

The causal effect of an industrial policy
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Discussion
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1. I am pleased to discuss this very interesting piece of empirical work, which estimates the causal effect on investment, employment and productivity of an important UK program, “Regional Selective Assistance” (RSA).

2. The paper helps economists and policy makers focus on an important issue, namely the effectiveness of aid to firms. As we know, the global downturn has brought industrial policy back into fashion, and in many countries, fiscal stimulus packages include substantial subsidies to firms. Public support to firms is not new, however. In the past a number of EU governments have been actively engaged in programs to spur investment, innovation and development mostly in lagging areas (in the EU-25 roughly 0.6 per cent of GDP is spent on this each year). The amount of public money earmarked for the support of private firms is also huge outside the EU (and the US is no exception in this regard). Despite the importance of these programs, little is known about how effective they are in reaching their stated goals. This paper by Criscuolo, Martin, Overman and Van Reenen is certainly a step towards filling this gap.

3. The paper poses the right question: what would have happened without the program? In other words, evaluating a program is a counterfactual exercise. Accordingly, the paper exploits program-evaluation methods to come up with the answer about the effectiveness of the intervention. Moreover, program-evaluation methods are skillfully used, in two respects.

4. First, the authors are well aware that selection may be a prominent source of bias. The point is that irrespective of their having received RSA assistance, recipient firms may differ from non-recipient firms in important ways. For instance, if subsidies go to the most productive and innovative firms, then these firms might well invest without aid. If this is the case, then the comparison between recipients and non-recipients might reflect differences that predated the start of the program. A credible estimate of the effect of RSA must therefore be selection-free.

5. The way in which selection is differentiated away is probably the most innovative part of the paper. The authors exploit the EU rules governing the eligibility of UK territories for the policy. As the regulation at the EU level is reasonably exogenous to UK territories’ economic performance, this is a clever empirical strategy for identification.

6. Second, the authors are well aware that the comparison might reflect significant substitution effects. Substitution might occur *within firms*: 1) over time (for instance, to take advantage of the program firms anticipate investment that would have been made later without the subsidy); 2) between techniques (instead of using the most profitable one, firms might choose to use the K/L combination that maximizes the likelihood of receiving the subsidy); 3) between plants (for instance, if a firm has two plants, one in an area eligible for the RSA and another in an ineligible area, it could decide to concentrate production in the first plant). Substitution might also occur *between firms*: 4) located in the same area (for instance, when subsidized firms take some investment opportunities that, without the incentive, would have been exploited by other firms); 5) located in different areas (the program could boost economic activity in an assisted area at the expense of decreasing growth in an unassisted area).

7. The possibility of substitution is dealt with by estimating the effect of the program at different levels of aggregation. For instance, if an effect is found at the plant level and vanishes at the firm level (firms might have plants located in non-program areas), then it is reasonable to suspect that the effect only comes from moving production from unsubsidized to subsidized plants. Similarly, if the effect found for subsidized areas disappears when the estimation also includes surrounding unsubsidized districts, then it is reasonable to suspect that the effect only comes from moving activities from unsubsidized to subsidized areas.

8. By brilliantly solving the difficulties related to selection and providing an extensive check of the potential substitution effects, the paper shows that the RSA has had a positive impact on investment and employment. On the negative side, the effect is found to be limited to small and medium-sized enterprises and does not extend to productivity. Although the paper is sound and the results are reliable, a few critical remarks can be offered.

9. The paper does not say much about the RSA scheme. This is unfortunate, not only because the details of this successful scheme could be of use to policy makers wishing to replicate the program elsewhere, but also because some details would help us to trust the identification strategy. Two examples: First, the assumption maintained is that the RSA discriminates in favor of firms that have experienced negative unobserved transitory shocks (technically, $E(\epsilon_{it}, D_{it}) < 0$). That is, the RSA policy was of the *picking-the-losers* type. This may be reasonable, but it could also be the case that the RSA policy was instead of the *picking-the-winners* type - the kind of approach widely implemented in Europe during the 1990s (in part to justify the use of taxpayers' money). The details about just how the grants were actually allocated among applicant firms are needed to clarify this important point. Second, it is realistic to assume that the disbursement of the money was not a smooth process. Disbursements were probably irregular over the long evaluation period (1986-2004). Having the time profile of the disbursement and correlating, over time, funding with the investment triggered by the program could strengthen the claim that the extra investment was due to the RSA and not to some confounding factor.

