

## Some Causal Effects of an Industrial Policy<sup>†</sup>

By CHIARA CRISCUOLO, RALF MARTIN, HENRY G. OVERMAN,  
AND JOHN VAN REENEN\*

*We exploit changes in the area-specific eligibility criteria for a program to support jobs through investment subsidies. European rules determine whether an area is eligible for subsidies, and we construct instrumental variables for area eligibility based on parameters of these rule changes. Areas eligible for higher subsidies significantly increased jobs and reduced unemployment. A 10-percentage point increase in the maximum investment subsidy stimulates a 10 percent increase in manufacturing employment. This effect exists solely for small firms: large companies accept subsidies without increasing activity. There are positive effects on investment and employment for incumbent firms but not Total Factor Productivity. (JEL E24, G31, H25, L25, L52, R23)*

The Great Recession brought industrial policy back into fashion.<sup>1</sup> Governments around the world granted huge subsidies to private firms: most dramatically in financial services, but also in other sectors like autos. Business support policies are not new, however. Most governments offer subsidies that claim to protect jobs, reduce unemployment, and foster productivity, particularly in disadvantaged geographical

\* Criscuolo: OECD, 2, Rue André Pascal 75775 Paris Cedex 16, France, and Centre for Economic Performance (email: Chiara.CRISCUOLO@oecd.org); Martin: Imperial College Business School, South Kensington Campus, Exhibition Road, Kensington, London SW7 2AZ, United Kingdom, and Centre for Economic Performance (email: r.martin@imperial.ac.uk); Overman: Centre for Economic Performance, London School of Economics, Houghton Street, London WC2A 2AE United Kingdom (email: H.G.Overman@lse.ac.uk); Van Reenen: Department of Economics, MIT, Morris and Sophie Chang Building, E52-514, 50 Memorial Drive, Cambridge, MA 02142, and Centre for Economic Performance (email: vanreene@mit.edu). This paper was accepted to the *AER* under the guidance of Penny Goldberg, Coeditor. Helpful comments have come from anonymous referees, the editor, and 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, Energy, and Industrial Strategy for data access and Paul David, Fernando Galindo-Rueda, Pete Klenow, Enrico Moretti, Beatrice Parrish, Marjorie Roome, David Southworth, and Alex Wilson for very helpful insights. The ONS Virtual Microdata Lab ensured access to ONS Data, Alberta Criscuolo helped with the EU legislation, Cong Peng helped with maps, and Mehtap 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.

<sup>†</sup>Go to <https://doi.org/10.1257/aer.20160034> to visit the article page for additional materials and author disclosure statement(s).

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

areas. For example, the United States spends around \$40 to \$50 billion per annum on local development policies (Moretti 2011). Increasing geographical polarization has fostered social and political pressure for more place-specific policies. However, despite the ubiquity of such schemes, rigorous micro-econometric evaluations of the causal effects of these policies are rare. This is unfortunate given the mounting evidence on the persistent effect of negative economic shocks on local communities and the social and political implications of these pockets of disadvantage (e.g., Autor, Hansen, and Dorn 2016).

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

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

We reach four substantive conclusions. First, there is an economically large and statistically significant program effect: a 10 percentage point increase in an area’s rate of maximum investment subsidy causes about a 10 percent increase in manufacturing employment and a 4 percent decrease in aggregate unemployment. These effects are underestimated if endogeneity is ignored, as the areas that become eligible for the program are those which, on average, experience negative shocks and whose establishments would otherwise perform badly in the absence of the policy. This conclusion is robust to many controls including other place-based policies such as EU structural funds (for which we also develop a rules-based IV). Second, we show that these positive effects are not purely due to substitution of jobs toward eligible areas and away from neighboring (ineligible) areas. Third, we find that the

<sup>2</sup>For examples, see Krueger and Tuncer (1982), Beason and Weinstein (1996), and Lawrence and Weinstein (2001).

positive treatment effect is confined to establishments in smaller firms (e.g., with under 50 workers). We suggest that this is due to larger firms being more able to “game” the system and take the subsidy without changing their level of economic activity. Finally, there appear to be no additional effects on productivity after controlling for the program’s positive investment effects.

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

We also relate to a broader literature concerning evaluations of business support policies and place-based interventions (see Neumark and Simpson 2014 for a review). Several papers consider direct research subsidies to industrial R&D. Unlike the generally positive assessments of R&D tax credits (e.g., Fowkes, Sousa, and Duncan 2015), the evidence on these direct subsidies is mixed (e.g., the survey in Jaffe and Le 2015).<sup>4</sup> Several recent studies have used regression discontinuity designs to assess the causal effects of direct grants. For example, both Howell (2017) on the US Small Business Innovation Research program and Bronzini and Iachini (2014) on Italian data use a proposal’s application score by an independent committee as the running variable when analyzing the effects of receiving R&D subsidies. Interestingly, these studies are consistent with us in that they uncover much larger positive program effects on investment for small firms.

Our paper is not the first to look at the impact of the RSA program. Unfortunately, most of the previous evaluation studies are based on “industrial survey” techniques where senior managers at a sample of assisted firms are asked to give their subjective assessment of what the counterfactual situation would have been had they not received the grant (e.g., see National Audit Office 2003, for a survey). In contrast to the OLS approaches discussed above that are likely to underestimate positive policy effects, these survey techniques typically overestimate program impacts since firms receiving money are likely to exaggerate the scheme’s benefits. Some other studies have also used firm-level econometric techniques to evaluate the direct impact of

<sup>3</sup>Holmes (1998); Albouy (2009); and Wilson (2009) consider other place-based tax policies, while Wren and Taylor (1999); Bronzini and de Blasio (2006); Martin, Mayer, and Mayneris (2011); and Becker, Egger, and von Ehrlich (2010, 2012, 2013) provide evidence for regional policy in Europe. Gibbons, Overman, and Sarvimäki (2011); and Einiö and Overman (2015) discuss similar place-based schemes in the United Kingdom, while Gobillon, Magnac, and Selod (2012) and Mayer, Mayneris, and Py (2017) provide estimates for France and Cerqua and Pellegrini (2014) for Italy. In contrast to RSA, which targets specific firms within eligible areas, these schemes are generally not discretionary (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.

<sup>4</sup>See Takalo, Tanayama, and Toivanen (2013) or Einiö (2014) for recent contributions.

RSA.<sup>5</sup> Relative to existing studies, our contribution is to exploit a policy rule change experiment on the population of plants to identify causal effects.

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

The structure of the paper is as follows. Section I describes the policy in more detail and outlines how eligibility changes over time. Section II sets out a simple theoretical framework to help interpret the results and Section III describes the econometric modeling strategy. Section IV describes the data, Section V reports our results at the area level, and Section VI at the plant and firm level. Section VII provides conclusions. In the online Appendices we report more details on the RSA policy (online Appendix A), the changes in EU rules (online Appendix B), data details (online Appendix C), aggregation issues (online Appendix D), other regional policies (online Appendix E), further econometric results (online Appendix F), and cost per job estimates (online Appendix G).

## **I. Institutional Framework: Description of the Regional Selective Assistance (RSA) Program**

### *A. Overview*

Regional Selective Assistance started in 1972 and from the early 1980s was the main business support scheme in the United Kingdom. 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 percent of the cost of an investment project.<sup>7</sup>

Because RSA had the potential to distort competition and trade, it had to comply with European Union state aid legislation. European law, except in certain cases, prohibits this type of assistance. In particular, Article 87(3) of the Treaty of Amsterdam allows for state aid only in support of the EU’s regional development objectives. The guidelines designate very disadvantaged “Development (subsequently called Tier 1)

<sup>5</sup> For example, Devereux, Griffith, and Simpson (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.

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

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

Areas” in which higher rates of investment subsidy can be offered, and somewhat less disadvantaged “Intermediate (Tier 2) Areas” where lower subsidy rates were offered. There was an upper threshold to the investment subsidy called Net Grant Equivalent (NGE),<sup>8</sup> which sets a maximum proportion of a firm’s investment that can be subsidized by the government. These EU determined maximum NGE rates differed over time and across geographical areas.

Since the formula that determines which areas were eligible (and at which NGE rate) was set about every seven years by the European Commission for the whole of the European Union and not by the UK government, this mitigates concern of policy endogeneity. Although the overall budget for RSA is determined by the United Kingdom and not the European Union, the United Kingdom had to conform to EU rules when deciding which areas are eligible to receive RSA. Changes to area-level eligibility are driven by EU-wide policy parameters and are therefore the key form of identification in our paper.

### *B. Changes in Eligibility over Time*

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

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

This institutional setup 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 European Union, average EU GDP per capita fell, meaning some poorer UK areas were no longer eligible for subsidies from the RSA program. Third, the

<sup>8</sup>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 *Official Journal of the European Union* C74/19 10.03.1998.

Panel A. Pre-2000

Panel B. Post-2000

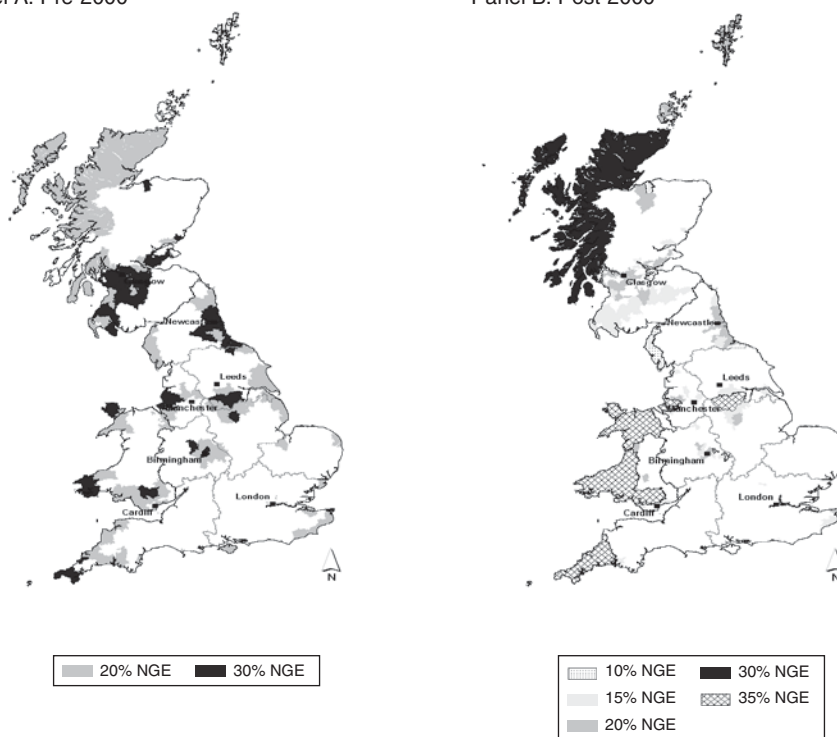


FIGURE 1. THE CHANGE IN THE LEVEL OF MAXIMUM INVESTMENT SUBSIDY (NGE) BETWEEN 1993 AND 2000

*Notes:* The shaded areas are those that are eligible for some Regional Selective Assistance. In the 1993–1999 period, the dark shaded areas are the very deprived areas eligible for an investment subsidy of up to 30 percent NGE (the maximum investment subsidy, net grant equivalent). The light shaded areas are eligible for up to 20 percent NGE (see panel A). After 2000 Tier 1 areas had 35 percent NGE and Tier 2 (see panel B) areas ranged between 10 percent and 30 percent.

