Assessing the Impact of Financial Aids to Firms: Causal Inference in the presence of Interference

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ABSTRACT

We consider policy evaluations when SUTVA is violated because of the presence of interference among units. We propose to explicitly model interactions as a function of units characteristics. Our approach is applied to the evaluation of a policy implemented in Tuscany (a region in Italy) on small handicraft firms. Results show that the benefits from the policy are reduced when treated firms are subject to high levels of interference. Moreover, the average causal effect is slightly underestimated when interference is ignored. These findings point to the importance of considering possible interference among units when evaluating and planning policy interventions.

KEY WORDS: Causal inference, Interference, Policy evaluation, Potential outcomes, SUTVA

1 Introduction

Regional and national development policies are an important tool for setting up and supporting local enterprise. Most countries spend significant amounts of money on programs intended to promote R&D investments (Takalo et al., 2013). The justification for this support comes from the correction of market failures arising from the fact that social returns to R&D activities are greater than private returns making the market allocation of these resources sub-optimal (Duch et al., 2009). Incentives to private investment in R&D are usually allocated in the form of tax incentives, credits or direct funding of innovation programs.

Many business policy evaluation studies employ the potential outcome framework to evaluate the causal effect of policy interventions or programs on productivity, investments, returns on capital, sales or employment (e.g., Almus and Czarnitzki, 2003; Battistin et al., 2001; Pellegrini and Carlucci, 2003; Bronzini and De Blasio, 2006). Under this framework, different statistical techniques (such as, for example, matching) are used to estimate what the supported firms would have experienced had they not been supported and compare it with the factual outcome (see Klette et al., 2000, for a survey). A standard assumption in these evaluation studies, even if often it is not made explicit, is the Stable Unit Treatment Value Assumption (SUTVA). SUTVA combines the "no-interference" assumption that one unit's treatment assignment does not affect another unit's potential outcomes with the assumption that there are "no hidden versions" of the treatment (Rubin, 1980, 1990). The "no hidden versions" assumption implies that there are no unrepresented treatments, and we maintain this assumption throughout. The no-interference assumption is a critical component of SUTVA and in many settings, it may be untenable. In studies aimed at evaluating programs that provide services or financial assistance to firms, firms operating in the same geographical area and/or sector of activity are likely to interact each other, and so a treatment received by one firm may affect its competitors' potential outcomes.

In the literature, research on drawing inference on causal effects in the presence of interference is not yet common, although some exceptions exist (see, e.g., Verbitsky and Raudenbush, 2004; Sobel, 2006; Rosenbaum, 2007; Hudgens and Halloran, 2008; Tchetgen Tchetgen and VanderWeele, 2012; Kao et al., 2012). However, most of the existing works are theoretical and/or focus on randomized experiments. Applications in the context of observational studies addressing violation of SUTVA are somewhat rare (one example is Hong and Raudenbush, 2006).

In this paper, we propose a simple approach to draw inference on causal effects in observational studies accounting for the presence of interference. As a motivating example we use data from a policy intervention implemented in Tuscany, a region in central Italy. The intervention consists of a set of programs, named "Programs for the Development of Crafts (PDC)" (Regional Law n.36, 4/4/95), and it is targeted at artisans firms with registered office in Tuscany. The main objective of the program incentive is to ease access to credit by making it less costly, in order to improve firm performances in terms of investment policies, employment levels and sales. Most Tuscan artisan firms are small-sized, and generally operate in a limited geographical area. Therefore, they plausibly interact each other, casting considerable doubt on the scientific validity of inference that would be drawn under the "no-interference" assumption.

In some contexts it is reasonable to assume that interactions are limited within well-defined groups. In these situations, one approach to overcome violations of the "no-interference" assumption is to conduct the analysis at the minimum aggregate level for which SUTVA is plausible. For example, Stuart (2007) argues that when evaluating educational interventions, the "no-interference" assumption may be more reasonable in school-level analyses than in student-level analyses. In business incentives policy evaluations, several studies use local areas as units of analysis (e.g., De Castris and Pellegrini, 2012). However this approach does not allow one to estimate micro-level effects of the policy.

Similarly to the previous approach but maintaining the analysis at the micro level, some studies assumed that interactions are limited to units within groups, with the intensity of the interactions being constant within the same group (e.g., Hong and Raudenbush, 2006). Instead, we address violation of the "no-interference" assumption by explicitly modeling interactions among units. In our empirical application this approach involves specifying which firms interact with each other, and the relative magnitudes of these interactions. We extend previous approaches by allowing the intensity of interactions to depends on a distance metric, based on firms' characteristics, namely, size and geographical location. This idea is in line with the extensive literature on social interactions (see, for instance, Brock and Durlauf, 2001, for a survey), where interactions are often found to be stronger for geographically, or economically, or socially close units.

Our approach consists of a three-step procedure. In the first step, we use propensity score matching (Rosenbaum and Rubin, 1983; Dehejia and Wahba, 1999; Imbens, 2004) to create a matched data set so that the group of untreated firms is as similar as possible to the treatment group in terms of the distribution of pretreatment covariates. In the second step, for each firm in the matched dataset we estimate the interference with other firms as function of its own and its competitors' characteristics. Finally, in the third step, we use a regression model to estimate the causal effect of the policy intervention taking into account interference. As suggested by Ho et al. (2007), combining a matching preprocessing of the data with regression analysis, as done here, should improve inference because estimates of causal effects are less dependent on modeling assumptions and specifications.

The paper is organized as follows. In Section 2 we introduce the potential outcome approach to causal inference and formally define the Stable Unit treatment Assumption (SUTVA). In Section 3 we briefly review some of the approaches proposed in the literature to address violations of the no-interference component of SUTVA and describe our approach explicitly formulating the key assumptions and defining the causal estimands of interest. In Section4 we apply the proposed approach to our motivating example. Section 5 concludes.

2 The Potential Outcomes Framework and the SUTVA

Consider a group of firms, index by $i = 1, \dots, N$ and suppose we want to assess the causal effect of receiving a given benefit. Let T_i be a binary treatment indicator, taking on value 1 for firms that received the benefit (treated/assisted firms) and 0 for those that did not receive the benefit (control/non-assisted firms). Let $\boldsymbol{T}$ be the $N-$dimensional vector of assignments with ith element T_i, and let $\boldsymbol{T}_{-i}$ be the vector of assignments with T_i removed. Let

$Y_i(\boldsymbol{T}) \equiv Y_i(T_i, \boldsymbol{T}_{-i})$ denote the potential outcome for firm i (e.g., number of employees, sales, production innovation) given the treatment vector $\boldsymbol{T}$. In the potential outcome framework, SUTVA is an usually invoked assumption. SUTVA rules out hidden versions of treatments as well as interference between units. Formally,

Assumption 1 *(SUTVA, Rubin, 1980, 1990)*

$$\textit{If } T_i = T_i', \textit{ then } Y_i(\boldsymbol{T}) = Y_i(\boldsymbol{T}') \textit{ for all } \boldsymbol{T}, \boldsymbol{T}' \in \{0,1\}^N$$

SUTVA allows us to write $Y_i(T_i, \boldsymbol{T}_{-i})$ as $Y_i(T_i)$. Therefore, under SUTVA, for each firm there exist just two potential outcomes, $Y_i(0)$ and $Y_i(1)$.

