The Annals of Applied Statistics
2016, Vol. 10, No. 3, 1170–1194
DOI: 10.1214/15-AOAS902

ASSESSING THE CAUSAL EFFECTS OF FINANCIAL AIDS TO FIRMS IN TUSCANY ALLOWING FOR INTERFERENCE

BY BRUNO ARPINO AND ALESSANDRA MATTEI[1]

Universitat Pompeu Fabra and University of Florence

We consider policy evaluations when the Stable Unit Treatment Value Assumption (SUTVA) is violated due to the presence of interference among units. We propose to explicitly model interference 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. We stress the importance of considering possible interference among units when evaluating and planning policy interventions.

1. Introduction. Regional and national development policies are an important tool for setting up and supporting local enterprise. Most countries spend relevant amounts of money on programs intended to promote R&D investments [Takalo, Tanayama and Toivanen (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 suboptimal [e.g., Duch, Montolio and Mediavilla (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 outcomes framework, commonly referred to as Rubin's Causal Model [Holland (1986)], to evaluate causal effects of policy interventions or programs on business performances such as productivity, investments, returns on capital, sales or employment [e.g., Almus and Czarnitzki (2003), Battistin, Gavosto and Rettore (2001), Bronzini and De Blasio (2006), Pellegrini and Carlucci (2003)]. 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 observed outcome [see Klette, Møen and Griliches (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

Received May 2014; revised June 2015.

[1]Supported in part by the Italian grant Futuro in Ricerca 2012 RBFR12SHVV_003, funded by the Italian Ministry of Research and Higher Education.

Key words and phrases. Interference, causal inference, policy evaluation, potential outcomes, Rubin Causal Model, 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 some settings it may be untenable. Firms operating in the same geographical area and/or sector of activity are likely to interact with each other, and studies aimed at evaluating programs that provide services or financial assistance to firms should consider that a treatment received by one firm may affect its competitors' potential outcomes.

In the causal inference literature, research on drawing inference on causal effects in the presence of interference is not yet common, although some exceptions exist [see, e.g., Aronow (2012), Crépon et al. (2013), Hudgens and Halloran (2008), Kao et al. (2012), Rosenbaum (2007), Samii and Aronow (2013), Sobel (2006), Tchetgen Tchetgen and VanderWeele (2012), Verbitsky and Raudenbush (2004)]. 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), targeted at artisan firms with a registered office in Tuscany. The main objective of the program 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 with each other, casting 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. Similarly, some works evaluating subsidies to firms 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 depend on a distance metric, based on firms' characteristics, namely, a measure of firms' size and their 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 focus on objectively designing our observational study using only background information on the units in the study, without access to the outcome. Specifically, we use propensity score matching [Dehejia and Wahba (1999), Imbens (2004), Rosenbaum and Rubin (1983)] 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, we model interference among firms in the matched dataset assuming that the level of interference which a firm is subject to depends on both its own and its competitors' characteristics. Finally, in the third step, we use a regression approach to estimate the causal effect of the policy intervention taking into account interference. Although we use regression methods for inference, the design phase of the study, aiming at reconstructing the hypothetical broken randomized experiment that led to the observed data, makes inference on causal effects less dependent on modeling assumptions and specifications [Ho et al. (2007)].

The paper is organized as follows. In Section 2 we introduce the potential outcomes 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 Sections 4 and 5 we apply the proposed approach to our motivating example. Section 6 concludes.

2. The potential outcomes framework and the SUTVA. Consider a group of firms, indexed 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/nonassisted firms). Let $\mathbf{T}$ be the N-dimensional vector of assignments with ith element T_i, and let $\mathbf{T}_{-i}$ be the vector of assignments with T_i removed. Let $Y_i(\mathbf{T}) \equiv Y_i(T_i, \mathbf{T}_{-i})$ denote the potential outcomes for firm i (e.g., number of employees, sales, production innovation) given the treatment vector $\mathbf{T}$. In the potential outcomes framework, SUTVA is usually an invoked assumption. SUTVA rules out hidden versions of treatments as well as interference between units. Formally, we have the following:

ASSUMPTION 1 [SUTVA, Rubin (1980, 1990)].

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

SUTVA allows us to write $Y_i(T_i, \mathbf{T}_{-i})$ 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 for each unit there is only a single version of each treatment level, ruling out that a particular unit could be exposed to treatment levels of varying efficacy. This component of SUTVA is violated, for example, in evaluation studies that only distinguish beneficiary and nonbeneficiary firms in the presence of different amounts or types of benefits of varying efficacy 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.

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 [Hudgens and Halloran (2008), Rosenbaum (2007), Sobel (2006)].[2] In business policy evaluation, firms operating in the same geographical area and sector of activity are likely to compete for scarce resources (e.g., credits) and customers, especially in small-business markets, such as the local market in Tuscany. This implies that a policy intervention assigning an incentive to a firm may also affect 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). Our approach can be, however, extended to multi-valued 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. In these cases, measuring the magnitude of the generated spillovers is by itself a crucial part of the evaluation

[2] 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.

study [Eberhardt, Helmers and Strauss (2013), Klette, Møen and Griliches (2000), Takalo, Tanayama and Toivanen (2013)].

In recent years, studies on spillover or peer effects have literally exploded, especially in economics. However, these studies usually adopt different types of regression models, such as versions of the linear-in-means model [see, e.g., Bramoullé, Djebbari and Fortin (2009)], without adopting a formal causal framework that allows to clearly define the causal estimands of interest and explicitly state the critical assumptions.

We adopt the potential outcomes framework and focus on the evaluation of the causal effect of a policy intervention for treated firms in the presence of interference; we do not aim at assessing the effects of interference *per se*.[3] 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 depend not only 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: $Y_i(T_i, \mathbf{T}_{-i})$ versus $Y_i(T_i', \mathbf{T}_{-i}')$, $T_i, T_i' \in \{0, 1\}$, and $\mathbf{T}_{-i}, \mathbf{T}_{-i}' \in \{0, 1\}^{(N-1)}$.

To address the complications due to completely relaxing 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 strengths 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 let N_j the number of firms in sector j, $j = 1, \dots, K$. The vector of treatment assignments $\mathbf{T}$ can be conveniently decomposed as follows: $\mathbf{T} = [\mathbf{T}_1, \dots, \mathbf{T}_K]^{\mathrm{tr}}$, where $\mathbf{T}_j = (T_{ij}, \mathbf{T}_{-ij})$, and ij represents firm i in sector j[4].