*Source:* Department of Business, Innovation and Skills “Industrial Development Reports,” various years

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 predetermined as it is lagged at least two (and up to ten) years before the policy change and therefore many years prior to current outcomes. However, there may be unobservable area trends that are correlated with eligibility status and outcomes. Areas which are in long-run decline are more likely to have falling employment and output and are therefore more likely to become eligible for the program, generating a downward bias on a difference-in-differences estimate of the program effect on jobs. Alternatively, there could be a temporary negative shock. This would increase the probability of an area becoming eligible, but it would also generate an upward bias on the treatment effect as the area mean reverts (an “Ashenfelter Dip” problem). To deal with endogeneity, we focus on using only *changes* in the cross EU policy rules to construct instrumental variables for program participation and ignore all changes in area characteristics. As described more formally in Section III, we fix the area characteristics relevant for eligibility prior to the policy change and interact these with the changes in the EU-wide policy parameters.



### C. Formal Criteria for Receipt of RSA Investment Subsidies

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

## II. Modeling the Effects of an Investment Subsidy

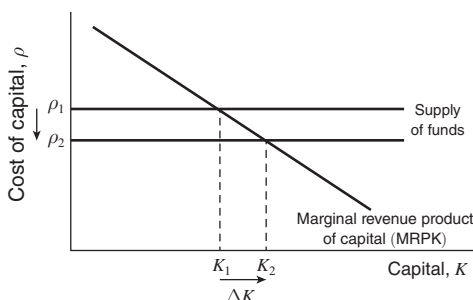
### A. Effects of the RSA Policy on Capital Investment

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

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

where  $\delta$  is the depreciation rate,  $\tau$  is the statutory corporate tax rate,  $r$  is the interest rate, and  $\theta$  is the depreciation allowance. It is clear from equation (1) that the cost of capital is falling in the generosity of the investment grant  $\left(\frac{\partial \rho}{\partial \phi} = -\frac{r}{1 - \tau} < 0\right)$ . 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  $\rho_1$  and a firm's capital stock is  $K_1$ . The RSA program reduces the effective cost of capital to  $\rho_2$  and capital rises to  $K_2$ .

Panel A. Perfect capital markets



Panel B. Imperfect capital markets

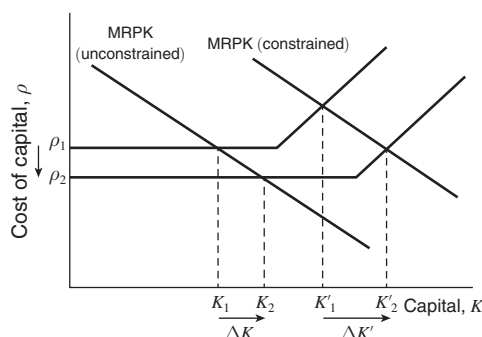


FIGURE 2. EFFECTS OF THE RSA POLICY ON CAPITAL

*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).

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 inframarginal investments that the firm would have made even in the absence of government intervention. The extreme case is where the government has zero monitoring ability and the firm simply accepts the subsidy without making any additional investment. The level of capital stays the same, but there is a direct transfer of funds from the taxpayer to shareholders. The firm will not voluntarily make investments that earn a rate of return below the outside market cost of capital<sup>9</sup> 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

<sup>9</sup>  $\text{MRPK} < \delta + \frac{r(1 - \theta\tau)}{1 - \tau}$ , i.e., the value of  $\rho$  in equation (1) when  $\phi = 0$ .



from the financially constrained firm than from the unconstrained firm.<sup>10</sup> This is illustrated in panel B of Figure 2 ( $\Delta K' > \Delta K$ ) and can be seen from considering the cross partial derivative of equation (1):  $\frac{\partial^2 \rho}{\partial \phi \partial r} = -\frac{1}{1-\tau} < 0$ . For firms facing an effective interest rate ( $r$ ) higher than the risk free rate due to financing constraints, the marginal effect of a subsidy on the cost of capital is greater and so the effect on investment is larger. If small firms are more likely to be financially constrained, this is a second reason over and above lower monitoring difficulties why the program may have a larger treatment effect on small firms. As with the case of perfect financial markets, if the government cannot target marginal investments there will be zero effect on the financially unconstrained firms.

### B. Effects of the RSA Policy on Labor

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

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

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

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

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

### C. General Equilibrium Effects

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

### D. Summary of Model

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

## III. Econometric Modeling Strategy

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

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

where  $NGE_{r,t}$  (Net Grant Equivalent) is the key policy variable and is defined as the maximum investment subsidy available in ward area  $r$  in year  $t$  and ranges from 0 to 35 percent. The main outcome,  $y_{r,t}$ , we examine is employment: a variable which is available at all levels of aggregation. However, we also examine unemployment and various other outcomes (e.g., investment, output, productivity, and entry/exit).<sup>12</sup> Unemployment is useful to examine as we can assess whether increased

<sup>11</sup> For example, RSA expenditure is 0.065 percent of total investment in 2004. Online Appendix Table A1 contains some descriptive statistics including the fact that total RSA grants were £149 million in 2004 compared to £227 billion spent in gross fixed capital formation (ONS 2014).

<sup>12</sup> Unlike the United States, the UK Office for National Statistics does not collect data on productivity, investment, and wages at the plant level. The surveys are conducted at the firm (reporting unit) level, including for

employment is coming from drawing in people who were previously not working. The  $\eta_r$  is an area fixed effect,  $\tau_t$  are time dummies, and  $v_{r,t}$  is an error term.

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

To examine this formally, denote eligibility in 2000 (and afterward) as a discrete variable  $S_{r,00}$  and similarly eligibility for the 1993–2000 period as  $S_{r,93}$ . So  $S_{r,\tau} = 1$  if the area is eligible in period  $\tau = \{93, 00\}$  and 0 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 European Union also explicitly gives the period over which these data are dated which is from 1998 and earlier due to lags in data collection. Similarly, the policy states the (lagged) characteristics used to define eligibility in 1993 (which were dated 1991 and earlier). Some of the characteristics determining 1993 eligibility were the same as 2000 and some were not (see online Appendix Table A2). We define the superset of all the area characteristics relevant in 1993 and 2000 as  $X_{r,r}$ . Therefore, the *propensity* of an area to be eligible in 2000 can be written as

$$(3) \quad S_{r,00}^* = \theta_{00} X_{r,00}.$$

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

$$(4) \quad S_{r,93}^* = \theta_{93} X_{r,93}.$$

Now consider the change in the propensity to be eligible:

$$(5) \quad \begin{aligned} S_{r,00}^* - S_{r,93}^* &= \theta_{00} X_{r,00} - \theta_{93} X_{r,93} \\ &= (\theta_{00} - \theta_{93}) X_{r,93} + (X_{r,00} - X_{r,93}) \theta_{93} + (\theta_{00} - \theta_{93}) (X_{r,00} - X_{r,93}). \end{aligned}$$

The change in eligibility will depend on the changes in the policy parameters ( $\theta_{00} - \theta_{93}$ ) and changes in area characteristics, ( $X_{r,00} - X_{r,93}$ ). An obvious concern is that those areas that were declining may be more likely to become eligible for the policy and hence more likely to have worse outcomes. Consequently, we construct instrumental variables based solely on  $\Delta \mathbf{z}_{r,t} \equiv (\theta_{00} - \theta_{93}) X_{r,93}$ , the leading term in equation (5) instead of the actual change in eligibility which is a function of ( $X_{r,00} - X_{r,93}$ ). These are “synthetic instrumental variables” in the spirit of Gruber

---

multi-plant firms. This means that we cannot accurately calculate productivity measures at very detailed geographical level (e.g., ward).

and Saez (2002) that should be purged of any suspected bias as they are constructed based solely on the rule changes and not changes in area characteristics.

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

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

A second issue is that the maximum subsidy rate varies across the eligible areas (see Figure 1). For example, pre-2000 there were Tier 1 areas with an NGE of 30 percent and Tier 2 areas with an NGE of 20 percent. To deal with this, we estimate the policy parameters by performing ordered probit models<sup>13</sup> separately for 1993 (three grouped outcomes) and 2000 (six grouped outcomes<sup>14</sup>). We use the  $X_{r,93}$  observables for both ordered probits. From the  $\hat{\theta}_\tau$  we calculate the probability that an area will be in each subsidy regime in both pre- and post-2000 periods. We then multiply these probabilities by the NGE in each regime to calculate an *expected* maximum subsidy level for an area based on pre-1993 characteristics and the policy parameters. This is the IV used which varies by area and across the policy change solely due to the policy parameters. Estimating by ordered probit also means that all probabilities are bounded between 0 and 1, which is not the case for OLS regression versions of (3) and (4).

<sup>13</sup> Nothing hinges on the particular distributional assumptions of the ordered probit. We have qualitatively similar results using ordered logits, OLS, etc. Online Appendix B discusses these alternatives.

<sup>14</sup> The 6 maximum subsidy rates after 2000 are 0 for ineligible areas, 10 percent, 15 percent, 20 percent, and 30 percent for Tier 2 areas and 35 percent for Tier 1 areas. Pre-2000 the rates were 0, 20 percent and 30 percent (see Figure 1).

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

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

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

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

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

#### IV. Data and Estimating Policy Rules

##### A. Datasets

Details on the data are in online Appendix C, but we summarize the most important features here. We combine administrative data on program participants with official

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

<sup>16</sup> For example, when we are looking at employment,  $L$ , as an outcome, the dependent variable is  $y = \ln(1 + L)$ . One hundred of our 10,737 areas have 0 manufacturing employment in all years (0.9 percent of the sample) and 21 areas have no unemployed in all years (0.2 percent of the sample). Our results are robust to dropping all areas with zeros in any year or all wards which had a zero in any year. We also obtain near identical results using the Inverse Hyperbolic Sign transformation where we use  $\ln[L + \sqrt{(1 + L^2)}]$  as the dependent variable (see Card and Della Vigna 2017).

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).<sup>17</sup> The IDBR is the population business register containing every establishment's employment, location, and industry. We use this to construct jobs by area, our primary dependent variable, distinguishing between manufacturing jobs (where RSA is targeted) and non-manufacturing jobs. We match in unemployment from the local areas labor market statistics through the ONS Nomis service.

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

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

The ONS draws a stratified random sample of firms from the population of firms in the IDBR to form the ARD (Annual Respondents Database) from the Annual Business Inquiry (ABI) which is a mandatory survey. From the ARD we obtain information on investment, wages, and productivity of firms. For multi-plant firms, the ARD reports this information only at the aggregate firm level rather than the plant level available in the IDBR. Overall, 80 percent of firms are single-plant and located at a single mailing address. The ARD does not consist of the complete population of all UK manufacturing firms, since the sample is stratified with smaller businesses sampled randomly. However, it does contain the population of larger businesses, which cover 90 percent of total UK manufacturing employment.

### *B. Descriptive Statistics*

Table 1 reports the number of wards broken down by the initial level of NGE 1993–1999 and the new post-policy change NGE after 2000. Before 2000 column

<sup>17</sup>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.

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



TABLE 1—NUMBER OF AREAS (WARDS) ELIGIBLE FOR DIFFERENT MAXIMUM INVESTMENT SUBSIDIES (NGE) PRE AND POST POLICY CHANGE IN 2000

		Rate after 2000 (%)						
Rate in 1993–1999 (%)		Total	0	10	15	20	30	35
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
1.	0	7,309	6,823	26	192	72	15	181
2.	20	2,012	841	102	539	33	118	379
3.	30	1,416	265	16	30	717	0	388
Total		10,737	7,929	144	761	822	133	948

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

1 shows that 3,428 out of Britain's 10,737 areas were eligible for some investment subsidy (2,012 areas had a maximum subsidy rate of 20 percent and 1,416 areas had a maximum rate of 30 percent). After the policy change, row 1 shows that 486 areas (summing columns 3 through 7) which were previously ineligible for any subsidy became eligible and 1,106 areas which used to be eligible became ineligible (summing 841 and 265 in column 2). The total number of ineligible areas rose from 7,309 to 7,929. There were also a large number of areas that were eligible in both periods, but still switched their level of NGE. For example, row 3 shows that of the areas which, pre-2000, were eligible for up to a 30 percent subsidy rate, 388 became eligible for up to a 35 percent subsidy, while 717 saw their NGE fall to 20 percent, 30 to 15 percent, 16 to 10 percent, and 265 to 0. Unsurprisingly, the majority of areas were ineligible for subsidies in all periods (6,823 areas out of a total of 10,737).

