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DO INNOVATION INCENTIVES WORK?
EVIDENCE FROM THE ITALIAN MANUFACTURING SECTOR

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Abstract

Understanding and estimating the impact of fiscal incentives on innovation are crucial elements for policy evaluation. It is so because innovation- being it of the product or the process type- is able to move the production frontier and hence, ultimately, enlarge society's consumption possibilities (see the endogenous growth literature). However, despite the fact that innovation outcomes are the really important policy variables, in the past most studies have been looking at the relationship between R&D intensity and fiscal policy, perhaps considering that the relationship between innovation and R&D is strong and deterministic. However, there exists evidence that such a relationship is neither strong nor deterministic. In fact, the innovation creation process is a sort of black box to which many factors contribute (including R&D) and so the interesting policy question for us becomes: do fiscal incentives designed–directly or indirectly (i.e. through R&D) - to promote innovation work? The fact that the relationship between R&D is weak casts some doubts on the impact of R&D subsidies on innovation but we have to remember that some tax incentives are often geared towards activities that foster innovation directly, without having to pass through R&D (this is the case if there are incentives for the acquisition of capital goods embedding a better technology).

Hence, the main purpose of this study is to investigate upon the impact that fiscal incentives have on firm's innovative performance. For this we use data from the 7th, 8th and 9th waves of the "*Indagine sulle Imprese Manifatturiere Italiane*" by Unicredit (previously managed by Capitalia-Mediocredito Centrale), which contains information on both product and process innovation by manufacturing firms, on the amount of resources invested in R&D (if such amount is positive) and it is also informative of the existence of forms of fiscal incentive for R&D and investment in innovative activities. This information is crucial for our study since it permits us to link firm's innovation (the dependent variable in our exercise) to fiscal incentives.

In our work we use different techniques. First we look at Average Treatment Effects, under the assumption of "selection on observables", implying that the econometrician has access to all the variables affecting the likelihood of being treated (i.e. have access to some sort of fiscal incentive for innovative activities). In this part of the paper we just want to verify whether- everything else

constant (i.e. for a given value of the propensity score obtained with the conditioning variables) - there is evidence that firms that have access to fiscal incentives tend to innovate more. In the second part of our study we cast some doubts on the plausibility of the "selection on observables" assumption and we look more in depth at one specific case of fiscal incentive: the one provided by Law 140/1999 to firms located in "depressed areas" (as defined by the law itself). We focus on this law because it is particularly important from a policy perspective within the Italian dual economy, but also because it allows us a more precise estimate of the treatment effect in a situation where treatment status (i.e. access to the incentive) is likely to depend to the same (unobserved) factors that affect the innovation outcome. In such a situation OLS estimated are biased and inconsistent and we have to use instrumental variable estimation. We choose to instrument treatment using the eligibility rules for treatment and we find the confirmation that indeed an endogeneity issue exists and that its effects are stronger the weaker is the impact of the treatment on the outcome variable.

1. INTRODUCTION

Understanding and estimating the impact of fiscal incentives on innovation (being it of the product or the process type) are crucial elements for policy evaluation. It is so because innovation is able to shift upwards the production frontier and hence, ultimately, to enlarge society's consumption possibilities. However, despite the fact that innovation outcomes are the main policy targets, in the recent past most studies have focussed on the relationship between R&D intensity and fiscal policy, considering the link between innovation and R&D strong and easily predictable. However, various studies (see Cohen and Klepper, 1996) have documented that such a relationship is sometimes weak and, in most of the cases, unpredictable. Observing that R&D is one of the inputs of the innovation creation process, this paper aims at addressing the following question: do fiscal incentives designed-directly or indirectly (i.e. through R&D) to promote innovation work?

The fact that the relationship between R&D is weak casts some doubts on the impact of R&D subsidies on innovation but often tax incentives are geared towards activities that foster innovation directly, without having to pass through R&D (this is the case if there are incentives for the acquisition of capital goods embedding a better technology). Hence, the main purpose of this study is to investigate the impact of fiscal incentives on firms' innovative performance, controlling for R&D intensity. For this we use data from the 7th, 8th and 9th waves of the "*Indagine sulle Imprese Manifatturiere Italiane*" by Unicredit (previously managed by Capitalia-Mediocredito Centrale). This Italian survey contains information on both product and process innovation by manufacturing firms, on the amount of resources invested in R&D (if such amount is positive), and on the use of fiscal incentives for R&D and investment in innovative activities. This information is crucial for our study since it allows us to link firm's innovative activity (the dependent variable in our exercise) to fiscal incentives.

This paper proceeds as follows. Section2 discusses the problem of evaluating the impact of policies directed at stimulating investments and innovation, while Section3 contains an estimation of the

Average Treatment Effect of the impact of fiscal subsidies to innovative activities. In that part of the paper we just want to verify whether, everything else constant, there is evidence that firms having access to some sort of fiscal incentives tend to innovate more. Section 4 contains the detailed study of the impact of Law 140/1997, first implemented in 1998, which introduced a tax credit for firms investing in innovative activities (directly or indirectly through R&D) in depressed areas (as defined by the Law). We focus on this law because it is particularly important from a policy perspective within the Italian dual economy, but also because it provides us with a more precise estimate of the treatment effect in a situation where the treatment status (i.e. access to the incentive) is might depend on the same (unobserved) factors that affect the innovation outcome. Since in such a situation OLS estimations are biased, we use the instrumental variables methodology. We choose to instrument treatment according to the eligibility rules for treatment and we find that there is indeed confirmation of an endogeneity issue whose effects are stronger the weaker the impact of the treatment on the outcome variable. Finally, section 5 concludes our work.

2. LITERATURE REVIEW

Evaluating the impact of incentives for R&D/innovation is a complex task: as documented by David, Hall and Toole in their review paper, there is no conclusive support in favor of the hypothesis of a positive impact of subsidies on R&D expenditure. This is mainly due to substitution effects between public and private R&D effort[1] (David et al., 2000).

