Synthetic Control Group Methods in the Presence of Interference: The Direct and Spillover Effects of Light Rail on Neighborhood Retail Activity

Giulio Grossi\textsuperscript{1}, Patrizia Lattarulo\textsuperscript{2}, Marco Mariani\textsuperscript{2}, Alessandra Mattei\textsuperscript{1,3}, and Özge Öner\textsuperscript{4,5}

\textsuperscript{1}Department of Statistics, Computer Science, Applications, University of Florence
\textsuperscript{2}IRPET - Regional Institute for Economic Planning of Tuscany, Florence, Italy
\textsuperscript{3}Florence Center for Data Science, Florence, Italy
\textsuperscript{4}Department of Land Economy, Cambridge University
\textsuperscript{5}Research Institute of Industrial Economics, Stockholm, Sweden

Abstract

In recent years, Synthetic Control Group (SCG) methods have received great attention from scholars and have been subject to extensions and comparisons with alternative approaches for program evaluation. However, the existing methodological literature mainly relies on the assumption of non-interference. We propose to generalize the SCG method to studies where interference between the treated and the untreated units is plausible. We frame our discussion in the potential outcomes approach. Under a partial interference assumption, we formally define relevant direct and spillover effects. We also consider the “unrealized” spillover effect on the treated unit in the hypothetical scenario that another unit in the treated unit’s neighborhood had been assigned to the intervention. Then we investigate the assumptions under which we can identify and estimate the causal effects of interest, and show how they can be estimated using the SCG method. We apply our approach to the analysis of an observational study, where the focus is on assessing direct and spillover causal effects of a new light rail line recently built in Florence (Italy) on the commercial vitality of the street where it was built and of the streets in the treated street’s neighborhood.

Keywords: Synthetic control; Direct effects; Spillover effects; Light rail; Retail location
1 Introduction

Synthetic Control Group (SCG) methods (Abadie and Gardeazabal 2003, Abadie et al. 2010, 2015) are an increasingly popular approach used to draw causal inference under the potential outcome framework (e.g., Rubin 1974, 2005) in panel comparative case studies. In these studies, the outcome of interest is observed for a limited number of treated units, often only a single one, and for a number of control units, with respect to a number of periods both prior and after the assignment of the treatment. See (Abadie 2020) for a review of the empirical and methodological aspects of SCG methods. The SCG method focuses on causal effects for treated units. Under the Stable Unit treatment Value assumption (SUTVA), which rules out the presence of interference and hidden versions of treatments Rubin (1980), and suitable assumptions on the treatment assignment mechanism (e.g., Athey et al. 2018), for each point in time after the assignment of the treatment, a weighted average of the observed potential outcomes of control units is used to reconstruct the potential outcomes under control for treated units. These weighted averages, named synthetic controls, are constructed minimizing some distance between pre-treatment outcomes and covariates for the treated units. In the last two decades, SCG methods have gained widespread popularity, and there has been a growing number of studies applying them – under SUTVA – to the investigation of the economic effects on particular locations of a wide range of events or interventions including natural disasters (e.g., Coffman and Noy 2012, Cavallo et al. 2013, Barone and Mocetti 2014); terrorism or organized crime (Abadie and Gardeazabal 2003, Montalvo 2011, Pinotti 2015, Becker and Klößner 2017); major political events (Sanso-Navarro 2011, Abadie et al. 2015, Grier and Maynard 2016, Campos et al. 2018); economic, fiscal or labor-regulation reforms (Billmeier and Nannicini 2013, Kleven et al. 2013, Bohn et al. 2014, Eren and Ozbeklik 2016); health or social policies (Abadie et al. 2010, Hinrichs 2012, Bassok et al. 2014, Bauhoff 2014, Kreif et al. 2016); hydrocarbon extraction (Mideksa 2013, Munasib and Rickman 2015); local and regional development policies (Ando 2015, Liu 2015, Barone et al. 2016, Gobillon and Magnac 2016, Di Cataldo 2017).

Initially, SCG methods have been used in panel studies where the outcome of interest is observed for a single treated unit (e.g., Abadie and Gardeazabal 2003, Abadie et al.
Recently, they have been generalized to draw causal inference in panel studies where focus is on the average causal effects for multiple treated units (Cavallo et al. 2013, Acemoglu et al. 2016, Gobillon and Magnac 2016, Kreif et al. 2016). Additional important theoretical and conceptual contributions include the comparison of SCG methods with alternative approaches for program evaluation, the definition of synthetic control units and the development of new estimators (Doudchenko and Imbens 2017, Gardeazabal and Vega-Bayo 2017, Xu 2017, Athey et al. 2018).

In this methodological and applied causal inference literature, SCG methods have been implemented using the potential outcome approach under the no-interference assumption, which states that the treatment received by one unit does not affect the outcomes of any other unit Rubin (1980). Nevertheless, there are many studies in which this assumption is not plausible and one cannot rule out that the events or interventions of interest produce their effect not only on the units that are exposed to them (direct effects), but also on other unexposed units (spillover effects). In economics, externalities are a consequence of interference. For instance, consider a local policy focusing on, e.g., redevelopments, regeneration and creation of public transports or other infrastructures in particular locations within a city or a region. In this setting, the direct effect of the intervention on the location where the policy is implemented can be reasonably accompanied by spillover effects on other nearby locations.

The presence of interference entails a violation of the SUTVA, and makes causal inference particularly challenging. A possible approach to address the issue is to redefine the unit of analysis by aggregating first-level units, so that the no-interference assumption is plausible at the aggregate level. This choice is not always appropriate and may prevent telling all the relevant stories (Imbens and Wooldridge 2009). Indeed, both scientists and policy makers may be interested not only in the direct effect of an intervention on the unit(s) where it actually takes place, but also in the effects that the same intervention may have – though in an indirect fashion – on other units, who are not directly exposed to the intervention. Therefore, disentangling direct and spillover effects becomes the key objective of the analysis.

Over the last years, causal inference in the presence of interference has been a fertile area
of research. Important theoretical works have dealt with the formal definition of direct and spillover effects and with the development of design and inferential strategies to conduct causal inference under various types of interference mechanisms, in both randomized and observational studies (e.g., Hong and Raudenbush 2006, Sobel 2006, Rosenbaum 2007, Hudgens and Halloran 2008, Aronow 2012, Tchetgen Tchetgen and VanderWeele 2012, Bowers et al. 2013, Arpino and Mattei 2016, Cerqua and Pellegrini 2017, Athey et al. 2018, Forastiere et al. 2018, Papadogeorgou et al. 2018, Arduini et al. 2019, Huber and Steinmayr 2019). Despite such increasing interest, to the best of our knowledge, only the recent works by Cao and Dowd (2019) and Di Stefano and Mellace (2020) deal with the application of synthetic control methods to comparative case studies where the no-interference assumption is not plausible. In particular, Cao and Dowd (2019) introduce – under the assumption that spillover effects are linear in some unknown parameter – estimators for both direct treatment effects and spillover effects. They also investigate their asymptotic properties when the number of pre-treatment periods goes to infinity. Instead, Di Stefano and Mellace (2020) define a procedure that allows to include units potentially affected by spillovers in the donor pool and to eliminate post-intervention effects from these units.

Motivated by the evaluation of causal effects of a new tramway line recently built in Florence (Italy) on the commercial vitality of the surrounding area, we propose to contribute to the nascent literature on the generalization of the SCG approach to a setting with interference. To that end, our paper makes both methodological and substantive contributions.

