (although there was a minor effect of the DotCom crash in 2001 as the rate of improvement slowed). It is clear though that there is a significantly faster fall in unemployment in the areas

[18] We checked that these co-linearity issues are not spuriously driving our key findings. We constructed the rule change instruments using the ordered probit estimates in Table 2 where exclude the problematic variables – i.e. current and residual unemployment rates - in all periods. Our results are robust to using these experiments. For example, the coefficient (standard error) on the IV estimates in the unemployment equation became -0.495(0.094) compared to -0.318(0.064) in the baseline estimates of column (4) in Table 2, Panel A. We also obtain similar results when basing the instruments on ordered probits that included all indicators used in the either the 1993 or 2000 period. For example, the IV coefficient (standard error) becomes -0.595(0.057) in the same unemployment equation.

[19] There are no areas where predicted support levels stay precisely constant because the probabilities are continuous.

which where exogenously more likely to become eligible for investment subsidies after 2000 (dashed line). By 2004, these areas enjoyed falls in unemployment about 10% faster than elsewhere. By contrast, prior to 2000 the falls in unemployment were statistically identical across the two groups of areas. Figure 4 reports the same results for employment. Since this is for manufacturing, a sector in long-run decline, both lines are on a downward trend, but again there is no sign of significant differential trends prior to the 2000 policy change. The figure clearly suggests that employment fell significantly less in areas where predicted eligibility for investment subsidies increased after 2000 compared to those areas where predicted eligibility fell.[20]

Table 3 reports the first area-level regression results. Panel A contains results for unemployment. In column (1), we report fixed effect regressions using the area's maximum investment subsidy (NGE) as the main explanatory variable. There is a negative and significant correlation with unemployment: increasing the available investment subsidy by 10 percentage points is associated with a 1.7% reduction in unemployment. Because of endogeneity concerns column (2) presents the reduced form regression using our instrument – exogenous changes eligibility for subsidies using the change in EU wide policy parameters. There is the expected negative and significant sign on the IV as suggested by Figure 3. Column (3) reports the first stage regression with NGE as the outcome and shows that this is strongly predicted by our IV. The final column reports the IV results suggesting that the causal effect of RSA is actually about twice as larger as the OLS estimate of column (1). A 10 percentage point increase in support leads to a 3.2% fall in unemployment. This is consistent with what we would expect: a positive shock to an area decreases the probability of it becoming eligible for investment subsidies, so OLS underestimates the unemployment reducing effects of the policy.

In Panel B of Table 2 we present identical specifications, but use ln(employment) as the dependent variable. This is of independent interest as unemployment falls could in principle be into non-participation instead of jobs. In fact, the employment results are consistent with the unemployment regressions, suggesting that the policy is drawing more people into work in the affected areas. In Panel C we look at the extensive margin by using the number of plants as outcomes. Does the increase in jobs come from incumbents expanding or is part of it from higher net entry? The results suggest that at least part of the employment effect comes through increasing

[20] The standard errors are larger than in Figure 3 because we focus on employment in a sub-sample for where we also have micro-data (the universe of firms in the manufacturing sector).

the number of plants. For example, column (4) of Panel C implies that a 10 percentage point increase in investment subsidy eligibility causes a 2.2% increase in the number of plants.

Table 4 reports the same set of regressions except that we use the ln(amount of subsidies spent in an area) as our main variable of interest. Hence, we can interpret the estimated coefficient as the elasticity of the outcome with respect to subsidy payments. We obtain qualitatively similar results to Table 3. For example, the final column in Panel B suggests that a 10% increase in subsidy spending leads to a 0.6% fall in unemployment, a 1.6% increase in jobs and a 0.4% increase in the number of plants.

VB. Area level (Ward) Robustness Tests

We conducted a large number of robustness tests, some of which we detail here. First, although Figures 3 and 4 do not suggest any spurious differential pre-2000 trends we also ran placebo tests where we introduced "pseudo policies" of the same form as RSA in the pre-2000 period. These were always insignificant. For example, we estimated the employment reduced form on the 1995-2000 data but used the post-2000 policy instruments as if they were introduced in 1997. The IV has a coefficient (standard error) of 0.125(0.123) compared to 0.713(0.174) in the main specification in column (2) of Table 3 Panel B (see Table A5).

Second, we estimated the regressions over a longer time period (from 1986 to 2004) which includes the policy change in 1993 as well as the one we use in 2000. Unfortunately, detailed information on the construction of the policy rules for the period before 1993 does not exist so we cannot construct the rule change instruments. Hence, we only run regressions of outcome variables on *actual* NGE support levels.[21] We find coefficients implying that an increase of support intensity by 10 percentage points leads to a growth of 2.8% more jobs and 0.8% more plants. This is somewhat higher, but not significantly different from the earlier results using only the 2000 rule change (see Table A6).

Third, we were concerned that there could be other policies conflating our main results. The only significant policy was EU "structural funds" which support infrastructure projects like roads and bridges.[22] The map of supported areas also changed in 2000 and these are also focused

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

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

on disadvantaged areas. Fortunately, the areas that are eligible for structural funds are not all the same as the ones eligible for RSA. To address this issue, we re-estimated our regressions excluding all the 561 areas that had a change in their eligibility for structural funds after 2000. The results are robust to this experiment. For example, for this sub-sample the coefficients (standard errors) on the policy change IV in the reduced form of column (2) of Table 3 were -0.158(0.058), 0.759(0.182) and 0.179(0.075) for unemployment, employment and number of plants respectively.[23]

Fourth, we examined trimming the sample on a common support; i.e. we exclude observations that fall into the extremes of the distribution of employment and unemployment across wards. We successively drop larger bands from the edges of the distribution (1%, 5%, and 10%). None of this has much effect on the estimates (last six columns of Table A6).

Fifth, instead of estimating our models by including a full set of areas dummies (i.e. within groups) we controlled for the fixed effects by estimating differences. This leads to qualitatively similar results (see odd columns of Table A7). Importantly, the differenced specification also allows us easily to implement a very tough test of our identification strategy. We can estimate the equations of interest including the lagged *levels* of the area characteristics that were used to compute the rule change instrumental variable (in Table 2). The identification is solely from the non-linear interaction between the weights on the rules and lagged area characteristics. This addresses concerns that past (pre-1993) values of these characteristics might still be correlated with trends in our outcome variables over the year 2000 change. Our results suggest that this is not an issue. These additional controls have little effect on our results (even numbered columns of Table A7).

Finally, there are several issues with the appropriate treatment of the standard errors. Since the instruments are generated regressors (from Table 2), formally we should allow for this in the calculation of the variance-covariance matrix. Doing so, however, made very little difference to the results as shown, for example, in Table A4. Consequently, we keep to the simpler method of simply clustering standard errors at the Ward level in most of the paper. A more conservative approach would be to allow for more spatial autocorrelation using. We tackle this in the next sub-section by considering estimates at a higher level of aggregation.

---

[23] If we estimate on the full sample and include the EU structural fund eligibility dummy the coefficients are significant and take their expected signs. The signs and significance on the coefficients of RSA policy instruments continue to be the same, however.

VC. Higher levels of aggregation (TTWA)

Comparison of the policy effects at the ward level and the more aggregate "Travel to Work Areas" (TTWA) level enables us to examine spillover effects across areas.[24] When an area becomes eligible for investment subsidies 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:

$$y_{at} = \mu NGE_{at} + \eta_a + v_{at} \tag{6}$$

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 (6), 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 program simply causes shifting between areas (as Wilson, 2009, suggests for R&D tax credits across American states) the coefficient of NGE in equation (6) will be zero (μ=0) for two symmetric wards.

To construct TTWA level measures of investment subsidies we take weighted averages of the ward-level investment subsidies.[25] If there are significant negative spillovers, estimates of policy effects at the TTWA level should be lower than at the disaggregated ward level. We replicate all the results from Table 3 at this higher level in Table 5. The qualitative results are similar and there is no evidence of any under-estimation of the results in Table 3. For example, the IV results in column (4) of Table 4 suggest that a 10 percentage point increase in support exposure of a TTWA leads to an 11% increase in employment (compared to 8.3% increase in our ward level regressions). Hence, in both cases the effect is, if anything, *stronger* than the ward level effect,

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

[25] The weights are based on 1996 employment levels to mitigate any endogeneity concerns.

thus rejecting the hypothesis of negative effects on neighboring areas. It may actually be that revitalizing one area helps strengthen neighbors.

VD. Plant level Results

We also present results disaggregated to the plant-level for two reasons. First, it will reveal if the ward level effects found earlier are solely due to extensive margin effects – the net entry effect we showed in Panel C of Table 2 or if they are also driven by intensive margin effects (existing establishments expanding their employment). Secondly, it allows us to examine if there is heterogeneity in the response of different types of firms to the policy. 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 plant sample as credit constraints or the gaming of the system depends on the size of the firm, not the plant per se (e.g. a ten worker factory owned by General Electric still benefits from GE's deep pockets). In addition, to mitigate endogeneity biases we use the firm's employment level in 1996 - the year before our estimation period (for firms born after 1996 we use size in the first year and drop this observation from the estimation).

Since we observe the population of manufacturing plants we can estimate for plant *i* the equation:

$$y_{it} = \alpha NGE_{rt} + \eta_i + \tau_t + v_{it} \tag{7}$$

This is the plant level analog of the area level equation (2). 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.[26]

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.[27] 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 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:

$$y_{jt} = \alpha NGE_{jt} + \beta X_{jt} + \eta_j + \tau_t + v_{jt} \tag{8}$$

[26] 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.
[27] We call this the firm level, *j*, but there could be many reporting units in one large firm.

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

Table 6 presents ln(employment) regressions on the support intensity of the area where a plant is located. The results are qualitatively similar to those at the ward level in Table 2. Column (4) of Panel A implies that an increase of NGE by 10 percentage points leads to a 4.2% increase in plant level employment. This is about half of the figure found at the ward-level suggesting that both the intensive and extensive margin contribute to the ward level results. We also find a larger difference between the OLS and IV coefficients that is consistent with stronger selection effects at the plant level. It is not just the weaker areas that get help, but the struggling plants within the areas that are more likely to be selected for assistance. Hence, the OLS is a severe underestimate of the positive treatment effect at the plant level (indeed the OLS coefficient is not even significant).

In Panels B and C of Table 6 we report regressions for plant level employment separately for small and large firms (firm level employment of less or more than 50 in 1996). The first stages are strong for both types of firms. The IV effect are positive and significant for plants in small firms but insignificant and under a third of the size for plants in big firms.[28] This implies that plants that are part of small firms drove the aggregate area effect identified in Table 2.