At the theoretical level, the justification for an aid arises from the fact that the socially efficient level of R&D investment (taken as a proxy for innovation) is higher than the optimal private value. At the empirical level, however, we would like to know what would have been the value for the outcome variable (R&D intensity or the existence of process or product innovation) in the absence of the incentive, but such value –by definition- cannot be observed for firms that have received the subsidy. In other words, we do not know what would have been the behavior of a treated firm in absence of treatment. Similarly, we have no counterfactuals for the non-treated firms. This is a well-known problem in policy evaluation analysis (see for instance Neyman, 1923 and Rubin, 1974, 1978, 1980, 1986) and several methods can be used to circumvent it. What is common to all these approaches is that they attempt to identify the most appropriate control group[2].

A solution can be found in case of randomized processes (this happens when the incentive is made available to firms on the basis of a random process). In this situation we do not expect structural differences between those who are treated and those who are not, so that we can use the second

[1] Perfect substitution happens when an increase in R&D financed by the public sector is followed by a one-to-one reduction in private sector financing, so that the overall value for R&D expenses is left unchanged. In practice it is rare to observe perfect substitution, but a significant amount of substitution is documented in the study by David et al. (2000).

[2] For an introduction to policy evaluation see Khandker, Koolwal and Samad (2010).

one as a control group for the former. However, randomized processes are very rare in social sciences, due both to the nature of the intervention and to ethical reasons (e.g. Rossi et al., 2004). In fact, most of the times we face situations in which: i) a given subsidy is offered to firms that satisfy some eligibility criteria; ii) it is not possible to assume that all the firms satisfying these criteria apply for the subsidy. Therefore the process determining the treatment exposition is quite complex and likely to depend upon many factors, some of which are not observed by the econometrician.

One solution that has been proposed in the literature is the use of the so called “regression discontinuity design”. This method can be applied to situations in which it is possible to identify a clear cut-off in access to the treatment and in which treatment status is based on observable characteristics. In this case the cut-off is defined by the eligibility rules of the incentives so that the treatment group is made of the firms that just satisfy these criteria and have access to the subsidy, whereas the control group is composed of the firms that are just below the cut-off level and do not have access to the subsidy. In such a circumstance it is reasonable to assume that the control group and the treated groups are very similar on every ground, and that the small difference in the variables guaranteeing access to treatment are not sufficient to justify a different value of the outcome variable, so that a difference in the latter can be entirely attributed to treatment. An application of this methodology to a set up close to ours can be found in de Blasio, Fantino and Pellegrini (2010). In that paper the authors study the impact of the *Italian Fund for Technological Innovation* on firms’ innovation, exploiting the fact that the financing of the fund was unexpectedly suspended for 5 years. The intuition is that firms who applied just before the suspension are not different from firms applying right after the suspension.

An alternative method is the use of quasi-natural experiments. This happens, for instance, when a new legislation affecting all the firms is implemented. In this case it is possible to appraise the impact of a reform by comparing firms’ behavior before and after its adoption. However this does not work when treatment exposure (i.e. the application of the reform) is not mandatory and depends upon some selection process that needs to be controlled for. Since the decision to use fiscal incentives depends upon firms’ not fully observable characteristics this approach is rarely applied to the case of firms’ subsidization (but could be applied in case of a ,say, a fiscal reform that reduces the marginal tax rate for every firm: see Hasset and Hubbard, 2002).

A second alternative is the use of propensity score matching. This approach, quite common in the literature that examines the impact of fiscal incentives on R&D intensity, is based on the intuition that, for each firm that has been treated, it is possible to find at least one non-treated firm that is “close” enough to the first one. In this context “close” means that it exhibits a value for the propensity score very similar (if not identical) to the one observed for the treated firm. The propensity score is defined as the conditional probability of receiving the treatment. To improve the likelihood of exogeneity, conditioning variables are often evaluated at time *t-1*,where *t* is the time of treatment. After having computed the propensity scores for all the firms in the dataset, it is possible to use this value to match firms in the treated group with at least one firm in the control group. There are various techniques for doing this, some use replacement while others do not, and some use more complex definitions of distance, but the logic in all these cases is very similar: find a close match for the treated within the group of untreated, using the values for the propensity scores. Notice that this approach works if the analyst is able to control for all the variables determining the treatment status (the so called “selection on observables” assumption); if not, there is a selection bias issue. The advantage of the propensity score matching over the alternative of directly inserting into the main regression the conditioning variables used for its estimation is that –with propensity score- the estimates are less dependent upon the functional form used to model the impact of the exogenous variables.

A method resulting from the combination of difference-in-difference and propensity score approaches can be used in presence of firm’s specific fixed effect (for an application see Bondonio and Engberg, 2000). This method consists in the following two steps: first, the dependent variable is expressed as first-differences and, second, the coefficient on the treatment status is estimated controlling for the propensity score. In this way the fixed effects are controlled for by taking the first-differences, while the propensity score controls for the factors affecting the variation in the dependent variable. This type of approach works well when the dependent variable is continuous, but can create problems when (as in our case) the dependent variable is dichotomous.

Applications of propensity score methodology can be found in studies that try to estimate the impact of R&D incentives on R&D intensity or innovation.

Bérubè and Mohnen (2009) look at the impact of R&D grants on firms’ innovation among a subset of firms already receiving tax credits[3]: using data from the 2005 Survey of Innovation from Statistics Canada and applying a non-parametric matching estimator and find a positive impact of tax grants. Gonzalez and Pazò (2008) look at the impact of public R&D incentives on R&D expenses and R&D intensity for a sample of Spanish manufacturing firms. The authors are particularly interested in exploring the extent of substitution between public and private financing of R&D and, using a bias-corrected matching estimator, they find no evidence of crowding-out by public subsidies (i.e. firms do not use public funding to reduce their private funding of R&D). For a similar study on Ireland see Gorg and Strobl, (2007), while for Germany see Czarnitzki and Frier (2002)

Furthermore, in alternative to the methodologies previously discussed, the analyst can use an instrumental variable estimation approach. This technique no longer assumes that the researcher is able to control for all the factors affecting the treatment status (directly or indirectly, through the propensity score) but explicitly assumes that the treatment status may be endogenous (i.e. there might be non-observable factors affecting both the treatment status and the dependent variable). In this case it is necessary to find an instrument for treatment: such instrument has to be strongly correlated with the treatment variable and not correlated with the endogenous one. The first condition can be tested, while the second cannot and has to be justified in the context of the empirical exercise.