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

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

TABLE 2—DESCRIPTIVE STATISTICS

	Years	Mean	Standard deviation	Observations	Number of units
<i>Panel A. All areas</i>					
NGE (maximum investment subsidy percent)	1997–1999	0.1	0.1	32,211	10,737
	2000–2004	0.1	0.1	53,685	10,737
Average RSA payment (£)	1997–1999	18,265.2	197,955	32,211	10,737
	2000–2004	9,837.1	134,036	53,685	10,737
Total unemployment (claimant count)	1997–1999	113.5	150.6	32,211	10,737
	2000–2004	80.9	112.5	53,685	10,737
Manufacturing employment	1997–1999	267.4	626.2	32,211	10,737
	2000–2004	233.0	535.9	53,685	10,737
<i>Panel B. Areas eligible for RSA subsidies</i>					
NGE (maximum investment subsidy percent)	1997–1999	0.2413	0.0492	10,284	3,428
	2000–2004	0.2367	0.08896	14,040	2,808
Average RSA payment (£)	1997–1999	56,132.2	346,827	10,284	3,428
	2000–2004	35,712.8	260,039	14,040	2,808
Total unemployment (claimant count)	1997–1999	161.85	179.04	10,284	3,428
	2000–2004	123.85	147.95	14,040	2,808
Manufacturing employment	1997–1999	350.96	823.91	10,284	3,428
	2000–2004	338.10	745.40	14,040	2,808
<i>Panel C. Plant-level</i>					
Plant employment across all areas	1997–1999	21.2	102.30	406,615	167,415
	2000–2004	19.8	91.90	631,089	183,061
Plant employment across eligible areas	1997–1999	27.4	139.00	131,431	53,575
	2000–2004	26.8	125.85	176,902	50,926
<i>Panel D. Characteristics of recipients and non-recipients in eligible areas (£), firms</i>					
Employment of recipients	1997–2004	87.2	323.4	16,413	4,550
Employment of non recipients	1997–2004	31.3	199.6	188,899	39,308
Investment of recipients	1997–2004	1,717.0	105,686	3,048	1,488
Investment of non recipients	1997–2004	953.3	8,001	15,314	7,449
(Value added/worker) recipients	1997–2004	38.6	25.8	3,048	1,488
(Value added/worker) non-recipients	1997–2004	44.9	231.51	15,314	7,449
TFP of recipients (indexed to industry $\times$ year average in logs)	1997–2004	−0.042	0.371	3,048	1,488
TFP of non recipients (indexed to industry $\times$ year average in logs)	1997–2004	−0.015	0.414	15,314	7,449

*Note:* TFP is computed using a Solow residual “factor share” method and relative to an industry  $\times$  year average (see online Appendix C).

### C. Estimating the Policy Rules

The estimates used to construct the policy rule instrument are presented in columns 1 and 2 of Table 3.<sup>19</sup> As discussed in Section III, these results are from ordered probit estimates of the area support levels (NGE). Note that three of the variables used as indicators in 2000 by policymakers were not used in 1993 (the employment rate, the ILO unemployment rate, and the share of manufacturing workers), so we drop these variables from the regressions in column 1, i.e., setting the coefficients on these variables to be 0. Similarly, there were two indicators that were used in 1993 but not in 2000 (the business start-up rate and the long-duration unemployment rate). Similarly, we set the coefficients on these variables to be 0 in column 2.

Looking across Table 3, the signs are generally intuitive. Areas with higher GDP per capita, higher population density, more skilled workers, higher business start-up rates, lower structural unemployment rates, higher activity rates, and higher

<sup>19</sup> Online Appendix Table A2 has definitions and descriptive statistics.

TABLE 3—ESTIMATES OF PARAMETERS ON ELIGIBILITY RULE CHANGES

Dependent variable: level of NGE ordered variable	Main specification		Restricted variables	
	1993 (1)	2000 (2)	1993 (3)	2000 (4)
GDP per capita	−0.022 (0.002)	−0.040 (0.002)	−0.034 (0.002)	−0.055 (0.002)
Population density	−0.028 (0.002)	−0.034 (0.002)	−0.043 (0.002)	−0.015 (0.002)
Share of high skilled workers	−0.584 (0.129)	−1.438 (0.149)	−0.904 (0.125)	−2.268 (0.142)
Business start-up rate	−2.414 (0.240)		−0.490 (0.183)	
Structural unemployment rate	83.251 (2.483)	32.681 (2.315)		
Activity rate	−1.147 (0.250)	−1.934 (0.263)	−1.235 (0.237)	−1.879 (0.252)
Employment rate		−8.201 (0.462)		−11.259 (0.444)
Current unemployment rate (claimant count)	−9.148 (3.240)	18.276 (3.565)	84.330 (1.846)	
ILO unemployment rate		−5.682 (0.824)		−0.122 (0.760)
Long-duration unemployment rate	0.472 (1.216)		5.501 (1.163)	
Share of manufacturing workers		−1.122 (0.202)		−1.870 (0.196)
Observations (wards)	10,737	10,737	10,737	10,737
Cut-off 10%	0.000 (0.220)	−9.503 (0.420)	−0.579 (0.210)	−14.697 (0.377)
Cut-off 15%	1.202	−9.426	0.478	−14.629
Cut-off 20%	(0.221)	(0.420)	(0.210)	(0.377)
Cut-off 30%		−8.938 (0.419)		−14.201 (0.375)
Cut-off 35%		−8.272		−13.600
log likelihood	−5,525.405	−6,879.521	−6,126.595	−7,325.151

Notes: Coefficients (robust standard errors) from ordered probits of NGE maximum support categories for 1993 and 2000. Dependent variable in columns 1 and 3 takes values of 1 to 3 depending on the level of NGE (0, 20, and 30 percent) and in columns 2 and 4 a value of 1 to 6 (0, 10, 15, 20, 30, and 35 percent). See online Appendix Table A2 for variable definitions. *Restricted variables* drops (collinear) structural unemployment in columns 3 and 4 and claimant unemployment in column 4.

employment rates are all significantly less likely to be eligible for higher investment subsidies. The only surprises are that the claimant count unemployment rate (in 1993) and the ILO unemployment rate (in 2000) take counterintuitive negative signs. This seems to be due to collinearity among the many unemployment measures. In 1993 (2000) there are four (five) labor market indicators that are all highly correlated. We illustrate the collinearity issue by estimating similar regressions with fewer unemployment variables. For 1993, when we drop structural unemployment, the results in column 3 show that the coefficient on the current unemployment rate

takes its expected positive sign. For the period from 2000 onward, column 4 shows that the coefficient on ILO unemployment reduces in absolute size and is no longer significant when we drop the claimant count and structural unemployment rate.<sup>20</sup>

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

## V. Area-Level Analysis

### A. Main Results

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

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

Figure 4 reports the same results for unemployment. The 1997–2004 period was one of strong growth in the UK economy and unemployment was falling across the country. It is clear that there is a significantly faster fall in unemployment in the areas which were exogenously more likely to become eligible for investment subsidies after 2000 (dashed line). By 2004, these areas enjoyed falls in unemployment about 7 percent higher than elsewhere. By contrast, prior to 2000 the falls in unemployment were statistically identical across the two groups of areas.<sup>22</sup>

<sup>20</sup>We checked that these collinearity issues are not spuriously driving our key findings. We constructed the rule change instruments dropping some of the potentially collinear variables and found that our results are robust. For example, using the estimates from the last two columns of Table 3 (instead of our baseline estimates using the first 2 columns) generated a coefficient (standard error) on the IV estimates in the employment equation of 0.653 (0.322) compared to 0.953 (0.260) in the baseline estimates of column 4 in Table 4, panel A.

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

<sup>22</sup>We also reproduced Figures 3 and 4 using the *actual* changes in areas eligible for RSA rather than the predicted changes. Consistent with our concern over endogeneity, there is evidence of pre-trends in the expected

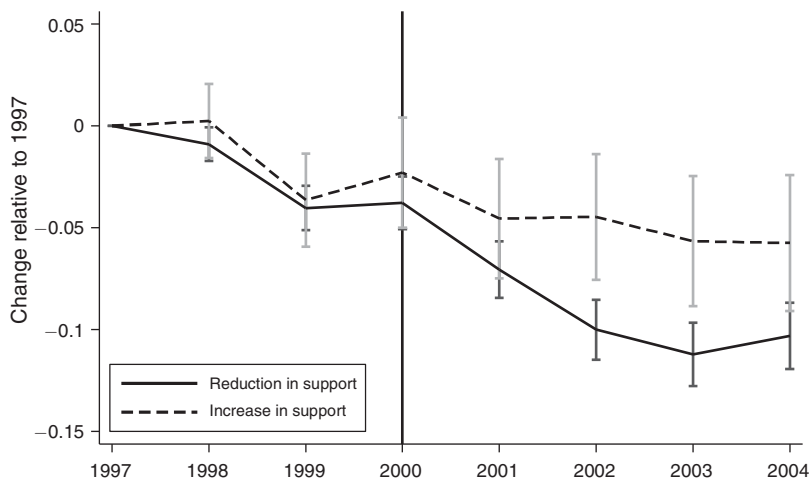


FIGURE 3. CHANGES IN MANUFACTURING EMPLOYMENT IN AREAS WITH INCREASING VERSUS DECREASING SUPPORT PROBABILITY

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

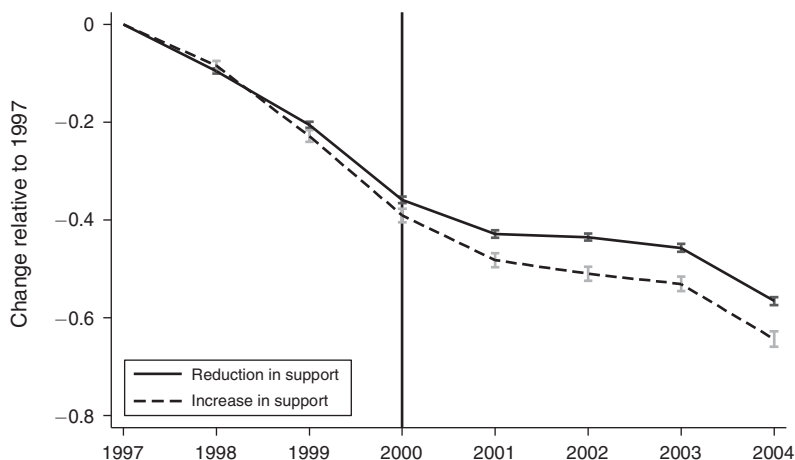


FIGURE 4. CHANGES IN UNEMPLOYMENT IN AREAS WITH INCREASING VERSUS DECREASING SUPPORT PROBABILITY

*Notes:* Average changes relative to base year of 1997 in  $\ln(\text{number of unemployed})$  in a geographical area (ward). The dashed line shows average unemployment in wards that had an increase in support (as predicted by our policy rule IV). The solid line is average unemployment in wards that had a decrease in support (as predicted by our policy rule IV). Ninety-five percent 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.

direction using the actual changes. For example, areas that were ineligible for RSA, but became eligible after 2000 had larger average falls in employment than areas which did not change their eligibility status (or lost it).