In the context of the evaluation of policy interventions targeted on firms, the "no hidden versions of treatments" assumption means that the same treatment is administered to all units in the treatment group (and likewise for the control group). This component of SUTVA is violated, for example, in evaluation studies that only distinguish beneficiary and non-beneficiary firms in the presence of different amounts or types of benefits allocated to firms. In such a case $Y_i(1)$ is not stable because it will depend on which amount or type of benefit is chosen. A solution would be to consider the treatment to be multi-valued (Imbens, 2000; Lechner, 2001) or continuous (Hirano and Imbens, 2004; Imai and Van Dyk, 2004), instead of considering it as binary. For example, Bia and Mattei (2012) apply the generalized propensity score methodology to estimate the effect on occupational levels of the amount of contribution received by firms. On the other hand, the "no interference" component of SUTVA assumes that potential outcomes for each firm do not depend on the treatment assignment of the other firms.

The “no interference” assumption is plausible in many applications, but there are also many cases in which interactions between units are a major concern and the assumption is not plausible (Sobel, 2006; Rosenbaum, 2007; Hudgens and Halloran, 2008) [1]. In business policy evaluation, it is reasonable to think that firms operating in the same geographical area and sector of activity are likely to compete for scarce resources (e.g., credits) and customers. This implies that a policy intervention assigning an incentive to a firm may affect also the performance of its competitors, violating the no-interference assumption. Here, we stress the importance of considering possible interference among firms in development policy evaluation studies. We maintain the “no hidden versions of treatments” assumption and consider a binary treatment (receiving versus not receiving a benefit). However, our approach can be extended to multivalued or continuous treatments.

In some settings, interference is a nuisance while in other settings it defines the effects of interest. In many business policy evaluation studies firms not receiving support may be still affected by the programs due to spillover effects, which are often the main justification for R&D subsidies. Measuring the magnitude of the generated spillovers is by itself a crucial part of evaluating the programs in these cases (Eberhardt et al., 2013; Klette et al., 2000; Takalo et al., 2013).

Our focus will be on the evaluation of the causal effect of a policy intervention for treated firms in the presence of interactions and not on the quantification of the effects of the inter-

[1] See Greiner and Rubin (2011) for a discussion of SUTVA, the possible violation of its components and ways of relaxing them in the context of the evaluation of the causal effect of the perception of immutable characteristics.

actions *per se*[2]. However, we shall notice that by considering different levels of interference we can define interesting causal estimands above and beyond the standard average treatment effect.

3 Causal Inference in the Presence of Interference

Without imposing the no-interference assumption potential outcomes for each firm do not only depend on its treatment assignment but also on the treatment assignment of all the other $N-1$ firms. Therefore for each firm, potential outcomes are not two anymore, but 2^N. In this setup, an individual causal effect may be defined as a comparison between any two potential outcomes: $Y_i(T_i, \boldsymbol{T}_{-i})$ versus $Y_i(T_i', \boldsymbol{T}_{-i}')$, $T_i, T_i' \in \{0,1\}$, and $\boldsymbol{T}_{-i}, \boldsymbol{T}_{-i}' \in \{0,1\}^{(N-1)}$.

To address the complications due to completely relax the no-interference assumption, in the following we introduce alternative weaker versions of the no-interference assumption and develop a framework to account for differential strength of interference a unit can be subject to.

3.1 Restricting the Interference within Activity Sectors

In business policy evaluation studies, it is plausible to assume that interference among firms is limited within activity sectors. We introduce some additional notation in order to account for firms' activity sector. Let K be the number of activity sectors and denote by N_j the number of firms in sector j, $j = 1, \ldots, K$. The vector of treatment assignments $\boldsymbol{T}$ can be conveniently

[2]For an interesting approach to the measurement of spillover effects see Kao et al. (2012)

decomposed as follows: $\boldsymbol{T} = [\boldsymbol{T}^{(1)}, \dots, \boldsymbol{T}^{(K)}]^{\mathrm{tr}}$, where $\boldsymbol{T}^{(j)} = (T_{ij}^{(j)}, \boldsymbol{T}_{-ij}^{(j)})$, and ij represents firm i in sector j[3].

The following assumption implies that interference is limited among firms in the same sector of activity, i.e. the SUTVA holds only with respect to firms in different sectors:

Assumption 2

$$\textit{If } \boldsymbol{T}^{(j)} = \boldsymbol{T}'^{(j)}, \textit{ then } Y_{ij}(\boldsymbol{T}) = Y_{ij}(\boldsymbol{T}') \textit{ for all } \boldsymbol{T}, \boldsymbol{T}' \in \{0,1\}^N$$

Assumption 2, defined by Sobel (2006) as partial interference, implies that $Y_{ij}(\boldsymbol{T})$ is equal to $Y_{ij}(\boldsymbol{T}^{(j)})$, and so each firm has 2^{N_j} potential outcomes corresponding to alternative treatments allocations for itself and its competitors in the same activity sector.

3.2 Modeling Interference within Activity Sectors

Hong and Raudenbush (2006) consider the estimation of the causal effect of retaining low-achieving children in kindergarten rather than promoting them to first grade. They argue that a student's learning outcome can be affected by the treatments assigned to other students. So, for example, the retention effect on a student may depend on the proportion of peers retained at the same time. Hong and Raudenbush (2006) relax the standard SUTVA by assuming that interference is limited within school and that peer effects can be summarized through the proportion of retained students in the school. Similarly, in our context, we could assume that interference only depends on the proportion of treated firms in each sector. Formally,

[3]In this paper we use square brackets to denote an ordered sequence of vectors and round brackets for a collection of elements. The superscript tr denotes the transpose of a vector/matrix.

Assumption 3

$$\textit{If } T_{ij}^{(j)} = T_{ij}^{\prime(j)} \textit{ and } p(\boldsymbol{T}_{-ij}^{(j)}) = p(\boldsymbol{T}_{-ij}^{\prime(j)}) \textit{ then } Y_{ij}(\boldsymbol{T}) = Y_{ij}(\boldsymbol{T}') \textit{ for all } \boldsymbol{T}, \boldsymbol{T}' \in \{0,1\}^N$$

where $p(\boldsymbol{T}_{-ij}^{j})$ is the proportion of treated firms in sector j excluded firm ij.

Assumption 3 imposes the strength of interference to be constant for all units within a group, given their treatment status. As an alternative, we can allow interference to depend on units' characteristics. For instance, as noted by Wooldridge and Imbens (2009), interactions may decline in importance depending on some distance metric, either geographical distance or proximity in some economic sense. In our case, it is reasonable to assume that within the same activity sector, interference depends on firms' characteristics, such as location and some measure of firms' size.