[3] So that they are really comparing firms that receive only tax credit to firms receiving both a tax credit and a grant.

Finally, researcher on the impact of fiscal policies can opt for structural econometric models. In this case it is customary to express the dependent variable (innovation or R&D expenditure) as a function of explanatory variables, among which the user's cost of capital. Fiscal incentives generally reduce the user's cost of capital (through effective tax rates), but their impact on the dependent variable can be estimated only if there is enough variation in effective tax rates. However this condition is not always satisfied, given that tax reforms are rare and that nominal tax rate tend not to vary much across firms. Hence, if one is interested in pursuing this type of modeling strategy, it is necessary to obtain the maximum variation in effective rates. This can be obtained, for instance, by exploiting tax asymmetries (see Biagi and Arachi, 2005).

3. PROPENSITY SCORE

Our data come from the 7th, 8th and 9th waves of the "*Indagine sulle Imprese Manifatturiere Italiane*" by Unicredit, previously managed by Capitalia-Mediocredito Centrale. These surveys were conducted in 1998, 2001 and 2004 respectively, through questionnaires handed to a representative sample of manufacturing firms within the national borders, and supplemented with standard balance-sheet data. Each questionnaire collects retrospective information over the previous three years.

Each survey contains about 4.500 manufacturing firms[4], and the structure of its questionnaire imposes some restrictions on our research. In each wave the sample is selected with a stratified random method based on geographical area, industry and firm size for firms with up to 500 workers, whereas firms above this threshold are all included. As a result of this sampling method, each surveys contains on average about 32% of the firms included in the previous survey.

While some variables are recorded for each of the three years covered by each wave of the survey (for instance, revenues), for other variables (such as innovation) we have a unique value per wave. In particular, for innovation, the questionnaire asks the firm whether in the previous three years it has implemented either product or process innovation or both. Similarly, the questionnaire asks whether in the previous three years the firm has engaged in R&D and-if the answer is yes- how much it has invested in R&D.

In the remaining part of the section we look at the estimation of an Average Treatment Effect on the Treated, using propensity score matching.

The first step in implementing propensity score matching is the creation of our dataset. We start creating two separate longitudinal datasets. The first is made up by firms observed in waves 7 and 8, while the second is made up by firms observed in waves 8 and 9. For each of these panels we then select the conditioning variables used when computing the propensity score for the exposure to treatment. Propensity scores are computed separately for each panel. Treatment (*T*) is a

[4] More precisely, the 7th wave contains 4.497 observations; the 8th wave 4.680 and the 9th 4.289.

variable taking a value of 1 when a firm receives some public subsidy to innovation (either directly or indirectly, through R&D).

The variables used to estimate the propensity score for each panel are: a dummy which takes a value of one if the firm in the previous wave has performed some R&D (*R&D_dummy*), a dummy equal to one if the firm in the previous wave has obtained subsidies (*subs*), the average number of workers employed in the previous wave (capturing size effects), the firm's average market share in the previous wave (*share*), the average value for the Herfindahl-Hirschman Index in the previous wave (this is a sector-wave specific controller), dummies for the area of the country in which the firm is located (*North East* – the reference area-, *North West*, *Center*, *South and Islands*), and dummies for Pavitt sector taxonomy (*supplier dominated* –the reference sector-,*scale-intensive sector*, *specialized sector*, and *science based*). Table A1 in the Annex presents some descriptive statistics of the variables used in this paper.

Notice that the values for the conditioning variables are all computed using values as of wave *t-1* (i.e. the 7th for the panel 7-8, the 8th for the panel 8-9). The results for the estimation of the propensity scores for the two panels are presented in Table 1.

TABLE 1: Propensity score estimation

	Panel7-8	Panel8-9
T	coeff (sd)	coeff (sd)
R&D_dummy	.7179859	.7172595
	(.1067535)**	(.1015383)**
Subs	.5072874	.9449705
	(.1807864)**	(.1169555)**
workers	.0001226	.0002602
	(.0002151)	(.0002414)
share	.1675722	-3.109477
	(.90698)	(1.862039)*
HHI	-.2846284	-1.252725
	(.4817199)	(.560182)**
Center	-.0380264	.0154553
	(.153488)	(.1257803)
South-Islands	.2707846	-.4702384
	(.1748939)	(.1853125)**
North-West	-.0880901	-.0020616
	(.1193123)	(.1038401)
Scale_intensive	.1869146	.1915241
	(.1451308)	(.1577649)
specialized	.3363143	.2692738
	(.1238822)**	(.1043036)**
science_based	.4176959	.3780443
	(.2495953)**	(.2174704)*
_cons	-1.627732	-1.688407
	(.1269978)**	(.1119425)**
Number of obs	1159	1774
Prob > chi2	0.0000	0.0000

Log likelihood	-402.42513	-511.23658

**p≤.05; *.05<p≤.1

Our approach implies that, for instance, when computing the propensity score in the panel made up by firms observed in the 7th and 8th wave, the conditioning variables appear with their value in the 7th wave, while the treatment status is defined relative to the 8th wave: hence we are effectively using pre-treatment values to compute the propensity score. This solution is preferable because it reduces (but not eliminates) endogeneity issues that might arise if we used contemporaneous values[5]. We compute the propensity scores using a kernel methodology[6] and we restrict our attention to the common support area, i.e. in the main regression we do not consider treated firms that have a propensity score lower than the minimum or higher than the maximum propensity score for the control group). At this point, for each panel we only keep the latest wave (because we want treatment defined on the latest year and the first year is just used for conditioning).

Once we have computed the propensity scores separately for the two panels we put them together, basically obtaining two cross-sections (one with firms observed in waves 7 and 8, and another one with firms observed in waves 8 and 9). Notice that in each cross-section we have only one observation per firm (belonging to the latest wave: the 8th for panel 7-8 and the 9th for panel 8-9). However some firms might be present in both cross-sections and this is controlled for when computing standard errors for the main regression.