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

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

[4] 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 2.

$$\text{If } \mathbf{T}_j = \mathbf{T}'_j, \text{ then } Y_{ij}(\mathbf{T}) = Y_{ij}(\mathbf{T}') \text{ for all } \mathbf{T}, \mathbf{T}' \in \{0, 1\}^N.$$

Assumption 2, defined by Sobel (2006) as partial interference, implies that $Y_{ij}(\mathbf{T})$ is equal to $Y_{ij}(\mathbf{T}_j)$, 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.* Interactions occurring among firms in the same sector of activity may be of different intensity, depending on firms' characteristics. We assume that for each firm ij, interference can be summarized by a multi-valued function of treatment assignments of firm ij's competitors, $f(\mathbf{T}_{-ij})$, so that $Y_{ij}(T_{ij}, \mathbf{T}_{-ij}) = Y_{ij}(T_{ij}, f(\mathbf{T}_{-ij}))$. Formally, we make the following assumption:

ASSUMPTION 3. There exists an m-valued function of treatment assignments $f(\mathbf{T}_{-i}) = [f_1(\mathbf{T}_{-i}), \dots, f_m(\mathbf{T}_{-i})]^{\text{tr}}$, with $m \geq 1$, such that $f(\mathbf{T}_{-i}) = f(\mathbf{T}_{-ij})$ and

if $T_{ij} = T'_{ij}$ and $f(\mathbf{T}_{-ij}) = f(\mathbf{T}'_{-ij})$, then $Y_{ij}(\mathbf{T}) = Y_{ij}(\mathbf{T}')$ for all $\mathbf{T}, \mathbf{T}' \in \{0, 1\}^N$.

It is worth noting that while Assumption 2 implies that potential outcomes for each firm ij depend on the whole vector of treatment assignments in sector j, Assumption 3 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 3 the influence of competitors' treatment assignments on firm ij potential outcomes may depend, through f, on firm ij's competitors 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.

Here we focus on linear combinations of treatment assignments: $f_r(\mathbf{T}_{-i}) = f_r(\mathbf{T}_{-ij}) = (\mathbf{w}^r_{ij})^{\text{tr}}\mathbf{T}_{-ij}$, where $\mathbf{w}^r_{ij} = [w^r_{i1,j}, \dots, w^r_{ii-1,j}, w^r_{ii+1,j}, \dots, w^r_{iN_j,j}]^{\text{tr}}$ is a $(N_j - 1)$-dimensional vector of weights for firm ij, $r = 1, \dots, m$. As a simple example consider the real function $f(\mathbf{T}_{-ij}) = (\mathbf{w}_{ij})^{\text{tr}}\mathbf{T}_{-ij}$, where $w_{ih,j} = \dfrac{1}{N_j - 1}$ for each $h \neq i = 1, \dots, N_j$, $j = 1, \dots, K$. In words, in this case $f(\mathbf{T}_{-ij})$ is the proportion of treated firms in sector j excluded firm ij. Under this specification of the function f, Assumption 3 implies the strength of interference to be constant for all units within a group, given their treatment status. This relaxed version of the no-interference assumption was first introduced by Hong and Raudenbush (2006), who considered the estimation of the causal effect of retaining low-achieving children in kindergarten rather than promoting them to first grade. They argued 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) relaxed the standard no-interference component of 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.

Another simple case is when each element of $\mathbf{w}^r_{ij}$ 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 prespecified distance threshold. This amounts to assuming that treatment assignment of firms with a zero weight does not affect firm ij's potential outcomes.

These simple specifications of the weights might be somewhat restrictive. More generally, the weights, $\mathbf{w}^r_{ij}$, can be specified as any real-value function and can also depend on firms' characteristics. Specifically, let $\mathbf{Z}_j$ be the $N_j \times p$-dimensional matrix of variables affecting the strength of interference among firms in sector j, with the ith row equal to $\mathbf{Z}_{ij}$. Then we assume that $w^r_{ih,j} = g_r(\mathbf{Z}_{ij}, \mathbf{Z}_{hj})$, with $h = 1, \dots, N_j$, $h \neq i$. A downside of this approach is that inference based on nonparametric methods might raise serious challenges. We propose a regression approach that explicitly uses the weights $w^r_{ih,j}$ to account for interference.

Under Assumption 3, an average causal effect can be defined as

$$\mathbb{E}\big[Y\big(T_{ij}, f(\mathbf{T}_{-ij})\big) - Y\big(T'_{ij}, f\big(\mathbf{T}'_{-ij}\big)\big)\big].$$

In this paper we focus on average causal effects for the treated [ATT, e.g., Imbens (2004)], which can be generally defined as follows:

$$\mathbb{E}\big[Y\big(T_{ij}, f(\mathbf{T}_{-ij})\big) - Y\big(T'_{ij}, f\big(\mathbf{T}'_{-ij}\big)\big)|T_{ij} = 1\big].$$

Specifically, we consider the following estimands:

$$\begin{aligned}\tau(f^*) = \mathbb{E}\big[Y\big(T_{ij}, f(\mathbf{T}_{-ij})\big) - Y\big(T'_{ij}, f\big(\mathbf{T}'_{-ij}\big)\big)|\\ T_{ij} = 1, f(\mathbf{T}_{-ij}) = f\big(\mathbf{T}'_{-ij}\big) = f^*\big]\end{aligned} \tag{1}$$

and

$$\begin{aligned}\tau = \mathbb{E}\big[\mathbb{E}\big[Y\big(T_{ij}, f(\mathbf{T}_{-ij})\big) - Y\big(T'_{ij}, f\big(\mathbf{T}'_{-ij}\big)\big)|\\ T_{ij} = 1, f(\mathbf{T}_{-ij}) = f\big(\mathbf{T}'_{-ij}\big) = f\big]\big],\end{aligned} \tag{2}$$

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 prefixed 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 in planning 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. Studying the impact of financial aids to firms in Tuscany: Background and study design.

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 prefixed 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, firms that applied proposing low-quality investment projects also 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 the 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 modifying 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, 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 information on geographic coordinates (UMT—Universal Transverse Mercator), 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 nonassisted 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 majority 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 firms with missing values on relevant variables. The selection procedure leads to a sample of 94 assisted firms and 528 nonassisted firms.[5]

[5]Restricting the analysis to the subsample of firms with no missing values on the relevant variables (complete-case analysis) implies that we are assuming that the missing data mechanism was "Missing Completely At Random" [Mealli and Rubin (2015), Rubin (1976)]. The missing completely at random assumption may be a strong assumption that does not need to be generally applicable. It has, however, testable implications: Under the missing completely at random assumption, the joint distribution of the observed variables should be the same between complete cases and units with missing values. In our study, the distribution of the observed variables is similar between firms with no missing values and firms with missing values. Therefore, the missing completely at random assumption is not falsified by the data, and we invoke it being the focus of the paper on the role of the no-interference assumption in casual inference. Nevertheless, it is worth noting that if missing completely at random does not hold, a complete case analysis may lead to biased estimates. In addi-

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 nonassisted firms: the initial absolute standardized percent bias [ASB, Rosenbaum and Rubin (1985)] is greater than 20% for 11 out of 28 covariates, and greater than 30% for 5 covariates including pretreatment number of employees (see Table 1). In our observational study, however, we have data on the most important determinants of the decision to apply for public assistance and of firms' performance, including performance indicators measured before treatment. Therefore, we can reasonably assume strong ignorability of the treatment conditional on observed covariates:

ASSUMPTION 4 (Strong ignorability). For each $\mathbf{T} \in \{0, 1\}^N$

$$\text{Unconfoundedness:} \quad P\big(\mathbf{T}|\mathbf{X}, \{\mathbf{Y}(\mathbf{T})\}_{\mathbf{T}\in\{0,1\}^N}\big) = P(\mathbf{T}|\mathbf{X}),$$

$$\text{Probabilistic assignment:} \quad 0 < \sum_{\mathbf{T}:T_i=1} P(\mathbf{T}|\mathbf{X}) < 1,$$

where $\mathbf{X}$ is the matrix of pretreatment variable, and $\{\mathbf{Y}(\mathbf{T})\}_{\mathbf{T}\in\{0,1\}^N}$ is the $N \times 2^N$-dimensional matrix of potential outcomes.