Formally, we assume that for each firm ij, interference can be summarized by a m-valued function of treatment assignments of firm ij's competitors, $\boldsymbol{T}_{-ij}^{(j)}$: $f(\boldsymbol{T}_{-ij}^{(j)}) = [f_1(\boldsymbol{T}_{-ij}^{(j)}), \ldots, f_m(\boldsymbol{T}_{-ij}^{(j)})]^{\mathrm{tr}}$, so that, $Y_{ij}(T_{ij}^{(j)}, \boldsymbol{T}_{-ij}^{(j)}) = Y_{ij}(T_{ij}^{(j)}, f(\boldsymbol{T}_{-ij}^{(j)}))$. Formally, we make the following assumption:

Assumption 4

$$\textit{If } T_{ij}^{(j)} = T_{ij}^{\prime(j)} \textit{ and } f(\boldsymbol{T}_{-ij}^{(j)}) = f(\boldsymbol{T}_{-ij}^{\prime(j)}), \textit{ then } Y_{ij}(\boldsymbol{T}) = Y_{ij}(\boldsymbol{T}')$$

for all $\boldsymbol{T}, \boldsymbol{T}' \in \{0,1\}^N$.

While Assumption 2 implies that potential outcomes for each firm ij depend on the whole vector of treatment assignments in sector j, Assumption 4 attempts to model interference within sectors.

Alternative specifications of the function f can be considered depending on subject matter knowledge and background characteristics can be included in the specification of such a function. Thus, under Assumption 4 the influence of competitors' treatment assignments on firm ij potential outcomes depends, through f, on firm ij's competitors characteristics. Here we focus on linear combinations of treatment assignments: $f_r(\boldsymbol{T}^{(j)}_{-ij}) = {}_r\mathbf{w}^{(ij)\prime}\boldsymbol{T}^{(j)}_{-ij}$, where ${}_r\mathbf{w}^{(ij)} = [{}_rw^{(ij)}_{1j}, \ldots, {}_rw^{(ij)}_{(i-1)j}, {}_rw^{(ij)}_{(i+1)j}, \ldots, {}_rw^{(ij)}_{N_jj}]$ is a $(N_j - 1)$-dimensional vector of weights for firm ij. A simple case is when each element of ${}_r\mathbf{w}^{(ij)}$ takes on two values, 0 and 1, depending on firms' characteristics. For instance, we might assign a zero weight to firms that are geographically faraway from ij given some pre-specified distance threshold. This amounts to assume that treatment assignment of firms with a zero weight does not affect firm ij's potential outcomes. However, this specification of the weights might be somewhat restrictive. More generally, weights, ${}_r\mathbf{w}^{(ij)}$, can be specified as any real-value function and can also depend on firms' characteristics.

Specifically, let $\boldsymbol{Z}^{(j)}$ be the $N_j \times p-$dimensional matrix of variables affecting the strength of interference among firms in sector j, with ith row equal to $\boldsymbol{Z}^{(j)}_{ij}$. Then, we assume that ${}_rw^{(ij)}_h = g_r(\boldsymbol{Z}^{(j)}_{ij}, \boldsymbol{Z}^{(j)}_{hj})$, with $h = 1, \ldots, N_j$, $h \neq i$. A downturn of this approach is that inference based on nonparametric methods might raise serious challenges. We propose a model-based approach that explicitly uses the weights ${}_rw^{(ij)}_h$ to account for interference.

Various causal estimands can be defined. Under Assumption 4, an average causal effect can be defined as:

$$\mathbb{E}\left[Y(T^{(j)}_{ij}, f(\boldsymbol{T}^{(j)}_{-ij})) - Y(T'^{(j)}_{ij}, f(\boldsymbol{T}'^{(j)}_{-ij}))\right].$$

In this paper we are interested in average causal effects for the treated (ATT, e.g., Imbens, 2004), which can be generally defined as follows:

$$\mathbb{E}\left[Y(T_{ij}^{(j)}, f(\boldsymbol{T}_{-ij}^{(j)})) - Y(T_{ij}^{\prime(j)}, f(\boldsymbol{T}_{-ij}^{\prime(j)})) \mid T_{ij}^{(j)} = 1\right].$$

Specifically, we focus on the following estimands:

$$\tau(f^*) = \mathbb{E}\left[Y(T_{ij}^{(j)}, f(\boldsymbol{T}_{-ij}^{(j)})) - Y(T_{ij}^{\prime(j)}, f(\boldsymbol{T}_{-ij}^{\prime(j)})) \mid T_{ij}^{(j)} = 1, f(\boldsymbol{T}_{-ij}^{(j)}) = f(\boldsymbol{T}_{-ij}^{\prime(j)}) = f^*\right], \quad (1)$$

and

$$\tau = \mathbb{E}\left[\mathbb{E}\left[Y(T_{ij}^{(j)}, f(\boldsymbol{T}_{-ij}^{(j)})) - Y(T_{ij}^{\prime(j)}, f(\boldsymbol{T}_{-ij}^{\prime(j)})) \mid T_{ij}^{(j)} = 1, f(\boldsymbol{T}_{-ij}^{(j)}) = f(\boldsymbol{T}_{-ij}^{\prime(j)}) = f\right]\right], \quad (2)$$

where the outer expectation in Equation (2) is over the distribution of the interference function, f.

The estimand in Equation (1) represents the effect of the policy under a pre-fixed level of interference, f^*. The variability of these effects with respect to different values of f^* will indicate to what extent different levels of interference affect the possibly beneficial effects of the policy. Evidence on heterogeneity of these effects could be useful to plan future policy interventions. For example, if the policy benefit is reduced because of the presence of geographically close treated competitors, then the policy maker could introduce some geographical constraints in the future allocation of benefits. The estimand in Equation (2) is the (marginal) average causal effect of the treatment, and it is the estimand of main interest if interference is merely viewed as a nuisance factor.

4 Estimation of the Impact of Financial Aids to Tuscan Firms

4.1 Programs for the Development of Crafts in Tuscany (Italy)

With the goal of promoting innovation and regional development, the Tuscan Regional Administration (Italy) in collaboration with "ArtigianCredito Toscano," a consortium aimed at easing the access to credit for small firms, introduced the "Programs for the Development of Crafts (PDC)" (Regional Law n.36, 4/4/95), targeted at Tuscan small-sized handicraft firms. Access to PDC was based on eligibility criteria and a voluntary application by firms. The eligibility criteria required firms to plan an investment project involving costs above a pre-fixed threshold, which varied across programs and over years. Beneficiary firms were selected on the basis of a score, accounting for both firms' characteristics and the quality of the investment project proposal.

The main objective of the program incentive was to ease access to credit by making it less costly, in order to improve firm performances in terms of investment policies, employment levels and sales.

The first PDC calls, published in 2001 and 2002, provided subsidies without requiring any refund or interest payment. This type of financial aid raised various issues. The lack of a commitment to refund boosted an extremely high number of firms to apply. As the access criteria were not very tight, also firms that applied proposing low-quality investment projects received a grant. Moreover, access to 2001/02 programs required that the investment project for which firms applied for a grant were ongoing at the moment of the application. This access rule implied that most of applicant firms had already received some financial support from a lending institution at the moment of application.