After having obtained our final dataset we run the main regression, which is expressed as

$$Prob(Innovation_{it}) = F(pscore78_{it}, pscore89_{it}, T_{it}, Panel89_{it}, T_{it} * Panel9_{it})$$

where the relationship between the probability of observing process or product innovation (or both) and the explanatory variables is modeled with a *probit* (allowing errors to be correlated across time for the firms that we observe in both panels). The explanatory variables are the values for the propensity score computed for the panel 7-8 (*pscore78*), the propensity score for the panel 8-9 (*pscore89*), the dummy for treatment (*T*), a dummy for observations belonging to the panel 8-9 (*Panel89*, which effectively controls for wave-specific common effects), and an interaction term between the treatment dummy and the wave 8-9 dummy (*T*Panel89*), controlling for the possibility that the impact of treatment changes from one wave to the other (i.e. from one cross-section to the other). Results are presented in Table 2.

[5] Effectively, the panel made up by firms existing in two adjacent waves is simply a cross section with lagged observations for the variables used in the computation of the propensity score.

[6] Each treated firm is compared to a weighted average of all the control units. The weights are inversely proportional to the distance between the propensity score of the treated and those of the control units.

TABLE 2: Probability of observing process or product innovation（propensity scores as regressors）

innoboth_pp	Coeff (sd)
T	.611503
	(.1176149)**
pscore78	3.202711
	(.393088)**
pscore89	2.662894
	(.2869996)**
Panel89	.3910907
	(.0780681)**
T*Panel89	.052756
	(.1647838)
_cons	-.9974763
	(.065634)**
Number of obs	2933
Prob > chi2	0.0000
Log likelihood	-1738.0604

**p≤.05; *.05<p≤.1

We can observe that the coefficients on the two propensity scores are both positive and highly significant, showing that our conditioning variables capture cross-firm variation in initial conditions. Coming now to the treatment effect, we find a positive and highly significant coefficient on the treatment dummy, showing that indeed, firms having access to some kind of subsidy tend to innovate more. We also notice that there is a strong panel effect (firms in panel 8-9 tend to be more innovative altogether). However we find no evidence that the impact of treatment changes across waves (the coefficient on the interaction term between the treatment and the panel 8-9 dummy is not significant).

Overall, we interpret these results as reassuring, in the sense that tax incentives (i.e. access to public subsidies) show an economic and significant impact on the likelihood that firms obtain some kind of product or process innovation. However, as previously mentioned, these results are valid only if the assumption of "selection on observables" holds. In the next session we explore the possibility that the treatment status is endogenous and determined by factors that are likely to be correlated with the outcome variable. Yet, since such exercise requires the finding of appropriate instruments, we are able to conduct our analysis only for a specific type of subsidy (incentives to innovation for firms located in depressed areas, as of Law 140/1997).

4. INSTRUMENTAL VARIABLES

In the previous section we have documented that fiscal incentives have a positive impact on innovation (being it process or product). This result was obtained through a propensity score methodology, in which we have first computed the likelihood that treatment is observed, conditional on a set of variables, and then –controlling for the value of the propensity score- we have tested whether the treatment status (i.e. access to some kind of fiscal incentive for innovative activity) is positively associated with the likelihood of observing product and/or process innovation. The propensity score methodology allows us to compare the treatment group with the proper control group, as expressed by the value of the propensity score, avoiding the problem of functional form dependency. However, such a methodology is appropriate if we think that –when computing the propensity score- we are controlling for all the factors that might affect the probability of observing treatment. This is equivalent to say that there are no unobserved variables that could explain treatment exposition. As such, this is an assumption and cannot be tested (the only test possible is to add another variable and see if it affects the likelihood of treatment) but we suspect that there might exists factors not observed by the researcher which could explain treatment. Even worse, some of these factors might be correlated with the probability of innovating. To solve this problem we need to adopt a different methodology, one using instrumental variables. That is, we have to find variables that are significantly correlated with the treatment dummy and that, in turn, do not affect our dependent variable (presence of innovation). A particularly good instrumental variable can be found in the eligibility criteria for the fiscal incentive. That is, we could instrument the treatment dummy by the criteria that firms have to fulfill in order to be eligible for the treatment. This requires some observations prior to the introduction of the fiscal incentive and some following its introduction (e.g. if a given incentive is introduced in time *t=1*, we need observations at time *t=0* and *t=2*): in the period after the introduction some firms will have access to the incentive while other do not, and –given that we cannot exclude that access to treatment is due to some non-observable factors- we have to instrument for treatment access. The implication of this is that we cannot simply introduce a dummy for treatment because such dummy –even when estimated via a propensity score methodology- could be capturing the effects of unobserved factors (such as management style) that are both correlated to the likelihood of treatment and to the outcome variable. The introduction of a new incentive schema (so that we have an ex-ante and an ex-post period) with its specific eligibility criteria (so that not every firm has access to fiscal incentives and for the same amount) allows us to instrument for the treatment.

Still, the drawback of this procedure is that we have to restrict our analysis to a sub-sample of our dataset. This is because for many types of fiscal incentives used by firms in the relevant interval (1995-2003) we cannot find a clear pre/post reform separation. Most fiscal incentives were introduced prior to 1995 and they lasted for the whole period. So our strategy is to look for the impact of one reform for which the conditions for identification are satisfied. This is the so called “Visco reform” of 1997, as expressed by Law 140/1997 and by ”Circolare 9002/1988”. This is a legislation introducing a tax credit for firms investing in technological improvement for innovation purposes and located in “disadvantaged areas”. These areas coincide with Objective 1 and

Objective 2 areas[7]. In practice, all firms located in Southern Regions have access to such incentives, but also some firms in the Center and Northern Regions, as long as they reside in some of the Municipalities that are specifically mentioned by the decree. Finally, the amount of the tax credit varies depending upon firms' size (three categories, small, medium and large) and upon its degree of disadvantagement (larger for firms located in Objective 1 areas). All this variation in the accessibility criteria is very important for us, since it creates variation in the likely impact of treatment on the endogenous variable.