There could be at least two different reasons for the heterogeneity of the policy effect by firm size. Firstly, although large firms are often based in areas that receive support – hence the highly significant first stage in column (3) – the size of their grants is less generous. Alternatively, they are equally well supported, but the subsidies generate less jobs. Table 7 explores this by reporting regressions of employment on actual RSA support. We show results using both a dummy variable for any support (participation: "*RSA* > 0") and the amount of subsidy ("RSA subsidy"). Panels A and B perform this exercise pooled across plants from all firms and show that regardless of whether we use a dummy or continuous treatment indicator there is a large and significant positive effect of receiving investment subsidies when we estimate by IV and small effects when we estimate by OLS. Panels C and D repeat the exercise for plants from small firms (under 50 employees) and show that we obtain even stronger IV results. For example, in the final column of Panel D, employment of a plant in a small firm grew by 1.5% if it received 10% more investment subsidy. By contrast, in Panel F, a plant in a large firm would grow by only (a statistically insignificant)

[28] The effects are also significantly different at 10% level for large firms vs. small firms.

0.2% if it received 10% more subsidies.[29] Note that first stage effects in column (2) for plants in large firms is highly significant – exposure to a higher support eligibility area matters. In fact, the coefficient on the IV in the first stage for plants in large firms is almost twice as large as that for plants in small firms (e.g. 0.454 vs. 0.237 in column (2) Panel E vs. C). Hence, these results are consistent with our second hypothesis - large firms receive equivalent subsidies but this support does not lead to as many jobs.[30]

What could explain the different treatment effects between small and large firms? One possibility is that small firms might be (more) financially constrained than larger ones. With asymmetric information between borrower and lender, young firms will be at a disadvantage because the credit markets will have less time observe their performance. Recent evidence, however, stresses that although there is a correlation between youth and size, many small firms are not young (Haltiwanger et al, 2013). A simple test of the credit constraint hypothesis is to interact the treatment effects with firm age since younger firms are more likely to be subject to credit constraints. We run reduced form employment regressions where we include the linear rule change IV as well as two interactions between the instrument and (i) whether the firms is small and (ii) whether the firms is young. The interaction between the instrument and size is always significant and positive whereas the interaction between the instrument and being young is actually always significantly *negative* and this finding is robust to the exact measure of being young.[31] Since young firms respond *less* to the policy, the bigger program effect for small firms does not seem consistent with a financial constraints story.

An alternative explanation of these results is that large firms might have more scope to "game" the system; i.e. receive the subsidy without actually being constraint by the requirements of the program to create jobs. For instance, they might have more scope pretend to create jobs while

[29] Could it be the case that despite smaller magnitudes the jobs effects for big firms are still larger as the smaller percentage effect applies to a larger employment base? We explore this by calculating the marginal effect of a £ of subsidy on the number of jobs. Denote γ as the elasticity between jobs (L) and subsidies received (ϕ). Since $\gamma = \frac{\partial \ln \mathrm{L}}{\partial \ln \phi}$ the marginal effect is: $\frac{\partial \mathrm{L}}{\partial \phi} = \gamma \frac{L}{\phi}$. Since we have estimated γ in in column (3) of Table 7 separately by small (Panel D) and large (Panel F) firms, we can evaluate the marginal effect at the empirical mean subsidy and sizes for large and small firms separately. Small firms had on average 18 employees and £29,450 of subsidies whereas large firms had 211 employees and £107,190 subsidy. When we substitute these values in Table A8 we find that the marginal effect on jobs for £1,000 of subsidy remains over twice as big for small firms (0.090) than for large firms (0.035).

[30] In Table A9 we vary the exact definition of a "small" firm and show our results are robust.

[31] For example, in Tale A10 we included interaction between the IV and firms under 50 employees in the employment reduced form regression of Panel A column (2) of Table 6 which had a coefficient(standard error of 0.118(0.046). We then included a further interaction between the IV and firms who have only been alive for two years or less in this specification. It took a coefficient (standard error) of -0.081(0.007) and the size interaction remained positive and significant.

actually reducing employment in another location of the business.[32] Although we do not have direct evidence of this, this explanation is consistent with the pattern of results described above.

VE. Firm level Results: Employment, Capital and Productivity

We report regressions at the *firm* level in Table 8, motivated by two considerations. First, it could be the case that the nationwide effect is zero if multi-plant firms are simply switching jobs within the firm across eligible and ineligible areas. Secondly, there is richer administrative data at the firm level including output, capital and materials for a stratified random subsample of firms - the ARD. Panel A reports employment regressions at the firm level using the IDBR population and showing that these are very similar to the plant level results, suggesting that within firm re-allocation is not a major issue. In the other panels we use the ARD sub-sample that has information on other outcomes such as investment. In Panel B we report results for employment estimated using the ARD sub-sample and confirm our earlier finding of a positive causal impact on jobs. In Panel C we find larger impacts on capital investment than we did for employment (although these results are slightly less significant) consistent with the simple theory model in Section II. Panel D shows that there is also an impact on output. Finally, Panel E uses a Solow residual based TFP measure and finds no significant effect of the policy (for more details see Appendix C). We looked at a variety of other methods of calculating TFP, but in no case do we find a significant impact on productivity (see Table A11). There was no significant program effects on wages.[33]

Motivated by the theory in Section II – suggesting that more capital-intensive firms are more response to the policy - we interacted the treatment effect with a dummy for whether the firm had a high level of capital costs in revenue prior to the 2000 policy change. Consistent with the model firms where the capital share was high (big s_K) had stronger positive employment effects. As shown in Table A13 (a generalization of column (2) in Table 7 Panel B) the interaction of the rule change IV and a dummy for high capital share firms had a coefficient(standard error) of 0.461(0.209) in the employment reduced form.

V.F Magnitudes

The most straightforward way to consider the overall magnitude of the RSA policy effect is to use the area-level reduced forms. The treatment effect from column (2) of Table 3 Panel B is 0.713

[32] Recall that absent the requirement to create or safeguard jobs the RSA is effectively a subsidy to capital and might reduce optimal employment depending on the elasticity of substitution between labor and capital.

[33] For example, when we replaced the dependent variable by wages in the reduced form of column (2) the coefficient on the policy rule IV was 0.113 with a standard error of 0.099.

indicating that a 10 percentage point NGE investment subsidy would increase area-level manufacturing employment by 7.13%. Table A2 shows that there were about 1.04m workers in manufacturing in eligible areas over the sample period 1997-2004 and the mean level of the maximum investment subsidy is 23.8%. We estimate that about 176,000 extra jobs were created because of the policy (0.713*0.238*1.04m). The nominal average annual cost of RSA was about £164m. Using official estimates of administrative costs (17% of the aggregate grant value)[34] and a deadweight cost of taxation of 50%, this implies a total annual cost of £288m, or a "cost per job" of £1,636 (=288/0.176) or $2,533. If we took the more conservative OLS estimates from column (1) which has a treatment effect of 0.169, the cost per job would be £6,885 (or $10,656). Since there do not appear to be large substitution effects from neighboring non-eligible areas, these do not need to be scaled down.[35]

The cost per job is higher than for 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 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 this selective place-based industrial policy than the existing literature.

V.G. Big Push: Asymmetries of subsidy removal?

Recent work on place-based policies have emphasized that their long-run success depends on whether there are be dynamic effects (e.g. Kline and Moretti, 2014a). Is continued support needed in order to achieve lasting gains in employment or can a "big push" move an area into a new equilibrium were employment gains continue even after the subsidy has been removed? We found no evidence for the big push hypothesis in our data for manufacturing employment and net entry,

[34] 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 (2005). We believe this is because no other study has exploited the exogenous changes in RSA eligibility to deal with the downward endogeneity bias.

[35] 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.

nor for total unemployment. In one experiment, we defined a series of dummy variable for the amount and length of time that an area had received RSA support and interacted these with our treatment effects, but there was no significant heterogeneity in this dimension.[36]

We also tried differentiating between areas that experienced an increase compared to a decrease in investment subsides 2000. We found that areas which lost subsidies had a significant decrease in jobs and that the magnitude of this job loss was broadly the same regardless of whether the area had benefited from being eligible for high subsidy levels for many years in the past. The effects of a reduction in a big push area is not significantly different from a reduction in a non-big push area.[37]

The absence of dynamic effects could be because the magnitude of the RSA policy is much less intense than the Tennessee Value Authority studied by Kline and Moretti (2014b) - it does not include infrastructure, for example. Nevertheless, our evidence does not seem supportive of the view that support of regions through these type of policies is likely to be transformational.

VI. CONCLUSIONS

There are surprisingly few micro-econometric analyses of the causal effects of business support policies, despite their ubiquity across the world. In this paper, we have examined one business support policy – Regional Selective Assistance (RSA). We use exogenous changes in the eligibility of areas to receive investment subsidies driven by 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 jobs in the eligible areas and on employment and investment for treated firms. We also find that the program effects are strong for smaller firms but effectively zero for larger firms. This is consistent with large firms being able to "game" the system and/or financial constraints being unimportant for these firms (although we do not find much

[36] For example, we created a dummy variable equal to one if an area received the maximum investment subsidy rate (NGE=30%) continuously between 1986 and 1999 (and zero otherwise) and interacted this with our rule change instrument. When included in the employment regression of column (3) of Table A7 (alongside the linear dummy), this interaction variable had an insignificant coefficient (standard error) of 0.486 (0.429).

[37] For example, we ran regressions of the form: $\Delta \ln EMP_{rt} = \beta_1 (BP_r * [I\{\Delta z_r \leq 0\} \times \Delta z_r]) + \beta_2 (I\{\Delta z_r \leq 0\} * \Delta z_r) + \beta_3 (BP_r * [I\{\Delta z_t > 0\} * \Delta z_r]) + \beta_4 (I\{\Delta z_r > 0\} * \Delta z_r) + \beta_5 BP_r + \alpha_t + \epsilon_{rt}$ where $\Delta \ln EMP_{rt} = \ln EMP_{rt} - \ln EMP_{r1997}$, BP = 1 if an area had maximum support (NGE=30%) for the years 1986-1999 and zero otherwise, $I\{\Delta z_r \leq 0\}$ is an indicator variable equal to one if the instrument falls in value – i.e. an area is predicted to have a decrease in subsidy levels for exogenous reasons. The test of whether removing support from (i) an area which has been supported for many years vs. (ii) other types of areas that have been less generously supported is: $\beta_1 + \beta_2 = \beta_3 + \beta_4$. We could not reject the hypothesis that the effects were identical between "Big Push" areas and all others – the F-statistic was 1.41 with a p-value of 0.23.

evidence for this latter hypothesis). Interestingly, this stronger effect of business support policies on smaller firms is found in many other studies.[38] The fact that the treatment effect is confined to smaller firms strengthens arguments for restricting subsidies that go to 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. Finally, we find no effects on (total factor) productivity. From a policy perspective, the fact that the subsidies were effective in raising employment and investment in these deprived areas at a modest "cost per job" should be regarded as a positive outcome. Although measured aggregate productivity falls as the RSA supported firms were on average less productive, this probably carries a modest welfare cost compared to the counterfactual where these employees enter unemployment (rather than be reallocated to firms that are more productive).

In terms of future work, an important 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, 2014b).

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.