The numerous drawbacks of the 2001/02 PDC led to modify the grant assignment rules in 2003: the grant type was changed from subsidies to soft-loans and the minimum investment cost was increased. In 2003, the minimum admissible investment cost was 12 500 Euros and the grant covered 70% of the financed investment. In 2004 these thresholds were slightly changed: the minimum admissible investment cost was increased to 25 000 Euros, and the percentage of the financed investment covered by the grant was reduced to 60%. The grants were distributed using a revolving fund in the form of interest-free grants one-off upon request from the assisted firms, given either a bank guarantee or the final investment financial statement.

From an economic perspective, soft loans are more advisable than capital grants, in the sense that with the same amount of public funds, the loans allow the government to provide incentives to a much larger number of assisted firms, generating a greater leverage. In fact the new grant allocation rule was successful: Among firms participating in the PDC between 2003 and 2005 only a few projects were not funded and the percentage of insolvencies was really low (lower than 3%). Also, previous studies found that the post 2003 PDC had statistically significant positive effects on firms' performance. Conversely, the 2001/02 PDC were found to have small and statistically negligible effects (Mattei and Mauro, 2007). We have information on assisted firms that participated in the program either before 2003 (2001/02 PDC) or between 2003 and 2005 (2003/05 PDC). However, given the advantages of the grant allocation rule of the post 2003 PDC and the results from previous impact evaluation studies on the PDC, we will focus on the 2003/05 PDC henceforth.

4.2 Data

We use an integrated data set, including longitudinal information on the PDC in Tuscany, collected by "ArtigianCredito Toscano," and a wealth of information on firms' characteristics coming from administrative archives provided by the Chamber of Commerce ($2001 - 2004$) and by the Internal Revenue Service (2002), and from an "ad hoc" telephone survey (see Mattei and Mauro, 2007, for details on the survey). The survey was conducted on a sample of 119 assisted firms (participating in 2003/05 PDC) and of 721 non-assisted firms, in order to gather additional information, such as 2005 outcome variables of firms' performances (number of employees, sales, production innovation).

In our analysis we focus on a subsample of firms. We first select firms operating in the following 4 economic activity sectors: Manufacturing activities (D); Construction (F); Wholesale and retail trade and repair of motor vehicles, motorcycles, and personal and household goods (G); and Real estate business, rental services, computer, research, business services (K). It is worth noting that these four economic activity sectors comprise the most of the Tuscan artisan firms. In fact only a very small number of firms in our sample operate in economic activity sectors different from those we select. We also discard 23 assisted firms and 135 non-assisted firms with missing values on relevant variables. The selection procedure leads to a sample of 94 assisted firms and 528 non-assisted firms.

Firms' decision to apply for public assistance, as well as the selection mechanism operated by the authorities, implies that the benefits are not randomly allocated. In fact, there is substantial imbalance in the distributions of several characteristics between assisted and non-assisted firms: the initial absolute standardized percent bias (ABS, Rosenbaum and Rubin, 1985) is greater than 20% for 11 out of 29 covariates, and greater than 30% for 5 covariates including pretreatment number of employees (see Table 1). In our observational study, however, we can reasonably assume strong ignorability of the treatment conditional on observed covariates, i.e., conditional independence of potential outcomes on the treatment assignment given pretreatment covariates (e.g., Imbens, 2004).

Under strong ignorability we employ a matching strategy to select a group of control units such that the distribution of pretreatment characteristics for the treated and matched control groups are as similar as possible. We investigated alternative matching procedures, including coarsened exact matching with alternative coarsening of covariates (Blackwell et al., 2009; Iacus et al., 2012) and propensity score matching with different specifications of the propensity score model and different matching algorithms. We selected the matching procedure that guaranteed the best average ASB and a satisfactory ASB for important covariates (such as pretreatment sales). In particular, we selected a subset of 94 non-assisted firms using one-to one nearest neighbour propensity score matching (without replacement) combined with exact matching on sector of activity.

As it can be seen in Table 1, the ASB after matching is dramatically reduced for most of the covariates and overall the matching solution was quite satisfactory. However, imbalance remains in some pretreatment variables. To adjust for such residual imbalance, we control for pretreatment variables in the regression-based analyses implemented on the sample of treated and matched control firms to estimate the causal effects of interest.

Our main substantive objective is to evaluate the effects of the PDC on employment levels, accounting for the presence of interference. Employment is a key component of the market, and a policy that is effective in increasing firms' labor demand may be worthwhile from a socio-economic perspective. Alternative outcome variables, such as sales and production innovation, can be considered, but they suffer from the presence of a large proportion of missing data, which may compromise the study, substantially complicating the analysis. The evaluation of the effects of the PDC on those outcome variables accounting for both the presence of interference as well as missingness will be a valuable topic for future research.

4.3 Modeling Interference among Tuscan Small-Handicraft Firms

Tuscany is a region in the center of Italy consisting of 10 provinces: Arezzo, Firenze, Grosseto, Livorno, Lucca, Massa-Carrara, Pisa, Pistoia, Prato, Siena. Figure 1 shows the borders of the provinces in Tuscany with the distribution of the assisted firms and matched firms classified by economic activity sector. Note that firms' geographical location refers to their registered office, although the vast majority of the firms in our sample only have one branch. The most noticeable pattern in the maps is that most of the firms operating in the economic activity sector D are located in the north and north-east of Tuscany (especially in the provinces of Arezzo, Firenze, Prato and Pistoia) and are relatively close to one another.

The geographical distribution of firms operating in the other economic activity sectors is more sparse, although the provinces in the north/north-east of Tuscany are still those where more firms concentrate. In the analyses, we aggregated some geographically contiguous provinces with a small number of firms in our sample.

Tuscany is traditionally a land of small-sized companies and individual traders, and firms operating in the same market usually have similar needs, interests, and knowledge, especially if they are located relatively nearby. These features of the Tuscan business market suggest that providing a benefit to one firm can also affect the outcomes of others and interactions are expected to be stronger among geographically close firms and to mainly affect firms that are smaller according to some measure of size. In fact, firms' performance may be strongly influenced by the policy choices of their competitors, especially if competitors' size is bigger. In our application study we use pretreatment sales as measure of firm size and assume that interference depends on pretreatment sales and geographical location. Formally, we set $m = 2$ and $f_r(\boldsymbol{T}^{(j)}_{-ij}) = {}_r\mathbf{w}^{(ij)tr}\boldsymbol{T}^{(j)}_{-ij}$, $r = 1, 2$, where the weights, ${}_r\mathbf{w}^{(ij)tr}$, $r = 1, 2$, are defined as follows. Let Z_{ij1} be pretreatment sales for firm ij and let $Z^{(1)}_{ij2}$ and $Z^{(2)}_{ij2}$ be the pair of variables measuring UTM (Universal Transverse Mercator) coordinates for firm ij.