TABLE 4—AREA-LEVEL REGRESSIONS: INSTRUMENTING MAXIMUM INVESTMENT SUBSIDIES (NGE) WITH RULE CHANGE

	OLS (1)	Reduced-form (2)	First-stage (3)	IV (4)
<i>Panel A. Dependent variable: <math>\ln(\text{manufacturing employment})</math></i>				
Maximum investment subsidy, NGE	0.124 (0.070)			0.953 (0.260)
Policy rule instrument		0.839 (0.228)	0.881 (0.033)	
<i>Panel B. Dependent variable: <math>\ln(\text{unemployment})</math></i>				
Maximum investment subsidy, NGE	-0.137 (0.024)			-0.414 (0.078)
Policy rule instrument		-0.365 (0.069)	0.881 (0.033)	
<i>Panel C. Dependent variable: <math>\ln(\text{non-manufacturing employment})</math></i>				
Maximum investment subsidy, NGE	0.006 (0.044)			0.177 (0.161)
Policy rule instrument		0.156 (0.141)	0.881 (0.033)	
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896

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

Table 4 reports area-level regression results. Panel A contains results for manufacturing employment. In column 1, we report regressions using the change in the area's maximum investment subsidy rate (NGE) as the main explanatory variable. There is a positive correlation with employment, but it is only significant at the 10 percent level and is small in magnitude: increasing the available investment subsidy by 10 percentage points is associated with a 1.2 percent increase in employment. Column 2 presents the reduced form using our policy rule instrumental variable constructed from exogenous changes in subsidy eligibility using the change in EU-wide policy parameters. The coefficient on the IV is positive and significant as suggested by Figure 3. Column 3 reports the first-stage regression with NGE changes as the outcome and shows that this is strongly predicted by our IV. The final column reports the IV results suggesting that the causal effect of RSA is over 7 times as large as the OLS estimate of column 1. A 10 percentage point increase in the maximum investment subsidy (e.g., an increase in NGE from 0 to 0.1) leads to a 10 percent  $(= (\exp(0.0953 \times 0.1) - 1) \times 100)$  increase in jobs. This OLS bias is consistent with what we would expect: a positive shock to an area decreases the probability of it becoming eligible for investment subsidies, so OLS underestimates the employment increasing effects of the policy.<sup>23</sup>

<sup>23</sup> All results are robust to using the level instead of the logarithm of the dependent variable. For example, in levels the coefficient on NGE in the IV employment equation of column 4 of panel A is 644.6 with a standard error of 112.9. This implies a 10 percentage point increase in NGE increases the number of manufacturing jobs in a ward by 64 or 18 percent at the mean level of employment (351 in panel B of Table 2). This larger effect is driven by outliers which are dampened by the log transformation. For example, if we winsorize the upper and lower 5 percent of the employment distribution and reestimate in levels the coefficient on NGE becomes 303.9 with a standard error



As noted above, RSA is focused on the manufacturing sector. Consequently, the increase in manufacturing employment in panel A of Table 4 could come from decreases in jobs in non-manufacturing sectors. To assess this, we do two things. First, in panel B we estimate identical specifications to panel A, but instead use  $\ln(\text{unemployment})$  as a dependent variable to see if joblessness falls in eligible areas. Second, in panel C we directly examine non-manufacturing employment. Panel B shows that unemployment falls significantly in areas that become eligible for higher levels of investment subsidy. Just like manufacturing employment, the beneficial effects of the policy on unemployment is underestimated by the OLS estimates in column 1 compared to the IV estimates. A 10 percentage point increase in NGE causes a 4.2 percent fall in unemployment in column 4. By contrast, there appears to be no significant effect of NGE on non-manufacturing employment in panel C. For example, the coefficient in column 4 is 0.177 with a standard error of 0.161 (compared to 0.953 for manufacturing). Consequently, NGE increases the share of manufacturing jobs as well as the total number of jobs in an area.<sup>24</sup>

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

### B. Other Policies

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

---

of 34.4, which for a 10 percentage point NGE increase implies a 9 percent rise in employment, much closer to our baseline results. Similarly, other transformations such as using the Inverse Hyperbolic Sign gave similar results. For example, the IV coefficient (standard error) was 0.968 (0.286) in a specification like column 4 of panel A.

<sup>24</sup>Using the share of manufacturing jobs as the dependent variable and estimating column 4 leads to a coefficient (standard error) of 0.137 (0.033) on NGE. Using the  $\ln(\text{total number of jobs})$  has a coefficient (standard error) of 0.353 (0.144) on NGE in this IV specification.

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

<sup>26</sup>The structural funds are the financial tools the European Union uses to implement regional policy (see [http://ec.europa.eu/regional\\_policy/en/funding/](http://ec.europa.eu/regional_policy/en/funding/)). Past evaluations report mixed results for the effect of structural funds. Recently, Becker, Eggert, and von Ehrlich (2010, 2012, 2013) have a more positive assessment especially for regions with higher absorptive capacity (those that are richer and hence closer to the cut-off point for EU funding).

TABLE 5—AREA LEVEL REGRESSIONS: INSTRUMENTING AMOUNT OF SUBSIDY WITH RULE CHANGE

Method	OLS (1)	Reduced-form (2)	First-stage (3)	IV (4)
<i>Panel A. Dependent variable: <math>\ln(\text{manufacturing employment})</math></i>				
$\ln(\text{RSA subsidy})$	0.012 (0.002)			0.288 (0.134)
Policy rule instrument		0.839 (0.228)	2.909 (1.140)	
<i>Panel B. Dependent variable: <math>\ln(\text{unemployment})</math></i>				
$\ln(\text{RSA subsidy})$	−0.002 (0.001)			−0.125 (0.053)
Policy rule instrument		−0.365 (0.069)	2.909 (1.140)	
<i>Panel C. Dependent variable: <math>\ln(\text{non-manufacturing employment})</math></i>				
$\ln(\text{RSA subsidy})$	0.001 (0.002)			0.054 (0.052)
Policy rule instrument		0.156 (0.141)	2.909 (1.140)	
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896

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

also changed in 2000. Fortunately, the areas that saw a change in their eligibility for structural funds are not all the same as those that saw a change in their eligibility for RSA. In fact, there is considerable variation in the areas that switched in and out of RSA and structural funds eligibility (see online Appendix Table A3). Total SF spending is higher than RSA, although the direct SF grants to business are an order of magnitude smaller than RSA. For example, in 1997 the total amount of RSA grants accepted was £158.3 million while the total amount of Structural Regional Development Funds was £621 million, only £15.6 million of this amount was paid as business grants (1997 Annual Report of the Industrial Development Act).

As with RSA, changes in structural fund eligibility are unlikely to be exogenous to local shocks as the structural funds are designed to provide support for declining areas. Consequently, we implement the same methodology used for RSA to develop an IV for structural funds based on the criteria that the EU used in determining whether an area is eligible for structural fund support. Despite considerable overlap with the variables used to determine RSA eligibility there are sufficient differences in the EU criteria to make this strategy viable. For example, local crime rates were a criterion for structural funds (but not RSA), and the start-up rate and activity rates were criteria for RSA (but not structural funds). We exploit these differences when estimating the structural fund policy rules.<sup>27</sup> From the estimated weights on the

<sup>27</sup> See online Appendix Table A4 (the analog to Table 3).

TABLE 6—AREA-LEVEL REGRESSIONS ACCOUNTING FOR STRUCTURAL FUNDS (SF)

	OLS (1)	Reduced-form (2)	First-stage NGE (3)	First-stage SF (4)	IV (5)
<i>Panel A. Dependent variable: <math>\ln(\text{manufacturing employment})</math></i>					
Maximum investment subsidy, NGE	0.098 (0.081)				0.999 (0.328)
Structural fund, SF	0.038 (0.037)				−0.029 (0.079)
NGE IV		0.792 (0.224)	0.816 (0.034)	0.805 (0.067)	
Structural fund IV		0.094 (0.059)	0.124 (0.011)	1.029 (0.026)	
<i>Panel B. Dependent variable: <math>\ln(\text{unemployment})</math></i>					
Maximum investment subsidy, NGE	−0.099 (0.027)				−0.409 (0.098)
Structural fund, SF	−0.061 (0.012)				−0.050 (0.027)
NGE IV		−0.374 (0.066)	0.816 (0.034)	0.805 (0.067)	
Structural fund IV		−0.103 (0.021)	0.124 (0.011)	1.029 (0.026)	
Number of areas (wards)	10,737	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896	85,896

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

structural funds criteria and lagged characteristics we construct a policy rule change IV for EU structural funds and reestimate our main specifications augmented to include these new variables.

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

As noted above, we also considered a wide range of other place-based policies. We identified six other place-based policies that changed in our sample period: Employment Zones, Coalfields Regeneration Scheme, Regional Venture Capital

TABLE 7—CONTROLLING FOR OTHER POLICIES (IN EMPLOYMENT REGRESSIONS)

	ln(manufacturing employment)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A. Reduced-form</i>								
Policy rule IV	0.900 (0.228)	0.824 (0.228)	0.840 (0.228)	0.837 (0.228)	0.813 (0.229)	0.831 (0.228)	0.815 (0.231)	0.768 (0.232)
Employment zones	−0.037 (0.025)						−0.024 (0.025)	−0.029 (0.025)
Coalfield regeneration trust		−0.052 (0.021)					−0.057 (0.020)	−0.058 (0.020)
Regional venture capital funds			0.031 (0.018)				0.023 (0.018)	0.026 (0.018)
Enterprise grants				−0.003 (0.015)			−0.009 (0.016)	−0.012 (0.016)
New deal for communities					−0.046 (0.023)		−0.057 (0.023)	−0.051 (0.023)
Devolution to Wales and Scotland						−0.029 (0.020)	−0.041 (0.021)	−0.055 (0.022)
Structural fund								0.134 (0.063)
<i>Panel B. IV</i>								
NGE	1.090 (0.277)	0.943 (0.262)	0.954 (0.260)	0.972 (0.266)	0.914 (0.259)	0.931 (0.255)	0.966 (0.274)	0.895 (0.319)
Employment zones	−0.074 (0.028)						−0.048 (0.027)	−0.053 (0.028)
Coalfield regeneration trust		−0.03 (0.022)					−0.040 (0.021)	−0.040 (0.021)
Regional venture capital funds			0.044 (0.018)				0.032 (0.018)	0.034 (0.018)
Enterprise grants				0.033 (0.019)			0.018 (0.018)	0.019 (0.018)
New deal for communities					−0.059 (0.023)		−0.080 (0.023)	−0.077 (0.024)
Devolution to Wales and Scotland						−0.072 (0.023)	−0.075 (0.023)	−0.076 (0.022)
Structural fund								0.055 (0.075)
Number of areas	10,737	10,737	10,737	10,737	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896	85,896	85,896	85,896	85,896

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

Funds, Enterprise Grants, the New Deal for Communities, and Devolution to Scotland and Wales. The details of each of these are discussed in online Appendix E. These policies do not have an explicit set of EU rules that we can use to construct the same instruments as RSA and structural funds. Therefore, to control for the effect of these policies, we simply include a dummy variable which switches on when an area becomes eligible for the policy. Table 7 displays the results for employment reduced forms (panel A) and IV regressions (panel B) with specifications based on those of

columns 2 and 4 of Table 4, panel A.<sup>28</sup> We include each policy variable one by one in columns 1–6, and then all together in column 7. As is clear from the table, the effect of RSA is robust to the inclusion of all these other policy controls, remaining statistically significant with a very similar coefficient throughout (the coefficient in the reduced form is now 0.815 compared to 0.839 in the baseline Table 4 results and for IV is now 0.966 compared to 0.953 in the baseline). As for the other policies, some appear to have perverse negative and significant coefficients on jobs (e.g., New Deal for Communities and Devolution to Scotland and Wales) whereas others have generally positive coefficients (e.g., Regional Venture Capital Fund). Given that we do not have instruments for these policies, we should not read too much into the coefficients. Finally, column 8 also adds in structural funds to the specification of column 7, treated endogenously as in Table 6. The SF coefficient is significant in the reduced form of panel A, but insignificant for the IV specification of panel B.<sup>29</sup> More importantly for us, the RSA treatment effect remains significant.