From a methodological perspective we formally define direct and spillover effects in comparative studies where the outcome of interest is observed for a single treated unit, and a number of control units, for a number of periods before and after the assignment of the treatment. We introduce two types of spillover effects. The first type represents the effect of the treatment on untreated units belonging to treated unit’s neighborhood. The second type would flow from untreated units towards the treated unit, in the hypothetical scenario where the untreated units were exposed to the treatment rather than the actual treated unit. In a sense, we can view this type of spillover effect as an “unrealized spillover effect.”
These causal estimands are defined under a partial interference assumption (Sobel 2006), which states that interference takes place between units located near to each other, but not between units that are sufficiently faraway from one another. Under partial interference, we introduce the assumptions that allow us to identify the direct and spillover effects of interest and, then, propose to estimate them using SCG methods.

From a substantive perspective we assess the direct effect of a new light rail line built in Florence (Italy) on the retail activity of the street where it was built, its spillover on neighboring streets, and the spillover on the treated street that would have emanated from hypothetical, alternative locations of the light rail within the same neighborhood. We measure the commercial vitality of a street using the number and the median sales of the stores located on that street. This kind of application is original with respect to the previous field literature, which has often examined whether the creation of urban rail infrastructure is accompanied by changes in real estate values or gentrification of the area (e.g., Cervero and Landis 1993, Bowes and Ihlandfeldt 2001, Baum-Snow and Kahn 2000, Kahn 2007, Pagliara and Papa 2011, Grube-Cavers and Patterson 2015, Budiaikvskas and Casolario 2018, Nilsson and Delmelle 2018, Delmelle and Nilsson 2019) and, only more seldom, whether it is accompanied by higher firm density (Mejia-Dorantes et al. 2012, Mejia-Dorantes and Lucas 2014) or by the settlement of new retailers (Schuetz 2015, Credit 2018). Nevertheless, it is worth noting that not all these empirical studies are fully embedded in an explicit causal framework, and that none of them addresses the issue of spillovers within such framework.

The paper is organized as follows. Section 2 describes the application that motivates the methodological development we propose and the available data. Section 3 presents the methodology. In Section 4, we discuss how the methodology is applied to study the case of the Florentine light rail and present the results of the analysis. Section 5 concludes the paper.
2 Motivating application and related data

2.1 A new light rail in Florence, Italy

In addition to being a renowned art capital, Florence is also a city with nearly 400,000 residents and the hub of a very wide commuting area. Away from the artworks and the pedestrian footpaths packed with store windows in the city center, the thoroughfares of peripheral Florence are often congested with cars. From the early 1900s, the city of Florence developed an extensive public tram network on street running tracks. Such network was dismissed in 1958 in favor of public bus transport. In the following decades, the city of Florence suffered from soaring private motor vehicle transport, which led to congested traffic and undermined both the effectiveness and the attractiveness of public transport. In order to face these issues, the city administration launched, after a long controversy, the construction of a brand new light rail network (also referred to as tramway hereinafter). Such network should mostly run on reserved tracks, thus guaranteeing a more reliable public transport service, especially on long-distance journeys. Once completed, the planned network will develop radially from the city center towards all the main surrounding suburbs.

The first line of the network was constructed between 2006 and 2010. It connects the main railway station, in the city center, with the Southwestern urban area. The aim of this first light rail line was to facilitate access to the railway station and, more in general, to the city center, which is subject to very restrictive traffic regulations. The most intensive phase of works, when tracks were laid and stations were built, started in 2007. The first line was completed in 2010. It has a total length of 7.6 kilometers, with stops approximately every 400 meters. After the inauguration of this line, some previous long-distance bus services were suppressed, whereas other ones were re-designed as short-distance services to ease the access to the tramway from adjacent areas. The completion of the planned light rail network requires the construction of four additional lines. The construction of two of these lines started in 2014 and was completed in 2018, while the remaining two lines are still in their design phase. The analysis in this paper looks at the 2004-2013 period and focuses on the first line of the tramway. In particular, we consider the section of the line that goes along Talenti St. (1.2 kilometers, 3 stops: Talenti, Batoni, and Sansovino), one of the
main thoroughfares in the densely inhabited Soutwestern urban neighborhood of Legnaia-Isolotto (Legnaia hereinafter). There are other important thoroughfares and streets in Legnaia, most of which run parallel to Talenti St. but do not host tramway tracks and stations. These are: Pollaiolo St. (about 300 meters far from Talenti St.); Pisana St. (450 meters far); Scandicci St. (650 meters far); and Magnolie St. (650 meters far). For each of these streets we consider a section of maximum length of 1.2 kilometers, which we select to be geographically the closest to Talenti St.. All these streets fall within the half-mile range from the light rail and its transit stations (corresponding to a walking distance of about 10 minutes), which is considered a reasonable area of impact by the field literature (Guerra et al. 2012). It is worth noting that, unlike previous studies, where streets within a given radius from transit infrastructures are aggregated to form a cluster level unit, we consider each street as a distinct statistical unit.

2.2 Conjectures on how light rail could affect the streets’ retail activity

Light rail is generally expected to raise accessibility through the improvement of transit times between different points within a urban area (e.g., see Papa and Bertolini 2015, and the literature review therein). However, citywide accessibility improvements are likely to occur in the presence of an extensive transit network. This is not the case in our study, where there is only one tramway line, which was mainly conceived to make access to the city center easier from one particular section of urban periphery. A single light rail line like the one subject to our study is expected to yield a rather localised accessibility improvement. At the same time, the light rail may be expected to trigger a process of revitalization of peripheral areas and of the retail sector therein. In particular, in peripheral and suburban areas, a new light rail could boost the retail component of a so-called “mixed-use transit-oriented development”, which is a high-density mix of residential and commercial uses within walking distance of light rail stations (e.g., Calthorpe 1993, Cervero 2004, Nilsson and Smirnov 2016). This may occur once the light rail is in operation thanks to high flows of transit users and renewed site image. However, the previous empirical literature suggests that the boost of the local retail sector, if any, can be small or transitory (Mejia-Dorantes
Before the light rail inauguration, construction works may temporarily undermine the area’s attractiveness and livability. Faced with the light rail construction site in front of their shop windows, incumbent store owners often complain about the risk of lost sales owed to poor site image, traffic diversions, very limited street parking, and so forth. For the store owners located on other thoroughfares belonging to the same neighborhood of Talenti St., but with no construction site, the story might go the other way around during the tramway construction, with increased sales opportunities owed to temporarily higher flows guaranteed by traffic diversions, unchanged image and street parking possibilities, increased relative competitiveness, and so forth.

When the new infrastructure goes into operation on Talenti St., the prospects of the commercial environment of adjacent sites are hard to envisage. On the one hand, they could also benefit from having the light rail at walking distance, which may increase the footfall for the retailers, constituting a positive spillover effect. On the other hand, they might return to business as usual, or even be crowded out and lose footfall due to the soaring relative attractiveness of the street where the tramway stations are located, which may then constitute a negative spillover effect (Credit 2018).

The effect of the tramway on the commercial environment of a given shopping site may be heterogeneous depending on the different types of stores. Since stores may belong to a high number of categories, an attractive way to group them into few meaningful classes is to distinguish between purveyors of non-durable goods/frequent-use services (non-durables hereinafter) and purveyors of durable goods/seldom-use services (durables hereinafter). This distinction reflects a difference in the frequency of purchase of the two types of goods and services (Brown 1993, Klaesson and Öner 2014, Larsson and Öner 2014) and may help characterize in greater detail the effects of the light rail on the urban neighborhood’s retail sector.