The weights ${}_1\mathbf{w}^{(ij)tr}$ and ${}_2\mathbf{w}^{(ij)tr}$ are respectively based on the Canberra distance between sales and the Euclidean distance between UTM coordinates. Formally, let ${}_1d^{(ij)}_{hj}$ denote the Canberra distance between sales of firm ij and sales of firm hj, $h \neq i$.:

$$ {}_1d^{(ij)}_{hj} = \frac{\mid Z_{ij1} - Z_{hj1} \mid}{Z_{ij1} + Z_{hj1}}. $$

We choose the Canberra distance because it allows us to standardize with respect to the total size of the firms being compared. An element ${}_1w^{(ij)}_{hj}$ of the vectors of weights ${}_1\mathbf{w}^{(ij)tr}$ is defined as follows:

$$
{}_1w_{hj}^{(ij)} = \begin{cases} 1 + {}_1d_{hj}^{(ij)} & \text{if } Z_{hj1} \geq Z_{ij1} \\ 1 - {}_1d_{hj}^{(ij)} & \text{if } Z_{hj1} < Z_{ij1}. \end{cases}
$$

This type of weighting implies that if firm hj is bigger (smaller) than firm ij, then its weight in the function f_1 for firm ij increases (decreases) with the difference between the size of the two firms. The weight of firm hj is one if it has the same size as firm ij.

Similarly, let ${}_2d_{hj}^{(ij)}$ be the Euclidean distance between the UTM coordinates of firm ij and the UTM coordinates of firm hj, $h \neq i$. An element ${}_2w_{hj}^{(ij)}$ of the vector of weights ${}_2\mathbf{w}^{(ij)\prime}$ is defined as the reciprocal of the Euclidean distance between the UTM coordinates of firm ij and the UTM coordinates of firm hj, $h \neq i$: ${}_2w_{hj}^{(ij)} = 1/{}_2d_{hj}^{(ij)}$.

The rationale behind this system of weights is that for each firm the interference will be stronger the higher is the number of treated competitors which are geographically close and have higher sales levels.

Let $\boldsymbol{T}^{obs} = [\boldsymbol{T}^{(1),obs}, \ldots, \boldsymbol{T}^{(K),obs}]^{\mathrm{tr}}$ be the vector of treatments intakes, where $\boldsymbol{T}^{(j),obs} = (T_{ij}^{(j),obs}, \boldsymbol{T}_{-ij}^{(j),obs})$, and let Y_{ij}^{obs} be the actual outcome (number of employees) for firm ij in sector j. We standardized the observed values of the interference functions $f_1(\boldsymbol{T}_{-ij}^{(j),obs})$ and $f_2(\boldsymbol{T}_{-ij}^{(j),obs})$ using their means and standard deviations within sectors: Let $\overline{f}_r(\boldsymbol{T}_{-ij}^{(j),obs})$ denote the within sector standardized interference function f_r, $r = 1, 2$. Then, we model the conditional expectation of Y_{ij}^{obs} given T_{ij}^{obs}, $\overline{f}_r(\boldsymbol{T}_{-ij}^{(j),obs})$, $r = 1, 2$ and $\boldsymbol{X}_{ij}$ as a flexible function of its arguments. Formally,

$$\mathbb{E}[Y_{ij}^{obs}|T_{ij}^{(j),obs},\overline{f}_1(\boldsymbol{T}_{-ij}^{(j),obs}),\overline{f}_2(\boldsymbol{T}_{-ij}^{(j),obs}),\boldsymbol{X}_{ij}] = \alpha_0 + \alpha_1 T_{ij}^{(j),obs} + \alpha_2 \overline{f}_1(\boldsymbol{T}_{-ij}^{(j),obs}) + \alpha_3 \overline{f}_2(\boldsymbol{T}_{-ij}^{(j),obs}) + \alpha_4 T_{ij}^{(j),obs}\overline{f}_1(\boldsymbol{T}_{-ij}^{(j),obs}) + \alpha_5 T_{ij}^{(j),obs}\overline{f}_2(\boldsymbol{T}_{-ij}^{(j),obs}) + \boldsymbol{X}_{ij}\boldsymbol{\beta}. \tag{3}$$

Under strong ignorability and Model (3),

$$\begin{aligned}
&\mathbb{E}\left[Y(T_{ij}^{(j)}, f(\boldsymbol{T}_{-ij}^{(j)})) - Y(T_{ij}^{\prime(j)}, f(\boldsymbol{T}_{-ij}^{\prime(j)}))\right] = \\
&\quad\left[\alpha_1 T_{ij}^{(j)} + \alpha_2\overline{f}_1(\boldsymbol{T}_{-ij}^{(j)}) + \alpha_3\overline{f}_2(\boldsymbol{T}_{-ij}^{(j)}) + \alpha_4 T_{ij}^{(j)}\overline{f}_1(\boldsymbol{T}_{-ij}^{(j)}) + \alpha_5 T_{ij}^{(j)}\overline{f}_2(\boldsymbol{T}_{-ij}^{(j)})\right] - \\
&\quad\left[\alpha_1 T_{ij}^{\prime(j)} + \alpha_2\overline{f}_1(\boldsymbol{T}_{-ij}^{\prime(j)}) + \alpha_3\overline{f}_2(\boldsymbol{T}_{-ij}^{\prime(j)}) + \alpha_4 T_{ij}^{\prime(j)}\overline{f}_1(\boldsymbol{T}_{-ij}^{\prime(j)}) + \alpha_5 T_{ij}^{\prime(j)}\overline{f}_2(\boldsymbol{T}_{-ij}^{\prime(j)})\right].
\end{aligned}$$

We estimate the model parameters by ordinary least squares: Let $\hat{\alpha}_\ell$, $\ell = 0, 1, \ldots, 5$, and $\hat{\boldsymbol{\beta}}$ the ordinary least squares estimates of the model parameters. Given the estimated parameters, causal effects and their standard errors can be easily estimated using the estimating equation. For instance, the average causal effect for a given allocation of the assignment $\boldsymbol{T} = [\boldsymbol{T}^{(1)}, \ldots, \boldsymbol{T}^{(K)}]'$ and a pre-fixed interference level f^*, i.e., the causal estimand in Equation (1), is estimated as

$$\hat{\tau}(f^*) = \hat{\alpha}_1 + \hat{\alpha}_4 f_1^* + \hat{\alpha}_5 f_2^*,$$

and its standard error is estimated as

$$\hat{s.e.}(\hat{\tau}(f^*)) = \sqrt{\hat{\mathbb{V}}(\hat{\alpha}_1) + (f_1^*)^2\hat{\mathbb{V}}(\hat{\alpha}_4) + (f_2^*)^2\hat{\mathbb{V}}(\hat{\alpha}_5) + 2f_1^*\widehat{Cov}(\hat{\alpha}_1,\hat{\alpha}_4) + 2f_2^*\widehat{Cov}(\hat{\alpha}_1,\hat{\alpha}_5) + 2f_1^* f_2^*\widehat{Cov}(\hat{\alpha}_4,\hat{\alpha}_5)}.$$

4.4 Results

The first two columns in Table 2 show parameter estimates of the conditional distribution of number of employees given treatment status and covariates:

$$E[Y_{ij}^{obs}|T_{ij}^{(j),obs}, \boldsymbol{X}_{ij}] = \gamma_0 + \gamma_1 T_{ij}^{(j),obs} + \boldsymbol{X}_{ij}\boldsymbol{\beta}.$$

Under SUTVA, the coefficient on the treatment indicator in this outcome model, γ_1, is the causal effect of interest. Its estimate is 1.37 $(se = 0.62)$ suggesting a positive and statistically significant impact of the policy intervention. The estimated effect is also substantially quite strong given that the pretreatment average number of employees is about 10 for assisted firms before the implementation of the policy. The last two columns of Table 2 show parameter estimates of the conditional distribution of number of employees taking into account interference (Equation (3)).