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

### C. Higher Levels of Aggregation (TTWA)

In this subsection we compare the policy effects at the ward level to the more aggregate travel to work area (TTWA) level in order to examine spillover effects across areas.<sup>30</sup> When an area becomes eligible for investment subsidies firms may relocate jobs from neighboring ineligible areas. For example, consider a ward,  $r$  and its neighbor  $r'$ , in a single TTWA (the example is easily generalized to  $r = 1, 2, \dots, R$  contiguous wards). The ward employment regression (in long differences) can take the form:

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

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

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

where  $y_{a,t}$  is the log of TTWA employment and  $NGE_{a,t}$  is the average NGE change in the 2 wards weighted by the lagged ward-level employment levels; i.e.,  $\Delta NGE_{a,t} = w_r \Delta NGE_{r,t} + (1 - w_r) \Delta NGE_{r',t}$  where  $w_r = \frac{L_{r,0}}{L_{r,0} + L_{r',0}}$  is the share of

<sup>28</sup> Equivalent results for unemployment are contained in online Appendix Table A5.

<sup>29</sup> The structural funds coefficient is significant for both specifications in online Appendix Table A5 when we use unemployment as the dependent variable.

<sup>30</sup> A TTWA is similar to a US commuting zone. There is variation within TTWA in ward eligibility. Post-2000, in a TTWA with at least one eligible ward only 31.5 percent of wards had positive NGE. Pre-2000 the number was 35 percent.

TABLE 8—HIGHER LEVEL OF AGGREGATION: TRAVEL TO WORK AREA (TTWA)

	OLS (1)	Reduced-form (2)	First-stage (3)	IV (4)
<i>Panel A. Dependent variable: <math>\ln(\text{manufacturing employment})</math></i>				
Maximum investment subsidy, NGE	0.538 (0.114)			1.006 (0.319)
Policy rule instrument		1.053 (0.362)	1.047 (0.222)	
<i>Panel B. Dependent variable: <math>\ln(\text{unemployment})</math></i>				
Maximum investment subsidy, NGE	−0.263 (0.062)			−0.803 (0.265)
Policy rule instrument		−0.840 (0.249)	1.047 (0.222)	
Number of areas (TTWAs)	322	322	322	322
Observations	2,576	2,576	2,576	2,576

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

employment in region  $r$  in the base year 0.<sup>31</sup> In online Appendix D we show that if there are no spillovers (i.e.,  $\chi = 0$ ) we would expect to see that  $\mu \approx \lambda_1$ . If there are negative spillovers we would expect  $\mu < \lambda_1$ . In the extreme case where the program simply causes shifting between areas (as Wilson 2009 suggests for R&D tax credits across American states) the coefficient of NGE in equation (7) will be zero ( $\mu = 0$ ).

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

#### D. Other Area-Level Robustness Tests

We conducted a large number of other robustness tests, some of which we sketch here with details in online Appendix F. First, our baseline regression results in Table 4 control for the levels of all variables in Table 3 that enter the policy rules (the  $X_{r,93}$ ). We checked the robustness of the results to using higher-order polynomial functions of the  $X_{r,93}$  (quadratic and interaction terms), dropping them completely or

<sup>31</sup> The weights are based on 1996 employment levels to mitigate endogeneity concerns.

<sup>32</sup> We are assuming that displacement is most likely to occur across neighboring areas. It is possible that displacement occurs from other areas of the United Kingdom, but it is likely that local displacement would be strongest.



adding in the predicted probabilities from the ordered probits (see online Appendix Table A6). The results were robust to these experiments.

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

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

Fourth, we considered regression discontinuity (RD) designs (see online Appendix Sections F.2 through F.4). In principle, since we know the variables underlying the rules, conditioning on polynomials of the rules should remove the correlation of NGE with unobservable influences on our outcomes. Implementing this design is empirically challenging in our context as we do not directly observe the running variable, the threshold is unknown, and the variables underlying the policy rules are high dimensional (e.g., 8 indicators pre-2000 and 9 thereafter) and are likely measured with error. However, for one indicator, GDP per capita, we do know the cut-off for eligibility (75 percent of the EU average GDP per capita in the NUTS2 region). We implement an RD design using this threshold and find a significant effect of the cut-off on NGE as well as treatment effects that are larger than our main estimates, although very imprecisely estimated (see online Appendix Table A9). For example, a 10 percentage point increase in NGE causes a (insignificant) 19 percent increase in employment compared to 10 percent in our baseline.<sup>35</sup>

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

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

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

Finally, we conducted a large number of other robustness tests such as using a longer time period (from 1986 onward instead of 1997), examining general equilibrium effects on factor prices (wages), and using matching estimators. Our results are robust to these tests.<sup>36</sup>

## VI. Micro-Analysis at Firm and Plant Level and Overall Magnitudes

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

### A. *Extensive versus Intensive Margins: Number of Plants as an Outcome*

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

### B. *Heterogeneous Policy Effects by Firm Size*

Table 10 presents  $\ln(\text{employment})$  regressions at the plant level (rather than the area level as in the previous tables) where the treatment variable continues to be the maximum investment subsidy available in the area where a plant is located (NGE). The IV results of column 4 of panel A implies that an increase of NGE by 10 percentage points leads to a 4.7 percent increase in plant-level employment. We also find a large difference between the OLS and IV coefficients that is consistent with strong selection effects at the plant level.

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

<sup>36</sup>Details in online Appendix Table A11 and online Appendix F5.

TABLE 9—EFFECTS AT THE EXTENSIVE MARGIN? NUMBER OF MANUFACTURING PLANTS AS AN OUTCOME

	ln(number of manufacturing plants)			
	OLS (1)	Reduced-form (2)	First-stage (3)	IV (4)
<i>Panel A. Baseline</i>				
Maximum investment subsidy, NGE	−0.025 (0.033)			0.209 (0.106)
Policy rule instrument		0.184 (0.094)	0.881 (0.033)	
<i>Panel B. RSA subsidy levels</i>				
ln(RSA subsidy)	0.003 (0.001)			0.063 (0.039)
Policy rule instrument		0.184 (0.094)	2.909 (1.140)	
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896
			First-stage	
			NGE	SF
<i>Panel C. Including structural funds in baseline</i>				
Maximum investment subsidy, NGE	−0.062 (0.036)			0.097 (0.135)
Structural fund	0.037 (0.017)			0.047 (0.034)
NGE IV		0.117 (0.093)	0.816 (0.034)	0.805 (0.067)
Structural fund IV		0.060 (0.026)	0.124 (0.011)	1.029 (0.026)
Number of areas (wards)	10,737	10,737	10,737	10,737
Observations	85,896	85,896	85,896	85,896
<i>Panel D. Travel to work area</i>				
Maximum investment subsidy, NGE	−0.036 (0.053)			0.029 (0.126)
Policy rule instrument		0.030 (0.132)	1.047 (0.222)	
Number of areas (TTWA)	322	322	322	322
Observations	2,576	2,576	2,576	2,576

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

1996, the year before the start of our estimation period (for firms born after 1996 we use size in the first year and drop this observation from the regressions).

We report plant-level employment regressions separately for small firms (firm employment under 50) and large firms (over 50 employees) in panels B and C respectively in Table 10. The first stages are strong for both types of firm with a near identical coefficient (0.68 versus 0.64). However, the IV effect is positive and significant for plants in small firms but insignificant and around one-sixth of the size for

TABLE 10—PLANT-LEVEL EMPLOYMENT REGRESSIONS, SPLITS BY FIRM SIZE

	ln(manufacturing employment)			
	OLS (1)	Reduced- form (2)	First- stage (3)	IV (4)
<i>Panel A. Pooled across all plants, 653,385 observations on 96,768 plants; 9,975 wards</i>				
Maximum investment subsidy, NGE	0.011 (0.025)			0.463 (0.089)
Policy rule instrument		0.312 (0.058)	0.675 (0.040)	
<i>Panel B. Small (plants in firm with under 50 employees), 594,356 observations on 87,728 plants; 9,883 wards</i>				
Maximum investment subsidy, NGE	0.006 (0.026)			0.441 (0.095)
Policy rule instrument		0.299 (0.063)	0.678 (0.040)	
<i>Panel C. Large (plants in firms with over 50 employees), 59,025 observations on 9,036 plants; 3,708 wards</i>				
Maximum investment subsidy, NGE	0.027 (0.055)			0.070 (0.203)
Policy rule instrument		0.045 (0.130)	0.642 (0.050)	

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

plants in big firms.<sup>37</sup> Similarly, there is a large and significant reduced-form effect in column 2 for small firms but a small and insignificant effect for large firms.<sup>38</sup> This implies that plants that are part of small firms drove the aggregate area effect identified in the previous section.

There could be at least two different reasons for the heterogeneity of the policy effect by firm size. Firstly, although large firms are often based in areas that receive support—hence the highly significant first stage in column 3—the size of their grants could be relatively less generous. An alternative story is that they are equally well supported, but the subsidies generate less jobs. We explored this by examining regressions of employment on actual RSA support.<sup>39</sup> Regardless of whether we use a dummy or a continuous treatment indicator there is a large and significant positive effect of receiving investment subsidies when estimated by IV for small firms, but not for large firms. Hence, these results reject the hypothesis that the absence of a large firm effect is because they simply obtain less subsidies, but it is rather that small firms create more jobs from the subsidies they receive compared to big firms.<sup>40</sup>

<sup>37</sup>In online Appendix Table A12 we vary the definition of a “small” firm and show our results are robust to varying the exact size threshold.

<sup>38</sup>The effects are also significantly different at the 5 percent level for large firms versus small firms (see online Appendix Table A13, column 1).

<sup>39</sup>Online Appendix Table A14 is the analog of Table 5.

<sup>40</sup>An objection is that the relevant quantity is not the *elasticity* of employment with respect to subsidies for large versus small firms, but rather the marginal effect on the absolute number of jobs created with respect to a \$1 increase in subsidy. Online Appendix Table A15 conducts this analysis and shows that \$1 of subsidy to a small firm still creates over 8 times as many jobs as \$1 of subsidy to a large firm according to our estimates.

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

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

### C. Firm-Level Results: Employment, Capital, and Productivity

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

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

Panel A of Table 11 reports employment regressions at the firm level using the IDBR population. These are very similar to the plant level results, suggesting that

<sup>41</sup> See online Appendix Table A13.

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

<sup>43</sup> We call this the firm level,  $j$ , but there could be many reporting units in one large firm.

TABLE 11—FIRM LEVEL: EFFECTS ON JOBS, INVESTMENT, OUTPUT, AND TFP. INSTRUMENTING MAXIMUM INVESTMENT SUBSIDY WITH RULE CHANGE

	OLS (1)	Reduced- form (2)	First-stage (3)	IV (4)
<i>Panel A. Dependent variable: <math>\ln(\text{employment})</math>, full sample (449,514 observations, 91,546 firms)</i>				
NGE	0.039 (0.024)			0.670 (0.078)
Policy rule instrument		0.493 (0.057)	0.735 (0.011)	
<i>Panel B. Dependent variable: <math>\ln(\text{employment})</math>, ARD sub sample (45,511 observations, 21,389 firms)</i>				
NGE	0.163 (0.057)			0.564 (0.168)
Policy Rule Instrument		0.444 (0.132)	0.787 (0.032)	
<i>Panel C. Dependent variable: <math>\ln(\text{capital investment})</math>, ARD sub sample (45,511 observations, 21,389 firms)</i>				
NGE	0.249 (0.304)			1.668 (0.750)
Policy rule instrument		1.313 (0.588)	0.787 (0.032)	
<i>Panel D. Dependent variable: <math>\ln(\text{output})</math>, ARD sub sample (45,511 observations, 21,389 firms)</i>				
NGE	0.031 (0.065)			0.399 (0.182)
Policy rule instrument		0.314 (0.143)	0.787 (0.032)	
<i>Panel E. Dependent variable: <math>\ln(\text{TFP})</math>, ARD sub sample (45,511 observations, 21,389 firms)</i>				
NGE	-0.034 (0.043)			-0.071 (0.099)
Policy rule instrument		-0.056 (0.078)	0.787 (0.032)	

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

within firm reallocation across plants in response to the policy is not a major issue. In the other panels we use the ARD data that have information on other outcomes such as investment. In panel B we report results for employment estimated using the ARD subsample and confirm our earlier finding of a positive causal impact on jobs. In panel C we find larger impacts on capital investment than we did for employment consistent with the simple theory model in Section II. Panel D shows that there is also an impact on output. Finally, panel E uses a Solow residual-based TFP measure (for more details on the calculation, see online Appendix C) and finds no significant effect of the policy. We looked at a variety of other methods of calculating TFP, but in no case do we find a significant impact on productivity (see online Appendix Table A16). There were also no significant program effects on wages.<sup>44</sup>

<sup>44</sup>For example, when we replaced the dependent variable by wages in the reduced form of column 2 the coefficient on the policy rule IV was 0.287 with a standard error of 0.877. This is consistent with the absence of an area-level wage effect of NGE (see online Appendix F.5).