2.3 Data

The dataset used to examine the impact of the new light rail on the local retail environment includes information on 5 streets in the peripheral urban neighborhood of Legnaia
(Talenti St., Pisana St., Pollaiolo St., Scandicci St., and Magnolie St.) and on 38 further thoroughfares and streets of Florence, clustered in other 10 peripheral neighborhoods that are far from Legnaia. For each of these streets, we consider a section having a maximum length of 1.2-1.5 kilometers. The definition of urban neighborhoods is based on the areas identified by the Real Estate Observatory of the Italian Ministry of Finance. We do not consider any street in the city center, as its commercial environment is completely different from what can be found in the surrounding residential neighborhoods.

Background and outcome variables for each street originate from two main sources: the Statistical Archive of Active Local Units (SAALU, English translation of ASIA, the Italian acronym for “Archivio Statistico delle Unità Locali delle Imprese”) and Tax Records (TR). The SAALU is held by the Italian National Institute of Statistics (ISTAT). This dataset is available from 2004 onwards. It collects some basic, individual information on all the active local units of firms, including the exact location of the activity and the sector of activity (classified according the Statistical Classification of Economic Activities in the European Community, usually referred to as NACE). Annual tax records are held by the Ministry of Finance and collect information about firms’ annual sales and other quantities that are required to calculate the amount of taxes due by firms. Using these data sources, we construct background and outcome variables for each street as follows.

We first select firms that are active in the retail sector in the city of Florence, then we collect information about their annual sales from 2004 to 2013 using their TRs. Since the TRs report only aggregate information on sales by multi-site stores, we are forced to focus on mono-site retail outlets, for which the sales of the site coincide with the sales of the whole firm. Mono-site outlets, however, account for the overwhelming majority of stores in the peripheral neighborhood of Florence.

Second, we select only those stores having their shop windows on the streets involved in the study or that are located within an extremely short distance from such streets (50 meters). Then, in line with the reasoning developed in the previous subsection, we classify each of these stores into a NACE sector of activity in order to elicit the product/service these stores sell, and group them into two categories: purveyors of non-durable goods (or frequent-use services); and purveyors of durable goods (or seldom-use services).
For each street and year, we finally construct background and outcome variables aggregating information across stores belonging to the same category. In our application, we focus on the following four outcome variables: number of purveyors of non-durable/durable goods every 500 meters; median of the local sales distribution of purveyors of non-durable/durable goods.

Figure 1 reports the observed value of these four variables over the time period 2004-2013. The left-hand vertical line marks the start of light rail construction, the right-hand vertical line marks the start of its operation. These descriptive graphs suggest that in Talenti St. both the number of purveyors of non-durable goods (every 500 meters) and their median sales increase after the tramway goes into operation. On the other hand, the median sales of stores selling durables on Talenti St. decrease during construction but increase in the first phase of the operational period, when the number of this type of shops starts to diminish. On Pollaiolo St., the number of purveyors of non-durables grows during construction and wanes afterwards; their median sales first decrease then increase. The number stores selling durables is overall quite stable, but their median sales have ups and downs. After an initial jump, Pisana St. retains stores selling durables but loses some purveyors of non-durables when the light rail is operational. Median sales are rather stable throughout. On Scandicci St., the number of purveyors is overall stable, but median sales of purveyors of both durables and non-durables increase during the light rail construction phase. Median sales of stores selling durables dwindle as the light rail eventually goes into operation. Finally, Magnolie St. sees a continuous decrease in the number of stores selling durables, while the decline in the number of purveyors of non-durables begins as the light rail service starts. Median sales of both types of stores tend to increase during the light rail construction, but only those of stores selling non-durables continue to grow after the tramway is completed.
Figure 1: Observed values 2004-2013 of the main variables of interest in the treated street (Talenti St.) and in other streets belonging to the same urban neighborhood (Pollaiolo St., Pisana St., Scandicci St., and Magnolie St.)
3 Methodology

3.1 Potential outcomes and causal estimands

Consider a panel data setting with a set \( N \) of \( 1 + N \) units observed in time periods \( t = 1, \ldots, T \), \( T_0 + 1 \ldots, T, 1 < T_0 < T \). Suppose that from period \( T_0 + 1 \) onwards, a single unit, say unit 1, is exposed to the intervention of interest, so that we have \( N \) remaining units as potential controls. For each unit \( i, i = 1, 2, \ldots, N + 1 \), let \((i, N_i, N_{-i})\) be a partition of \( N \) around \( i \), where \( N_i \) is the neighborhood of unit \( i \), that is, the set of all neighbors of units \( i \), and \( N_{-i} \) is the set of all units other than \( i \) that do not belong to \( N_i \). Let \(|N_i|\) and \(|N_{-i}|\) denote the number of units belonging to \( N_i \) and \( N_{-i} \), respectively. Throughout the paper we focus on settings where the population can be partitioned into clusters, therefore \( (i, N_i, N_{-i}) = (i', N_{i'}, N_{-i'}) \), if \( i \) and \( i' \) belong to the same cluster, but \( (i, N_i) \cap (i', N_{i'}) = \emptyset \) if \( i \) and \( i' \) belong to two different clusters.

In our motivating study, units are streets of Florence. Our dataset includes information on \( 1 + N = 1 + 42 = 43 \) streets from 2004 to 2013. Only one of these streets, namely Talenti St., which we label as street 1, is exposed to the intervention of interest: the construction of a new light rail line. Since construction works started in 2006 and ended in 2010, we have two pre-treatment, five treatment, and three post-treatment years.

In the study, clusters are naturally defined by urban neighborhoods. The partition of \( N \) around, e.g., Talenti St., \((1, N_1, N_{-1})\), consists of: Talenti St. \((i = 1)\); the set, \( N_1 \), of four streets belonging to the same urban neighborhood where Talenti St. is also located (the Legnaia neighborhood): \( N_1 \equiv \{2, 3, 4, 5\} = \{\text{Pollaiolo, Pisana, Scandicci, Magnolie}\} \); the set, \( N_{-1} \equiv \{6, 7, \ldots, N + 1\} \), of streets belonging to other urban neighborhoods of Florence sufficiently far from Legnaia, which comprises 38 streets clustered in 10 neighborhoods. See Figure 2 for a stylized map.

Let \( W_t \in \{0, 1\}^{1+N} \) be the observed treatment vector at time \( t, t = 1, \ldots, T \). Because we focus on settings where only the first unit (Talenti St.) is exposed to the intervention after timepoint \( T_0 \) \((T_0 = 2005)\), we have that \( W_t = 0_{1+N} \) if \( t \leq T_0 \), and \( W_t = [1, 0_N]' \) if \( t > T_0 \), where \( 0_K \) denotes the zero vector in \( \mathbb{R}^K \).

Under the assumption that there is no hidden versions of treatment (Consistency As-
Figure 2: Streets involved in the analysis, clustered in their own urban neighborhoods
sumption: Rubin 1980, Imbens and Rubin 2015), let $Y_{it}(w_t)$ denote the potential outcome that would be observed for unit $i$ at time $t$ if treatment vector $W_t$ were set to the value $w_t$, $w_t \in \{0,1\}^{1+T}$. We assume that the intervention has no effect on the outcome before the treatment period, $T_0+1, \ldots, T$ (Abadie et al. 2010), so we have that for all $i = 1, \ldots, N+1$ and $w_t \in \{0,1\}^{1+T}$, $Y_{it}(w_t) = Y_{it}(0_{1+N})$ for $t \leq T_0$.

