Presentation by Tito Bianchi

As the only civil servant on this panel, rather than limiting my remarks to any one paper, I wish to discuss some general issues emerging from this seminar. I shall dwell mainly on the finding of two of the three papers presented today, namely that subsidies are effective only for small firms and not for large ones. This result is of great relevance for public policy, and I will reflect on how we can interpret it and on its implications.

My short presentation is divided into three parts. The first aims at circumscribing or qualifying the main results of this seminar. The second accepts these results as true and interprets them. The third reflects on specific aspects of industrial research policy, which justify the adoption of distinctive methods of inquiry in this field.

Let me cut the **first part** very short because some of the points I meant to make have been covered by previous discussants.

Using balance-sheet data to measure investment in R&D has its limits. Depending on their characteristics, firms may have different incentives to report this kind of expenditure as investment in their accounts.

With respect to Carmignani's paper it should be ensured that the transfers recorded by the database exclude the export subsidies intermediated by banks. Those subsidies are pre-eminently granted to large firms in central and northern Italy and have little to do with investment.

From a regional development perspective, the lack of measurable effects on investment for large firms does not preclude that incentives of this kind could be additional from a geographical point of view. In other words, large firms may have been induced by regional development incentives to invest in lagging areas of the country resources that they would otherwise have invested in central and northern Italy, or abroad for that matter. Several policy measures have precisely this objective and would thus be considered effective from a territorial viewpoint, even if the incentives they provide are found to be ineffective at the firm level.

Turning to the **second part** of my discussion, I shall now assume that the main result of two of the three papers presented today is true and try to provide alternative explanations to those provided in the papers discussed today. The finding I refer to is that incentives appear to be effective only in changing the behaviour of small and medium-sized firms, and ineffective with respect to large ones. How we explain this phenomenon is very relevant to economic development policy because it calls for specific courses of action to eliminate market imperfections.

This result seems reasonable and consistent with the anecdotal evidence that many of us have collected, to the point that policy makers should come to terms with it and design policy accordingly. In fact, I take this result so seriously that these papers leave me with a very practical question unanswered: what is the threshold used in each paper to discriminate the small and medium-sized firms from large ones, for which the policy is declared ineffective? What I understand is that the papers employ a relative threshold, set to divide the sample into two convenient halves. However for policy purposes it is essential to know the absolute value of the threshold. For example, in which of the two classes would we find the typical Italian manufacturing firms employing 40 to 90 employees, whose owners are directly involved in management? As we all know, the standard classification of firms used for policy purposes divides them into three size brackets – small, medium and large – where the central one (medium firms) range from 50 to 249 employees.

When it comes to interpreting this result, the papers explain it mostly by referring to the notion of financial constraints on investment. Large firms are not credit rationed on the financial markets and therefore when they are offered grants or subsidized credit, they replace the credit they would have collected at market rates with the less expensive subsidized finance, without changing the amount they invest. However, what we know from case studies is that with respect to investment in research, it is not only access to finance, but the very decision-making process that differs between small and large firms. For large firms, especially when they are multinational corporations, investment in research seems to be absolutely necessary for survival and is therefore planned in cycles that are largely independent from the financial source eventually used to fund them. If this is indeed the behaviour of large firms, its explanation can hardly be reduced to the notion of financial constraints alone; there may be other ways to explain and model it.

I leave it to the academics in this room to think of ways to model firm investment in research, which correctly represent and explain their different investment behaviour based on size. However,

here are some ideas. First, we might expect the level of investment of large firms to be rigid in the short-run because of discontinuities or lumpiness. Indeed, applied case-based research seems to confirm that for large firms or groups, reaching the (optimal) levels of planned investment takes time as research projects are multi-annual. One could reach the same formal result in terms of firm behaviour by hypothesizing that it is the marginal return line, and not only the marginal cost, which could have a different slope for large firms compared with small ones. For example, it could have a steeper or even vertical slope. This goes to show that there may be alternative explanations not based on differential access to finance, which are at least as convincing for explaining the lack of effectiveness or additionality of incentives to research activity for large firms.

Finally, I would like to point to one element of these otherwise very admirable examples of evaluation research, that I find unsatisfactory when it comes to the specific sectoral policy being evaluated. I am referring to the choice of the level of investment in R&D as the only variable for measuring the results of industrial research policy. I don't mean to deny the merits of this measure, of which I am aware, but I would like to point out the limits that stem from measuring the results of research policy in this way. First of all, the sheer level of investment does not capture the degree of innovativeness or risk of each research project, which cannot in any way be approximated by its monetary cost. This qualitative characteristic of research activity should be an explicit target of public policy, as the authors implicitly acknowledge when they introduce the notion of marginal research projects and declare that it is only this type of project that public incentives should subsidize. The marginal projects are the more ambitious and risky ones, which would not be undertaken by firms without the extra incentives provided by the state.

The point I would like to make here is that neglecting this aspect is not only a problem of evaluation, but of policy implementation itself. Very few incentive measures lend this aspect the weight it deserves amongst the criteria used to select the research projects.

The riskiness of research may be harder to measure, but represents a closer proxy than the value of investment per se, of the outcomes that we should be really interested in measuring when we try to evaluate research policy. In fact, as everyone in this room knows, the rationale for funding private research conducted by firms lies in the externalities they produce in the economy and society at large, and certainly not in the results they produce for the recipient firms. These results should accordingly be measured at the societal level in terms of general competitiveness, propensity

to innovate, new products and processes, enhanced competitiveness, expansion of the knowledge economy, and so on.

The rebuttals I expect the professional evaluators like those whose work was presented today, to oppose to this critic are: (1) that there are very few alternatives to the use of this measure if we want to use quasi-experimental counterfactual methods; (2) that the monetary additionality is necessary in order to obtain results in terms of externalities at the societal level. All in all, most evaluators of these kinds of policy would probably agree that broader outcomes on industry and in society are what we should be really measuring when we evaluate the impact of industrial research policy. However, they may still defend their work by saying that the level of investment of the recipient firms relative to the others is an intermediate result towards those ends, and the only one that we can try to connect to the policy intervention in causal terms.

There is a final argument that I wish to make against the use of this variable as the only measure of the result, which has less to do with evaluation methods and more with the policy context in which we currently operate. The shortage of public funds to promote traditional private investment that we are experiencing leaves the incentives for industrial research as the main, and in some cases only, form of public subsidy available for firms. Under these conditions, there is an increasing possibility that traditional investment plans will be somehow disguised as industrial research and submitted for public support by funding lines meant to support only very ambitious and risky investment such as research. In this situation, evaluating the results of these policies by measuring the level of investment only, even if done in good faith by researchers who are very aware of the limits of this method, tends to legitimate the inefficient allocation of these funds to firms whose investment projects do not deserve them. It fails to recall what the correct policy goals of these policies are to those who have lost sight of them or have an interest in “forgetting” them; nor does it provide the right incentives to policy managers to implement these measures more effectively.

Evaluating this form of industrial policy exactly in the same way as we evaluate traditional incentives perpetuates the view that industrial research is akin to other forms of support for firm investment. The ultimate goal of evaluation, after all, is to push public expenditure towards effectiveness: in this case the effectiveness of public intervention should consist in funding very ambitious and risky projects, the majority of which are inevitably going to fail.