Unconfoundedness amounts to assuming that within cells defined by the values of pretreatment variables $\mathbf{X}$, the treatment is assigned independently of potential outcomes. The second condition requires that the assignment mechanism is probabilistic, namely, that the probability of assignment to treatment for each unit i is strictly between zero and one.

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, King and Porro (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 number of employees). In particular, we selected a subset of 94 nonassisted firms using one-to one nearest neighbor propensity score matching (without replacement) combined with exact matching on sector of activity.

---

tion, throwing away data can lead to estimates with larger standard errors due to reduced sample size. Therefore, a valuable topic for future research is developing a framework to simultaneously address the complications due to the presence of both interference and unintended missing values using, e.g., multiple imputation methods.

TABLE 1
Summary statistics of pretreatment covariates before and after matching

Variable	Mean: Assisted firms	Mean: Nonassisted firms	Mean: Matched nonassisted firms	ASB %: Before matching	ASB %: After matching
Economic activity sector					
D	0.76	0.72	0.76	8.95	0.00
F	0.12	0.17	0.12	14.27	0.00
G	0.06	0.10	0.06	12.72	0.00
K	0.06	0.02	0.06	22.68	0.00
Province					
Arezzo	0.18	0.23	0.18	11.99	0.00
Florence	0.39	0.38	0.38	2.66	2.18
Grosseto, Siena	0.06	0.05	0.06	5.46	0.00
Prato, Pistoia	0.19	0.18	0.21	3.96	5.49
Lucca, Massa, Pisa	0.17	0.16	0.16	1.97	2.86
Sales (2002)					
Up to 50,000	0.07	0.07	0.07	2.44	0.00
(50,000, 100,000]	0.06	0.08	0.09	5.39	8.30
(100,000, 250,000]	0.15	0.22	0.16	17.87	2.76
(250,000, 500,000]	0.14	0.25	0.13	28.97	2.71
(500,000, 1,000,000]	0.29	0.18	0.27	24.58	5.05
Greater than 1,000,000	0.29	0.20	0.29	20.24	0.00
Legal status					
Individual	0.14	0.27	0.13	32.45	2.68
Partnership	0.52	0.56	0.53	7.90	2.14
Capital companies	0.34	0.17	0.34	39.23	0.00
Objective 2 or phasing out area	0.67	0.67	0.66	0.35	2.26
Year of start up					
Before 1980	0.24	0.26	0.27	3.84	4.89
1980–1990	0.26	0.27	0.23	3.52	4.83
1990–2000	0.29	0.35	0.30	13.18	2.29
After 2000	0.21	0.12	0.20	25.31	2.88
Main target market (local vs international)					
Local market	0.53	0.72	0.56	38.69	6.71
Main distribution channel (private vs other)					
Private distribution	0.32	0.44	0.32	25.75	0.00
Gender of the owner(s): Female owner	0.50	0.34	0.51	33.07	2.19
Age of the owner(s): Young owner	0.34	0.26	0.33	17.74	2.33
Number of employees (2002)	10.05	7.75	9.93	36.19	2.00

As can be seen in Table 1, the ASB after matching is dramatically reduced for most of the covariates and overall the matching solution is satisfactory. To adjust for residual imbalance, and also improve efficiency, we conduct the outcome analysis, implemented on the sample of treated and matched control firms, conditioning on pretreatment variables.

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 socioeconomic perspective.

4.3. *Modeling interference among tuscan small-handicraft firms*. The specification of the interference function(s) should depend on substantive knowledge on the phenomenon under study. In our study, the specification of the interference function(s) is mainly driven by the features of the business market in Tuscany.

Tuscany is a region in the center of Italy consisting of 10 provinces: Arezzo, Florence, 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 northeast of Tuscany (especially in the provinces of Arezzo, Florence, 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/northeast of Tuscany are still those where more firms concentrate. When considering provinces as a pretreatment variable 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, indeed, most handicraft firms in Tuscany are small- or medium-sized enterprises. Also, firms operating in the same market usually have similar needs, interests and knowledge, especially if they are located relatively nearby. Therefore, competition is expected to be higher among firms operating in the same market and located nearby [e.g., Dei Ottati (1994)].

These features of the Tuscan business market suggest that interference among handicraft firms in the same sector of activity might mainly depend on some measure of firms' size and geographical distance. In other words, we can reasonably expect that providing a benefit to one firm can also affect the outcomes of others in the same sector of activity and that interactions are stronger among geographically close firms and for smaller firms. In fact, firms' performances 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

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

and assume that interference depends on pretreatment sales and geographical location. Formally, we assume that interference can be summarized by a $m = 2$-valued function $f = (f_1, f_2)$ with $f_r(\mathbf{T}_{-ij}) = (\mathbf{w}_{ij}^r)^{\mathrm{tr}}\mathbf{T}_{-ij}$, $r = 1, 2$, where the weights, $\mathbf{w}_{ij}^r$, are defined as follows. Let Z_{ij1} be pretreatment sales for firm ij, and let $Z_{ij2}^{(1)}$ and $Z_{ij2}^{(2)}$ be the pair of variables measuring UTM (Universal Transverse Mercator) coordinates for firm ij. The weights $\mathbf{w}_{ij}^{r=1}$ and $\mathbf{w}_{ij}^{r=2}$ are respectively based on the Canberra distance between sales and the Euclidean distance between UTM coordinates. Formally, let $d_{ih,j}^{r=1}$ denote the Canberra distance between sales of firm ij and sales of firm hj, $h \neq i$:

$$d_{ih,j}^{r=1} = \frac{|Z_{ij1} - Z_{hj1}|}{Z_{ij1} + Z_{hj1}}.$$

We choose the Canberra distance because it allows us to standardize with respect to the total size of the firms being compared. An element $w_{ih,j}^{r=1}$ of the vectors of weights $\mathbf{w}_{ij}^{r=1}$ is defined as follows:

$$w_{ih,j}^{r=1} = \begin{cases} 1 + d_{ih,j}^{r=1}, & \text{if } Z_{hj1} \geq Z_{ij1}, \\ 1 - d_{ih,j}^{r=1}, & \text{if } Z_{hj1} < Z_{ij1}. \end{cases}$$

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 $w_{ih,j}^{r=1}$ is one if firms ij and hj have the same size.