For each $i$, the partition $(i,N_i,N_{-i})$ defines the following partition of each treatment vector $w_t \in \{0,1\}^{1+T}$ at time $t$: $w_t = [w_{it}, w_{N_{it}}, w_{N_{-it}}]'$. Therefore the potential outcome $Y_{it}(w_t)$ can be also written as $Y_{it}(w_{it}, w_{N_{it}}, w_{N_{-it}})$. For instance, for $i = 1$ and $t > T_0$, we have that the observed treatment vector is $W_t = [W_{1t}, W_{N_{1t}}, W_{N_{-1t}}]' = [1,0_{|N_1|},0_{|N_{-1}|}]'$. In our application study, for $i = 1$ and $t > 2005$, we have that $W_t = [1,0_{|N_1|},0_{|N_{-1}|}]' = [1,0_4,0_{38}]'$.

When the population can be partitioned into clusters, it is often plausible to invoke the partial interference assumption (Sobel 2006). Such assumption states that interference may occur within, but not between, groups (e.g. Hong and Raudenbush 2006, Hudgens and Halloran 2008, Papadogeorgou et al. 2018). Formally, we can formulate the partial interference assumption as follows:

**Assumption 1. (Partial Interference).**

For $t > T_0$, for all $[w_{it}, w_{N_{it}}, w_{N_{-it}}]'$ and $[w_{it}^*, w_{N_{it}}^*, w_{N_{-it}}^*]'$ with $w_{it} = w_{it}^*$ and $w_{N_{it}} = w_{N_{it}}^*$,

$$Y_{it}(w_{it}, w_{N_{it}}, w_{N_{-it}}) = Y_{it}(w_{it}^*, w_{N_{it}}^*, w_{N_{-it}}^*)$$

for all $i = 1, \ldots, N+1$.

Partial interference implies that potential outcomes for each unit $i$ only depend on its own treatment status and on the treatment statuses of the units belonging to the same cluster/neighborhood as unit $i$, but they do not depend on the treatment statuses of the units belonging to different clusters/neighborhoods. Therefore, partial interference allows us to write $Y_{it}(w_t) \equiv Y_{it}(w_{it}, w_{N_{it}}, w_{N_{-it}})$ as $Y_{it}(w_{it}, w_{N_{it}})$ for all $i = 1, \ldots, N+1$ and for $t > T_0$.

In our application study, where streets are partitioned into clusters defined by urban neighborhoods, it is rather plausible to assume that interference occurs within streets belonging to the same neighborhood, but not between streets belonging to different, geographically distant, urban neighborhoods. Indeed, we can reasonably expect that customers
patronizing stores in a given peripheral area will hardly switch over to other distant, peripheral areas because of a single light rail line connecting only one of these peripheries with the city center, but with none of the other peripheries.

In a setting where only the first unit (Talenti St.) is exposed to the intervention after timepoint \( T_0 \) (with \( 1 \leq T_0 < T \)), and under the assumption of partial interference, we are interested in the following direct and spillover causal effects at timepoints \( t = T_0 + 1, \ldots, T \).

We define the (individual) direct causal effect of treatment 1 versus treatment 0 for the treated unit/street as

\[
\tau_{it} = Y_{it}(1, 0, |N_1|) - Y_{it}(0, 0, |N_1|) \quad t = T_0 + 1, \ldots, T. \tag{1}
\]

For \( i, i' \in \{1\} \cup N_1 \), let \( e_{N_1}^{(i')} \) be a \( |N_1| \)-dimensional vector with all of its entries equal to 0 except the \( i' \)th entry that is equal to 1. Note that in cluster settings \( \{1\} \cup N_1 = \{i\} \cup N_i \) and thus \( |N_1| = |N_i| \) for all \( i \in N_1 \). For all \( i \in N_1 \), let

\[
\delta_{it}^{N_i} = Y_{it}(0, e_{N_i}^{(1)}) - Y_{it}(0, 0, |N_i|)
\]

be the individual spillover causal effect of treatment 1 versus treatment 0 at time \( t \) on unit \( i \) belonging to the treated unit’s cluster. We define the average spillover causal effect at time \( t \) as

\[
\delta_{i}^{N_i} = \frac{1}{|N_1|} \sum_{i \in N_1} \delta_{it}^{N_i} = \frac{1}{|N_1|} \sum_{i \in N_i} \left[ Y_{it}(0, e_{N_i}^{(1)}) - Y_{it}(0, 0, |N_i|) \right]. \tag{2}
\]

Finally, we define the (individual) spillover causal effect at time \( t \) of unit \( i, i \in N_1 \), on the treated unit as

\[
\gamma_{it}^{(i)} = Y_{it}(0, e_{|N_1|}^{(i)}) - Y_{it}(0, 0, |N_1|) \quad i \in N_1. \tag{3}
\]

The quantity \( \gamma_{it}^{(i)} \), measures what the spillover effect on unit 1 could have been in the hypothetical scenario where another unit, say unit \( i \), belonging to the same cluster as the treated unit 1 was exposed to the intervention rather than unit 1. In our application study, \( \gamma_{it}^{(i)} \) is the effect of the light rail on Talenti St. if the light rail was not located on Talenti St. but on another street belonging to Talenti St.’s urban neighborhood. We can interpret \( \gamma_{it}^{(i)} \) as the spillover that unit 1, namely Talenti St., has not realized precisely because of its exposure to treatment. It recalls the concept of opportunity cost used in public economics.
for the comparative study of alternative investment plans. The difference between the
direct effect and the unrealized spillover,

\[ \tau_{1t} - \gamma^{(i)}_{1t} = Y_{1t}(1, 0_{|N_1|}) - Y_{1t}(0, e_{i|N_1|}^{(i)}), \quad i \in N_1 \] (4)

may provide useful insights on whether, among a set of alternatives, the original treatment
allocation choice has brought about a gain or a loss for the treated unit. If \( \tau_{1t} > \gamma^{(i)}_{1t} \),
the actual treatment allocation brought about a gain for unit 1 with respect to unit \( i \); if
\( \tau_{1t} < \gamma^{(i)}_{1t} \), then some alternative allocation of the intervention within the cluster would
have been preferable for the treated unit; if \( \tau_{1t} = \gamma^{(i)}_{1t} \), an alternative allocation of the
intervention, where unit \( i \) rather than unit 1 were exposed to the treatment, would have
been equivalent to the actual one for the treated unit.

Throughout the paper, we refer to \( Y_{1t}(0, 0_{|N_1|}) \) and \( Y_{it}(0, 0_{|N_i|}) \), \( i \in N_1 \), for \( t = T_0 + 1, \ldots, T \) as control potential outcomes for the treated unit and for units who belong to the
treated unit’s cluster, respectively, and to units who do not belong to the treated unit’s
cluster as control units.

It is worth noting that we are not interested in assessing causal effects for units/streets
belonging to clusters/urban neighborhoods different from the treated unit’s cluster (Leg-
naia), but the availability of information on them is essential for inference, as we will show
in the next Sections.

### 3.2 Observed potential outcomes

For each \( t = 1, \ldots, T \) and \( i = 1, \ldots, 1+N \), let \( Y_{it} \) be the observed outcome for unit \( i \)
at time \( t \). Under consistency and partial interference we have that \( Y_{it} = Y_{it}(W_{it}, W_{N_1}) \).
Specifically, in the setting we focus on we have that for \( t = 1, \ldots, T_0, Y_{it} = Y_{it}(0, 0_{|N_i|}) \) for all
\( i = 1, \ldots, 1+N \); and for \( t = T_0+1, \ldots, T \), \( Y_{it} = Y_{it}(1, 0_{|N_i|}) \), \( Y_{it} = Y_{it}(0, e_{i|N_i|}^{(i)}) = Y_{it}(0, e_{i|N_1|}^{(i)}) \)
for all \( i \in N_1 \), and \( Y_{it} = Y_{it}(0, 0_{|N_i|}) \) for all \( i \notin \{1\} \cup N_1 \).