Becker, Sascha O., Peter Egger and Maximilian von Ehrlich, M. (2010), "Going NUTS: The Effect of EU Structural Funds on Regional Performance", *Journal of Public Economics* 94 (9–10): 578–90.

Becker, Sascha O., Peter Egger and Maximilian von Ehrlich, M. (2012a), "Absorptive Capacity and the Growth Effects of Regional Transfers: a Regression Discontinuity Design with Heterogeneous Treatment Effects", forthcoming *American Economic Journal: Economic Policy*.

Becker, Sascha O., Peter Egger and Maximilian von Ehrlich, M. (2012b), "Too Much of a Good Thing? On the Growth Effects of the EU's Regional Policy", *European Economic Review* 56 (4): 648–68.

[38] For example, Howell (2015) and Wallsten (2000) for the US, González et al (2005) for Spain, Lach (2002) for Israel, and Bronzini and Iachini (2014) for Italy and Gorg and Strobl (2007) for Ireland.

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* 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 (2014) “Are Incentives for R&D Effective? Evidence from a Regression Discontinuity Approach” *American Economic Journal: Economic Policy*, 6(4), 100-134.

Busso, Mattias, Jesse Gregory and Patrick Kline (2013) “Do Local empowerment programs work? Evidence from the Federal Empowerment Zone program”, *American Economic Review*, 103, 897-947.

Cerqua, Augusto and Pellegrini, Guido (2014) “Do subsidies to private capital boost firms' growth? A multiple regression discontinuity design approach”, *Journal of Public Economics*, 109(C), 114-126.

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 Report, http://cep.lse.ac.uk/textonly/_new/research/productivity/finalreportxxElseDTIs2.pdf

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.

Einio, Elias and Henry Overman (2015) “The (Displacement) Effects of Spatially Targeted Enterprise Initiatives: Evidence from UK LEGI”, mimeo LSE.

Einio, Elias (2014) “R&D subsidies and company performance” *Review of Economics and Statistics*, 96(4), 710-728.

Felix, R. Alison and James Hines (2013) “Who offers tax-based development incentives?” *Journal of Urban Economics*, 75(C), 80-91

Fowkes, Rigmor, Joao Sousa, and Neil Duncan (2015). “Evaluation of research and development tax credit.” HMRC Working Paper No. 17.

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

Glaeser, Edward and Joshua Gottlieb (2008) “The Economics of Place-Making Policies”, *Brookings Papers on Economic Activity*, 39(1) 155-253.

Gobillon, Laurent, Thierry Magnac and Harris Selod (2012) “Do Unemployed Workers Benefit from Enterprise Zones: the French experience” *Journal of Public Economics*, 96(9-10), 881-892.

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.
Gruber, Jonathan and Emmanuel Saez (2002) "The elasticity of taxable income: evidence and implications" *Journal of Public Economics*, 84, 1-32.
Haltiwanger, John, Ron Jarmin and Javier Miranda (2013) "Who Creates Jobs? Small vs. Large vs. Young" *Review of Economics and Statistics* 95(2), 347-361
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.
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.
Howell, Sabrina (2015) "Financing Constraints as a Barrier to Innovation", NYU mimeo
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.
Jaffe, Adam and Trinh Le (2015) "The Impact of an R&D subsidy on innovation: A study of New Zealand firms" NBER Working Paper 21479.
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
Kline, Pat and Enrico Moretti (2014a) "People, Places, and Public Policy: Some Simple Welfare Economics of Local Economic Development Policies" *Annual Review of Economics*, 6, 629-662.
Kline, Pat and Enrico Moretti (2014b) "Local Economic Development, Agglomeration Economies, and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority *Quarterly Journal of Economics*, 129, 275-331.
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 (2012) "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, Florian Mayneris and L. Py (2013) "The Impact of Urban Enterprise Zones on Establishment Location Decisions - Evidence from French ZFUs". Banque de France Working Paper 458

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*, MHSO: London.

Neumark, David and Helen Simpson (2014) "Place-Based Policies", NBER Working Paper 20049.

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

ONS (2014) "Investment - impact analysis of changes to the estimation of gross fixed capital formation and business investment for Blue Book 2014", London: HMSO.

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.

Tuomas Takalo, Tanja Tanayama and Otto Toivanen (2013) "Estimating the Benefits of Targeted R&D Subsidies" *Review of Economics and Statistics*, 95(1), 255-272.

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.

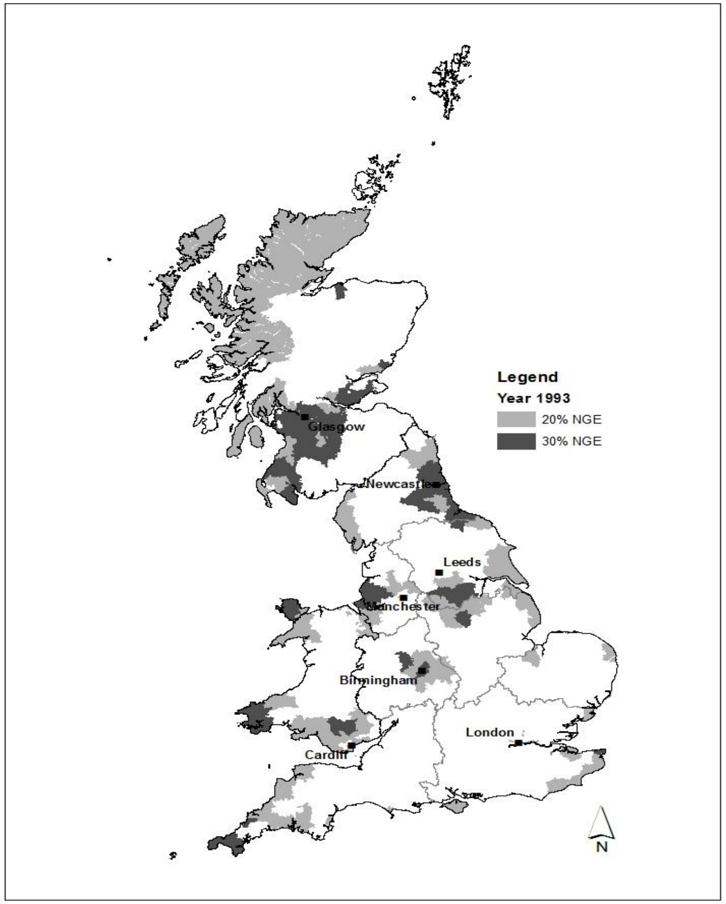
Wallsten, Scott (2000) "The effects of government-industry R&D programs on private R&D: the case of the Small Business Innovation Research program" *RAND Journal of Economics*, 31, 82-100.

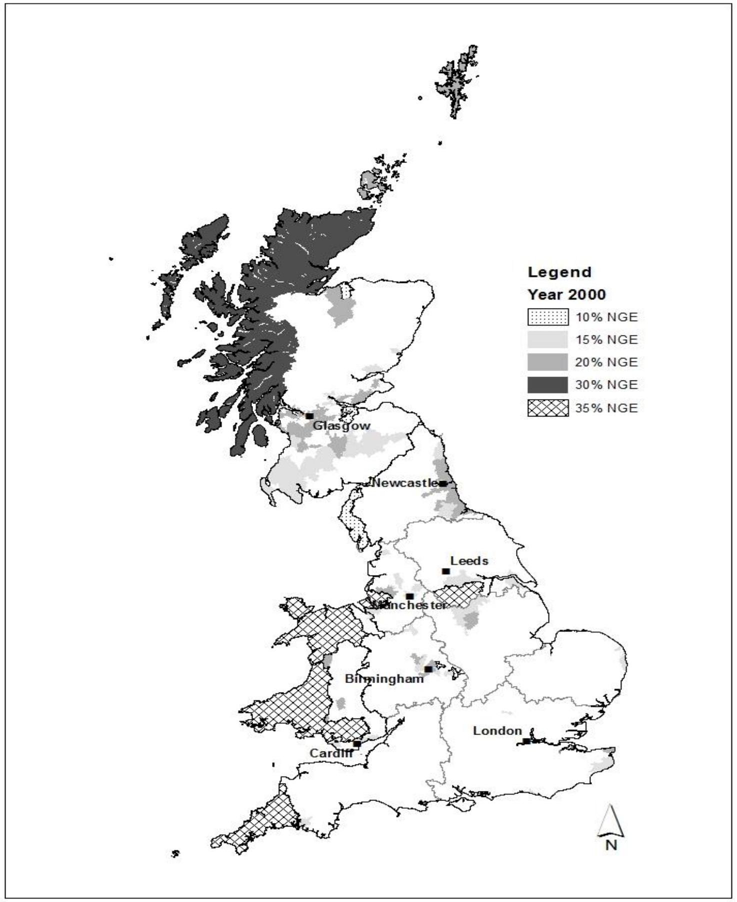
Wilson, Daniel. (2009) "Beggar thy Neighbor? The In-State, Out-of-State and Aggregate Effects of R&D Tax Credits" *Review of Economics and Statistics*, 91(2), 431-436.

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.