Similarly, let $d_{ih,j}^{r=2}$ be the Euclidean distance between the UTM coordinates of firm ij and the UTM coordinates of firm hj, $h \neq i$: $d_{ih,j}^{r=2} = \sqrt{(Z_{ij2}^{(1)} - Z_{hj2}^{(1)})^2 + (Z_{ij2}^{(2)} - Z_{hj2}^{(2)})^2}$. An element $w_{ih,j}^{r=2}$ of the vector of weights $\mathbf{w}_{ij}^{r=2}$ is defined as the reciprocal of the Euclidean distance between the UTM coordinates of firm ij and the UTM coordinates of firm hj, $h \neq i$: $w_{ih,j}^{r=2} = 1/d_{ih,j}^{r=2}$.

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

5. Studying the impact of financial aids to firms in Tuscany: Outcome analysis. Let $\mathbf{T}^{\text{obs}} = [\mathbf{T}_1^{\text{obs}}, \ldots, \mathbf{T}_K^{\text{obs}}]^{\text{tr}}$ be the vector of treatments intakes, where $\mathbf{T}_j^{\text{obs}} = (T_{ij}^{\text{obs}}, \mathbf{T}_{-ij}^{\text{obs}})$, and let Y_{ij}^{obs} be the actual outcome (number of employees) for firm ij in sector j. We model the conditional expectation of Y_{ij}^{obs} given T_{ij}^{obs}, $f_r(\mathbf{T}_{-ij}^{\text{obs}})$, $r = 1, 2$ and $\mathbf{X}_{ij}$ as a flexible function of its arguments. Formally,

$$\begin{aligned} &\mathbb{E}[Y_{ij}^{\text{obs}} | T_{ij}^{\text{obs}}, f_1(\mathbf{T}_{-ij}^{\text{obs}}), f_2(\mathbf{T}_{-ij}^{\text{obs}}), \mathbf{X}_{ij}] \\ &\quad = \alpha_1 T_{ij}^{\text{obs}} + \alpha_2 f_1(\mathbf{T}_{-ij}^{\text{obs}}) + \alpha_3 f_2(\mathbf{T}_{-ij}^{\text{obs}}) + \alpha_4 T_{ij}^{\text{obs}} f_1(\mathbf{T}_{-ij}^{\text{obs}}) \\ &\qquad + \alpha_5 T_{ij}^{\text{obs}} f_2(\mathbf{T}_{-ij}^{\text{obs}}) + \mathbf{X}_{ij}\boldsymbol{\beta} + \beta_0. \end{aligned} \tag{3}$$

Under strong ignorability and model (3),

$$\begin{aligned} &\mathbb{E}[Y(T_{ij}, f(\mathbf{T}_{-ij})) - Y(T'_{ij}, f(\mathbf{T}'_{-ij}))] \\ &\quad = [\alpha_1 T_{ij} + \alpha_2 f_1(\mathbf{T}_{-ij}) + \alpha_3 f_2(\mathbf{T}_{-ij}) + \alpha_4 T_{ij} f_1(\mathbf{T}_{-ij}) + \alpha_5 T_{ij} f_2(\mathbf{T}_{-ij})] \\ &\qquad - [\alpha_1 T'_{ij} + \alpha_2 f_1(\mathbf{T}'_{-ij}) + \alpha_3 f_2(\mathbf{T}'_{-ij}) \\ &\qquad + \alpha_4 T'_{ij} f_1(\mathbf{T}'_{-ij}) + \alpha_5 T'_{ij} f_2(\mathbf{T}'_{-ij})]. \end{aligned}$$

We estimate the model parameters by ordinary least squares: Let $\hat{\alpha}_\ell$, $\ell = 1, \dots, 5$, and $\hat{\boldsymbol{\beta}}$ and $\hat{\beta}_0$ be the ordinary least squares estimates of the model parameters. Given the estimated parameters, causal effects and their standard errors are estimated using the estimating equation. For instance, the average causal effect for a given allocation of the assignment $\mathbf{T} = [\mathbf{T}_1, \dots, \mathbf{T}_K]^{\mathrm{tr}}$ and a prefixed interference level f^*, that is, 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

$$\begin{aligned}
&\hat{\text{s.e.}}\ (\hat{\tau}(f^*)) \\
&\quad = \big(\hat{\mathbb{V}}(\hat{\alpha}_1) + (f_1^*)^2 \hat{\mathbb{V}}(\hat{\alpha}_4) + (f_2^*)^2 \hat{\mathbb{V}}(\hat{\alpha}_5) + 2 f_1^* \widehat{\mathrm{Cov}}(\hat{\alpha}_1, \hat{\alpha}_4) \\
&\qquad + 2 f_2^* \widehat{\mathrm{Cov}}(\hat{\alpha}_1, \hat{\alpha}_5) + 2 f_1^* f_2^* \widehat{\mathrm{Cov}}(\hat{\alpha}_4, \hat{\alpha}_5)\big)^{1/2}.
\end{aligned}$$

In order to investigate the role of interference in our study, we also conduct inference under SUTVA. Under SUTVA (Assumption 1), there are only two potential outcomes for each firm: $Y_{ij}(0)$ and $Y_{ij}(1)$. We specify the following regression model for the potential outcomes (number of employees) conditional on the pretreatment variables:

$$\mathbb{E}[Y_{ij}^{\mathrm{obs}} | T_{ij}^{(j),\mathrm{obs}}, \mathbf{X}_{ij}] = \tilde{\alpha}_1 T_{ij}^{\mathrm{obs}} + \mathbf{X}_{ij} \tilde{\boldsymbol{\beta}} + \tilde{\beta}_0. \tag{4}$$

Note that, under strong ignorability, the coefficient on the treatment indicator in model (4), $\tilde{\alpha}_1$, is the average causal effect of interest.

As discussed in Section 4.2, the analysis of the outcome data was preceded by a careful design phase where we matched treated and control units on pretreatment covariates by using propensity score matching. In the resulting matching dataset the distribution of covariates is much more similar than it was in the raw dataset. We also checked the balance of the distribution of the interference functions, f_1 and f_2. The values of the ASB for f_1 and f_2 were, respectively, 5.31% and 9.90% before matching and 0.02% and 7.43% after matching. Therefore, matching also reduced the ASB for the interference functions. Conducting inference on the matched dataset greatly reduces the possibility that estimated causal effects are affected by extrapolation and it also reduces the dependence of the results on the parametric assumptions [e.g., that the outcome is a linear function of the treatment and interference functions, Ho et al. (2007)]. Nonetheless, we included the pretreatment covariates as control variables in the regression model with the sole goal of reducing possible bias resulting from remaining imbalance and not to increase model fit, that was not our guiding principle. We also considered alternative models, including polynomial regressions with quadratic terms in the interference functions, and semiparametric regression models with spline smooth functions with respect to the interference functions. We found that our results were robust with respect to the model specification, but more complex models led to higher variability, probably due to the small sample size. We therefore opted for a linear model.[6]

5.1. *Results*. Table 2 shows point estimates and standard errors of the parameters of the regression models in equation (3) (columns 1 and 2) and in equation (4) (columns 3 and 4).