Then, we can re-write the (individual) direct causal effect for the treated unit in Equa-
tion (1) and the average spillover causal effect in Equation (2) at time \( t, t = T_0 + 1, \ldots, T \),
as function of the observed outcomes:

\[ \tau_{1t} = Y_{1t} - Y_{1t}(0, 0_{|N_1|}) \quad \text{and} \quad \delta_i^{N_1} = \frac{1}{|N_1|} \sum_{i \in N_1} [Y_{it} - Y_{it}(0, 0_{|N_1|})]. \]
These relationships make it clear that we need to estimate $Y_{1t}(0,0|N_1)$ and $Y_{it}(0,0|N_i)$ for $i \in N_1$ to get an estimate of $\tau_{1t}$ and $\delta_{N_1}^t$. The spillover not realized by the treated unit in Equation (3), $\gamma_{1t}^{(i)}$, depends on two unobserved potential outcomes, $Y_{1t}(0,e_{1|N_1}^{(i)})$, $i \in N_1$, and $Y_{1t}(0,0|N_1)$, and thus we need to estimate both of them to get an estimate of $\gamma_{1t}^{(i)}$.

In addition to the pre-treatment outcomes, $Y_{it}$, for $t = 1, \ldots, T_0$ and $i = 1, \ldots, 1 + N$, we observe a vector of time- and unit-specific covariates, $C_{it} = [C_{it}^{(1)}, \ldots, C_{it}^{(K)}]$, $i = 1, \ldots, 1 + N$, $t = 1, \ldots, T_0$. Using information on unit-level pre-treatment outcomes and covariates, for each unit $i$, we construct neighborhood-level pre-treatment outcomes, $Y_{N_i t}$, and neighborhood-level unit × time specific covariates, $C_{N_i t} = [C_{N_i t}^{(1)}, \ldots, C_{N_i t}^{(K)}]$, as average of the unit-level pre-treatment outcomes and covariates for units belonging to unit $i$’s cluster/neighborhood:

$$Y_{N_i t} = \frac{1}{|N_i|} \sum_{i' \in N_i} Y_{i' t}, \quad t = 1, \ldots, T_0$$

and

$$C_{N_i t}^{(k)} = \frac{1}{|N_i|} \sum_{i' \in N_i} C_{i' t}^{(k)} \quad k = 1, \ldots, K; \quad t = 1, \ldots, T_0$$

### 3.3 SCG estimators of direct and average spillover effects

We meet the challenge to impute the missing potential outcomes by extending the SCG approach (Abadie and Gardeazabal 2003, Abadie et al. 2010) to studies where units are grouped into clusters, so the presence of interference cannot be ruled out. Under partial interference (Assumption 1), the key insight characterizing our framework consists of using information on units within clusters different from the treated unit’s cluster to impute the missing potential outcomes.

Let us start by focusing on the (individual) direct causal effects for the treated unit in Equation (1), $\tau_{1t}$, and on the average spillover causal effects in Equation (1) $\delta_{N_1}^t$, $t = T_0 + 1, \ldots, T$. Ignoring for the moment the presence of covariates, the question is how information on pre-treatment outcomes for the treated unit and its neighbors, $\{Y_{it}, Y_{N_i t}\}_{t=1,\ldots,T_0}$ for $i \in \{1\} \cup N_1$, and information on pre- and post-treatment outcomes for units outside the treated unit’s cluster, $\{Y_{it}, Y_{N_i t}\}_{t=1,\ldots,T_0}$ and $\{Y_{it}, Y_{N_i t}\}_{t=T_0+1,\ldots,T}$ for $i \notin \{1\} \cup N_1$, can be used to impute post-treatment outcomes under control for the treated unit and its neighbors, $\{Y_{it}(0,0|N_i)\}_{t=T_0+1,\ldots,T}$ for $i \in \{1\} \cup N_1$. 

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For studies where the no-interference assumption holds, Doudchenko and Imbens (2017) and Athey et al. (2018) explicitly introduce the key assumption on the treatment assignment mechanism underlying SCG methods: for each post-treatment period \( t = T_0 + 1, \ldots, T \), the control outcome at time \( t \) for the treated units are independent of such units’ treatment assignment at time \( t \), conditional on the outcomes at time \( t \) for control units. Under this assumption, various SCG approaches have been proposed to impute the missing potential outcomes under control for the treated units (e.g., Abadie and Gardeazabal 2003, Abadie et al. 2010, Doudchenko and Imbens 2017, Athey et al. 2018). Most of these approaches exploit the idea of a stable relationship over time between the outcome of the treated units and the outcome of the control units in the absence of intervention (stable patterns across units, e.g., Abadie and Gardeazabal 2003, Abadie et al. 2010, Doudchenko and Imbens 2017). Recently, Athey et al. (2018) propose matrix completions methods for estimating causal effects in settings with panel data that exploit both cross-sectional and within-unit patterns in the data.

We generalize both the assumptions and the SCG approach initially developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010), to studies where units are organized into clusters and a single unit is exposed to the treatment. Our method exploits stable patterns across units belonging to different clusters. Specifically, for each \( i \in \{1\} \cup \mathcal{N}_1 \), we assume that the relationship between the outcome of unit \( i \), \( Y_{it} \), and the unit-level and neighborhood-level outcomes of control units, \( Y_{i't} \) and \( Y_{N_i't} \), \( i' \notin \{1\} \cup \mathcal{N}_1 \), is stable over time. This type of stable patterns implies that (i) the same structural process drives both the outcomes of control units’ clusters (clusters of units who do not belong to the treated unit’s cluster) as well as the outcomes of the treated unit and its neighbors in absence of treatment; and (ii) the outcomes of control units and their neighbors are not subject to structural shocks during the sample period of the study.

The key identifying assumption underlying our method is:

**Assumption 2.** (Vertical Unconfoundedness for clustered data). For each unit \( i \in \{1\} \cup \mathcal{N}_1 \),

\[
Y_{it}(0,0_{|\mathcal{N}_i|}) \perp (W_{it}, W_{N_{it}}) \mid \{Y_{i't}, Y_{N_{i't}}\}_{i' \notin \{1\} \cup \mathcal{N}_1}
\]
Assumption 2 implies that for each \( t \), within the cells defined by the individual- and neighborhood-level outcomes at time \( t \) of control units outside the treated unit’s cluster, the potential outcome under control for the treated unit and for the units inside the treated unit’s cluster are independent of the vector of their treatment assignments.

Under these assumptions, building on Abadie and Gardeazabal (2003) and Abadie et al. (2010), we propose to impute the missing control potential outcomes for the treated unit and the units who belong to the treated unit’s cluster as weighted average of outcomes of control units. Formally,

\[
\hat{Y}_{it}(0, 0_{|N_i|}) = \sum_{i' \notin \{1\} \cup N_1} \lambda_{i'}^{(i)} Y_{i't} \quad \text{for} \quad i \in \{1\} \cup N_1, \quad t = T_0 + 1, \ldots, T,
\]

where \( \lambda_{i'}^{(i)} \) are weights such that, for all \( i \in \{1\} \cup N_1 \),

\[
\lambda_{i'}^{(i)} \geq 0 \quad \text{for all} \quad i' \notin \{1\} \cup N_1 \quad \text{and} \quad \sum_{i' \notin \{1\} \cup N_1} \lambda_{i'}^{(i)} = 1.
\]

For each \( i \in \{1\} \cup N_1 \), the set of weights \( \lambda^{(i)} = \{\lambda_{i'}^{(i)}\}_{i' \notin \{1\} \cup N_1} \) defines the synthetic control unit of unit \( i \).