The types of activities for which Law 140/1997 provides a tax credit are: 1) acquisition of new knowledge finalized to the creation of new products, new services or new production processes or to the improvement of already existing products and projects; 2) implementation of new knowledge through the creation of pilot projects and prototypes directed at the creation of new processes, products and services or to the improvement of already existing ones. Hence, this law is trying to promote activities that directly or indirectly favor process or product innovation. Among these, some could be R&D activities (and hence R&D expenses), but R&D is not the main focus of the legislation. The costs for which tax credits are available are: a) labor costs for employees engages in activities described at point 1) and 2); b) costs for instruments and capital functional to activities mentioned at points 1) and 2); c) costs for technological counseling and for the acquisition of knowledge related to activities at point 1) and 2); d) a share of overall costs, which is set to be equal to 40% of labor costs described at point a).

The amount of the tax credit depends upon firm's size and upon the areas in which the firm is located, according to table 3:

TABLE 3

Firm Dimension	Areas ex art.92, par3 a) of EU Treaty	Areas ex art.92, par3 c) of EU Treaty	Other areas
Small	30%	25%	20%

[7] According to the 2000-2006 EU's regional policy framework, Objective 1 program operates among areas of most need and supports the development of regions that are significantly falling behind the rest of Europe. Objective 1 is "regionalised", meaning that it applies to designated NUTS level II areas in the Nomenclature of Territorial Units for Statistics developed by Eurostat. Of these geographical areas, only those with a per capita gross domestic product (GDP) lower than 75% of the Community average are eligible under Objective 1. Concerning Objective 2, it aims at supporting the economic and social conversion of areas experiencing structural difficulties. The areas eligible under Objective 2 are those undergoing socio-economic change in the industrial and service sectors, declining rural areas, urban areas in difficulty and depressed areas dependent on fisheries. Like Objective 1, Objective 2 is "regionalised", meaning that it applies to areas defined according to specific statistical and socio-economic criteria. Since the regions covered by this Objective are facing structural difficulties, their eligibility depends on a population ceiling, and on criteria specific to each area. See http://europa.eu/legislation_summaries/regional_policy/provisions_and_instruments/l60013_en.htm)

Medium	25%	20%	15%
Large	20%	15%	10%

The costs relevant for the tax credit are those that are documented by the balance sheet of the year prior to the one in which the firm applies.

Additional tax credits are available for firms engaging in R&D activities (and expenditures). These tax credits are once again changing according to firm's size and to the area in which the firm is located (see table 4).

TABLE 4

Firm Dimension	Areas ex art.92, par3 a) of EU Treaty	Areas ex art.92, par3 c) of EU Treaty	Other areas
Small	6%	5%	4%
Medium	5%	4%	3%
Large	4%	3%	2%

In this case the tax credit is given in relationship to the difference between R&D expenditures in a given year and the moving average of R&D expenditures in the previous three years.

Summarizing: Law 140/1997 introduces tax credits for expenditures that, directly or indirectly, are geared towards process or product innovation. R&D activities are also incentivized, but this is not the main purpose of the law. Finally, incentives are provided only for firms located in depressed areas (where the amount of the tax credit changes according to the type of area and to firms' size).

The model estimated in par. 4 is expressed by

$$y_{it} = \beta * x_{it} + \gamma * Law\_140_{it} + u_{it}$$

where y_{it} is a dummy variable taking a value of one if the firms obtains either process or product innovation (or both), x_{it} is a vector of exogenous explanatory variables (more on this later) and Law_140_{it} is an indicator variable that takes a value of one for firms having access to the tax credit granted by Law 140/1997. Notice that we allow for endogenous treatment, which is instrumented with the following variables: a dummy variable which takes a value of one in periods after the introduction of the reform (i.e. from 1998 onwards), interactions between the after-reform dummy variable and all the variables characterizing eligibility to treatment. By instrumenting with the proposed variables for treatment we are effectively assuming (and testing) that firms qualifying for treatment benefit more from the reform relative to firms that do not have access to fiscal benefits. This should then have an impact on the outcome variable.

Our IV methodology is hence conducted through a two-stage procedure. First we test the relevance of the proposed instruments, that is: i) we test if the conditions defining eligibility are positively related to treatment[8]; ii) then, under the assumption (which cannot be tested) that the eligibility criteria impact the outcome variable only through access to treatment, we are able to estimate the second stage, where the treatment dummy is substituted by the likelihood of treatment as estimated using all the exogenous first stage variables (including eligibility criteria). The use of eligibility criteria (and not simply the treatment status) is fundamental in making this assumptions credible: the fact that a firm has access to fiscal incentives after the reform is introduced (for instance because it is small and located in a depressed area) should not –per se- have an impact on its capability of generating product and/or process innovations[9].

Given our proposed methodology we hence create a longitudinal dataset made up by firms that are observed in the 7th wave (prior to the Visco reform) and then in the 8th and 9th wave.

Due to the structure of our data, at each successive wave only one third on the firms is interviewed. The level of attrition is hence substantial but we cannot control for it and we interpret it as a result of a randomized process, so that our sample of observed firms is still representative of the underlying population. Focusing on the panel made by firms observed in all the successive waves allows us to verify whether the short run effects of the reform (within the first three years from approval) differ from the long run effects[10] (between the first three years and the subsequent three years). Finally, since we want to instrument for the treatment dummy (equal to one for firms have receiving a tax-credit based on Law 140/1997) using the eligibility rules of the reform itself, we need to have all the data that characterize the eligibility rule (among which the municipality in which the firm has its fiscal residence).

With our longitudinal dataset we look at two different time intervals. First, we look at the 7th and 8th waves, and, second, we look at the 7th and 9th waves. In the first case we are really looking at the short-run impact of the reform (applicable for the first time in 1998, the first year of the 8th

[8] Remember that treatment status for a given firm depends upon both the eligibility rules and the firm's own characteristics, some of which are not observable to us. This means that treatment status is very likely to be endogenous.

[9] We are controlling for firms' characteristics even in the period prior to the reform, so that we are really assuming that such factors, after the reform, do not have a impact per se on the outcome variable, but only through the reform, by making possible for the firm to have access to the tax credit.