First, consider the last two columns in Table 2, presenting results for the case under the no-interference assumption. Under SUTVA and model (4), the estimate of the coefficient on the treatment indicator, $\tilde{\alpha}_1$, is 1.37 with standard error equal to 0.62, suggesting a positive and statistically significant impact of the policy intervention.

The first two columns of Table 2 show parameter estimates of the conditional distribution of number of employees taking into account interference [model (3)]. 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.

We use the model parameters to estimate the causal effects of interest. We first focus on average causal effects under prefixed levels of interference, $\tau(f^*)$. Interference levels f^* are chosen on the basis of the empirical distribution of the two components of the interference function f. The observed values of the interference function based on the sales distance, f_1, range from 1.04 to 141.88 with median equal to 56.62, and the observed values of the interference function based on the geographical distance, f_2, range from about 0 to 0.0272 with median equal to 0.0031. Table 3 shows the estimated conditional causal effects, $\tau(f^*)$, and their standard errors: (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 some percentiles of its empirical distribution (see the first block of columns in Table 3); and (b) fixing the interference function based on the geographical distance at its observed median value and ranging the interference function based on the sales distance over some percentiles of its empirical distribution (see the second block of columns in Table 3).

As we can see in Table 3, the estimated $\tau(f^*)$ effects steadily reduce as the strength of interference increases: High values of the interference functions lead to really small estimates of the causal effects with large standard errors, suggesting that the presence of interference may strongly affect the benefit of the policy, and so ignoring interference may lead to misleading conclusions. For medium/low values of interference, our results show some evidence that the PDC helps increasing employment levels. For example, for f_2 fixed at its observed median level, if we

[6]Note that given the limited sample size we did not include any interaction between the treatment and covariates and focused only on possible interactions between the treatment and the interference functions

TABLE 2

Parameter estimates of conditional expectations of number of employees under interference, model (3), and under no-interference, model (4). Reference group for categorical variables in parenthesis

	Model (3)		**Model (4)**	
Variable	**Coefficient**	**Standard error**	**Coefficient**	**Standard error**
Treatment status: T_{ij}	2.33	1.23	1.37	0.62
Interference functions				
Geographical distance: $f_1(\mathbf{T}_{-ij}) \cdot 100$	−0.28	1.18	–	–
Sales distance: $f_2(\mathbf{T}_{-ij})/100$	−1.84	2.91	–	–
Interaction				
$T_{ij} \times f_1(\mathbf{T}_{-ij}) \cdot 100$	−0.56	1.52	–	–
$T_{ij} \times f_2(\mathbf{T}_{-ij})/100$	−1.24	1.94	–	–
Economic activity sector (K)				
D	2.29	2.91	−0.34	1.47
F	2.69	2.00	1.60	1.69
G	1.13	2.19	0.16	1.93
Provinces (Arezzo)				
Florence	0.37	0.93	0.39	0.93
Grosseto, Siena	0.63	1.67	0.47	1.66
Prato, Pistoia	−0.40	1.03	−0.35	1.02
Lucca, Massa, Pisa	−0.71	1.18	−0.55	1.15
Sales (2002) (Greater than 1,000,000)				
Up to 50,000	−1.96	2.71	−3.75	1.96
(50,000, 100,000]	−1.12	2.48	−2.59	1.82
(100,000, 250,000]	−1.76	2.13	−3.05	1.44
(250,000, 500,000]	−2.34	1.65	−3.29	1.28
(500,000, 1,000,000]	−2.11	1.15	−2.71	0.98
Legal status (Individual)				
Partnership	0.56	1.12	0.65	1.10
Capital companies	1.09	1.30	1.08	1.28
Objective 2 or phasing out area	0.91	0.76	0.93	0.74
Year of start up (After 2000)				
Before 1980	0.15	1.25	0.48	1.21
1980–1990	0.71	1.25	1.04	1.21
1990–200	0.30	1.09	0.55	1.05
Main target market (local vs international)				
Local market	0.09	0.72	0.04	0.71
Main distribution channel (private vs other)				
Private distribution	−0.79	0.87	−0.89	0.85
Gender of the owner(s): Female owner	0.44	0.71	0.42	0.70
Age of the owner(s): Young owner	−0.33	0.75	−0.24	0.74
Number of employees (2002)	0.81	0.07	0.81	0.07
Constant	1.05	3.10	2.61	2.81

TABLE 3

Estimated $\tau(f^*)$ effects and their standard errors derived (a) *fixing the interference function based on the sales distance at its observed median value* ($f_1^{\mathrm{Me}} = 56.62$) *and ranging the interference function based on the geographical distance over observed percentiles*; *and* (b) *fixing the interference function based on the geographical distance at its observed median value* ($f_2^{\mathrm{Me}} = 0.003$) *and ranging the interference function based on the sale distance over observed percentiles*

	$\hat{\tau}(f^*)$			$\hat{\tau}(f^*)$		
Percentile	f_2	$f^* = (f_1^{\mathrm{Me}}, f_2)$	**S.E.**	f_1	$f^* = (f_1, f_2^{\mathrm{Me}})$	**S.E.**
Min	0.0000	1.62	0.87	1.04	2.14	1.19
5%	0.0001	1.62	0.86	5.64	2.08	1.12
10%	0.0002	1.61	0.85	7.06	2.06	1.10
25%	0.0013	1.55	0.75	21.96	1.88	0.88
50%	0.0031	1.45	0.64	56.62	1.45	0.64
75%	0.0051	1.33	0.65	80.37	1.15	0.83
90%	0.0076	1.20	0.83	105.46	0.84	1.20
95%	0.0110	1.01	1.24	116.27	0.71	1.38
Max	0.0272	0.10	3.58	141.88	0.39	1.84

compare the estimated effect corresponding to the lowest level of the interference function based on sales distance, f_1, ($\tau(f^*) = 2.14$) to the one obtained for the highest level of f_1 ($\tau(f^*) = 0.39$), we find a difference of 1.75 units. This implies that, on average, if 100 firms are treated in a situation of low interference, the created employment is 214 units, while in the opposite case it would be 39 units only. Probably due to small sample sizes, the interaction effects are not statistically significant, and, as a consequence, most of the estimated effects for prefixed interference levels $\tau(f^*)$ are not statistically significant at the 5% level.

The estimate of the (marginal) average causal effect of the treatment, τ in equation (2), which is derived averaging the estimated $\tau(f^*)$ effects for f^* ranging over percentiles of the empirical distributions of f_1 and f_2, is approximately equal to 1.42 with standard error 0.43. The estimated effect is statistically significant and 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 estimated effect implies that for every 100 treated firms the policy is expected to generate 142 new employees.

The estimate of τ obtained averaging out the interference function is similar to the estimate of the average causal effect of the treatment under the no-interference assumption. Therefore, in our study the presence of interference does not seem to strongly affect the beneficial effects of the policy, on average. This result reflects the statistically negligible estimate of the coefficients of the interference functions and of their interactions with the treatment indicator. Nevertheless, we find some evidence that causal effects are heterogeneous with respect to different interference levels.