The observed (standardized) values of the interference function based on the geographical distance, f_1, ranges from -0.94 to 5.50 with median equal to -0.27, and the observed (standardized) values of the interference function based on the sales distance, f_2, ranges from -1.88 to 2.40 with median equal to -0.055. Although the estimated coefficients of the interference functions and of their interactions with the treatment indicator are statistically negligible, there is some evidence that the association between number of employees and interference is negative.

Figures 2 and 3 present our main results. Figure 2 consists of two parts showing the estimated $\tau(f^*)$ effects and their 95% point-wise confidence bands derived (a) fixing the interference function based on the sales distance at its observed median value and ranging the interference function based on the geographical distance over its observed percentiles; and (b) fixing the interference function based on the geographical distance at its observed median value and ranging the interference function based on the sale distance over its observed percentiles. Most of the estimated causal effects are positive and statistically significant at the 5% level: the 95% confidence bands do not cover zero over most of the range of the interference functions. Specifically, for interference levels based on the geographical distance lower than 1.6 (the 95th percentile), the estimated average causal effects are a statistically significant increase of employment levels ranging between 0.82 units and 1.28 units. Similarly, for interference levels based on sales lower than 1.0 (the 82th percentile), the estimated average causal effects indicate a statistically significant increase of employment levels ranging between 0.44 units and 2.26 units. However, the highest effects are estimated under minimum levels of interference, and as the strength of interference increases, the estimated $\tau(f^*)$ effects steady reduce. Extreme values (greater than the observed 95th percentile) of the interference function based on the geographical distance lead to small and non-significant effects. The consequences of large interference levels based on sales are even more dramatic: the estimated $\tau(f^*)$ effects reach also negative values for interference levels based on sales greater than the observed 94th percentile (1.68), although the estimated standard errors suggest these negative point estimates are statistically negligible.

Jointly ranging the interference functions over their observed percentiles (see Figure 3) further stresses the role of interference, clearly showing that the impact of PDC steady decreases as the strength of interference increases. Most of the estimated effects are positive and statistically significant (estimated standard errors are omitted), suggesting that PDC are effective in increasing employment levels, even in the presence of interference. However, in the presence of high levels of interference the estimated effects become small and statistically negligible, clearly highlighting that ignoring interference may lead to misleading results.

The estimate of the average treatment effect on the treated we obtain integrating out the interference functions, the causal effect τ in Equation (2), is positive and statistically significant ($\hat{\tau} = 1.08$, $se = 0.16$), but it is slightly smaller than the model-based estimate of the ATT effect under SUTVA.

In general the presence of interference seems to reduce the size of the effect. In fact the ATT effects estimated accounting for the presence of interference are generally smaller than the ATT effects estimated under SUTVA, unless interference is really small. These results are consistent with the idea that the beneficial effect of the policy for a firm is reduced when its competitors are also treated, highlighting that the impact of interference can be considerable and should not be neglected.

4.4.1 A Small Simulation Experiment

Previous results are based on the distribution of the interference functions as resulting from the observed allocation of treatment and firms characteristics. In order to investigate how the interference functions and treatment effects vary over the assignment distribution, we conducted a small simulation study where we estimate the treatment effects of interest under various allocations of the treatment but maintaining fixed the firms characteristics. We assume that a completely randomized experiment is conducted, where the number of firms assigned to receive a benefit is fixed to 94, the observed number of assisted firms. Formally, we randomly draw $K = 10\,000$ N-dimensional vectors with $N_T = 94$ ones and $N - 94 = 94$ zeros from the set of $\binom{N}{N_T}$ possible treatment vectors. For each draw from this set, we first calculate the (standardized) interference functions, $\overline{f}(\boldsymbol{T}^{(j)}_{-ij})$, and then we estimate $\tau(f^*)$, with f^* fixed at the median value of $\overline{f}(\boldsymbol{T}^{(j)}_{-ij})$ over the firms assigned to treatment.

Table 3 shows summary statistics of the medians of the interference functions over the simulated assignment distribution. The medians indicate low to moderate levels of interference. The distributions of the medians generated by the treatment allocations are centered on negative values and have a small variability, implying that most of the medians are negative. Consistently, the estimated $\tau(f^*)$ effects are relatively stable, ranging from 1.01 ($s.e. = 0.16$) to 1.38 ($s.e. = 0.18$). Nevertheless, Figure 4, showing the estimated $\tau(f^*)$ effects for each of the $K = 10\,000$ treatment allocations, gives a clear message, that is, the presence of interference can strongly affect the evaluation results.

In line with the results described above, Figure 4 suggests that the beneficial effect of the policy is higher, the lower the strength of interference. In our specific study, the interference function based on the sale distance seems to play a key role: the $\tau(f^*)$ effects reach their minimum values in the presence of high levels of the interference function based on the sale distance. Therefore, an allocation of treatments that does not account for the distribution of sales among assisted firms may reduce or even vanish the expected benefits of the policy.

As a general message, these results highlight that given budget constrains, which necessarily limit the number of assisted firms, firms' characteristics and the features of the business market, policy makers could make a policy intervention more effective, generating an higher average treatment effect if they apply allocation rules that keep interference into account.

5 Concluding remarks

The aim of this article was to discuss the violation of SUTVA, a standard assumption in the potential outcome literature, due to interference among units in the context of policy evaluations targeted on firms. Previous evaluations studies in this area uncritically used SUTVA either implicitly or explicitly. In general, applied papers trying to relax the standard SUTVA are very rare. Similarly to these limited works, we assume that SUTVA holds across groups (sectors of activity), while it may be violated within groups. However, contrary to previous attempts of relaxing SUTVA, we allow interference within groups to vary for each unit. In particular, we propose a framework where potential outcomes for each firm may depend on the treatment assignment of other firms in the same activity sector with the strength of interference being a function of firms' characteristics, such as geographical distance between firms and firms' size (as measured by pretreatment sales). With minor modifications this approach could be applied in different contexts by adequately specifying the weights entering the interference function.

It is also worth noting that our approach does not require the existence of different groups. If, for example, firms may be partially competing with firms in different sectors this can be taken into account by assigning a non-zero weights also to firms in different sectors.