[10] We could have looked at two separate panes: 1) the panel made up by firms observed in wave 7th and 8th; 2) the panel made up by firms observed in wave 7th and 9th. This would have generated two different longitudinal datasets. However in this case we would not have been able to use IV since we have information location on the firms' seat only for wave 9th. So we are constrained to look at firms which-besides being recorded in wave 7th (the pre-reform period)- are also present in wave 9th.

wave), while in the second case we look at the longer-run impact of the reform. It is likely that the impact over a extended period of time is stronger than the immediate impact of the reform because there is usually a learning effect (it takes time for firms to understand how a new legislation works).

Due to the structure of our data, we use only one observation for each wave and this observation captures the mean of the relevant variable. This is because the information on innovation activity is defined only with reference to the whole wave (i.e. it does not distinguish between the various years). Therefore, all the explanatory variables are computed as wave-specific means (where the means are computed with reference to the three different years covered by the survey). For instance, if among the regressors we want to consider cash flow over sales, a variable which is potentially available for every year, we are going to compute the average value of the ratio over the three years for which it is observable and use this as our explanatory variable. We do this for every variable for which we have yearly observations. This procedure implies that, at the end, for each survey we keep only one observation (the one representing the means).

In Table 5 we present our results for both OLS and IV estimates. Notice that we use a linear probability model (instead of a probit or logit model) because with such model we can easily perform IV estimation, which would not be true for the other types of estimates[11].

The variables assumed to be exogenous are[12]: *share* (the share of firm's i sales within any given sector), *HHI* (the value of the HHI index for the whole sector, computed using sales), *educ_ratio* (the ratio between workers with higher education and workers with low education[13]), *cash_ratio* (the ratio between cash flow and sales), *profit_ratio* (the ratio between profits and sales), *R&D_intensity* (the ratio between R&D expenditures and sales), *other_subsidies* (a dummy equal to one if a firm has benefited from a tax incentive different from those provided by L.140/1997), *group* (a dummy equal to one if the firm belongs to a group); *export* (a dummy equal to one if a firm has exported some of its output), two dummies for size (*small* is the reference group, so that we control for *medium* and *large*), three dummies for the Pavitt sectorial aggregation (the reference sector is *supplier dominated*; the others are: *scale intensive*, *specialized suppliers* and *science based*[14]).

[11] The drawback of this approach is that we cannot be sure that the estimated probabilities lie in the 0-1 interval. This is a not a major problem in our work given that we want to verify whether the Visco reform of 1997 had some impact on innovative behavior.

[12] The variable expressing treatment is *L140* (a dummy equal to one if a firm has benefited from the tax credit provided by L 140/1997).

[13] High education means with completed secondary education or more; low education means with less than secondary education.

[14] The number of firms belonging to the *science based* sector is very low, due to the fact that this meso-sector is made by large firms engaging in high intensity R&D in chemical, electronics and bio-engineering sectors, and such firms are very rare in our sample.

We also control for macro-area dummies: *North_west* (the reference group), *North_east*, *Centre* and *South_islands*, with the intent to capture structural differences in the outcome variables related to such macro-territorial differences. We also control for a finer territorial aspect, which turns out to be important in estimating the impact of the reform: we have dummies for firms located in areas ex art 92.c3a, ex art 92:c3c and other depressed areas. By doing this we can capture variation within macro-areas (we expect that firms located in depressed areas tend to be less innovative) and this also helps in the quest for identification (more on this later).

The variables that we have used are those that the theory usually considers as relevant in accounting for innovative effort. Firms' share might be positively related to the probability of observing innovation if larger firms have more to lose from not innovating (a prediction of some recent Industrial Organization models), while the value for the HHI index is capturing sectorial differences in concentration, which might affect the incentives for innovation (more concentration could lead to more innovation[15]); *cash_ratio* and *profit_ratio* are likely to be positively related to the probability of observing an innovation given that they signal abundance of resources that can be invested to obtain process and/or product innovation (either directly, by purchasing goods and services in which innovation is embedded, or indirectly, through R&D); firms belonging to a group are more likely to be innovative, both because they can have access to a larger knowledge capital (knowledge acquisition is cheaper) and because the gains from innovation might be larger (innovation can be passed to other firms belonging to the group); the export dummy is also likely to be positive related to the likelihood of innovation, given that firms exporting a significant part of their output tend to operate in more competitive environments and hence are more interested in capturing value through process and product innovation.

TABLE 5

	OLS1	IV(2SLS)1	OLS2	IV(2SLS)2
innoboth_pp	coeff (sd)	Coeff (sd)	Coeff (sd)	Coeff (sd)
share	-.1768324	-.2066029	.0634592	.0872179
	(.1769493)	(.1906168)	(.1990089)	(.1689088)
HHI	-.1140505	-.0890083	-.030937	-.0048964
	(.0992527)	(.1067649)	(.0982181)	(.0982693)
educ_ratio	.0012937	.0010234	.0011531	.0025045
	(.0042104)	(.0040701)	(.0045317)	(.0045428)
cash_ratio	-.002397	-.0015099	-.0007524	-.0017987
	(.0017355)	(.0020235)	(.0018543)	(.0019336)
prof_ratio	.0047163	.0040197	.0017462	.0029591
	(.002319)**	(.0023248)*	(.0021313)	(.0022017)
R&D_intensity	.0082378	.0194765	1.735418	1.178044
	(.0043331)*	(.0121035)	(1.148612)	(1.072177)
Other_subsidies	.2168958	.1934606	.1892369	.2516664

[15] Theoretical predictions about the impact of share and HHI on the likelihood of innovation are not unique. Some authors- in the Arrowian tradition- maintain that smaller firms and firms operating in less concentrated market structures gain more from product and process innovation and, hence, they are more likely to innovate.