Ideally we would like to find weights such that, in pre-treatment periods, unit- and neighborhood-level outcomes for each unit \( i \in \{1\} \cup N_1 \), and the weighted average of unit- and neighborhood-level outcomes for control units \( i' \notin \{1\} \cup N_1 \), are equal. Formally, for each \( i \in \{1\} \cup N_1 \), we would like to find weights, \( \lambda^{(i)} = \{\lambda_{i'}^{(i)}\}_{i' \notin \{1\} \cup N_1} \), such that for \( t = 1, \ldots, T_0 \),

\[
Y_{it} = \sum_{i' \notin \{1\} \cup N_1} \lambda_{i'}^{(i)} Y_{i't} \quad \text{and} \quad Y_{N_{it}} = \sum_{i' \notin \{1\} \cup N_1} \lambda_{i'}^{(i)} Y_{N_{i't}} \quad (5)
\]

In practice, sets of weights guaranteeing that Equation (5) holds exactly for each unit \( i \in \{1\} \cup N_1 \) might not exist in the data. Then, for each unit \( i \in \{1\} \cup N_1 \), the synthetic control unit is constructed so that Equation (5) holds approximately.

Let \( Y_{i,\text{pre}}^{(1)} = [Y_{i1}, \ldots, Y_{iT_0}, Y_{N_{i1}}, \ldots, Y_{N_{iT_0}}]' \) be a \( 2T_0 \)-dimensional vector of pre-treatment individual- and neighborhood-level outcomes for a unit \( i, i \in \{1\} \cup N_1 \); and let \( Y_{\text{pre}}^{(0)} = [Y_{i',\text{pre}}^{(0)}]_{i' \notin \{1\} \cup N_1}' \) be a \( 2T_0 \times (|N_1| + 1) \) matrix with \( i'th \) column equal to \( Y_{i',\text{pre}}^{(0)} = [Y_{i1}, \ldots, Y_{iT_0}, Y_{N_{i1}}, \ldots, Y_{N_{iT_0}}]' \), a \( 2T_0 \)-dimensional vector of pre-treatment individual-
and neighborhood-level outcomes for a control unit $i'$ outside the treated unit’s cluster, $i' \notin \{1\} \cup \mathcal{N}_1$. Similarly, let $\mathbf{Y}^{(1)}_{i,post} = [Y_{1T_0+1}, \ldots, Y_{iT}]^T$ be a $(T-T_0)$-dimensional vector of post-treatment outcomes for a unit $i$, $i \in \{1\} \cup \mathcal{N}_1$; and let $\mathbf{Y}^{(0)}_{i,post} = \left[ \mathbf{Y}^{(0)}_{i',post} \right]_{i' \notin \{1\} \cup \mathcal{N}_1}$ be a $(T-T_0) \times \left| (N+1)-(\mathcal{N}_1+1) \right|$ matrix with $i$th column equal to $\mathbf{Y}^{(0)}_{i',post} = [Y_{1T_0+1}, \ldots, Y_{iT}]^T$, a $(T-T_0)$-dimensional vector of post-treatment outcomes for a control unit $i'$ outside the treated unit’s cluster, $i \notin \{1\} \cup \mathcal{N}_1$.

For each $i \in \{1\} \cup \mathcal{N}_1$, the vector of weights $\lambda^{(i)} = \{\lambda^{(i)}_{i'}\}_{i' \notin \{1\} \cup \mathcal{N}_1}$ is chosen to minimize some distance between $\mathbf{Y}^{(1)}_{i,pre}$ and $\mathbf{Y}^{(0)}_{i,pre} \lambda^{(i)}$, subject to $\lambda^{(i)}_{i'} \geq 0$ for all $i' \notin \{1\} \cup \mathcal{N}_1$ and $\sum_{i' \notin \{1\} \cup \mathcal{N}_1} \lambda^{(i)}_{i'} = 1$. Specifically, let $\Lambda$ be the set of all vector $\lambda = \{\lambda_{i'}\}_{i' \notin \{1\} \cup \mathcal{N}_1}$ such that $\lambda_{i'} \geq 0$ for all $i' \notin \{1\} \cup \mathcal{N}_1$ and $\sum_{i' \notin \{1\} \cup \mathcal{N}_1} \lambda_{i'} = 1$. Following Abadie et al. (2010), for each $i \in \{1\} \cup \mathcal{N}_1$, we select the set of weights $\hat{\lambda}^{(i)}$ such that

$$\hat{\lambda}^{(i)}(\mathbf{V}_i) = \arg \min_{\lambda^{(i)} \in \Lambda} ||\mathbf{Y}^{(1)}_{i,pre} - \mathbf{Y}^{(0)}_{i,pre} \lambda^{(i)}||_{\mathbf{V}_i}$$

where $\mathbf{V}_i$ is a $(2T_0 \times 2T_0)$ symmetric and positive semi-definite matrix that assigns weights to linear combinations of the variables in $\mathbf{Y}^{(0)}_{i,pre}$ and $\mathbf{Y}^{(1)}_{i,pre}$ to minimize the mean squared error of the synthetic control estimator, that is, the expectation of $\hat{\mathbf{Y}}^{(i)}_{i,post} - \mathbf{Y}^{(0)}_{i,post} \hat{\lambda}^{(i)}$. Specifically, in our application study, the matrix $\mathbf{V}_i$ is chosen among all positive definite and diagonal matrices such that the mean squared prediction error (MSPE) of the outcome variable is minimized over the set of pre-treatment periods. Formally, let $\mathcal{V}$ be the set of all positive definite and diagonal matrices. Then $\hat{\mathbf{V}}_i$ is chosen such that

$$\hat{\mathbf{V}}_i = \arg \min_{\mathbf{V}_i \in \mathcal{V}} \left[ \mathbf{Y}^{(1)}_{i,pre} - \mathbf{Y}^{(0)}_{i,pre} \hat{\lambda}^{(i)}(\mathbf{V}_i) \right]' \left[ \mathbf{Y}^{(1)}_{i,pre} - \mathbf{Y}^{(0)}_{i,pre} \hat{\lambda}^{(i)}(\mathbf{V}_i) \right]$$

Therefore, $\hat{\lambda}^{(i)}(\mathbf{V}_i)$ and $\hat{\mathbf{V}}_i$ are obtained solving a nested optimization problem that solves Equation (7) for $\hat{\lambda}^{(i)}(\mathbf{V}_i)$ given by Equation (6).

It is worth noting that, in the original SCG approach, background covariates above and beyond pre-treatment outcomes enter the choice of the weights: the vector of weights is chosen to minimize the distance between the treated unit and the weighted combination of the other units in terms of the covariates, where covariates may include both background characteristics as well as some or all of the pre-treatment outcomes (see Abadie...
and Gardeazabal 2003, Abadie et al. 2010). We generalize this approach ignoring information on background characteristics but focusing on minimizing the distance, in terms of the pre-treatment outcomes only, between a unit $i$ belonging to the treated unit’s cluster and the weighted combination of control units. The main reason underlying our choice is that, in practice, pre-treatment covariates tend to play a relatively minor role relative to pre-treatment outcomes: in terms of predictive power, the lagged outcomes tend to be substantially more important (Doudchenko and Imbens 2017). The issue of how, and which, pre-treatment characteristics above and beyond the lagged outcomes should be involved when SCG methods are exploited is still open. A shared perspective does not exist in the literature yet (e.g., Xu 2017, Doudchenko and Imbens 2017, Becker et al., 2018, Ben-Michael et al. 2018), and we do not wish to enter into such discussion. Nevertheless, we believe that accounting for information on pre-treatment background characteristics might be worthwhile. Here we propose to adjust for pre-treatment variables, by pre-processing the data using the following matching procedure. Prior to choosing the weights, for each unit $i \in \{1\} \cup N_1$ we restrict unit $i$’s donor pool, that is, the set of control units $i' \notin \{1\} \cup N_1$ to use to impute unit $i$’s missing potential outcomes, $Y_{it}(0,0|N_i)$, $t = T_0 + 1 \ldots, T$, to control units $i' \notin \{1\} \cup N_1$ whose time×unit-level and neighborhood-level covariates and pre-treatment outcomes are similar to those of unit $i$. In our application study, for each unit $i \in \{1\} \cup N_1$ we select a matched donor pool among units $i' \notin \{1\} \cup N_1$ using a matching procedure based on the Mahalanobis distance (see Section 4 for details).