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

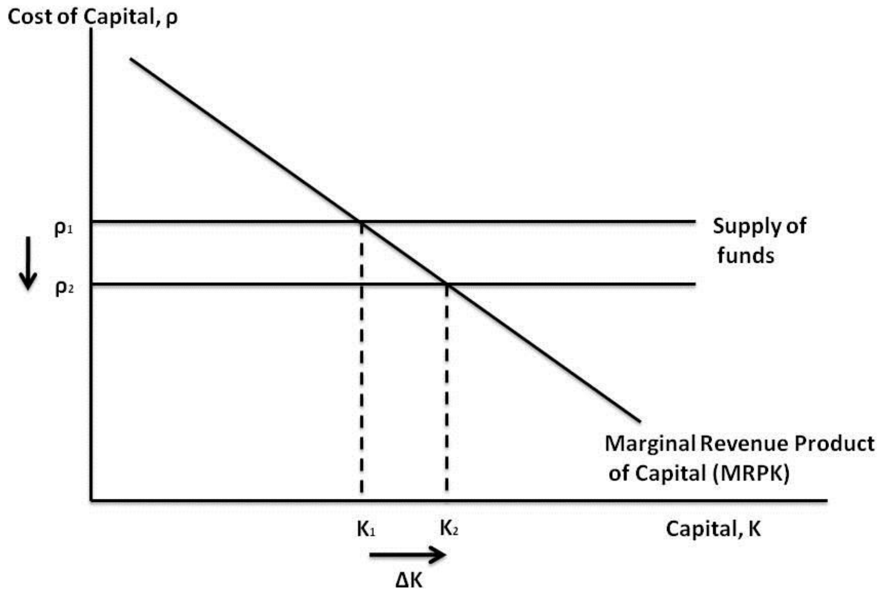




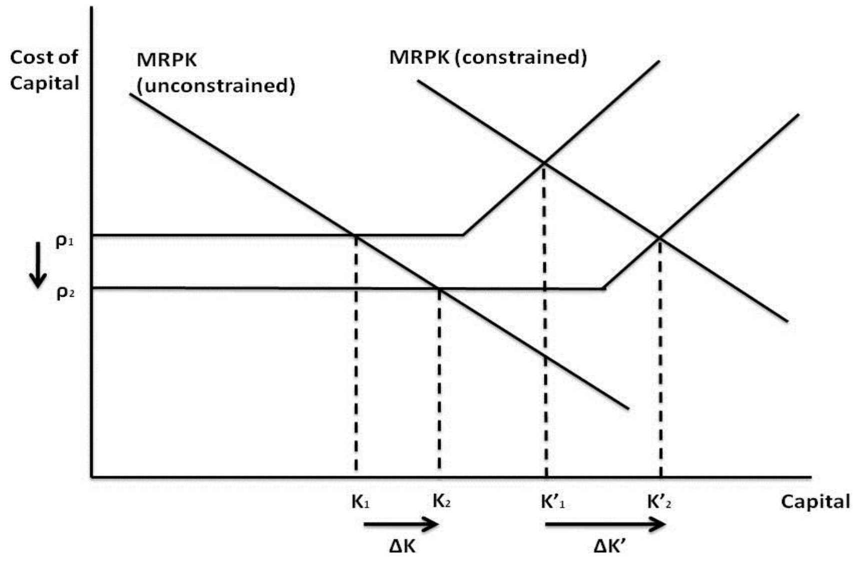
Notes: The shaded areas are those that are eligible for some Regional Selective Assistance. In the 1984-1993 period, the dark shaded areas are the very deprived areas eligible for an investment subsidy of up to 30% NGE (Net Grant Equivalent, the maximum investment subsidy). The light shaded areas are eligible for up to 20% NGE. After 2000 Tier 1 areas had 35% NGE and Tier 2 areas ranged between 10% and 35%.
Source: Department of Business, Innovation and Skills "Industrial Development Reports", various years.

Figure 2: Effects of the RSA policy on capital

Panel A – Perfect Capital Markets

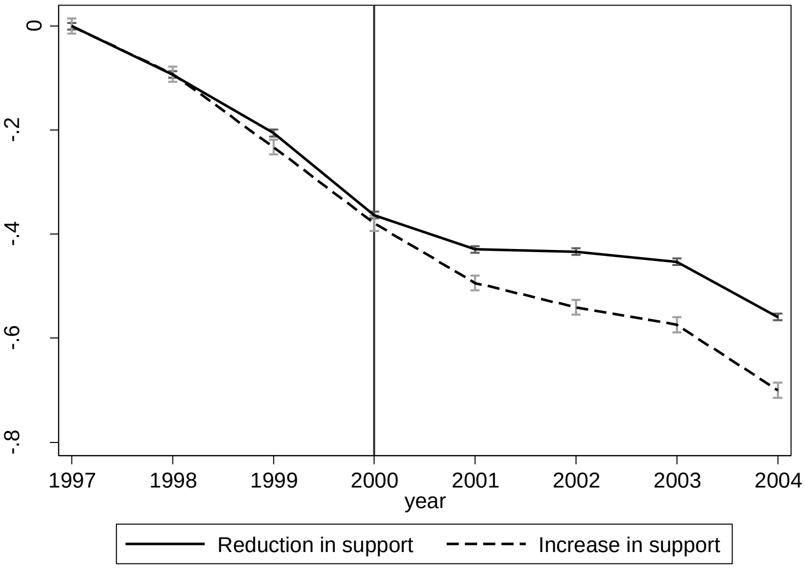


Panel B – Imperfect Capital Markets



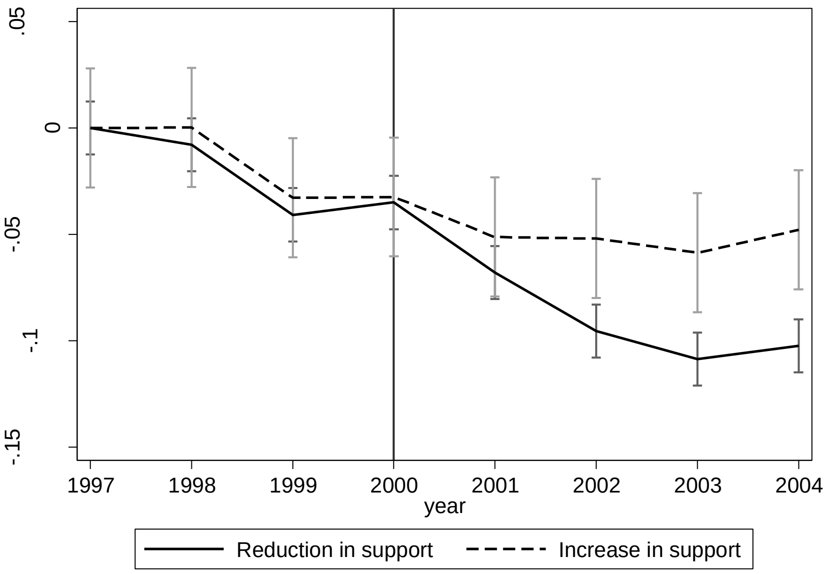
Notes: These figures examine the theoretical effect of the RSA policy reducing the cost of capital with perfect capital markets (Panel A) and imperfect capital markets (Panel B). For affected firms this is likely to raise capital, but the extent to which it does so will depend on a variety of factors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

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



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

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



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

Table 1 Descriptive Statistics

	Years	Mean	Std. Dev.	Obs.	#Units
Panel A. All areas					
NGE (Maximum Investment Subsidy %)	97-99	0.1	0.1	32,211	10,737
	00-04	0.1	0.1	53,685	10,737
Average RSA Payment (£)	97-99	18,265.2	197,955	32,211	10,737
	00-04	9,837.1	134,036	53,685	10,737
Total Unemployment (claimant count)	97-99	113.5	150.6	32,211	10,737
	00-04	80.9	112.5	53,685	10,737
Manufacturing Employment	97-99	267.4	626.2	32,211	10,737
	00-04	233.0	535.9	53,685	10,737
Panel B. Areas Eligible for RSA subsidies					
NGE (Maximum Investment Subsidy %)	97-99	0.2413	0.0492	10,284	3,428
	00-04	0.2367	0.08896	14,040	2,808
Average RSA Payment (£)	97-99	56,132.2	346,827	10,284	3,428
	00-04	35,712.8	260,039	14,040	2,808
Total Unemployment (claimant count)	97-99	161.85	179.04	10,284	3,428
	00-04	123.85	147.95	14,040	2,808
Manufacturing Employment	97-99	350.96	823.91	10,284	3,428
	00-04	338.10	745.40	14,040	2,808
Panel C. Plant –Level					
Plant employment across all areas	97-99	21.2	102.30	406,615	167,415
	00-04	19.8	91.90	631,089	183,061
Plant employment across eligible areas	97-99	27.4	139.00	131,431	53,575
	00-04	26.8	125.85	176,902	50,926
Panel D. Characteristics of recipients and non-recipients in eligible areas (£), Firms					
Employment of recipients	97-04	87.2	323.4	16,413	4,550
Employment of non- recipients	97-04	31,3	199.6	188,899	39.308
Investment of recipients	97-04	1,717.0	105,686	3048	1,488
Investment of non- recipients	97-04	953.3	8,001	15,314	7,449
(Value added/ worker) - recipients	97-04	38.6	25.8	3048	1,488
(Value added/worker) non recipients	97-04	44.9	231.51	15,314	7,449
TFP of recipients (Indexed to industry × year average in logs)	97-04	-.042	0.371	3,048	1,488
TFP of non- recipients (Indexed to industry × year average in logs)	97-04	-.015	0.414	15,314	7,449

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

Table 2: Estimates of parameters on eligibility rule changes

	(1)	(2)	(3)	(4)
	Main specification		Restricted variables	
Year	**1993**	**2000**	**1993**	**2000**
Dependent Variable: level of NGE ordered variable				
GDP per capita	-0.022***	-0.047***	-0.034***	-0.058***
	(0.002)	(0.003)	(0.002)	(0.003)
Population density	-0.028***	-0.042***	-0.043***	-0.013***
	(0.002)	(0.002)	(0.002)	(0.002)
Share of high skilled workers	-0.586***		-0.910***	
	(0.139)		(0.137)	
Start-up rate	-2.483***		-0.570***	
	(0.194)		(0.174)	
Structural unemployment rate	83.079***			
	(2.354)			
Activity rate	-1.129***		-1.219***	
	(0.257)		(0.236)	
Employment rate		-9.134***		-12.108***
		(0.462)		(0.447)
Current unemployment rate	-9.163***	62.192***	83.867***	
(claimant count)	(3.209)	(2.244)	(2.030)	
ILO unemployment rate		-6.364***		0.257
		(0.847)		(0.798)
Long-duration unemployment	0.421		5.436***	
Rate	(1.246)		(1.167)	
Share of manufacturing workers		-0.263		-0.518***
		(0.193)		(0.197)
Observations (wards)	10,737	10,737	10,737	10,737
Cut-off 10%		-8.662***		-13.517***
		(0.436)		(0.324)
Cut-off 15%		-8.590***		-13.452***
		(0.438)		(0.326)
Cut-off 20%	0.000	-8.135***	0.163	-13.038***
	(0.226)	(0.437)	(0.218)	(0.323)
Cut-off 30%	1.198***	-7.484***	1.359***	-12.451***
	(0.226)	(0.433)	(0.217)	(0.317)
Cut-off 35%		-7.352***		-12.327***
		(0.433)		(0.316)
Log Likelihood	-5,534.825	-7,098.669	-5,538.835	-7,509.456

Notes: Coefficients (robust standard errors) from Ordered Probits of NGE maximum support categories for 1993 and 2000. Dependent variable in columns (1) and (3) takes values of 1 to 3 depending on the level of NGE (zero, 20% and 30%) and in columns (2) and (4) a value of 1 to 6 (zero, 10%, 15%, 20%, 30% and 35%). See Table A3 for variable definitions. "Restricted variables" drops (co-linear) structural unemployment in column (3) and claimant unemployment in column (4).

Table 3: Area Level regressions – Instrumenting Maximum Investment Subsidies (NGE) with Rule change

Method	(1) OLS	(2) Reduced Form	(3) First Stage	(4) IV
A. Dependent variable: ln(Unemployment)				
Maximum investment subsidy	-0.169***			-0.318***
NGE	(0.020)			(0.064)
Policy Rule Instrument		-0.273***	0.859***	
		(0.055)	(0.031)	
B. Dependent variable: ln(Employment)				
Maximum investment subsidy	0.169***			0.831***
NGE	(0.057)			(0.202)
Policy Rule Instrument		0.713***	0.859***	
		(0.174)	(0.031)	
C. Dependent variable: ln(Number of Plants)				
Maximum investment subsidy	0.014			0.215***
NGE	(0.027)			(0.083)
Policy Rule Instrument		0.185***	0.859***	
		(0.071)	(0.031)	
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896

Notes: *** denotes significance at the 1% level, **5% level and *10% level. NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. All columns include a full set of area fixed effects time dummies. Standard errors below coefficients are clustered by area (ward level) in all columns. The time period is 1997-2004. Policy Rule instrument is described in text. All dependent variables in this and other tables are ln(1+Y) where Y is the raw value of the dependent variable.