	(.0600282)**	(.066464)**	(.0708407)**	(.0818015)**
L140/1997	.1767331	-.1001287	.3055718	.6050981
	(.072947)**	(.2888169)	(.0530523)**	(.1848516)**
North_East	.105798	.0957737	.0587668	.068394
	(.0465942)**	(.0503599)*	(.047382)	(.0486823)
Centre	.0789469	.0650832	.0291417	.0277444
	(.0429097)**	(.047447)	(.0449721)	(.0462824)
South_islands	.0574969	.0504615	.1058126	.1246276
	(.102971)	(.0842826)	(.1036248)	(.089359)
gruppo	.1094255	.1116421	.1196525	.1105353
	(.0485319)**	(.0457355)**	(.0452091)**	(.0428173)**
export	.0766634	.0788661	.1208317	.1140716
	(.0339117)**	(.0348473)**	(.0353746)**	(.0365118)**
Scale_intensive	.0990369	.0883629	.0528023	.0475816
	(.044994)**	(.0438241)**	(.0442068)	(.0444938)
Specialized	.1558945	.1648983	.0995944	.0698692
	(.0383753)**	(.0384657)**	(.0375446)**	(.0413136)*
Science_based	.0383753	.0693139	.100859	.085001
	(.1035301)	(.0994212)	(.0966624)**	(.0979082)
Medium_size	-.0675887	-.091463	-.0629276	-.0720293
	(.0747253)	(.0771013)	(.0727739)	(.0726561)
Large	-.2086068	-.245369	-.1842996	-.1775591
	(.0763389)**	(.0837503)**	(.0746536)**	(.0736287)**
area92a	.0121943	.0118013	-.1036773	-.0930249
	(.1116283)	(.0862501)	(.1126618)	(.097134)
area92c	.0314761	.0431727	-.0030095	.0006597
	(.0495326)	(.056822)	(.0495606)	(.057034)
Other_areas	-.0407214	-.0401748	-.0881384	-.0970624
	(.0477818)	(.04716)	(.0492004)*	(.0492403)**
_cons	.2740487	.3187153	.3103851	.2894076
	(.0927778)**	(.1062196)**	(.0941024)**	(.0942509)**
Number of obs	915	915	946	946
Prob > F	0.0000	0.0000	0.0000	0.0000

**p≤.05; *.05<p≤.1

As for size (measured by the workforce) theoretical predictions are not clear-cut, while we would expect that more innovative firms tend to have a higher fraction of workers with higher education. As for the macro-areas dummy we expect firms in the North-east to be more innovative than firms located in other parts of the country and we expect that firms located in depressed areas to be less innovative (but the latter effect might disappear once we control for macro-area dummies[16]). Finally, coming to Pavitt's sectorial aggregation, we expect the science sector to be mostly innovative, followed by specialized suppliers and by scale intensive sectors.

When looking at the short run (the panel made up by firms in the 7th and 8th wave), we notice that in the OLS specification (Table 5, col.1) most of the explanatory variables have the expected sign: *profit_ratio*, the *group* dummy, the *export* dummy are all significant and with a positive coefficient, as theory would predict. We also find that firms in *specialized suppliers* sectors have the highest likelihood of introducing innovation, followed by firms in *scale intensive* sectors. We

[16] We should remember that all the regions in the South of Italy are depressed areas as of art. 92:3a) of the EU Treaty.

do not find evidence that firms in *science based* sectors behave differently from firms in *supplier dominated* sectors (the reference one), but this is likely to depend upon the relatively small number of observations for firms in *science based* sectors[17]. As for our size dummies, we find that–as far as likelihood of innovation is concerned- *medium*-size firms do not perform differently from *small* firms. However, *large* firms seem to innovate less (but this effect might be due to the small number of large firms appearing in our dataset). We also find that *R&D_intensity* is positively and significantly (but only at 90% confidence) correlated with the likelihood of observing innovation, even after controlling for all the other explanatory variables.

As for the macro-areas dummies, we find that firms located in the *North_east* and in the *Center* tend to be more innovative than those located in the *North_west* (but the coefficient for the *Center* dummy is significant only at 90% confidence), while firms located in the *South* do not appear to be more innovative than the reference group. As for the depressed-area dummies (the reference here is given by firms located in non-depressed areas), we find that –once controlled for the macro-areas dummies- they are not significant (this is so by construction for firms located in southern regions).

Concerning *share* and *HHI*, our results indicate that these variables do not have a significant impact on the outcome variable. The same applies for *educ_ratio*, which does not appear to have any significant effect on the dependant variable[18].

Coming now to the variable which is our primary interest, we see that the dummy *L140* enters with a positive and significant coefficient, signaling that firms that benefited from the Visco tax incentives are more likely to innovate. Notice that this result is obtained controlling for both other types of fiscal incentives and for *R&D_intensity*. Given that the Law 140/1997 incentivize R&D effort (relative to the level of the previous three years), and given that R&D per se has a positive impact on the likelihood of innovation, our estimate on the impact of the Visco reform should be considered as an under-estimate of the overall impact (made up by the direct and the indirect effect –i.e. the one operating through R&D). It is important that we control for additional forms of incentives, especially since this might be capturing some of the firms' fixed effects not observable by us.

As previously explained, there are good reasons to believe that the OLS estimates might be affected by omitted variables bias: if treatment is endogenous we have to try to instrument it. Given that we are in presence of a reform we can use the quasi-natural experiment nature of our data and use the eligibility criteria of Law 140/1999 as instruments for treatment. This amount to say that in the first stage regression we regress treatment status on all the exogenous variables (size and Pavitt dummies, group and export dummies, *educ_ratio*, *cash_ratio*, *profit_ratio*,

[17] In both panels, the share of firms belonging to the science-based sector is about 3%.

[18] We have tried with other variables capturing the relative abundance of skills at the firm level and we have obtained very similar results. These results casts doubts about the impact of human capital distribution of firms' performance. Alternatively, they signal that our indexes of human capital are quite non-informative.

R&D_intensity, *other_subsidies*, *share* and *HHI*) and on the variables affecting eligibility. Such variables are: a dummy for post-reform period (i.e. for the 8th wave), interactions between such dummy and the size dummies and interactions between the post-reform dummy and the area specific dummies. These variables are mean to capture the eligibility criteria defined by the reform and the reform itself (captured by the *post_reform* dummy).