Given an estimate of the weights $\hat{\lambda}_{i}^{(1)}$ for each $i \in \{1\} \cup N_1$, we estimate the direct effects for the treated unit, $\tau_{it}$, and the individual indirect/spillover causal effects on unit $i$, $\delta_{it}^{N_1}$, $i \in N_1$, $t = T_0 + 1, \ldots, T$, as follows

$$\hat{\tau}_{it} = Y_{it} - \sum_{i' \notin \{1\} \cup N_1, i' \in M_i} \hat{\lambda}_{i}^{(1)} Y_{i't} \quad \text{and} \quad \hat{\delta}_{it}^{N_1} = Y_{it} - \sum_{i' \notin \{1\} \cup N_1, i' \in M_i} \hat{\lambda}_{i}^{(i)} Y_{i't},$$

where $M_i$, $i \in \{1\} \cup N_1$, is the set of matched control units for unit $i$. Following Cavallo et al. (2013), who focus on SCG methods in presence of multiple treated units, we estimate the average spillover causal effects $\hat{\delta}_{it}^{N_1}$, $t = T_0 + 1, \ldots, T$, as

$$\hat{\delta}_{it}^{N_1} = \frac{1}{|N_1|} \sum_{i \in N_1} \hat{\delta}_{it}^{N_1}.$$
3.4 Inference on SCG estimators of direct and average spillover effects

In this section we first briefly review the literature on inference with SCG methods under the no-interference assumption. Then, we discuss the approach we propose for doing inference when such an assumption is deemed implausible, but the partial interference assumption previously introduced (Assumption 1) holds.

Previous literature has proposed various approaches to conduct inference on causal effects estimated using SCG methods under the no-interference assumption. Abadie et al. (2010) and Abadie et al. (2015) propose to use falsification tests, also named “placebo studies,” developing a type of randomization inference (e.g., Imbens and Wooldridge 2009, Imbens and Rubin 2015). A particular synthetic control estimate for the treated unit is compared against “placebo estimates”, that is, synthetic control estimates for cases where the intervention did not take place. Results are interpreted arguing that if the size of the placebo estimates are similar to, or even greater than, the size of the synthetic control estimate, then we infer that there is not enough evidence that the synthetic control estimate reflects the impact of the intervention. Abadie et al. (2015) discuss two specific types of placebo studies and derive p-values. In the first case, the treated unit is viewed as exchangeable with the control units in the absence of the treatment, and placebo estimates are obtained by reassigning the intervention to control units of the donor pool (in-space placebos). In the second case, the period in which the treated unit first receives the treatment is stochastic, and placebo estimates are obtained by reassigning the intervention to dates when the intervention did not actually occur. Doudchenko and Imbens (2017) also consider these two inferential methods (above and beyond a combination of them), but they focus on deriving standard errors of the synthetic control estimators rather than p-values. Ando and Sävje (2013) and Firpo and Possebom (2018) focus on hypothesis testing with the SCG methods. Firpo and Possebom (2018) also develop a method to compute confidence sets by inverting a test statistic. Hahn and Shi (2017) and Ferman and Pinto (2016) discuss inference in SCG settings with a large number of pre-treatment periods. Cavallo et al. (2013), Acemoğlu et al. (2016) and Gobillon and Magnac (2016) generalize SCG methods and corresponding inferential tools to studies where there are
more than one treated unit. For inference, Cavallo et al. (2013) and Acemoğlu et al. (2016) put forward tests that are similar to the ones proposed by Abadie et al. (2010, 2015), and Gobillon and Magnac (2016) propose a way to compute bootstrap confidence intervals. Cao and Dowd (2019) generalize a test procedure based on the end-of-sample instability test (P-test, Andrews 2003) to draw inference on causal effects estimated using the synthetic control methods in comparative case studies with and without interference.

In this paper we propose to generalize the in-space placebo approach to conduct inference for direct and average spillover effects estimated using the SCG method described in Section 3.3, which focuses on studies where there is only one treated unit, units are organized in clusters and the partial interference assumption (Assumption 1) holds.

Our approach is based on viewing the treated unit's cluster, which comprises the treated unit and its neighbors, as exchangeable with the control units' clusters in the absence of the treatment. An in-space placebo study is conducted by artificially reassigning the intervention to one control unit. Specifically, we apply the SCG method described in Section 3.3 to estimate direct and average spillover placebo effects for every potential control unit who does not belong to the treated unit’s cluster. Thus, we obtain a distribution of direct and average spillover placebo effects against which we can compare the direct and spillover effects estimated for the unit actually treated and for its neighbors, respectively. In particular, we evaluate the synthetic control estimates of the direct effects as being statistically not significant if their magnitudes fall inside the distribution of the placebo direct effects. We can also derive $p-$values by calculating for each time point $t > T_0$ the fraction of placebo direct effects greater than or equal to the direct effect estimated for the treated unit. Similarly for the synthetic control estimates of the average spillover effects. Even if the treatment is not randomly assigned, we can still interpret the $p-$values as the probability of obtaining estimates of the direct effects (the average spillover effects) as large as or larger than the ones obtained for the treated unit (treated unit’s neighbors) when the intervention is reassigned at random to control units who do not belong to the treated unit’s cluster.

It is worth stressing that we discard control units that are not included in any donor pool, but we use only “matched” control units as potential controls, that is, units that do
not belong to the treated unit’s cluster, but have been included in the donor pool of either
the treated unit or of at least one untreated unit belonging to the treated unit’s cluster.
Therefore, control units’ clusters are indeed control units’ sub-clusters that only comprise
“matched” control units.

In principle, we could also generalize in-time placebo studies to our setting. Here we
focus on in-space placebo studies, because in our application study in-time placebo studies
are not feasible. In-time placebo tests rely on the availability of data for a large number
of pre-treatment time periods when no structural shocks to the outcome variable occurred
(Abadie et al. 2015). Unfortunately, our application study includes data on only two pre-
treatment time periods.

Building on the work of Doudchenko and Imbens (2017), we also estimate standard
errors of the synthetic control estimators of the direct, average spillover and unrealized
spillover effects (See Figure 1 in Web Supplementary Material).

3.5 Assessing the unrealized spillover through the SCG approach

Inference on the indirect causal effects, \( \gamma_{1t}^{(i)} = Y_{1t}(0, e_{iN_1}^{(i)}) - Y_{1t}(0, 0_{\mathcal{N}_1}), i \in \mathcal{N}_1 \), is par-
ticularly challenging because both potential outcomes, \( Y_{1t}(0, e_{iN_1}^{(i)}) \) and \( Y_{1t}(0, 0_{\mathcal{N}_1}) \), are unobserved. Under Assumption 2 and exploiting stable patterns across units’ clusters, we
can use information on control units outside the treated unit’s cluster and their neigh-
bors to construct an estimator for \( Y_{1t}(0, 0_{\mathcal{N}_1}) \) as described in Section 3.3. Unfortunately,
data contain no or little information on the potential outcomes of the form \( Y_{1t}(0, e_{iN_1}^{(i)}) \).
Therefore, in order to construct an estimator for \( \gamma_{1t}^{(i)} \), we need to introduce some addi-
tional assumptions, which allow us to extrapolate information on the potential outcomes
\( Y_{1t}(0, e_{iN_1}^{(i)}) \) from the observed data.