Table 4:
Area Level– Instrumenting amount of subsidy with Rule change

Method	(1) OLS	(2) Reduced Form	(3) First Stage	(4) IV
A. Dependent variable: ln(Unemployment)				
ln(RSA subsidy)	-0.002**			-0.062***
	(0.001)			(0.018)
Policy Rule Instrument		-0.273***	4.418***	
		(0.055)	(0.958)	
B. Dependent variable: ln(Employment)				
ln(RSA subsidy)	0.011***			0.161***
RSA	(0.002)			(0.051)
Policy Rule Instrument		0.713***	4.418***	
		(0.174)	(0.958)	
C. Dependent variable: ln(Number of Plants)				
ln(RSA subsidy)	0.003***			0.042**
	(0.001)			(0.018)
Policy Rule Instrument		0.185**	4.418***	
		(0.071)	(0.958)	
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896

Notes: *** denotes significance at the 1% level, **5% level and *10% level. Ln(subsidy), *RSA*, is the ln(1+total subsidies) in an area. All columns include a full set of area fixed effects time dummies. Standard errors below coefficients are clustered by area (ward level) in all columns. The time period is 1997-2004. Policy Rule instrument is described in text. These are the same specifications as in Table 3, but estimated at a higher level of aggregation (TTWA).

Table 5: Higher levels of aggregation - Travel to Work Area

Method	(1) OLS	(2) Reduced Form	(3) First Stage	(4) IV
A. Dependent variable: ln(Unemployment)				
ln(Max. investment subsidy)	-0.301***			-1.082***
NGE	(0.054)			(0.373)
Policy Rule Instrument		-0.433***	0.401***	
		(0.136)	(0.125)	
B. Dependent variable: ln(Employment)				
ln(Max. investment subsidy)	0.461***			1.105***
NGE	(0.089)			(0.386)
Policy Rule Instrument		0.443***	0.401***	
		(0.163)	(0.125)	
C. Dependent variable: ln(Number of Plants)				
ln(Max. investment subsidy)	0.061			0.236
NGE	(0.047)			(0.181)
Policy Rule Instrument		0.095	0.401***	
		(0.072)	(0.125)	
Number of areas (wards)	322	322	322	322
Observations	2,576	2,576	2,576	2,576

Notes: *** denotes significance at the 1% level, **5% level and *10% level. NGE ("Net Grant Equivalent") is the level of the employment weighted average maximum investment subsidy rate in the area. All columns include a full set of area fixed effects and time dummies. Standard errors below coefficients are clustered by area (TTWA level) in all columns. The time period is 1997-2004. Policy Rule instrument is described in text.

Table 6: Plant Level regressions of ln(employment)

	(1)	(2)	(3)	(4)
Method	OLS	Reduced Form	First Stage	IV
A. Pooled across all plants, 792,091 observations on 139,796 plants				
ln(Maximum investment subsidy)	0.021			0.416***
NGE	(0.021)			(0.073)
Policy Rule Instrument		0.282***	0.676***	
		(0.049)	(0.034)	
B. Small (Plants in Firm with under 50 employees), 720,151 observations on 126,508 plants				
ln(Maximum investment subsidy)	0.011			0.343***
NGE	(0.023)			(0.079)
Policy Rule Instrument		0.232***	0.678***	
		(0.053)	(0.035)	
C. Large Plants (Plants in Firms with over 50 employees), 71,940 observations on 13,288 plants				
ln(Maximum investment subsidy)	0.027			0.098
NGE	(0.044)			(0.155)
Policy Rule Instrument		0.064	0.661***	
		(0.103)	(0.043)	

Notes: *** denotes significance at the 1% level, **5% level and *10% level. NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. All columns include a full set of area fixed effects time dummies. Standard errors below coefficients are clustered by area (ward level) in all columns. The time period is 1997-2004. Policy Rule instrument is described in text.

Table 7: Is absence of policy effect on plants in large firms because they receive less subsidies?

	(1)	(2)	(3)
Method	OLS	First Stage	IV
Sample: Pooled across all plants, 792,091 observations on 139,796 plants			
A. Pooled, Dummy for subsidy receipt			
Receiving any subsidy?	0.006		1.034***
RSA>0	(0.009)		(0.219)
Policy Rule Instrument		0.272***	
		(0.035)	
B. Pooled, subsidy amount			
ln(subsidy)	0.002*		0.148***
RSA	(0.001)		(0.037)
Policy Rule Instrument		1.896***	
		(0.367)	
Sample: Small (Plants in Firm with under 50 employees), 720,151 obs on 126,508 plants			
C. Small, Dummy for subsidy receipt			
Receiving any subsidy?	-0.016		0.982***
RSA>0	(0.011)		(0.259)
Policy Rule Instrument		0.237***	
		(0.033)	
D. Small, subsidy amount			
Ln(subsidy)	0.000		0.148***
RSA	(0.001)		(0.046)
Policy Rule Instrument		1.567***	
		(0.347)	
Sample: Large (Plants in Firm with over 50 employees), 71,940 obs on 13,288 plants			
E. Large, Dummy for subsidy receipt			
Receiving any subsidy?	0.035**		0.142
RSA>0	(0.015)		(0.226)
Policy Rule Instrument		0.454***	
		(0.121)	
F. Large, subsidy amount			
ln(subsidy)	0.003**		0.018
RSA	(0.001)		(0.029)
Policy Rule Instrument		3.548***	
		(1.374)	

Notes: *** denotes significance at the 1% level, **5% level and *10% level. Each cell is from a different regression and each of the eight panels is a different sample and measure of the treatment effect. All columns include a full set of area fixed effects time dummies. Standard errors below coefficients are clustered by area (ward level) in all columns. The time period is 1997-2004. Policy Rule instrument is described in text. "Receiving any subsidy" (RSA>0) is a dummy switched on when the firm begins receiving an investment subsidy and ln(subsidy), RSA, is the log of (1+the amount of subsidy received).

Table 8: Firm Level – Effects on jobs, investment, output and TFP. Instrumenting maximum investment subsidy with Rule change

	(1)	(2)	(3)	(4)
Method	OLS	Reduced Form	First Stage	IV
A. Dependent variable: ln(Employment), Full Sample (596,994 observations, 91,593 firms)				
NGE	0.049**			0.566***
	(0.021)			(0.069)
Policy Rule Instrument		0.392***	0.693***	
		(0.048)	(0.010)	
B. Dependent variable: ln(Employment), ARD sub-sample (45,545 observations, 21,404 firms)				
NGE	0.131***			0.554***
	(0.050)			(0.151)
Policy Rule Instrument		0.412***	0.744***	
		(0.112)	(0.029)	
C. Dependent variable: ln(Capital Investment), ARD sub-sample (45,545 observations, 21,404 firms)				
NGE	0.161			1.255*
	(0.261)			(0.688)
Policy Rule Instrument		0.934*	0.744***	
		(0.512)	(0.029)	
D. Dependent variable: ln(Output), ARD sub-sample (45,545 observations, 21,404 firms)				
NGE	0.051			0.450***
	(0.055)			(0.159)
Policy Rule Instrument		0.335***	0.744***	
		(0.117)	(0.029)	
E. Dependent variable: ln(TFP), ARD sub-sample (45,545 observations, 21,404 firms)				
NGE	-0.014			-0.072
	(0.029)			(0.073)
Policy Rule Instrument		-0.054	0.744***	
		(0.055)	(0.029)	

Notes: *** denotes significance at the 1% level, **5% level and *10% level. Policy Rule instrument is described in text. “Receiving any subsidy” is a dummy switched on when the firm begins receiving an investment subsidy. All columns include a full set of area fixed effects time dummies. Standard errors below coefficients are clustered by area (ward level) in all columns. The time period is 1997-2004. The Policy Rule instrument is described in text. TFP is computed using a “factor share” method and relative to an industry × year average (see Appendix C).

ONLINE APPENDICES: NOT INTENDED FOR PUBLICATION

APPENDIX A: MORE DETAILS OF THE RSA POLICY

During the period of our study, Regional Selective Assistance (RSA) was the main business support scheme in the UK.[1] Since the early 1970s, RSA provided discretionary grants to firms in disadvantaged regions typically characterized by relatively low levels of per capita GDP and high unemployment ("Assisted Areas").[2] 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, European law except in certain cases prohibits this type of assistance. 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.[3] There is an upper threshold of support called the Net Grant Equivalent (NGE)[4] that essentially sets a maximum proportion of the firm's investment that can be subsidized by the government.

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

Changes in eligibility over time

The map of the areas eligible for RSA changes about once every seven years.[5] The maps were changed in 1984, 1993, 2000 and 2006. In the paper, we focus on the 2000 change because we could not (despite extensive investigation) discover the exact variables used in determining area eligibility in 1984 and previous years. Without this, we could not construct the rules change IV for the 1993 change, although we do show OLS results over the longer 1984-2004 period for employment. There were changes in the way that the administrative data were gathered after 2004, so we have to end our sample period in 2004 and cannot easily use the 2006 change. We begin the analysis in 1997, as unemployment data is unavailable on a consistent basis at the ward level before this year.

The map of the eligible areas is determined by using a series of quantitative indicators. The level of GDP per capita, unemployment and population density are key indicators that have been used in all years. A series of additional indicators is also used and the EU determines what these are and what years are used for their values – these are detailed in Table A3. The eligibility criteria are outlined in guidelines that are published before the

[1] 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.

[2] 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.

[3] 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.

[4] 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.

[5] Note that this happens in conjunction with the periodic revision of the Structural Funds, the EU's main policy for supporting economic development in less prosperous regions. Although the maps are different for RSA and Structural Funds, it is a potentially confounding influence we that we check as discussed in the main text (sub-section VB)

implementation of the map (in our case 1998). The UK government will then gather quantitative information on indicators at the relevant area level and will propose a new map that has to be approved by the EU. The changes before and after 2000 is shown in Figure 1 and Criscuolo et al (2006) discuss 2-4 shows the map changes at other points in time .