Motivated by the theory in Section II, suggesting that more capital-intensive firms are more responsive to the policy, we interacted the treatment effects with a dummy for whether the firm had a high level of capital costs in revenues prior to the 2000 policy change. Consistent with the model, firms where the capital share was high (big  $s_K$ ) had stronger positive employment effects. The interaction of the rule change IV and a dummy for high capital share firms had a coefficient (standard error) of 0.525 (0.200) in the employment reduced form.<sup>45</sup>

#### D. Magnitudes

To consider the overall magnitude of the impact of RSA, we model what would have happened if, instead of redrawing the map in 2000, the program had simply been abolished. Online Appendix G gives details of the calculations. We start with the IV coefficient from column 4 of Table 4, panel A of 0.953 (indicating that a 10 percentage point NGE investment subsidy would increase area-level manufacturing employment by 10 percent) and consider the area by area change in NGE (to 0) given employment levels. This calculation suggests a loss of just under 156,000 jobs. The nominal average annual cost of RSA was about £164 million. Using official estimates of administrative costs (17 percent of the aggregate grant value)<sup>46</sup> and a dead-weight cost of taxation of 50 percent, this implies a total annual cost of £288 million. This leads to a “cost per job” of £1,846 ( $= 288/0.156$ ), or \$3,541 (at 2010 prices). If we took the more conservative OLS estimates from column 1 which has a treatment effect of 0.124, we get smaller job effects of just under 22,400 and the cost per job would be £12,857 (or \$24,662). Since there do not appear to be substitution effects from neighboring non-eligible areas, these do not need to be scaled down.

In online Appendix G, we provide figures for the limited number of studies that report cost per job for similar policies to those we examine here. Two methodological differences help partly explain our lower cost per job numbers. First, three area-based studies (Busso and Kline 2008; Busso, Gregory, and Kline 2013; and Freedman 2012) do not use IV.<sup>47</sup> As noted already, we find much larger effects correcting for endogeneity using IV. Second, the three other studies only have estimates at the firm level. When we take into account that we find zero effects of RSA on large firms, we obtain a cost per job of \$26,572, higher than the US figure in Brown and Earle (2017), but lower than the two Italian studies (Pellegrini and Muccigrosso 2017, Cerqua and Pellegrini 2014). In addition to these methodological differences, the RSA program is different from the other studies in that it subsidizes capital and not labor, and the government agency selects firms who can show evidence of job additionality rather than providing subsidies for all eligible firms that locate in supported areas.

<sup>45</sup> See online Appendix Table A17 (a generalization of column 2 in Table 11, panel B).

<sup>46</sup> We use the administrative reports of the grants awarded averaging £164 million and add to this the estimations from the National Audit Office (2003) that there were 10 percent spent in government administration costs for RSA, and an average 7 percent 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 by 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.

<sup>47</sup> Cost per job figures for Busso and Kline (2008) are reported in Bartik (2010) and for Busso, Gregory, and Kline (2013) in Glaeser and Gottlieb (2008).

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

### E. “Big Push”: Asymmetries of Subsidy Removal?

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

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

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

## VII. Conclusions

There are surprisingly few micro-econometric analyses of the causal effects of industrial policies, despite their ubiquity across the world. In this paper, we have examined one business support policy: Regional Selective Assistance (RSA). We use exogenous changes in the eligibility of areas to receive investment subsidies driven by EU rule changes determining which areas were eligible for investment

<sup>48</sup>For example, we created a dummy variable equal to 1 if an area received the maximum investment subsidy rate (NGE = 30 percent) continuously between 1986 and 1999 (and 0 otherwise) and interacted this with support-level treatment. When included in the employment regression of column 4 of Table 4 (alongside the linear dummy), this interaction variable had an insignificant coefficient (standard error) of 0.121 (0.390). As an instrument for the interaction we use the interaction between the rule change instrument and the 30 percent NGE indicator.

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

subsidies. When we correct for endogeneity, we find evidence for a positive treatment effect on jobs in the eligible areas and that unemployment is significantly reduced. We also find that the program effects are strong for smaller firms but effectively zero for larger firms. This is consistent with large firms being able to game the system and/or financial constraints being unimportant for these firms (although we do not find much evidence for this latter hypothesis). Interestingly, this stronger effect of business support policies on smaller firms is found in many other studies.<sup>50</sup> The fact that the treatment effect is confined to smaller firms strengthens arguments for restricting subsidies that go to larger enterprises, although one must be careful that this does not create strong disincentives for firms to grow (as they may forfeit such size-related subsidies: see Garicano, Lelarge, and Van Reenen 2016).

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

## REFERENCES

- Albouy, David.** 2009. “The Unequal Geographic Burden of Federal Taxation.” *Journal of Political Economy* 117 (4): 635–67.
- Angrist, Joshua D.** 2004. “Treatment Effect Heterogeneity in Theory and Practice.” *Economic Journal* 114 (494): C52–83.
- Autor, David H., Gordon H. Hansen, and David Dorn.** 2016. “The China Shock: Learning from Labor Market Adjustment to Large Changes in Trade.” *Annual Review of Economics* 8: 205–40.
- Banerjee, Abhijit V., and Esther Duflo.** 2014. “Do Firms Want to Borrow More? Testing Credit Constraints Using a Directed Lending Program.” *Review of Economic Studies* 81 (2): 572–607.
- Bartik, Timothy.** 2010. “Estimating the Costs per Job Created of Employer Subsidy Programs.” Presented at Upjohn Institute conference on “Labor Markets in Recession and Recovery,” Kalamazoo, MI.
- Beason, Richard and David E. Weinstein.** 1996. “Growth, Economies of Scale, and Targeting in Japan (1955–1990).” *Review of Economics and Statistics* 78 (2): 286–95.
- Becker, Sascha O., Peter H. Egger, and Maximilian von Ehrlich.** 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 H. Egger, and Maximilian von Ehrlich.** 2012. “Too Much of a Good Thing? On the Growth Effects of the EU’s Regional Policy.” *European Economic Review* 56 (4): 648–68.

<sup>50</sup>For example, Howell (2017), Zwick and Mahon (2017), and Wallsten (2000) for the United States; Görg and Strobl (2007) for Ireland; Lach (2002) for Israel; Bronzini and Iachini (2014) for Italy; González, Jaumandreu, and Pazo (2005) for Spain; and Dechezleprêtre et al. (2018) for the United Kingdom.

- Becker, Sascha O., Peter Egger, and Maximilian von Ehrlich.** 2013. "Absorptive Capacity and the Growth and Investment Effects of Regional Transfers: A Regression Discontinuity Design with Heterogeneous Treatment Effects." *American Economic Journal: Economic Policy* 5 (4): 29–77.
- Black, Dan A., Jeffrey A. Smith, Mark C. Berger, and Brett J. 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): 1313–27.
- 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. "Microeconomic Models of Investment and Employment." In *Handbook of Econometrics*, Vol. 6A, edited by James Heckman and Edward Leamer, 4417–98. Amsterdam: Elsevier.
- Bronzini, Raffaello, 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): 327–49.
- 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): 100–34.
- Brown, J. David, and John S. Earle.** 2017. "Finance and Growth at the Firm Level: Evidence from SBA Loans." *Journal of Finance* 72 (3): 1039–80.
- Busso, Matias, Jesse Gregory, and Patrick Kline.** 2013. "Assessing the Incidence and Efficiency of a Prominent Place Based Policy." *American Economic Review* 103 (2): 897–947.
- Busso, Matias, and Patrick Kline.** 2008. "Do Local Economic Development Programs Work? Evidence from the Federal Empowerment Zone Program." Cowles Foundation Discussion Paper 1639.
- Card, David, and Stefano Della Vigna.** 2017. "What Do Editors Maximize? Evidence from Four Economics Journals." Unpublished.
- Cerqua, Augusto, and Guido Pellegrini.** 2014. "Do Subsidies to Private Capital Boost Firms' Growth? A Multiple Regression Discontinuity Design Approach." *Journal of Public Economics* 109: 114–26.
- Criscuolo, Chiara, Jonathan Haskel, and Ralf Martin.** 2003. "Building the Evidence Base for Productivity Policy Using Business Data Linking." *Economic Trends* 600: 39–51.
- Criscuolo, Chiara, Ralf Martin, Henry G. Overman, and John Van Reenen.** 2006. Longitudinal Micro Data Study of Selected DTI Business Support Schemes. London: Centre for Economic Performance.
- Criscuolo, Chiara, Ralf Martin, Henry G. Overman, and John Van Reenen.** 2019. "Some Causal Effects of an Industrial Policy: Dataset." *American Economic Review*. <https://doi.org/10.1257/aer.20160034>.
- David, Paul A., Bronwyn H. Hall, and Andrew A. 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.
- Dechezleprêtre, Antoine, Elias Einiö, Ralf Martin, Kieu-Trang Nguyen, and John Van Reenen.** 2018. "Do Tax Incentives for Research Increase Firm Innovation? An RD Design for R&D." Centre for Economic Performance Discussion Paper 1413.
- Dell, Melissa.** 2010. "The Persistent Effects of Peru's Mining Mita." *Econometrica* 78 (6): 1863–903.
- Department for Business, Innovation and Skills.** Various years. *Industrial Development Reports*. London: Her Majesty's Majestic Stationery Office.
- Devereux, Michael P., Rachel Griffith, and Helen Simpson.** 2007. "Firm Location Decisions, Regional Grants and Agglomeration Externalities." *Journal of Public Economics* 91 (3–4): 413–35.
- Einiö, Elias.** 2014. "R&D Subsidies and Company Performance: Evidence from Geographic Variation in Government Funding Based on the ERDF Population-Density Rule." *Review of Economics and Statistics* 96 (4): 710–28.
- Einiö, Elias, and Henry G. Overman.** 2015. "The (Displacement) Effects of Spatially Targeted Enterprise Initiatives: Evidence from UK LEGI." Unpublished.
- Felix, R. Alison, and James R. Hines, Jr.** 2013. "Who Offers Tax-Based Business Development Incentives?" *Journal of Urban Economics* 75: 80–91.
- Fowkes, Rigmor Kringelhol, João Sousa, and Neil Duncan.** 2015. "Evaluation of Research and Development Tax Credit." HM Revenue and Customs Working Paper 17.
- Freedman, Matthew.** 2012. "Teaching New Markets Old Tricks: The Effects of Subsidized Investment on Low-Income Neighborhoods." *Journal of Public Economics* 96 (11–12): 1000–14.
- Garicano, Luis, Claire Lelarge, and John Van Reenen.** 2016. "Firm Size Distortions and the Productivity Distribution: Evidence from France." *American Economic Review* 106 (11): 3439–79.
- Gibbons, Stephen, Henry G. Overman, and Matti Sarvimäki.** 2011. "The Impact of Subsidizing Commercial Space in Deprived Neighbourhoods." Unpublished.