Our IV results confirm the relevance of our instruments (the variables used in the first-stage are overall significant: pvalue=0.0000), but the picture that emerges for the variable of interest (the impact of the Visco reform) is quite different. Once controlled for all the exogenous variables, the second stage value for the coefficient on Law_140 is not statistically different from zero. Notice that the significance of the coefficients for the other variables remain similar to the OLS estimates, and so do the values for the estimated coefficients (however *R&D_intensity* is no longer significant, even at 90% confidence level). Finally, we notice that our estimate passes the Hansen's test for overidentification (that is, conditional on one instrument, the other instruments are valid), hence reinforcing our estimation strategy.

What our results hence document is that, once controlled for the endogeneity of treatment, the impact of the Law 140/1997 on the likelihood of innovation **in the short-run** is basically nihil. It is hence interesting to verify whether things change when we extend the view to the period 2001-2003 (**long run**). The first thing that appears from the OLS estimates for the 7th-9th wave transition is that the coefficient on the dummy for the Law 140/1997 is now more significant and higher in absolute value (almost twice as high as its value for the 7th-8th wave transition). This is expected since it takes some time to learn the existence and the functioning of the reform and to react to the new legislation (this is particularly true for the R&D tax incentive which operates when R&D intensity in a given year is higher than the average R&D intensity for the previous three years). We also notice that some variables which appeared to be significant in the short run are no longer significant when considered in the longer transition. This is the case for *profit_ratio*, *R&D_intensity* (signaling that –once controlled for law 140/1997- the additional effects of R&D intensity are small), the dummy for "scale intensive" sectors. We also find that none of the macro-area dummies is significant, but that the coefficient of *other_areas* (i.e. depressed areas different from ex art92.3a) and art. 92.3c) areas) is negative and significant at 90% confidence levels. This indicates that, within a given macro-area, firms located in (some) depressed areas tend to innovate less than the other firms.

Overall our OLS results for the long run are comparable with those obtained for the short run, with the clear indication that the Visco 1997 reform had a larger impact on innovation in the long run. When we turn to IV estimates[19], we notice that the long-run OLS results are confirmed: tax incentive provided by Law 140/1999 appears to significantly and positively affect the likelihood of innovating, and this is true even when controlling for *R&D_intensity* and other types of fiscal

[19] The F-test on the first stage of our IV estimation confirms that the exogenous variables are overall significantly correlated with the treatment status. Notice that this is true also when we regress treatment status on the instruments only.

advantages (different from those provided by the Visco tax credit for depressed areas). The Hansen's test confirms that conditional on one instrument, the other instruments are valid.

5. CONCLUSIONS

Average Treatment effects can be more precisely estimated when we can distinguish a pre-reform and a post-reform period. This simple point is often absent in the literature that studies the effects of tax incentives on R&D intensity (and a fortiori on the literature that studies their impact on innovation). But even in the presence of a structural break, we have to try to control for the endogeneity of treatment. One way to do this is to instrument treatment with the eligibility rules. This can work as long as in the same period there are no major factors independently influencing the observed outcome. In our case, this amounts to saying that the fact of being in a depressed area after 1997 (interacted with size dummies to take into account the size of the incentive), per se, should have no impact on the output variable (likelihood of innovation in our case), while it is positively and significantly correlated with the treatment status. Given all these caveats, our result show that, indeed, Law 140/1997 had a positive effect on the probability of observing process or product innovation only in the long run (i.e. the transition from the 1995-1997 to the 2001-2003 period) and not in the short run (i.e. the transition from the 1995-1997 to the 1998-2000 period). This is somehow expected and it reflects the fact that firms need time to learn and adjust to a new legislation. Notice that this result is obtained while controlling for the presence of other types of subsidies and for R&D intensity, so that it can be considered as a lower-bound estimate of the overall impact of Law 140/1997.

An important lesson that we learn from this study is that it takes time to adjust to reforms and this should induce policy makers to let in place the new legislation for some time before assessing its impacts or reforming it altogether.

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ANNEX

TABLE A1: Descriptive statistics

	Panel7-8		Panel8-9	
	Pre Treatment	Treatment	Pre Treatment	Treatment
	mean (sd)[missing%]	Mean (sd)[missing%]	mean (sd)[missing%]	mean (sd)[missing%]
workers	97.8 (247.5)[.002]	104.5 (273.2)	84.6 (246.2)	87.9 (272.5)
educ_ratio	3.05 (3.97)[.08]	3.87 (5.49)[.16]	3.66 (5.88)[.1]	2.85 (3.5)[.07]
cash_ratio	9.22 (20.64)[0.5]	15.7 (22.03)[.02]	13.8 (18.5)[.01]	14.5 (20.1)[.02]
R&D_intensity	.06 (.93)[.69]	.02 (.37)[63]	.02 (.31)[.66]	.007 (.01)[.63]
share	.004 (.01)[.002]	.008 (.03)	.006 (.03)	.003 (.01)[.003]
HHI	.04 (.05)	.05 (.05)	.05 (.06)	.04 (.05)
innovation[d]	.31 (.46)	.32 (.47)	.26 (.44)	.42 (.49)
R&D[d]	.34 (.48)[.003]	.42 (.49)[.01]	.37 (.48)[.01]	.42 (.49)[.01]
subsidies[d]	.05 (.22)	.13 (.34)	.11 (.31)	.12 (.33)
North_West[d]	.42 (.49)	.42 (.49)	.4 (.48)	.37 (.48)
North_East[d]	.30 (.46)	.30 (.46)	.29 (.46)	.3 (.46)
Center[d]	.18 (.38)	.18 (.39)	.2 (.4)	.2 (.4)
South_Islands[d]	.1 (.3)	.1 (.3)	.13 (.34)	.13 (.334)
Traditional_sectors[d]	.44 (.5)	.49 (.5)	.52 (.5)	.52 (.5)
Scale_Intensive sectors[d]	.25 (.43)	.19 (.39)	.17 (.38)	.17 (.38)
Specialized_sectors[d]	.27 (.44)	.29 (.45)	.26 (.44)	.27 (.44)
Science_based sectors[d]	.04 (.20)	.03 (.17)	.04 (.21)	.04 (.20)
Number of obs	1166		1794	

[d] dummy variable