(a) The 1993 change

The assisted area map for RSA was re-drawn in 1993 based on the 1988 guidelines using "Travel to Work Areas" as the underlying spatial units.[6] 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 fell from eight in 1993 to six in 2000. The most disadvantaged areas were re-named "Tier 1" - Cornwall and the Isles of Scilly, Merseyside, South Yorkshire and West Wales and the Valleys. The maximum investment subsidy allowed in these areas was 35% NGE. "Tier 2" areas were more scattered are were constructed based on groups of electoral wards.[7] 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 Tier 2 areas, that were geographically distant and sparsely populated, a maximum subsidy rate of 30% NGE was allowed.[8] The maximum NGE level for relatively less deprived areas was 10%. 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 (1997-2004), RSA targeted manufacturing sectors. The grants were discretionary and firms could only apply if the supported project satisfied the following criteria. (a) *Location*: The project had to be undertaken in an Assisted Area. (b) *Investment*: It had to involve capital expenditure on property, plant or machinery; (c) *Jobs*: It should normally have been expected to lead to the creation of new employment or directly protect jobs of existing workers which would otherwise have been lost; (d) *Viability*: The project should be viable and should help the business become more competitive; (e) *Need*: The applicant had to demonstrate that assistance was necessary for the project to proceed as envisaged in terms of nature, scale, timing or location;[9] (f) *Prior Commitments*: As RSA could only be offered when the project could not proceed without it, BIS must have completed its appraisal and issued a formal offer of assistance before the applicant entered into any commitment to proceed with the project; (g) *Other Funding*: The greater part of the funding for the project should be met by the applicant or other sources in the private sector. Note that location, which forms the basis for our instrumental variables, is objective, clearly defined and enforceable.

The process for application was as follows. Firms completed an application form, in which they needed to prove additionality, to provide business plans, accounts and reasons for wanting the grant. They then submitted this to the local office of the Department of Business, Innovation and Skills (BIS). During the period analyzed, the lag between submission and decision was normally between 35 and 60 days form standard grants, and 100 days or more for grants above £2 million. The lag depended on the amount applied for, the time needed to ensure that all of the criteria were met and on negotiations between the government agency and the firm. If the application was successful, the firm was paid the minimum necessary to get the project going. Additional payments started only after jobs were created/safeguarded and capital expenditure defrayed and were based on agreed targets. The payments were given in instalments – between two and seven and usually spread across more than one financial

[6] Travel to Work Areas (TTWA) 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 that of everyone working in the area; at least 75% actually live in the area.

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

[8] 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 area's population were in these areas).

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

year. The government agency monitored the project with visits (normally one per year, but more frequently for risky projects).

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

As noted in the main text, to deal with the issue that areas may be endogenously selected into being eligible for investment subsidies we use an instrument based on the probability that an area is assigned, based solely on the EU wide rule changes rather than changing area characteristics.

There are two practical issues in implementing this IV. First, although the EU reveals what is in the *X* vector determining eligibility for different subsidy levels, it does not reveal the exact policy parameters that determine eligibility.

A second issue is that the maximum subsidy differs in the eligible areas according to the severity of disadvantage. For example, after 2000 an area could fall into several categories with a maximum support share of 10%, 15%, 20%, 30% or 35% percent. Before 2000, there were two maximum support categories: 20% and 30%.

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

To construct instruments that are only driven by changes in the rules rather than changes in area conditions during we run two ordered probit regressions for the post and pre 2000 periods. Our vector of area characteristics $X_{r,93}$ includes all variables that were used by the EU for deciding about support status in the pre-2000 and post-2000 periods. Recall, however, we only estimate using values of the X variables dated prior to 1993 as used in 1993 rule change (in fact, given the lag structure used by the EU, the most recently dated value is 1988). This is because using values dated after 1993 could potentially be endogenous (recall equation (5) in the main text). This makes the estimates of the policy rule less precise, but so long as there is still some power of the first stage then the instruments will be valid. Formally, the model is:

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

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

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

We report results from the estimation of the ordered probits in Table 2. The signs generally look broadly sensible (with the caveat that these are not marginal effects). Areas with higher lower per person, lower labor force participation rates, lower population densities and greater increases in long-duration unemployment are more likely to be high subsidy areas.

From these ordered probit estimates we obtain the predicted probabilities ($\hat{P}_{j,r,\tau}$) of falling into each bin in each year for each area given their observables ($X_{r,93}$) and the estimated parameters ($\hat{\theta}_\tau$). The final stage is to create the predicted level of subsidy that we will use as an IV:

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

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

This specification has the advantage that we can interpret reduced form coefficients in a similar way as regressions of the actual support status (NGE). The distribution of the levels and changes of the IV are in Figures A1 and A2.

We experimented with many other ways of constructing the instruments to make sure that nothing hinges on modelling details and our results are robust. For example, in Table A12 we report our main results using instruments constructed from predictions of Linear Probability Model of the NGE values rather than the ordered probit of NGE categories. We also estimated models using ordered logit as well as simple logit and probit specifications on the binary event of a non-zero NGE value in an area.

APPENDIX C: MORE DETAILS ON DATA, MATCHING 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).[10] The IDBR is a list of all businesses in the UK, their addresses, type of activity and ownership structure. The list is compiled using a combination of tax records, accounting information (every UK firm has to lodge some information at Companies House). The smallest unit in the IDBR is a site that contains name, address and information on the number of employees and industry. We also know the enterprise (firm) that owns the site and whether this is part of a larger group ("enterprise group). Investigation showed that some of the most micro-units (the sites identifiers) are not reliable over time; we grouped all sites of a firm in a Ward into a single "local unit" which we refer to as a "plant" in the text.

A stratified random sample of enterprises is drawn every year from the IDBR to form the sampling frame for the ABI (Annual Business Inquiry), the mandatory annual survey of UK businesses. Data from the ABI is made available to researchers in the form of the ARD (Annual Respondents Database), which provides information on output, investment, intermediate inputs, employment, wages, etc.[11] The ARD is similar to the US Annual Survey of Manufacturing (ASM) with the caveat it covers all sectors (not just manufacturing) and is at a higher level of aggregation than the plant-level ASM. Not only is the ARD a sub-sample of the population 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 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

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

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

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 robust to dropping these few cases (they were). The ARD is a strict sub-set of the IDBR, so the issues discussed above apply in the same way to this dataset.

The SAMIS database has information on 54,322 program applications and whether or not the application was successful. Applicant numbers declined in the 2000s as the total budget for RSA fell. Using name, postcode and CRN numbers, the information in BIS files was linked to the IDBR over the whole period. The matching rate was 82% over the sample period (1997-2004).

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

Firm Size Definition

In some of the analysis, we split by firm size (e.g. Table 7). To mitigate endogeneity concerns we use firm size as measured by employment in a base period, for which we choose 1996, the year before our estimation period. For all plants belonging to a firm who were not alive in 1996 we use the year of birth to determine the size class and exclude data from the first year in our regressions of employment. We also experimented with dropping post 1996 entrants, which led to very similar results.

TFP (Total Factor Productivity) measures

There are numerous ways to obtain a TFP measure, a subject of ongoing debate in the literature (see inter alia Olley and Pakes, 1996 and Ackerberg et al, 2007). The results in Panel E of Table 8 are based on a simple "factor share" method and relative to an industry by year average. We define $TFP_{it} = \tau_{it} - \bar{\tau}_{I(i)t}$ where $\tau_{it} = r_{it} - \bar{S}_{MI(i)t} m_{it} - \bar{S}_{LI(i)t} l_{it} - \left(1 - \bar{S}_{MI(i)t} - \bar{S}_{LI(i)t}\right) k_{it}$. In this expression r_{it} is ln(firm revenue) for firm i in period t, m_{it} is ln(materials), l_{it} is ln(employment) and k_{it} is ln(capital). $\bar{S}_{MI(i)t}$ is the share of materials in revenue in the four-digit industry and $\bar{S}_{LI(i)t}$ is the share of labor costs in revenue at the industry level. $\bar{\tau}_{I(i)t}$ is the average value for τ_{it} in year t in the four digit industry.

We also considered alternative ways of computing TFP (see Table A11). Firstly, we consider a "regression-based" method where we use ln(revenue) as the dependent variables and include on the right hand side in addition to treatment controls ln(labor), ln(materials) and ln(capital). Secondly, we consider a more structural production function estimation approach as proposed in Martin (2012) which takes into account firm specific variation in market power when computing TFP. This requires running the following (first stage) regression: $\Xi_{it} = \beta_k k_{it} + \rho(\Xi_{it-1} - \beta_k k_{it-1}) + \nu_{it}$ where $\Xi_{it} = \frac{r_{it} - S_{Mit}(m_{it} - k_{it}) - S_{Lit}(l_{it} - k_{it})}{S_{Mit}}$ and S_{Mit}, S_{Lit} are the variables factor shares at the firm level. From this we can estimate a productivity index as $TFPMUOMEGA = \frac{\Xi_{it} - \hat{\beta}_k k_{it}}{\hat{\beta}_k}$.

APPENDIX D: AGGREGATING ACROSS SPATIAL UNITS

We consider the aggregation from lower (wards) to higher levels area (Travel to Work Areas) as discussed in sub-section VC. For simplicity consider the set-up of a single Travel to Work Area (TTWA, denoted a) consisting of two wards r1 and r2 and 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 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. $\ln L_{r1,1} - \ln L_{r1,0} = \alpha_{r1}$. And similarly for ward r2.

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:

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

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.[12]

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

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

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

This allows us to examine the case of negative spillovers as well. Suppose the policy leads to a positive effect of λ in region 1 at the expense of a negative spillover of χ in region 2. 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:

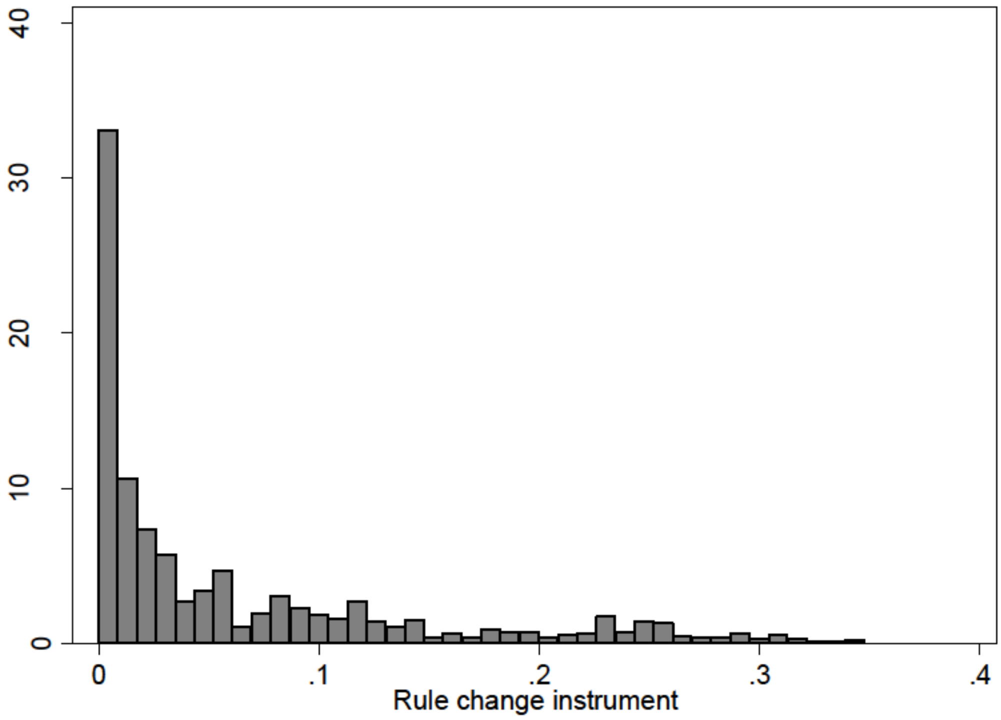
$$\alpha_r = \beta \; NGE_r$$

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 β_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. In the empirical estimates at the TTWA level in Table 5 we do indeed see that the treatment effects looks larger than at the ward level in Table 3.