- Glaeser, Edward L., and Joshua D. Gottlieb. 2008. "The Economics of Place-Making Policies." *Brookings Papers on Economic Activity*: 155–239.
- 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): 881–92.
- González, Xulia, Jordi Jaumandreu, and Consuelo Pazó. 2005. "Barriers to Innovation and Subsidy Effectiveness." *RAND Journal of Economics* 36 (4): 930–50.
- Goosbee, Austan. 1998. "Does Government R&D Policy Mainly Benefit Scientists and Engineers?" *American Economic Review* 88 (2): 298–302.
- Görg, Holger, and Eric Strobl. 2007. "The Effect of R&D Subsidies on Private R&D." *Economica* 74 (294): 215–34.
- Gruber, Jon, and Emmanuel Saez. 2002. "The Elasticity of Taxable Income: Evidence and Implications." *Journal of Public Economics* 84 (1): 1–32.
- Haltiwanger, John, Ron S. Jarmin, and Javier Miranda. 2013. "Who Creates Jobs? Small versus Large versus Young." *Review of Economics and Statistics* 95 (2): 347–61.
- Hamermesh, Daniel S. 1990. *Labor Demand*. Princeton, NJ: Princeton University Press.
- Harris, Richard, and Catherine Robinson. 2005. "Impact of Regional Selective Assistance on Sources of Productivity Growth: Plant-Level Evidence from UK Manufacturing, 1990–98." *Regional Studies* 39 (6): 751–65.
- 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)." Department for Business, Enterprise, and Regulatory Reform Occasional Paper 2.
- Heckman, James J., Hidehiko Ichimura, and Petra E. Todd. 1997. "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme." *Review of Economic Studies* 64 (4): 605–54.
- Holmes, Thomas J. 1998. "The Effect of State Policies on the Location of Manufacturing: Evidence from State Borders." *Journal of Political Economy* 106 (4): 667–705.
- Howell, Sabrina T. 2017. "Financing Innovation: Evidence from R&D Grants." *American Economic Review* 107 (4): 1136–64.
- Hsieh, Chang-Tai, and Peter J. Klenow. 2009. "Misallocation and Manufacturing TFP in China and India." *Quarterly Journal of Economics* 124 (4): 1403–48.
- Imbens, Guido W., and Joshua D. Angrist. 1994. "Identification and Estimation of Local Average Treatment Effects." *Econometrica* 62 (2): 467–75.
- Irwin, Douglas A., and Peter J. Klenow. 1996. "High-Tech R&D Subsidies: Estimating the Effects of Sematech." *Journal of International Economics* 40 (3–4): 323–44.
- Jaffe, Adam B., and Trinh Le. 2015. "The Impact of 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): 517–43.
- King, Mervyn A. 1974. "Taxation and the Cost of Capital." *Review of Economic Studies* 41 (1): 21–35.
- Kline, Patrick, and Enrico Moretti. 2014a. "People, Places, and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs." *Annual Review of Economics* 6: 629–62.
- Kline, Patrick, 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 (1): 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.
- Krueger, Anne O., and Baran Tuncer. 1982. "An Empirical Test of the Infant Industry Argument." *American Economic Review* 72 (5): 1142–52.
- Lach, Saul. 2002. "Do R&D Subsidies Stimulate or Displace Private R&D? Evidence from Israel." *Journal of Industrial Economics* 50 (4): 369–90.
- Lawrence, Robert Z., and David E. Weinstein. 2001. "Trade and Growth: Import Led or Export Led? Evidence from Japan and Korea." In *Rethinking the East Asian Miracle*, edited by Joseph E. Stiglitz and Shahid Yusuf, 379–408. Oxford, UK: Oxford University Press.
- Martin, Philippe, Thierry Mayer, and Florian Mayneris. 2011. "Public Support to Clusters." *Regional Science and Urban Economics* 41 (2): 108–23.
- Mayer, Thierry, Florian Mayneris, and Loriane Py. 2017. "The Impact of Urban Enterprise Zones on Establishment Location Decisions and Labor Market Outcomes: Evidence from France." *Journal of Economic Geography* 17 (4): 709–52.
- Mirrlees, James. 2010. *Mirrlees Review of Taxation*. London: Institute for Fiscal Studies.
- Moretti, Enrico. 2011. "Local Labor Markets." In *Handbook of Labor Economics*, Vol. 4B, edited by Orley Ashenfelter and David Card, 1237–313. Amsterdam: Elsevier.



- National Audit Office.** 2003. *The Department for Trade and Industry: Regional Grants in England*. London: National Audit Office.
- Neumark, David, and Helen Simpson.** 2014. "Place-Based Policies." NBER Working Paper 20049.
- Office for National Statistics.** 2014. *Investment-Impact Analysis of Changes to the Estimation of Gross Fixed Capital Formation and Business Investment for Blue Book 2014*. London: Office for National Statistics.
- Office for National Statistics.** 1973–2008. "Annual Respondents Database, 1973–2008: Secure Access." UK Data Service. <http://doi.org/10.5255/UKDA-SN-6644-5>.
- Pellegrini, Guido, and Teo Muccigrosso.** 2017. "Do Subsidized New Firms Survive Longer? Evidence from a Counterfactual Approach." *Regional Studies* 51 (10): 1483–93.
- Rodrik, Dani.** 2007. *One Economics, Many Recipes*. Princeton, NJ: Princeton University Press.
- Ruane, Frances P.** 1982. "Corporate Income Tax, Investment Grants, and the Cost of Capital." *Journal of Public Economics* 17 (1): 103–09.
- Takalo, Tuomas, Tanja Tanayama, and Otto Toivanen.** 2013. "Estimating the Benefits of Targeted R&D Subsidies." *Review of Economics and Statistics* 95 (1): 255–72.
- Wallsten, Scott J.** 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 (1): 82–100.
- Wilson, Daniel J.** 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): 431–36.
- Wren, Colin.** 2005. "Regional Grants: Are They Worth It?" *Fiscal Studies* 26 (2): 245–75.
- Wren, Colin, and Jim Taylor.** 1999. "Industrial Restructuring and Regional Policy." *Oxford Economic Papers* 51 (3): 487–516.
- Zwick, Eric, and James Mahon.** 2017. "Tax Policy and Heterogeneous Investment Behavior." *American Economic Review* 107 (1): 217–48.



This article has been cited by:

1. Yi Zhou, Chaoran Hu. 2021. Industrial policy and differentiated regional diversifications: Evidence from Chinese cities. *Cities* **119**, 103348. [[Crossref](#)]
2. Shan Zhou, Barry D. Solomon. 2021. The interplay between renewable portfolio standards and voluntary green power markets in the United States. *Renewable Energy* **178**, 720-729. [[Crossref](#)]
3. Alessandro Cusimano, Fabio Mazzola, Sylvain Barde. 2021. Place-based policy in southern Italy: evidence from a dose–response approach. *Regional Studies* **55**:8, 1442-1458. [[Crossref](#)]
4. Jack Strauss, Hongchang Li, Jinli Cui. 2021. High-speed Rail's impact on airline demand and air carbon emissions in China. *Transport Policy* **109**, 85-97. [[Crossref](#)]
5. Keisuke Takano, Hiroyuki Okamuro. 2021. Local R&D support as a driver of network diversification: A cross-regional comparison in Japan. *Science and Public Policy* **37**. . [[Crossref](#)]
6. Stjepan Srhoj, Bruno Škrinjarić, Sonja Radas, Janette Walde. 2021. Small matching grants for women entrepreneurs: lessons from the past recession. *Small Business Economics* **76**. . [[Crossref](#)]
7. Wessel N. Vermeulen. 2021. Stuck outside the Single Market. Evidence from firms in central and eastern Europe. *Journal of Comparative Economics* **41**. . [[Crossref](#)]
8. Wanda Dugiel, Anna Golejewska, Tomasz Skica, Anna Zamojska, Adriana Zabłocka-Abi Yaghi. 2021. Firm-level additionality effects of investments and employment tax credits in Polish Special Economic Zones. *Post-Communist Economies* **28**, 1-16. [[Crossref](#)]
9. Bradley Setzler, Felix Tintelnot. 2021. The Effects of Foreign Multinationals on Workers and Firms in the United States. *The Quarterly Journal of Economics* **136**:3, 1943-1991. [[Crossref](#)]
10. Li Song, Xiaoliang Zhou. 2021. How does industrial policy affect manufacturing carbon emission? Evidence from Chinese provincial sub-sectoral data. *Environmental Science and Pollution Research* **7**. . [[Crossref](#)]
11. Yajie Sun, Wen-Chi Liao. 2021. Resource-Exhausted City Transition to continue industrial development. *China Economic Review* **67**, 101623. [[Crossref](#)]
12. Ondřej Dvouletý, Stjepan Srhoj, Smaranda Pantea. 2021. Public SME grants and firm performance in European Union: A systematic review of empirical evidence. *Small Business Economics* **57**:1, 243-263. [[Crossref](#)]
13. Antonio Estache, Renaud Foucart. 2021. On the political economy of industrial, labor and social reforms as complements. *European Economic Review* **19**, 103789. [[Crossref](#)]
14. Jingting Fan, Ben Zou. 2021. Industrialization from scratch: The “Construction of Third Front” and local economic development in China's hinterland. *Journal of Development Economics* **21**, 102698. [[Crossref](#)]
15. Jingwen Yang, Zili Tang, Qingcui Yuan, Bing Xu. 2021. The economic and social benefits of the government-backed credit guarantee fund under the condition of an economic downturn. *Technological Forecasting and Social Change* **166**, 120632. [[Crossref](#)]
16. Wei Cao, Chunhua Chen, Dequan Jiang, Weiping Li, Ying Zhang. 2021. Industrial policy and non-financial corporations' financialization: evidence from China. *The European Journal of Finance* **3**, 1-19. [[Crossref](#)]
17. Ties Vanthillo, Joris Beckers, Ann Verhetsel. 2021. The changing nature of regional policy in Europe. *Oxford Review of Economic Policy* **37**:1, 201-220. [[Crossref](#)]
18. Ross Brown. 2021. Mission-oriented or mission adrift? A critical examination of mission-oriented innovation policies. *European Planning Studies* **29**:4, 739-761. [[Crossref](#)]