As illustrative example, we applied our approach to the evaluation of the effect of soft-loans provided to Tuscan artisans firms in 2003/2004 on employment levels in 2005. Given the characteristics of the Tuscan labor market, where most artisan firms are small-sized and generally operate in a limited geographical area, it was crucial to consider possible interference among firms. By allowing for interference, in addition the standard average treatment effect on the treated (ATT), it was possible to consider another interesting causal estimand: the causal effect of receiving the benefit (versus not receiving it) given a certain level of interference. By varying the level of interference, we find that the beneficial effect of the policy is highly heterogeneous. In particular, our findings show that the effect of the treatment is decreasing as interference increase. This happens both when we increase the level of interference due to having geographically close treated competitors as well as when interference increases because treated competitors are bigger in size. However, the average effect of the policy is positive and statistically significant for most of the levels of interference. Only in the presence of very high levels of interference the average treatment effect become statistically insignificant. Finally, we find that the average causal effect of the policy intervention is substantial and statistically significant. However, when we allow for interference, the ATT is lower than the same effect estimated under the standard SUTVA. This result indicates that ignoring SUTVA may not only hide heterogeneous effects of the policy for different interference levels but also produce misleading estimates of the average effect of a policy.

These results suggest that policy makers should carefully account for interactions among firms in the planning phase of a new intervention in order to define “optimal” treatment allocation rules that allow them to maximize the benefits of that intervention.

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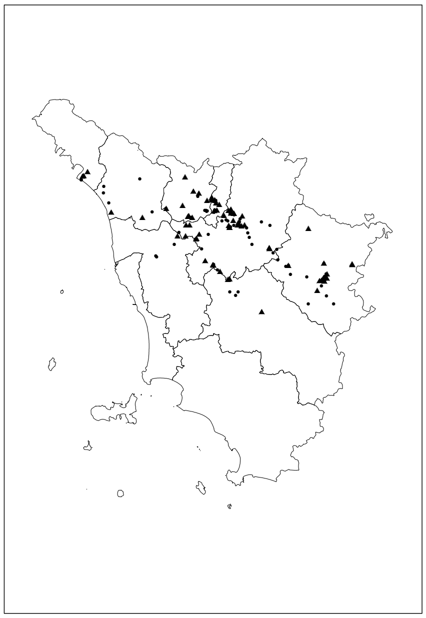
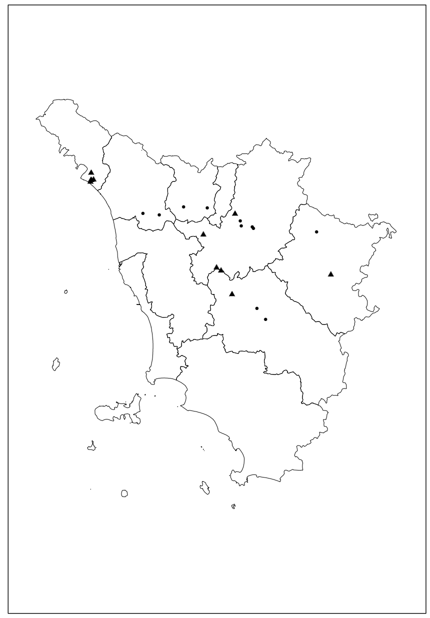
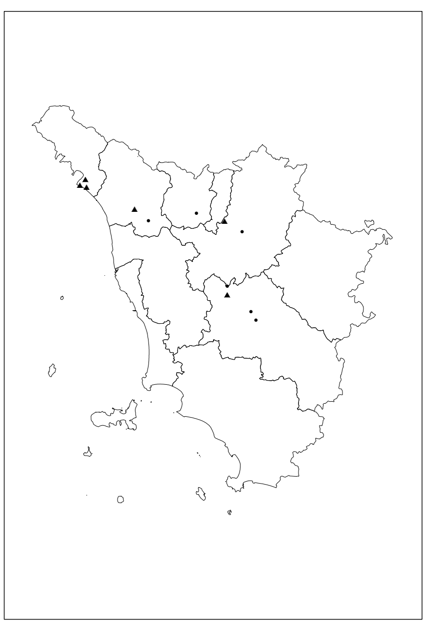
Figure 1: Provinces of Tuscany (Italy) with the PDC assisted firms (black triangles) and the matched firms (black circles) classified by economic activity sectors.



Activity Sector: D



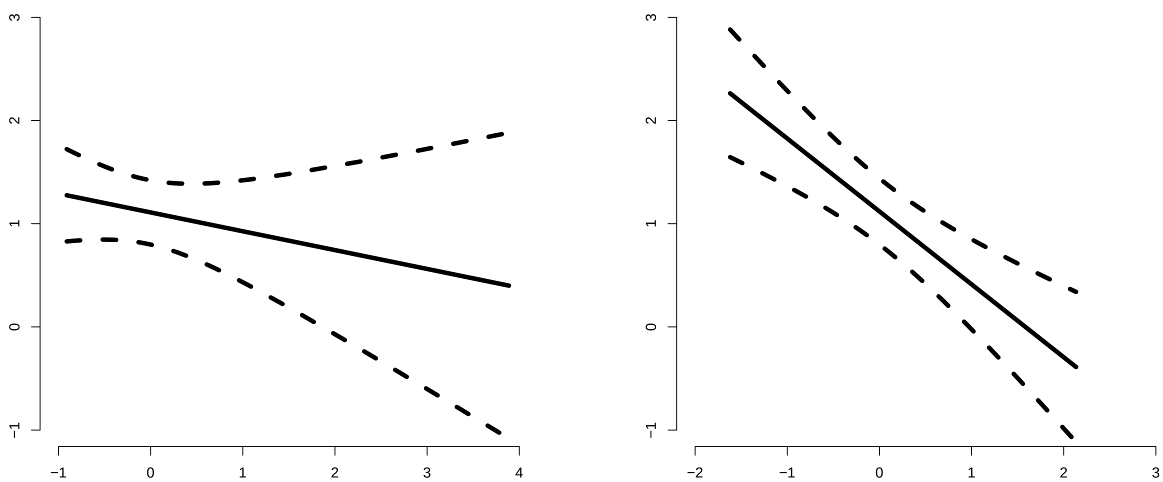
Activity Sector: F



Activity Sector: G

Activity Sector: K

Figure 2: Estimated $\tau(f^*)$ effects and their 95% point-wise confidence bands derived (a) fixing the interference function based on the sales distance at its observed median value and ranging the interference function based on the geographical distance over its observed percentiles; and (b) fixing the interference function based on the geographical distance at its observed median value and ranging the interference function based on the sale distance over its observed percentiles.



(a) (b)

Figure 3: Estimated $\tau(f^*)$ effects derived ranging the interference functions over their observed percentiles.

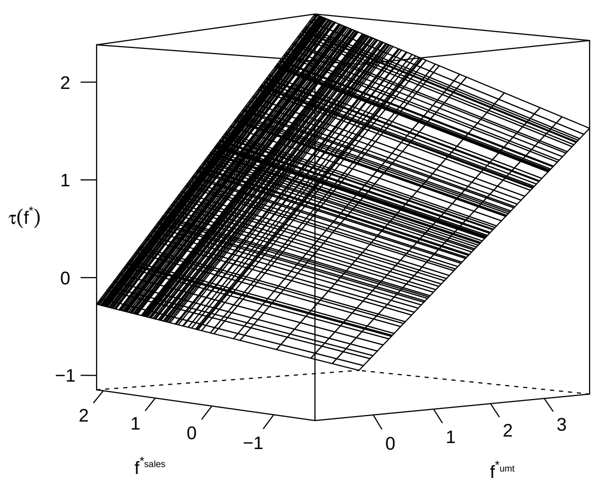


Figure 4: Estimated $\tau(f^*)$ effects under various allocations of the assignment with f^* fixed at the median value of the interference functions over the firms assigned to treatment.