10. Among the various sources of substitution that might endanger identification, time substitution can still represent a problem. Indeed, several studies carried out at the Bank of Italy have found it to be the most important source of substitution in the Italian case.¹ My fear is that time substitution can still have a role in the paper because there are changes in eligibility at the EU level in 2000 while the observation window ends in 2004. Therefore, the anticipation effect, which typically materializes just after a policy is introduced and vanishes as time goes by, might drive the increase in investment and employment that subsidized firms exhibited between 2000 and 2004. One way to check if this is the case is to use a longer time series. Another is to derive the results exploiting only the 1993 changes in eligibility. In the latter case, the data available up to 2004 represent a long enough period to make sure that an increase in investment and employment due to anticipation reverses itself through lower accumulation and job creation. On more technical grounds, the LATE, which is identified by comparing areas that become eligible with those that were always eligible, can reflect anticipation effects even more intensively.

11. To make sure that what is identified is the effect of the RSA program one has to be clear about the absence or irrelevance of concurrent programs. To be honest, the paper does provide a control for EU infrastructure funding. However, I believe that this issue should be tackled more carefully. It could be extremely useful to have a section discussing the other policies – both local and national – that were implemented over the long time span 1986-2004; the section could also explain why the effect of the concurrent policies are not going to impact on the results estimated for the RSA program. In addition to industrial policies, labour market policies should also be considered, since their effect can be relevant for all the outcomes considered in the paper. Clearly, concurrent programs or reforms that have across-the-board effects on the eligible areas do not necessarily constitute a source of bias for the within-area results (there would be a problem, instead, if the concurrent programs subsidized the same firms financed by the RSA); however, comparison across territories might be less credible.

12. Let me mention some very minor additional points. a) As reported in Table 2, substantial differences can be found in the pre-intervention observables between participants and non-participants. The matching technique should make these differences much lower. Perhaps a table with the descriptives for the matched sample should also be presented. b) In the first stage of the IV estimation the dummy variables for aid intensity enter with higher point estimates for the areas of higher NGE (the maximum proportion of a firm's investment that can be subsidized by the government). This amounts to saying that allocations of public money were more generous in these areas, as higher aid intensity is associated with a higher number of firms

¹ See R. Bronzini and G. de Blasio, "Evaluating the Impact of Investment Incentives: The Case of Italy's Law 488/1992," *Journal of Urban Economics*, vol. 60 (2006), 327-349, and L. Cannari, L. D'Aurizio and G. de Blasio, "The effectiveness of investment subsidies: Evidence from survey data", *Rivista Italiana degli Economisti*, vol. XII (2007), no. 3.

being treated. Again, this could be corroborated with evidence from administrative data or the public budget.

c) The variable that controls EU infrastructure financing enters with a negative sign (and it is also statistically significant). A line or two explaining what is going on here could be helpful.

13. Chiara Criuscuolo and her co-authors have done an excellent job in this paper. I should add that they have also been extremely fortunate to find signs of effectiveness. In 5 years and roughly 10 papers evaluating Italian programs, we at the Bank have never found any positive effect of aid to firms. This, again, underscores the need for more information on the program details, which are crucial for effectiveness. Hopefully, policy makers will be interested in replicating successful scheme elsewhere.

14. The positive effect of the program does not carry over larger firms. The reason provided in the paper is that larger firms may be effectively “gaming” the system by closing and opening plants. Gaps in effectiveness across firms of different size are thus not due to credit constraints, which are typically more severe for SMEs. This point is very interesting and should be investigated further, perhaps in a companion paper (even though the estimates of the RSA’s effect on productivity provided in Table 5 do not seem to lend much support to this argument).

15. However, the point that aid is effective only for SMEs underscores an important dilemma for countries, such as Italy, where productivity is stagnant in part because the industrial structure is tilted towards SMEs. To make sure that public money is not wasted, financial support should be targeted only to SMEs, but this might create an additional impediment for firms to bulk up. The challenge for the future is to design schemes that are effective but leave firms with the right set of incentives to grow.