TABLE 4
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}	0.0084	0.0006	0.0062	0.0080	0.0084	0.0088	0.0111
f_{size}	170.35	8.45	127.57	165.12	170.78	175.95	203.21
$\tau(f^*)$	1.46	0.04	1.30	1.43	1.46	1.49	1.67

5.2. *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. To better understand how the interference functions and treatment effects vary over the assignment distribution, we conducted a small simulation study. This simulation is intended to complement the empirical results. Empirical results in fact are obtained conditionally on the observed allocation of treatment. This generates a given level of interference. In the simulation study we can answer an important question for policy making: what would be the estimated effect of the policy in the case of different treatment allocation rules? In particular, in the context of our paper, a policy rule could refer to either spatial allocations of incentives or to the size of firms (for instance, policy makers may decide to limit the number of incentives in the same local market and/or allocate a minimum number of incentives to small firms).

In our simulation study we use the data on handicraft firms in Tuscany, but we increase the sample size to $N = 564$, appending three copies of the original data set, to avoid sampling variability issues due to the small sample size. Given a sample of 564 firms, 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 both the number of firms assigned and not assigned to receive a benefit is fixed to $N/2 = 282$. Formally, we randomly draw $H = 10{,}000$ N-dimensional vectors with $N_T = 282$ ones and $N - N_T = 282$ zeros from the set of $\binom{N}{N_T}$ possible treatment vectors. For each draw from this set, we first calculate the interference function, $f(\mathbf{T}_{-ij})$, and then we estimate $\tau(f^*)$, with f^* fixed at the median value of $f(\mathbf{T}_{-ij})$ for the firms assigned to treatment.

Table 4 show summary statistics of the medians of the interference functions and of the estimated $\tau(f^*)$ effects over the simulated assignment distribution. The estimated $\tau(f^*)$ effects are relatively stable, ranging from 1.30 (s.e. $= 0.37$) to 1.67 (s.e. $= 0.37$). Nevertheless, Figure 2, showing the estimated $\tau(f^*)$ effects for each of the $H = 10{,}000$ treatment allocations, clearly highlights that the presence of interference may affect the evaluation results.

In line with the results described above, Figure 2 suggests that the beneficial effect of the policy is higher the lower the strength of interference is. Therefore,

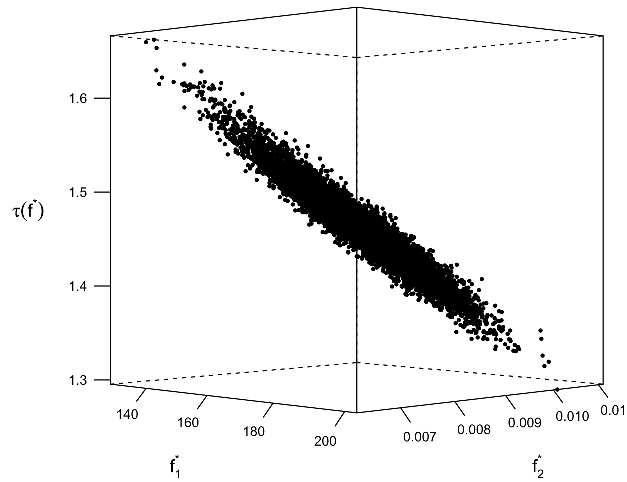

FIG. 2. *Estimated $\tau(f^*)$ effects under various allocations of the assignment with f^* fixed at the median value of the interference functions over the firms assigned to treatment.*

an allocation of treatments that does not account for either the distribution of sales among assisted firms or the geographical location of the assisted firms may reduce 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 features of the business market, policy makers could make a policy intervention more effective, generating a higher average treatment effect if they apply allocation rules that take interference into account.

6. Discussion and concluding remarks. Inspired by a program providing soft loans to Tuscan artisan firms in 2003/2004, the aim of this article was to discuss the violation of SUTVA, a standard assumption in the potential outcomes literature, due to interference among units. Given the characteristics of the Tuscan business market, where most artisan firms are small-sized and generally operate in a limited geographical area, interference among firms may be a relevant phenomenon and evaluating the role of interference is crucial. Previous evaluations studies in this area uncritically used SUTVA either implicitly or explicitly. In general, applied papers trying to relax the standard SUTVA are 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 applied works that relaxed SUTVA, we allow interference within groups to vary for each unit. However, it is 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, we can account for this by redefining the interference function so that it also depends on treatment assignments of firms in different sectors. 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.

We show that by allowing for interference, in addition to the standard average treatment effect on the treated (ATT), it is possible to consider another interesting causal estimand: the causal effect of receiving the benefit (versus not receiving it) given a certain level of interference. Both the empirical application and a small simulation study show that the beneficial causal effect of the policy is decreasing as the strength of interference increases. Therefore, ignoring SUTVA may hide heterogeneous effects of the policy for different interference levels and possibly produce misleading estimates of the average effect of a policy. Nevertheless, in our study we find that when we allow for interference, the average causal effect of the treatment on the treated firms is similar to the effect estimated under the standard SUTVA. 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.

It is possible to recognize some similarities between our approach and methods used in the spatial statistical literature. The approach typically used in spatial statistics to model interdependence among observations due to geographical proximity is through constructing spatial weights to reflect spatial interactions or spillovers [Anselin (2006), Harris, Moffat and Kravtsova (2011)]. Two types of modeling approaches have been proposed that differ on how spatial interactions are modeled. In the *spatial lag model* (or *spatial effects* model), interactions are modeled by the inclusion of (spatially weighted) variables directly into the model. In the *spatial error* model, spatial dependence is captured in the (spatially weighted) error term. The spatial lag model can be represented as

$$\mathbf{Y} = \rho \mathbf{W}\mathbf{Y} + \mathbf{X}\boldsymbol{\beta} + \mathbf{e}, \tag{5}$$

where $\mathbf{Y}$ represents the dependent variable, ρ is a scalar parameter measuring the strength of dependence between units, $\mathbf{X}$ is a matrix of independent variables with associated parameters $\boldsymbol{\beta}$, and $\mathbf{e}$ is a vector of independently and identically distributed disturbance terms with zero mean and variance σ^2. Finally, $\mathbf{W}$ is the spatial weights matrix that measures the strength of interactions between units as a function of their spatial closeness.