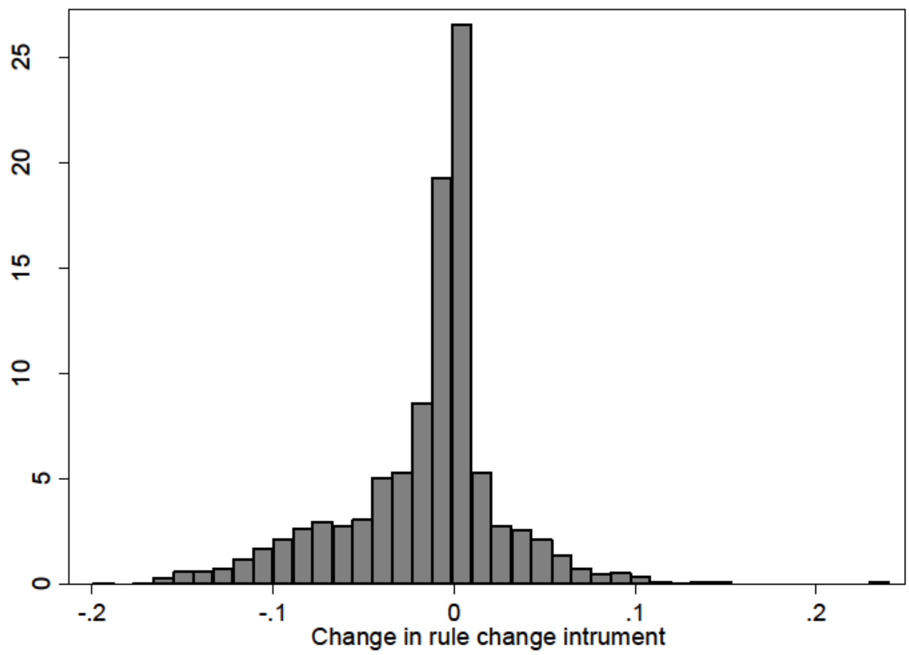
[12] Note that the two errors go in opposite directions with the first one overestimating and the second one underestimating the true figure. The second error is also likely larger so that on net we are underestimating the true figure. Simulations of the errors suggest that these are under 5%.

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

Panel A: Level of the instrument



Panel B: Change in the value of the instrumental variable



Notes: Histograms of the rule change instrument based on 10,737 wards. As described in Appendix B, this is constructed from the expected probability of being in each subsidy regime multiplied by the level of subsidy in that regime. It is constructed form the ordered probits in Table 2 from which we can calculate the probability that an area falls into a subsidy regime in all years and the actual level of NGE.

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**	**Units which changed their RSA eligibility**	**Increase in eligibility for subsidies**	**Decrease in eligibility for subsidies**
Areas (wards)					
	2000	10,737	4,048	1,424	2,624
	1993	10,737	1,893	1,034	859
Plants					
	2000	163,796	50,920	14,967	35,953
	1993	146,420	23,225	14,369	8,856

Notes: The first two rows relate to areas (wards) and the second two rows to plants. Column (2) indicates how many areas changed their eligibility status and this is divided into increases (column (3)) and decreases (column (4)). The eligibility maps changed in 1993 and 2000. The changes could be from zero or across different rates of support. In the paper we use only the 2000 change for the IV because we do not know all the criteria for the rules governing the changes before and after 1993.

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

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

Notes: Column (1) is total expenditure on RSA. Column (2) is the average NGE across the eligible wards. Column (3) is the share of wards that are eligible for RSA. Column (4) are the number of jobs in eligible areas. Column (5) is the number of plants in eligible areas. Column (6) reports the jobs in eligible areas as a fraction of all jobs. Column (7) reports the plants in eligible areas as a fraction of all plants. Column (9) is the fraction of plants in eligible areas that receive support. All data refer to manufacturing sector.

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

Table A3 - Variables that define Rules for eligibility

Variable	Definition	Timing of data used by EU for eligibility	Source	Used in which years for rules	Mean (std. dev)
GDP per capita	Value added in the area per person	1991 (for 1993); 1994-96 average (for 2000)	Eurostat	1993 and 2000	91.7 (15.9) 94.1 (28.8)
Population Density	Number of inhabitants per square km	1981 (for 1993) 1991	Census	1993 and 2000	10.4 (16.5)
Share of high Skilled workers	Share of working residents aged over 16 in high skilled occupation (SOC Groups 1 to 3)	1991	Census	1993	34 (12)
Start-Up rate	Annualized net percentage rate of growth company VAT registrations (except retail and agriculture); i.e. total registrations minus de-registrations	1987-1991	ONS Business Register	1993	2.3 (7.5)
Structural Unemployment rate	Average annual unemployment (based on ILO definition) rate over 5 years	1986-90	ONS	1993	3.5 (1.7)
Activity rate	Fraction of working age population who are economically active relative (men: 16-64; women: 16-59)	1991	Census	1993	61 (6.3)
Employment Rate	Residents in employment divided by population of working age	1992	Labor Force Survey	2000	73 (6.5)
Current Unemployment rate (Claimant Count)	Average monthly unemployment rate over year (residents claiming unemployment insurance divided by labor force)	1991 (for 1993); 1998 (for 2000)	ONS	1993 and 2000	3.9 (1.4)
ILO Unemployment Rate	Proportion of residential labor force who are "ILO" unemployed ,	1992	Labour Force Survey	2000	9.04 (3.6)
Long-duration Unemployment rate	Number claiming unemployed insurance for more than a year as a fraction of the labour force	1991	Census	1993	0.9 (1.4)
Share of manufacturing workers	Number of manufacturing employee jobs divided by total jobs	1991	Census	2000	16.9 (8.2)

Notes: These are the definitions of variables used by the EU to determine whether an area is eligible for RSA and if so, at what level of support. ILO unemployed are defined as individuals who are (i) without a job, want a job, have actively sought work in the last four weeks and are available to start work in the next two weeks, or (ii) are out of work, have found a job and are waiting to start it in the next two weeks. People who are not claimants can appear among ILO unemployed if they are not entitled to unemployment related benefits. Similarly, unemployment claimants may not appear in the LFS measure of unemployment if they state that they are not seeking, or are not available to start work.

Source: Official Journal of the European Communities (1998), OJ C 74, 10.3; and OJ C 88/C 212/02, 12.8.1988; Department of Trade and Industry (1999) "The UK Government's proposals for new Objective 2 areas" Official letter SG(2000) D/ 106293; Department of Trade and Industry (1993), "Review of the assisted areas of Great Britain. Background document on the new assisted areas map"

Table A4: Ward level - Bootstrapped standard errors to account for generated instruments

Dep. Variable:	Unemployment	Employment	Plants
Rule Change Instrument	-0.273***	0.713***	0.185***
	(0.077)	(0.178)	(0.069)
Observations	85,896	85,896	85,896
Wards	10,737	10,737	10,737

Notes: The standard errors in most of our tables ignore the fact that our rule change instrument emerges after regressing support status on various area level statistics taken into account by EU rules; i.e. as reported in Table 2. Here we provide bootstrapped results (clustered at the ward level) of the whole procedure for the reduced form estimates only. This shows that standard errors very similar to those simpler ones found in (column 2) of Table 2.

Table A5: Area Level regressions – Placebo regressions 1995-1999 of ln(Employment)

	Baseline:2000	Placebo: 1997
Years	1997-2004	1995-1999
Policy Rule Instrument	0.713***	0.125
	(0.174)	(0.123)
Observations	85,896	42,948
Wards	10,737	10,737

Notes: The policy rule instrument is constructed as if the policy change happened in 1997 in columns (2). Column (1) is baseline in 2000 when actual change took place

Table A6: Robustness of Ward Level regressions – Long time horizon (1986-2004) and Common Support

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	**ln(Employment)**	**ln(# Plants)**	**Ln(unemployment)**	**Ln(unemployment)**	**Ln(unemployment)**	**Ln(Employment)**	**Ln(Employment)**	**Ln(Employment)**
Trimming:			**1%**	**5%**	**10%**	**1%**	**5%**	**10%**
NGE	0.280***	0.079***						
	(0.071)	(0.029)						
Rules Change IV			-0.403***	-0.363***	-0.396***	0.809***	0.773***	0.844***
			(0.045)	(0.047)	(0.051)	(0.145)	(0.163)	(0.182)
Time period	1986-2004	1986-2004	1997-2004	1997-2004	1997-2004	1997-2004	1997-2004	1997-2004
Observations	204,003	204,003	82,576	70,040	55,880	82,576	70,040	55,880
#areas	10,737	10,737	10,322	8,755	6,985	10,322	8,755	6,985

Notes: *** denotes significance at the 1% level, **5% level and *10% level. Each cell is from a different regression and each of the eight panels is a different sample and measure of the treatment effect. All columns include a full set of area fixed effects and time dummies. Standard errors below coefficients are clustered by area (ward level) in all columns. The time period is 1986-2004 in columns (1) and (2) and 1997-2004 in the other columns. Policy Rule instrument is described in text. NGE is maximum investment grant subsidy. Columns (1) and (2) extend the sample back to 1986. Columns (3)-(8) trim the sample to get a closer common support; "1%" trims the sample at the lowest and top percentiles, "2%" trims from 2nd to 98th percentile, etc.

Table A7: Test of Rule change instrument (Reduced Forms)

Dependent variable:	(1) Ln(Unemployment)$_t$ - Ln(Unemployment)$_{1997}$	(2)	(3) Ln(Employment)$_t$ - Ln(Employment)$_{1997}$	(4)	(5) Ln(# Plants)$_t$ - Ln(# Plants)$_{1997}$	(6)
Rule Change IV	-0.650***	-0.569***	1.035***	0.924***	0.259***	0.226***
	(0.054)	(0.057)	(0.174)	(0.185)	(0.073)	(0.077)
GDP per person relative to EU Average		0.002***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.000)
Population density		0.013		-0.026		-0.031
		(0.023)		(0.057)		(0.026)
Share of high skilled		0.084***		0.047		0.015
		(0.021)		(0.051)		(0.023)
Start-up rate		0.267***		-0.301***		-0.124***
		(0.040)		(0.083)		(0.042)
Structural unemployment rate		2.036***		0.34		0.08
		(0.351)		(0.815)		(0.345)
Activity rate		0.215***		-0.097		-0.061
		(0.037)		(0.088)		(0.041)
Employment rate		-0.412***		0.136		0.150**
		(0.072)		(0.170)		(0.072)
Current unemployment rate (claimants)		-3.663***		-1.779		-0.369
		(0.481)		(1.106)		(0.503)
Unemployment rate (ILO)		0.006		-0.024		0.04
		(0.130)		(0.305)		(0.132)
Long-duration unemployment rate		0.546***		-0.566		-0.647***
		(0.165)		(0.493)		(0.195)
Share of manufacturing workers		0.568***		-0.421***		-0.127***
		(0.031)		(0.074)		(0.032)

Notes: *** denotes significance at the 1% level, **5% level and *10% level. These are reduced form estimates of the effects of the rule change. The dependent variables are in changes from their 1997 levels. The rule change instrument is the value of the IV after 2000 minus its pre-2000 level. The levels of the right hand variables are from Table A3. All columns include a full set of time dummies. Standard errors below coefficients are clustered by area (ward level). The time period is 1997-2004. There are 74,613 observations and 10,731 areas in all columns.