19. Isabela Manelici, Smaranda Pantea. 2021. Industrial policy at work: Evidence from Romania's income tax break for workers in IT. *European Economic Review* **133**, 103674. [[Crossref](#)]
20. Changqing Liu, Lei Li. 2021. Place-based techno-industrial policy and innovation: Government responses to the information revolution in China. *China Economic Review* **66**, 101600. [[Crossref](#)]
21. Giuseppe Albanese, Guido Blasio, Andrea Locatelli. 2021. Does EU regional policy promote local TFP growth? Evidence from the Italian Mezzogiorno. *Papers in Regional Science* **100**:2, 327-348. [[Crossref](#)]
22. Ondřej Dvouletý, Ivana Blažková, Oto Potluka. 2021. Estimating the effects of public subsidies on the performance of supported enterprises across firm sizes. *Research Evaluation* **4**. . [[Crossref](#)]
23. Eva Dettmann, Matthias Brachert, Lutz Schneider, Mirko Titze. 2021. Die Wirkung von GRW-Investitionszuschüssen — ein Beitrag zum Aufholprozess?. *Wirtschaftsdienst* **101**:S1, 26-31. [[Crossref](#)]
24. Barry D. Solomon, Shan Zhou. 2021. Renewable Portfolio Standards: Do Voluntary Goals vs. Mandatory Standards Make a Difference?. *Review of Policy Research* **38**:2, 146-163. [[Crossref](#)]
25. Adam Jakubik, Victor Stolzenburg. 2021. The 'China Shock' revisited: insights from value added trade flows. *Journal of Economic Geography* **21**:1, 67-95. [[Crossref](#)]
26. Stjepan Srhoj, Vanja Vitezić, Janette Walde. 2021. Do small public grants boost tourism firms' performance?. *Tourism Economics* **3**, 135481662199443. [[Crossref](#)]
27. 2021. OUP accepted manuscript. *The World Bank Economic Review* . [[Crossref](#)]
28. Liudmyla Deineko, Olena Tsyplitska, Nadiia Hrebenuk, Oleksandr Deineko. 2021. Transition from Vertical to Horizontal Industrial Policy in Ukraine: Effects on Industrial Sector Growth. *SHS Web of Conferences* **100**, 01005. [[Crossref](#)]
29. Nathaniel Lane. 2021. Manufacturing Revolutions: Industrial Policy and Industrialization in South Korea. *SSRN Electronic Journal* **84**. . [[Crossref](#)]
30. Giovanni Dosi, Andrea Roventini, Emanuele Russo. 2020. Public policies and the art of catching up: matching the historical evidence with a multicountry agent-based model. *Industrial and Corporate Change* **46**. . [[Crossref](#)]
31. Albert Bravo-Biosca. 2020. Experimental Innovation Policy. *Innovation Policy and the Economy* **20**, 191-232. [[Crossref](#)]
32. Hyejin Ku, Uta Schönberg, Ragnhild C. Schreiner. 2020. Do place-based tax incentives create jobs?. *Journal of Public Economics* **191**, 104105. [[Crossref](#)]
33. Antonio Accetturo, Giuseppe Albanese, Alessio D'Ignazio. 2020. A new phoenix? Large plants regeneration policies in Italy. *Journal of Regional Science* **60**:5, 878-902. [[Crossref](#)]
34. Yijia Song, Ruichen Deng, Ruoxi Liu, Qian Peng. 2020. Effects of Special Economic Zones on FDI in Emerging Economies: Does Institutional Quality Matter?. *Sustainability* **12**:20, 8409. [[Crossref](#)]
35. Wanxia Ren, Bing Xue, Jun Yang, Chengpeng Lu. 2020. Effects of the Northeast China Revitalization Strategy on Regional Economic Growth and Social Development. *Chinese Geographical Science* **30**:5, 791-809. [[Crossref](#)]
36. Richard Harris, John Moffat. 2020. The impact of product subsidies on plant-level total factor productivity in Britain , 1997–2014. *Scottish Journal of Political Economy* **67**:4, 387-403. [[Crossref](#)]
37. Augusto Cerqua, Guido Pellegrini. 2020. Local multipliers at work. *Industrial and Corporate Change* **29**:4, 959-977. [[Crossref](#)]
38. Pawel Krolikowski, Mike Zabek, Patrick Coate. 2020. Parental proximity and earnings after job displacements. *Labour Economics* **65**, 101877. [[Crossref](#)]
39. Elias Einiö, Henry G. Overman. 2020. The effects of supporting local business: Evidence from the UK. *Regional Science and Urban Economics* **83**, 103500. [[Crossref](#)]

40. Jingjing Li, Ahmed Abbasi, Amar Cheema, Linda B. Abraham. 2020. Path to Purpose? How Online Customer Journeys Differ for Hedonic Versus Utilitarian Purchases. *Journal of Marketing* **84**:4, 127-146. [[Crossref](#)]
41. Nicholas Bloom, Tarek A. Hassan, Aakash Kalyani, Josh Lerner, Ahmed Tahoun. 2020. The Geography of New Technologies. *Institute for New Economic Thinking Working Paper Series* 1-45. [[Crossref](#)]
42. Stjepan Srhoj, Michal Lapinski, Janette Walde. 2020. Impact evaluation of business development grants on SME performance. *Small Business Economics* **56**1066. . [[Crossref](#)]
43. Nathaniel Lane. 2020. The New Empirics of Industrial Policy. *Journal of Industry, Competition and Trade* **20**:2, 209-234. [[Crossref](#)]
44. Sander Ramboer, Jo Reynaerts. 2020. Indecent proposals: Estimating the impact of regional state aid through EU guideline compliance. *Regional Science and Urban Economics* **82**, 103424. [[Crossref](#)]
45. Yanfeng Lou, Yezhuang Tian, Kai Wang. 2020. The Spillover Effect of US Industrial Subsidies on China's Exports. *Sustainability* **12**:7, 2938. [[Crossref](#)]
46. Junxue Jia, Guangrong Ma, Cong Qin, Liyan Wang. 2020. Place-based policies, state-led industrialisation, and regional development: Evidence from China's Great Western Development Programme. *European Economic Review* **123**, 103398. [[Crossref](#)]
47. Yue Gao, Xian-Liang Tian. 2020. Prefabrication policies and the performance of construction industry in China. *Journal of Cleaner Production* **253**, 120042. [[Crossref](#)]
48. Daniel G. Garrett, Eric Ohrn, Juan Carlos Suárez Serrato. 2020. Tax Policy and Local Labor Market Behavior. *American Economic Review: Insights* **2**:1, 83-100. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
49. Daeheon Choi, Chune Young Chung, Soon-Ihl Samuel Hong, Jason Young. 2020. The Role of Political Collusion in Corporate Performance in the Korean Market. *Sustainability* **12**:5, 2031. [[Crossref](#)]
50. Stjepan Srhoj, Janette Walde. 2020. Getting ready for EU Single Market: The effect of export-oriented grant schemes on firm performance. *Structural Change and Economic Dynamics* **52**, 279-293. [[Crossref](#)]
51. Daniel Da Mata, Guilherme Resende. 2020. Changing the climate for banking: The economic effects of credit in a climate-vulnerable area. *Journal of Development Economics* 102459. [[Crossref](#)]
52. Riccardo Crescenzi, Mara Giua. 2020. One or many Cohesion Policies of the European Union? On the differential economic impacts of Cohesion Policy across member states. *Regional Studies* **54**:1, 10-20. [[Crossref](#)]
53. Bing Xu, Javier Sendra-García, Yanxia Gao, Xiaohui Chen. 2020. Driving total factor productivity: Capital and labor with tax allocation. *Technological Forecasting and Social Change* **150**, 119782. [[Crossref](#)]
54. Panagiotis E. Petrakis, Dionysis G. Valsamis, Kyriaki I. Kafka. Innovation, Creativity and Economic Growth 235-263. [[Crossref](#)]
55. Sumit Agarwal, Wenlan Qian, Ruth Tan. Financial Inclusion and Financial Technology 307-346. [[Crossref](#)]
56. Antonella Ferrara, Lewis Dijkstra, Philip McCann, Rosanna Nisticò. 2020. The Response of Regional Well-Being to EU Cohesion Policy Interventions. *SSRN Electronic Journal* . [[Crossref](#)]
57. Liu Hao. 2020. Patent Citation Network Analysis. *SSRN Electronic Journal* . [[Crossref](#)]
58. Ying Xu. 2020. Industrial Policy and State Ownership: Where Does Credit Go?. *SSRN Electronic Journal* . [[Crossref](#)]

59. Kalim-Ullah Bhat, Shihua Chen, Yan Chen, Khalil Jebran. 2020. Debt capacity, debt choice, and underinvestment problem: Evidence from China. *Economic Research-Ekonomska Istraživanja* **33**:1, 267-287. [[Crossref](#)]
60. Nadine Levratto, Aurelien Quignon. 2020. Innovation Performance and the Signal Effect: Evidence from a European Program. *SSRN Electronic Journal* **2**. . [[Crossref](#)]
61. Raphaela Andres, Timothy DeStefano, Thomas Niebel, Steffen Viete. 2020. Capital Incentive Policies in the Age of Cloud Computing: An Empirical Case Study. *SSRN Electronic Journal* . [[Crossref](#)]
62. Klaas Mulier, Ilia Samarin. 2020. Firm and Sector Heterogeneity in the Effects of Innovation Subsidies: Evidence from Horizon 2020. *SSRN Electronic Journal* . [[Crossref](#)]
63. Ross Brown, Suzanne Mawson. 2019. Entrepreneurial ecosystems and public policy in action: a critique of the latest industrial policy blockbuster. *Cambridge Journal of Regions, Economy and Society* **12**:3, 347-368. [[Crossref](#)]
64. Matthias Brachert, Eva Dettmann, Mirko Titze. 2019. The regional effects of a place-based policy – Causal evidence from Germany. *Regional Science and Urban Economics* **79**, 103483. [[Crossref](#)]
65. Xiuyan Liu, Jiangnan Zeng, Qiyao Zhou. 2019. The chosen fortunate in the urbanization process in China? Evidence from a geographic regression discontinuity study. *Review of Development Economics* **23**:4, 1768-1787. [[Crossref](#)]
66. Nicholas Bloom, John Van Reenen, Heidi Williams. 2019. A toolkit of policies to promote innovation. *Voprosy Ekonomiki* :10, 5-31. [[Crossref](#)]
67. Nicholas Bloom, John Van Reenen, Heidi Williams. 2019. A Toolkit of Policies to Promote Innovation. *Journal of Economic Perspectives* **33**:3, 163-184. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
68. Yi Lu, Jin Wang, Lianming Zhu. 2019. Place-Based Policies, Creation, and Agglomeration Economies: Evidence from China's Economic Zone Program. *American Economic Journal: Economic Policy* **11**:3, 325-360. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]
69. Qiuyun Zhao, Zeyu Li, Zuoxiang Zhao, Jinqiu Ma. 2019. Industrial Policy and Innovation Capability of Strategic Emerging Industries: Empirical Evidence from Chinese New Energy Vehicle Industry. *Sustainability* **11**:10, 2785. [[Crossref](#)]
70. Jan Schnellenbach, Sören Schwuchow. 2019. Die Nationale Industriestrategie 2030: Eine kritische Diskussion. *ORDO* **70**:1, 340-371. [[Crossref](#)]
71. Isabela Manelici, Smaranda Pantea. 2019. Industrial Policy at Work: Evidence from Romania's Income Tax Break for Workers in IT. *SSRN Electronic Journal* . [[Crossref](#)]
72. Salvatore Esposito De Falco, Nicola Cucari. Reasons and opportunism control in public grants policies for development and innovations of businesses 349-361. [[Crossref](#)]
73. Christophe Bellégo, Louis-Daniel Pape. 2019. Dealing with the Log of Zero in Regression Models. *SSRN Electronic Journal* . [[Crossref](#)]
74. Alexandros Fakos. 2019. Industrial Policy, Misallocation, and Aggregate Productivity: Policy Implications of Firm-Specific Distortions. *SSRN Electronic Journal* . [[Crossref](#)]
75. Yanyan Gao, Jianghuai Zheng. 2019. Cleaning the Air Through Pipes? An Evaluation of the Air Pollution Reduction Effect of China's Natural Gas Pipeline Projects. *SSRN Electronic Journal* . [[Crossref](#)]
76. Chang Liu, Li-An Zhou. 2019. Does International Travel Cause Economic Growth? Evidence from China's Deregulation on Foreigners' Traveling. *SSRN Electronic Journal* . [[Crossref](#)]
77. Jacquelyn Pless. 2019. Are 'Complementary Policies' Substitutes? Evidence from R&D Subsidies in the UK. *SSRN Electronic Journal* **60**. . [[Crossref](#)]

78. Matthew Denes, Ran Duchin, John Hackney. 2019. Does Size Matter? Evidence from Changes in Small Business Size Standards. *SSRN Electronic Journal* . [[Crossref](#)]
79. Jennifer Luchs, George A. Plesko, Steven Utke. 2018. Market and Firm Reaction to Targeted Tax Benefits: Evidence From the Tax Reform Act of 1986. *SSRN Electronic Journal* 2. . [[Crossref](#)]