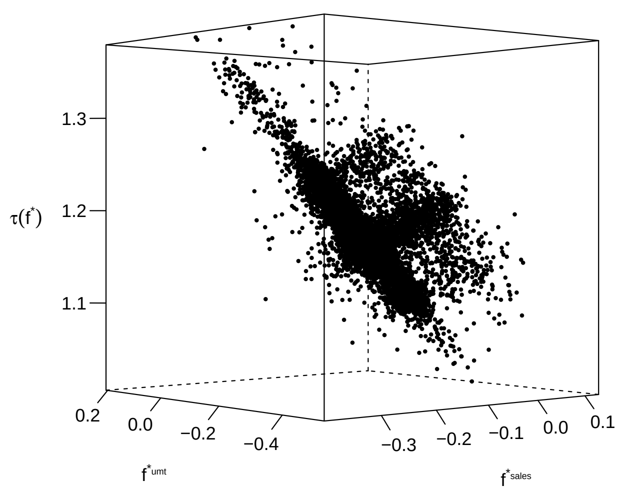


Table 1: Summary Statistics of covariates before and after matching

Variable	Mean: Assisted Firms	Mean: Non-Assisted Firms	Mean: Matched Non-Assisted Firms	%Absolute Bias Before Matching	%Absolute Bias After Matching
Economic activity sector					
D	0.76	0.72	0.76	8.9	0.0
F	0.12	0.17	0.12	14.2	0.0
G	0.06	0.10	0.06	12.7	0.0
K	0.06	0.02	0.06	22.6	0.0
Province					
Arezzo	0.18	0.23	0.19	12.0	2.7
Florence	0.39	0.38	0.35	2.6	8.8
Grosseto, Siena	0.06	0.05	0.12	5.4	18.5
Prato, Pistoia	0.19	0.18	0.19	4.0	0.0
Lucca, Massa, Pisa	0.17	0.16	0.15	2.0	5.8
Sales (2002)					
Up to 25 000	0.02	0.03	0.02	7.8	0.0
(25 000; 50 000]	0.05	0.03	0.02	9.3	16.8
(50 000; 100 000]	0.06	0.08	0.07	5.4	4.2
(100 000; 250 000]	0.15	0.22	0.14	17.8	3.0
(250 000; 500 000]	0.14	0.25	0.13	28.9	3.1
(500 000; 1 000 000]	0.29	0.18	0.27	24.5	4.7
Greater than 1 000 000	0.29	0.20	0.35	20.2	13.7
Legal status					
Individual	0.14	0.27	0.15	32.4	3.0
Partnership	0.52	0.56	0.54	7.9	4.2
Capital companies	0.34	0.17	0.31	39.1	6.8
Objective 2 or Phasing Out area	0.67	0.67	0.63	0.4	8.9
Year of start up					
Before 1980	0.24	0.26	0.27	3.8	4.9
$1980 - 1990$	0.26	0.27	0.32	3.5	14.1
$1990 - 2000$	0.29	0.35	0.22	13.1	14.6
After 2000	0.21	0.12	0.19	25.2	5.3
Main target market (local vs international)					
Local market	0.53	0.72	0.53	38.6	0.0
Main distribution channel (private vs other)					
Private distribution	0.32	0.44	0.39	25.7	15.5
Gender of the owner(s): Female owner	0.50	0.34	0.55	33.0	10.6
Age of the owner(s): Young owner	0.34	0.26	0.31	17.7	6.8
Number of employees (2002)	10.05	7.75	10.55	36.2	7.0

Table 2: Parameter estimates of conditional distribution of number of employees (reference group for categorical variables in parenthesis).

Variable	Coefficient	Standard Error	Coefficient	Standard Error
Treatment status: $T_{ij}^{(j),obs}$	1.37	0.62	1.07	0.50
Interference functions				
Geographical distance: $\overline{f}_1(\boldsymbol{T}_{-ij}^{(j),obs})$	–	–	0.04	0.35
Sales distance: $\overline{f}_2(\boldsymbol{T}_{-ij}^{(j),obs})$	–	–	-1.63	1.50
Interactions				
$T_{ij}^{(j),obs} \times \overline{f}_1(\boldsymbol{T}_{-ij}^{(j),obs})$	–	–	-0.71	0.83
$T_{ij}^{(j),obs} \times \overline{f}_2(\boldsymbol{T}_{-ij}^{(j),obs})$	–	–	-0.18	0.43
Economic activity sector (K)				
D	0.85	0.16	0.83	0.16
F	-0.14	0.73	-0.15	0.78
G	0.81	1.22	0.76	1.17
Province (Lucca, Massa, Pisa)				
Arezzo	0.39	1.10	0.63	1.12
Florence	-0.29	0.75	-0.48	0.73
Grosseto, Siena	0.16	1.70	2.62	2.89
Prato, Pistoia	1.68	1.79	4.62	3.69
Sales 2002 (Greated than $1\,000\,000$				
Up to $25\,000$	1.11	1.62	4.00	3.23
$(25\,000; 50\,000]$	-2.47	3.31	4.33	4.71
$(50\,000; 100\,000]$	-2.90	3.01	2.97	3.81
$(100\,000; 250\,000]$	-2.01	2.57	3.01	3.58
$(250\,000; 500\,000]$	-2.55	2.64	1.48	2.62
$(500\,000; 1\,000\,000]$	-2.07	2.12	0.69	1.83
Legal status (Individual)				
Partnership	-2.29	1.78	-0.46	1.34
Capital companies	0.20	0.85	0.27	0.85
Objective 2 or Phasing Out area	0.45	1.04	0.50	1.06
Year of start up (After 2000)				
Before 1980	0.37	0.76	0.31	0.78
$1980 - 1990$	-0.26	0.73	-0.65	0.86
$1990 - 2000$	0.80	0.69	0.44	0.70
Main target market: Local market	1.22	1.01	0.97	0.92
Main distribution channel: Private distribution	0.29	0.51	0.37	0.56
Gender of the owner(s): Female owner	-1.07	0.65	-0.95	0.65
Age of the owner(s): Young owner	0.32	0.71	0.52	0.79
Number of employees (2002)	0.03	0.76	0.03	0.78
Constant	1.84	2.76	-2.33	3.70

Table 3: Summary statistics of medians of the interference functions and the estimated $\tau(f^*)$ effects over 10000 simulated treatment allocations

Interference Function	Mean	SD	Minimum	25%	50%	75%	Maximum
f_{umt}	-0.17	0.07	-0.52	-0.18	-0.16	-0.14	0.21
f_{size}	-0.10	0.06	-0.40	-0.13	-0.10	-0.08	0.13
$\tau(f^*)$	1.17	0.04	1.01	1.15	1.17	1.20	1.38