In model (5) interdependence among units operates directly via the outcome of the other units. In the context of our paper that focuses on violations of SUTVA, interactions among firms impact on the outcome variable indirectly via the treatment vector, $\mathbf{T}$. Model (5) can be modified by including spatially lagged predictor variables instead of, or in addition to, the spatial lag for the outcome variable by multiplying the weights matrix to $\mathbf{X}$ [Halleck Vega and Elhorst (2015)]. A spatial lag model more similar to our regression model [model (3)] could be then formulated as

$$\mathbf{Y} = \rho\mathbf{W}\mathbf{T} + \mathbf{X}\boldsymbol{\beta} + \mathbf{e}. \tag{6}$$

In the social network literature, the spatial lag model (and similar modeling approaches) has been used to model interdependence among observations that arise from reasons different than geographical proximity [Doreian (1996)] as social or economic factors. In network analysis, the matrix $\mathbf{W}$ in Models (5) and (6) defines the existence of links (ties) among units and is known as an adjacency matrix or sociomatrix. We can think of our sample of firms as a particular type of social network where firms are assumed to be connected to all other firms in the same sector by a competition relationship. The adjacency matrix $\mathbf{W}$ would be represented in our case by a block diagonal matrix with elements equal to one for firms in the same sector and 0 otherwise. In this paper, we allowed interdependence among firms to depend on firms' characteristics (geographical distance and firms' sizes). This can be accomplished in network analysis models by specifying a weighted adjacency matrix [Börner, Sanyal and Vespignani (2007)]. Moreover, it is possible to generalize the spatial lag model to include two (or more) distinct adjacency matrices, $\mathbf{W}_1$ and $\mathbf{W}_2$, and estimate separate parameters ρ_1 and ρ_2 for the relative impact of each [Gleditsch, Ward and Kristian (2007)]:

$$\mathbf{Y} = \rho_1\mathbf{W}_1\mathbf{T} + \rho_2\mathbf{W}_2\mathbf{T} + \mathbf{X}\boldsymbol{\beta} + \mathbf{e}. \tag{7}$$

The spatial (or network) lag model (7) is similar to the model we used to estimate the effect of the treatment in the presence of interference, but our approach is different from the typical application of a spatial or network lag model. Our main goal and contribution was to show how to define causal estimands under plausible assumptions on the interference mechanism and how to estimate causal effects of policies in the presence of interference. We used a linear regression model to estimate our causal estimands, but, differently from the typical application of a spatial lag model, the definition of our estimands is not based on the regression model *per se*. Typically, in analyses using spatial or network lag models, the effect of the interference and that of the treatment correspond to parameters in the models which usually lack a well-defined causal interpretation.

Our approach was inspired by and tailored to a specific real case study, but it can be adapted to other applications. An interesting avenue for future research is to extend this approach allowing for more general forms of interference. We think that combining the potential outcomes framework with the spatial and network literatures can offer important insights. Some recent work [Kao et al. (2012), Samii and Aronow (2013)] is going in this direction.

Our approach has a limitation that is also shared with many studies in spatial and network analyses. One key problem that analysts in these fields typically have to face is how to construct and treat the weighting matrix(es) [Harris, Moffat and Kravtsova (2011)]. Empirical results can be influenced by the way the weighting matrix is defined. It is usually emphasized that theory and contextual knowledge should be the driving forces that determines the specification of the matrix of weights, **W** [see, e.g., the discussion in Corrado and Fingleton (2012)]. We also followed this approach to motivate the specification of the f_1 and f_2 interference functions. Future studies may develop, within the potential outcomes framework, methods to estimate interference rather than imposing an a priori specified structure [see, e.g., Aronow (2012), Bhattacharjee and Jensen-Butler (2013)].

Acknowledgments. The authors would like to thank the Editor, Professor Edoardo Airoldi, an anonymous Associate Editor and two anonymous reviewers for their comments which helped to substantially improve the manuscript. The authors also thank the Tuscan Regional Institute for Economic Planning (IRPET) for giving us insights on the industrial structure in Tuscany and for helping reconstruct the geographic coordinates of firms in the study.

REFERENCES

ALMUS, M. and CZARNITZKI, D. (2003). The effects of public R&D subsidies on firms' innovation activities: The case of Eastern Germany. *J. Bus. Econom. Statist.* **21** 226–236. MR1973746

ANSELIN, L. (2006). Spatial econometrics. In *Palgrave Handbook of Econometrics*: *Volume* 1, *Econometric Theory* (T. C. Mills and K. Patterson, eds.) 901–941. Palgrave Macmillan, Basingstoke, UK.

ARONOW, P. M. (2012). A general method for detecting interference between units in randomized experiments. *Sociol. Methods Res.* **41** 3–16. MR3190698

BATTISTIN, E., GAVOSTO, A. and RETTORE, E. (2001). Why do subsidised firms survive longer? An evaluation of a program promoting youth entrepreneurship in Italy. In *Econometric Evaluation of Labour Market Policies* 153–181. Springer, Berlin.

BHATTACHARJEE, A. and JENSEN-BUTLER, C. (2013). Estimation of the spatial weights matrix under structural constraints. *Regional Science and Urban Economics* **43** 617–634.

BIA, M. and MATTEI, A. (2012). Assessing the effect of the amount of financial aids to Piedmont firms using the generalized propensity score. *Stat. Methods Appl.* **21** 485–516. MR2992915

BLACKWELL, M., IACUS, S. M., KING, G. and PORRO, G. (2009). CEM: Coarsened exact matching in stata. *The Stata Journal* **9** 524–546.

BÖRNER, K., SANYAL, S. and VESPIGNANI, A. (2007). Network science. *Annual Review of Information Science and Technology* **41** 537–607.

BRAMOULLÉ, Y., DJEBBARI, H. and FORTIN, B. (2009). Identification of peer effects through social networks. *J. Econometrics* **150** 41–55. MR2525993

BROCK, W. A. and DURLAUF, S. N. (2001). Interactions-based models. In *Handbook of Econometrics* **5**. Elsevier, Amsterdam.

BRONZINI, R. and DE BLASIO, G. (2006). Evaluating the impact of investment incentives: The case of Italy's law 488/1992. *Journal of Urban Economics* **60** 327–349.

CORRADO, L. and FINGLETON, B. (2012). Where is the economics in spatial econometrics? *Journal of Regional Science* **52** 210–239.

CRÉPON, B., DUFLO, E., GURGAND, M., RATHELOT, R. and ZAMORA, P. (2013). Do labor market policies have displacement effects? Evidence from a clustered randomized experiment. *The Quarterly Journal of Economics* **128** 531–580.

DE CASTRIS, M. and PELLEGRINI, G. (2012). Evaluation of spatial effects of capital subsidies in the South of Italy. *Regional Studies* **46** 525–538.

DEHEJIA, R. H. and WAHBA, S. (1999). Causal effects in non-experimental studies: Re-evaluating the evaluation of training programs. *J. Amer. Statist. Assoc.* **94** 10053–1062.

DEI OTTATI, G. (1994). Cooperation and competition in the industrial district as an organization model. *European Planning Studies* **2** 463–483.

DOREIAN, P. (1996). When the data points are not independent. In *Developments in Data Analysis: Proceedings of the International Conference on Statistical Data Analysis and Data Collection, Bled, Slovenia, September* 19-21, 1994 (A. Ferligoj and A. Kramberger, eds.) 27–46. FDV, Fakulteta za družbene vede, Univerza v Ljubljani. Slovenia.