Table A8: RSA Impact in large vs small firms

	(1) Average Subsidy amount (£100s)	(2) Average number of employees	(3) Elasticity between employment and subsidies	(4) Marginal Impact of subsidy
Small Firm (under 50)	29.45	18	0.148	0.090
Large Firm (over 50)	107.19	211	0.018	0.035

Notes: The Table calculates the marginal effect of a £1,000 of subsidy on the number of jobs, split by large and small firms. Column (1) is the average subsidy received and column (2) is the average plant size from our data 1997-99. In column (3) we report the elasticity between jobs and subsidies received (γ) estimated in column (3) of Table 7 for small firms (Panel D) and large firms (panel F). Since $\gamma = \frac{\partial \ln L}{\partial \ln \phi}$ where L = employment and ϕ = subsidy, the marginal effect of a $ of subsidy on the number of jobs is: $\frac{\partial L}{\partial \phi} = \gamma \frac{L}{\phi}$. This is given in column (4). It shows that the marginal impact of subsidies on jobs is over twice as large in plants belonging to small firms than large firms.

Table A9: Alternative Size cut-offs, Plants

	(1) OLS	(2) Reduced Form	(3) First Stage	(4) IV
Panel A. Small Firm employment less than 40 in 1996 (707,745 observations; 124,467 firms; 10,279 area clusters)				
Subsidy amount	0.000			0.153***
	(0.001)			(0.050)
Policy Rule instrument		0.223***	1.462***	
		(0.054)	(0.347)	
Panel B. Large Firm employment greater than 40 in 1996 (84,346 observations; 15,329 firms; 4,677 area clusters)				
Subsidy amount	0.002*			0.031
	(0.001)			(0.026)
Policy Rule instrument		0.123	3.925***	
		(0.095)	(1.270)	
Panel C. Small Firm employment less than 60 in 1996 (728,839 observations; 127,965 firms 10,288 area clusters)				
Subsidy Amount	0.000			0.141***
	(0.001)			(0.043)
Policy Rule instrument		0.232***	1.646***	
		(0.052)	(0.349)	
Panel D. Large Firm employment greater than 60 in 1996 (63,252 observations;11,831 firms; 4,166 area clusters)				
Subsidy Amount	0.003**			0.032
	(0.001)			(0.036)
Policy Rule instrument		0.105	3.227**	
		(0.110)	(1.506)	

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

Table A10: Do small firms respond to treatment more because they are younger?

	(1)	(2)	(3)	(4)	(5)
Dependent variable: ln(employment)					
Definition of young is alive for no more than:		**One year**	**Two years**	**Three years**	**Four years**
Subsidy Amount	0.009	0.016	0.022	0.023	0.029
	(0.023)	(0.023)	(0.024)	(0.024)	(0.024)
Subsidy Amount × Small Firm	0.118***	0.125***	0.136***	0.148***	0.163***
	(0.046)	(0.047)	(0.049)	(0.051)	(0.054)
Subsidy Amount × Young firms		-0.102***	-0.081***	-0.076***	-0.085***
		(0.009)	(0.007)	(0.007)	(0.009)
Observations	618,098	618,098	618,098	618,098	618,098
Firms	95,404	95,404	95,404	95,404	95,404
Area Clusters	9,943	9,943	9,943	9,943	9,943

Notes: These are specifications equivalent to Table 6 Panel A column (2) except that we include additional interactions as specified. Column (1) is the baseline where "small" is defined as firms with less than 50 employees (as in Table 6). Columns (2) to (5) are based on different definitions of a "young" firm. Column (2) defines young to be a firm that is one year old or younger; in column (3) young = 2 years old or less, etc.

Table A11: Alternative ways of measuring firm-level productivity

	(1)	(2)	(3)
Method of measuring TFP:	**Factor Share**	**Regression**	**MU OMEGA**
Policy Rule instrument	-0.054	0.031	0.0103
	(0.055)	(0.054)	(1.377)
Observations	45,545	45,545	18,999
Firms	21,404	21,404	9,139

Notes: These are reduced form specifications corresponding to column (2) of Panel E in Table 8. **"**Factor Share" method in column (1) reproduces the results reported in Panel E of Table 8 for reference; i.e. TFP is computed using a "factor share" method and relative to an industry by year average "Regression" method in column (2) includes (the log of) labor, materials and capital as additional control variables in a specification where the dependent variable is ln(revenue). "MU OMEGA" in column (3) implements the structural production function framework proposed in Martin (2012) which takes into account firm specific variation in market power when computing TFP. The exact method of construction is in the final sub-section of Appendix C.

Table A12: Area Level regressions – Instrumenting NGE with Rule change – Simple linear regression instead of ordered probit

	(1)	(2)	(3)	(4)
Method	OLS	Reduced Form	First Stage	IV
A. Dependent variable: ln(Unemployment)				
Maximum investment subsidy	-0.169***			-0.658***
	(0.020)			(0.106)
NGE				
Policy Rule Instrument		-0.355***	0.539***	
		(0.056)	(0.026)	
B. Dependent variable: ln(Employment)				
Maximum investment subsidy	0.169***			1.671***
	(0.057)			(0.271)
NGE				
Policy Rule Instrument		0.901***	0.539***	
		(0.142)	(0.026)	
C. Dependent variable: ln(Number of Plants)				
Maximum investment subsidy)	0.014			0.413***
	(0.027)			(0.112)
NGE				
Policy Rule Instrument		0.223***	0.539***	
		(0.060)	(0.026)	
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896

Notes: *** denotes significance at the 1% level, **5% level and *10% level. NGE ("Net Grant Equivalent") is the level of the maximum investment subsidy in the area. All columns include a full set of area fixed effects time dummies. Standard errors below coefficients are clustered by area (ward level) in all columns. The time period is 1997-2004. This Table corresponds to Table 3, however, we use a slightly different version of the policy rule instrument. Instead of the ordered probit reported in Table A3, the instrument here is based on a binary Probit of the event "NGE>0". Note however, that the IV results in column 4 are very similar to those reported in Table 2.

Table A13: Firm level regressions with capital intensity interactions

Dependent variable: ln(employment)		
Rule Change	0.373***	0.195
	(0.088)	(0.121)
Rule Change × High Capital Intensity		0.346**
		(0.176)
Observations	94,937	94,937
Firms	14,790	14,790

Notes: These are specifications equivalent to column (2) of Table 8 Panel B. Capital intensity is firm level average capital to labor ratio before 2000. "High capital intensity" is a dummy equal to one if capital intensity is above the sample median and zero otherwise. Sample is smaller than in other firm level results because firms without valid observations for pre 2000 capital intensity are excluded.

CENTRE FOR ECONOMIC PERFORMANCE
Recent Discussion Papers

1112	Alex Bryson Richard Freeman Claudio Lucifora Michele Pellizzari Virginie Perotin	Paying for Performance: Incentive Pay Schemes and Employees' Financial Participation
1111	John Morrow Michael Carter	Left, Right, Left: Income and Political Dynamics in Transition Economies
1110	Javier Ortega Gregory Verdugo	Assimilation in Multilingual Cities
1109	Nicholas Bloom Christos Genakos Rafaella Sadun John Van Reenen	Management Practices Across Firms and Countries
1108	Khristian Behrens Giordano Mion Yasusada Murata Jens Südekum	Spatial Frictions
1107	Andrea Ariu Giordano Mion	Service Trade and Occupational Tasks: An Empirical Investigation
1106	Verónica Amarante Marco Manacorda Edward Miguel Andrea Vigorito	Do Cash Transfers Improve Birth Outcomes? Evidence from Matched Vital Statistics, Social Security and Program Data
1105	Thomas Sampson	Assignment Reversals: Trade, Skill Allocation and Wage Inequality
1104	Brian Bell Stephen Machin	Immigrant Enclaves and Crime
1103	Swati Dhingra	Trading Away Wide Brands for Cheap Brands
1102	Francesco Caselli Antonio Ciccone	A Note on Schooling in Development Accounting
1101	Alan Manning Barbara Petrongolo	How Local Are Labour Markets? Evidence from a Spatial Job Search Model
1100	Fabrice Defever Benedikt Heid Mario Larch	Spatial Exporters

1099	John T. Addison Alex Bryson André Pahnke Paulino Teixeira	Change and Persistence in the German Model of Collective Bargaining and Worker Representation
1098	Joan Costa-Font Mireia Jofre-Bonet	Anorexia, Body Image and Peer Effects: Evidence from a Sample of European Women
1097	Michal White Alex Bryson	HRM and Workplace Motivation: Incremental and Threshold Effects
1096	Dominique Goux Eric Maurin Barbara Petrongolo	Worktime Regulations and Spousal Labor Supply
1095	Petri Böckerman Alex Bryson Pekka Ilmakunnas	Does High Involvement Management Improve Worker Wellbeing?
1094	Olivier Marie Judit Vall Castello	Measuring the (Income) Effect of Disability Insurance Generosity on Labour Market Participation
1093	Claudia Olivetti Barbara Petrongolo	Gender Gaps Across Countries and Skills: Supply, Demand and the Industry Structure
1092	Guy Mayraz	Wishful Thinking
1091	Francesco Caselli Andrea Tesei	Resource Windfalls, Political Regimes, and Political Stability
1090	Keyu Jin Nan Li	Factor Proportions and International Business Cycles
1089	Yu-Hsiang Lei Guy Michaels	Do Giant Oilfield Discoveries Fuel Internal Armed Conflicts?
1088	Brian Bell John Van Reenen	Firm Performance and Wages: Evidence from Across the Corporate Hierarchy
1087	Amparo Castelló-Climent Ana Hidalgo-Cabrillana	The Role of Educational Quality and Quantity in the Process of Economic Development
1086	Amparo Castelló-Climent Abhiroop Mukhopadhyay	Mass Education or a Minority Well Educated Elite in the Process of Development: the Case of India

The Centre for Economic Performance Publications Unit
Tel 020 7955 7673 Fax 020 7955 7595
Email info@cep.lse.ac.uk Web site http://cep.lse.ac.uk