DUCH, N., MONTOLIO, D. and MEDIAVILLA, M. (2009). Evaluating the impact of public subsidies on a firm's performance: A two-stage quasi-experimental approach. *Investigaciones Regionales* **16** 143–165.

EBERHARDT, M., HELMERS, C. and STRAUSS, H. (2013). Do spillovers matter when estimating private returns to R&D? *The Review of Economics and Statistics* **95** 436–448.

GLEDITSCH, K. S., WARD, M. D. and KRISTIAN, S. (2007). An introduction to spatial regression models in the social sciences. Technical report, Duke Univ., Durham, NC.

GREINER, D. J. and RUBIN, D. B. (2011). Causal effects of perceived immutable characteristics. *The Review of Economics and Statistics* **93** 775–785.

HALLECK VEGA, S. and ELHORST, J. P. (2015). The SLX model. *Journal of Regional Science* **55** 339–363.

HARRIS, R., MOFFAT, J. and KRAVTSOVA, V. (2011). In search of "W". *Spatial Economic Analysis* **6** 249–270.

HIRANO, K. and IMBENS, W. G. (2004). The propensity score with continuous treatments. In *Applied Bayesian Modeling and Causal Inference from Incomplete-Data Perspectives* (A. Gelman and X. L. Meng, eds.) **226164** 73–84. Wiley, Hoboken, NJ.

HO, D. E., IMAI, K., KING, G. and STUART, E. A. (2007). Matching as nonparametric preprocessing for reducing model dependence in parametric causal inference. *Political Analysis* **15** 199–236.

HOLLAND, P. W. (1986). Statistics and causal inference. *J. Amer. Statist. Assoc.* **81** 945–970. MR0867618

HONG, G. and RAUDENBUSH, S. W. (2006). Evaluating kindergarten retention policy: A case study of causal inference for multilevel observational data. *J. Amer. Statist. Assoc.* **101** 901–910. MR2324091

HUDGENS, M. G. and HALLORAN, M. E. (2008). Toward causal inference with interference. *J. Amer. Statist. Assoc.* **103** 832–842. MR2435472

IACUS, S. M., KING, G. and PORRO, G. (2012). Causal inference without balance checking: Coarsened exact matching. *Political Analysis* **20** 1–24.

IMAI, K. and VAN DYK, D. A. (2004). Causal inference with general treatment regimes: Generalizing the propensity score. *J. Amer. Statist. Assoc.* **99** 854–866. MR2090918

IMBENS, G. W. (2000). The role of the propensity score in estimating dose-response functions. *Biometrika* **87** 706–710. MR1789821

IMBENS, G. W. (2004). Nonparametric estimation of average treatment effetcs under exogeneity: A review. *The Review of Economics and Statistics* **86** 4–29.

KAO, E., TOULIS, R., AIROLDI, E. and RUBIN, B. D. (2012). Causal estimation of peer influence effects. In *NIPS 2012 Workshop 'Social Network and Social Media Analysis: Methods, Models and Applications´*. Stanford Network Analysis Project, Stanford University, Lake Tahoe, Nevada.

KLETTE, T., MØEN, J. and GRILICHES, Z. (2000). Do subsidies to commercial R&D reduce market failures? Microeconometric evaluation studies. *Research Policy* **29** 471–495.

LECHNER, M. (2001). Identification and estimation of causal effects of multiple treatments under the conditional independence assumption. In *Econometric Evaluation of Labour Market Policies* (M. Lechner and F. Pfeiffer, eds.). *ZEW Economic Studies* **13** 43–58. Physica-Verlag, Heidelberg.

MATTEI, A. and MAURO, V. (2007). Valutazione di politiche per le imprese artigiane. Research report. IRPET—Istituto Regionale Programmazione Economica della Toscana.

MEALLI, F. and RUBIN, D. B. (2015). Clarifying missing at random and related definitions, and implications when coupled with exchangeability. *Biometrika* **102** 995–1000. MR3431570

PELLEGRINI, G. and CARLUCCI, C. (2003). Gli effetti della legge 488/92: Una valutazione dell'impatto occupazionale sulle imprese agevolate. *Rivista Italiana degli Economisti* **8** 267–286.

ROSENBAUM, P. R. (2007). Interference between units in randomized experiments. *J. Amer. Statist. Assoc.* **102** 191–200. MR2345537

ROSENBAUM, P. R. and RUBIN, D. B. (1983). The central role of the propensity score in observational studies for causal effects. *Biometrika* **70** 41–55. MR0742974

ROSENBAUM, P. R. and RUBIN, D. B. (1985). Constructing a control group using multivariate matched sampling methods that incorporate the propensity score. *Amer. Statist.* **3** 33–38.

RUBIN, D. B. (1976). Inference and missing data. *Biometrika* **63** 581–592. MR0455196

RUBIN, D. B. (1980). Discussion of "Randomization analysis of experimental data: the Fisher randomization test" by D. Basu. *J. Amer. Statist. Assoc.* **75** 591–593.

RUBIN, D. B. (1990). Comment on J. Neyman and causal inference in experiments and observational studies: "On the application of probability theory to agricultural experiments. Essay on principles. Section 9" [Ann. Agric. Sci. **1**0 (1923), 1–51]. *Statist. Sci.* **5** 472–480. MR1092987

SAMII, C. and ARONOW, P. (2013). Estimating average causal effects under general interference. Technical report. Available at http://arxiv.org/abs/1305.6156.

SOBEL, M. E. (2006). What do randomized studies of housing mobility demonstrate?: Causal inference in the face of interference. *J. Amer. Statist. Assoc.* **101** 1398–1407. MR2307573

STUART, E. A. (2007). Estimating causal effects using school-level data sets. *Educational Researcher* **36** 187–198.

TAKALO, T., TANAYAMA, T. and TOIVANEN, O. (2013). Estimating the benefits of targeted R&D subsidies. *The Review of Economics and Statistics* **95** 255–272.

TCHETGEN TCHETGEN, E. J. and VANDERWEELE, T. J. (2012). On causal inference in the presence of interference. *Stat. Methods Med. Res.* **21** 55–75. MR2867538

VERBITSKY, N. and RAUDENBUSH, S. W. (2004). Causal inference in spatial settings. In *Preceedings of the Social Statistics Section* 2369–2374. Amer. Statist. Assoc., Alexandria, VA.

WOOLDRIDGE, J. M. and IMBENS, G. W. (2009). Recent developments in the econometrics of program evaluation. *Journal of Economic Literature* **47** 5–86.

DEPARTMENT OF POLITICAL AND SOCIAL SCIENCE
UNIVERSITAT POMPEU FABRA
EDIFICI JAUME I
RAMON TRIAS FARGAS, 25-27
E-08005 BARCELONA
SPAIN
E-MAIL: bruno.arpino@upf.edu

DEPARTMENT OF STATISTICS,
COMPUTER SCIENCE, APPLICATIONS
UNIVERSITY OF FLORENCE
VIALE MORGAGNI, 59
50134 FLORENCE
ITALY
E-MAIL: mattei@disia.unifi.it