We first assume that the spillover causal effects on the treated unit are constant across
treated unit’s neighbors: \( \gamma_{1t} = \gamma_{1t}^{(i)} \) for all \( i \in \mathcal{N}_1 \). This assumption states that the unreal-
ized spillover on the treated unit would be the same irrespective of which unit, within the
treated unit’s neighborhood, were exposed to the treatment. In our application study, it
amounts to assume that, no matter which of the other four streets in the urban neigh-
borhood of Legnaia acts as hypothetically alternative light rail location, the spillover causal
effects on the commercial vitality of Talenti St. is the same. The assumption of constant spillover causal effects on the treated unit appears to be reasonable in our application study, where retailers located on these streets likely rely on the same catchment area and the streets are geographically close to each other. The assumption of constant spillover causal effects on the treated unit implies that \( Y_{1t}(0, e_{|N_1|}^{(s)}) \equiv Y_{1t}(0, e_{|N_1|}^{(i)}) \) for all \( i \in N_1 \), and thus the number of missing potential outcomes of the form \( Y_{1t}(0, e_{|N_1|}^{(i)}) \) we need to impute reduces from \( |N_1| \) to one.

Our key assumption is that there exists a vector of weights, \( \xi^* = \{\xi^*_i\}_{i \in N_1} \), with \( \xi^*_i \geq 0 \) for each \( i \in N_1 \) and \( \sum_{i \in N_1} \xi^*_i = 1 \), such that

\[
Y_{1t} = \sum_{i \in N_1} \xi^*_i Y_{it} \quad \text{for } t = 1, \ldots, T_0,
\]

and, for each \( t = T_0 + 1, \ldots, T \), the missing potential outcome, \( Y_{1t}(0, e_{|N_1|}^{(s)}) \), has the same distribution as the weighted average of the (observed) potential outcomes for units belonging to the treated unit’s cluster with weights \( \xi^* = \{\xi^*_i\}_{i \in N_1} \), \( \sum_{i \in N_1} \xi^*_i Y_{it}(0, e_{|N_1|}^{(i)}) \).

The vector of weights \( \xi^* = \{\xi^*_i\}_{i \in N_1} \) defines the synthetic \((0, e_{|N_1|}^{(s)})\)-unit of the treated unit 1.

We construct the synthetic \((0, e_{|N_1|}^{(s)})\)-unit by applying the procedure proposed by Abadie et al. (2010) to the sub-sample of units comprising the treated unit and the treated unit’s neighbors. Specifically, let \( Y_{1,\text{pre}} = [Y_{11}, \ldots, Y_{1,T_0}]' \) be a \( T_0 \)-dimensional vector of pre-treatment individual-level outcomes for the treated unit 1, and let \( Y_{N_1,\text{pre}} = [Y_{i,\text{pre}}]_{i \in N_1} \) be a \( T_0 \times |N_1| \) matrix with \( i \text{th} \) column equal to \( Y_{i,\text{pre}} = [Y_{i,1}, \ldots, Y_{i,T_0}]' \), \( a T_0 \)-dimensional vector of pre-treatment individual-level outcomes for a unit \( i \) who do belong to the treated unit’s cluster, \( i \in N_1 \). We select the weights \( \hat{\xi} \) by solving the following nested optimization problem:

\[
\hat{\xi}(Q) = \arg \min_{\xi \in \Xi} \| Y_{1,\text{pre}} - Y_{N_1,\text{pre}} \xi \| Q = \arg \min_{\xi \in \Xi} \sqrt{\| Y_{1,\text{pre}} - Y_{N_1,\text{pre}} \xi \| Q Y_{1,\text{pre}} - Y_{N_1,\text{pre}} \xi }\]

where \( Q \) is a \( (T_0 \times T_0) \), which must be a symmetric and positive semi-definite matrix, is chosen in the set of all positive definite and diagonal \( (T_0 \times T_0) \) matrices, \( Q \), such that

\[
\hat{Q} = \arg \min_{Q \in \mathcal{Q} \Xi} \left[ Y_{1,\text{pre}} - Y_{\text{pre}} \hat{\xi}(Q) \right]' \left[ Y_{1,\text{pre}} - Y_{N_1,\text{pre}} \hat{\xi}(Q) \right].
\]

Note that this procedure generally defines a set of weights, \( \hat{\xi} \), so that Equation (8) holds approximately.
Given an estimate of the weights $\hat{\xi}$, we estimate the indirect effect on the treated unit 1 as

$$\hat{\gamma}_{1t} = \hat{Y}_{1t}(0, e_{i[N_1]}^{(e)}) - \hat{Y}_{1t}(0, 0_{i[N_1]}) = \sum_{i \in N_1} \hat{\xi}_i Y_{it} - \sum_{i' \notin \{1\} \cup N_1, i' \in M_1} \hat{\lambda}_{(1)i'}(1) Y_{i't}, \ t = T_0, \ldots, T.$$

We use an in-space placebo approach to conduct inference on $\gamma_{1t}, t = T_0, \ldots, T$. We artificially reassign the intervention to one control unit, $i'$, who does not belong to the treated unit's cluster, $i' \notin \{1\} \cup N_1$, and we estimate placebo indirect effects for that unit, $\hat{\gamma}_{i't}, t = T_0, \ldots, T$, using the SCG approach described above. Thus, we obtain a distribution of the indirect effects for the treated unit that we use to evaluate the statistical significance of the indirect effect estimated for the unit actually treated.

4 Causal effects of a new light rail line on streets’
commercial vitality

In this section, we apply the SCG methods described in Section 3 to estimate the direct, the average spillover and the unrealized spillover causal effects of a new light rail line on the retail sector vitality in a number of streets belonging to the same urban neighborhood in peripheral Florence (Italy). Talenti St., where the light rail is located, is subject to direct effects and unrealized spillovers. The nearby streets – namely Pollaiolo St., Pisana St., Scandicci St., and Magnolie St. – may only be subject to spillovers originating from Talenti St.

Streets’ commercial vitality is measured using various street-level outcome variables: number and median sales of stores selling durables; and number and median sales of stores selling non-durables. It is worth noting that we draw inference on the causal effects of interest, conducting placebo tests and calculating standard errors, on each outcome variable, separately. Therefore we ignore the inferential problems typically arising with multiple outcome variables. Accounting for the problem of multiplicities with SCG methods in the presence of interference is beyond the scope of this paper, although it is a valuable topic for future research (see Firpo and Possebom 2018, for inference based on hypothesis tests and confidence sets about causal effects estimated using SCG methods under no-interference in...
the presence of multiple outcome variables).

4.1 Imputation of unobserved potential outcomes

To estimate the direct, the average spillover and the unrealized spillover causal effects of interest using the methodological framework described in the previous Section, we need to impute the potential outcomes $Y_{it}(0, 0_{\mathcal{N}_1})$ for each $i \in \{1\} \cup \mathcal{N}_1$ and $t > T_0$; and the potential outcome $Y_{1t}(0, e^{(*)}_{\mathcal{N}_1})$ for Talenti St., for each $t > T_0$.

For each street $i$ within the urban neighborhood of Legnaia, $i \in \{1\} \cup \mathcal{N}_1$, we construct a synthetic street as weighted average of other streets belonging to Florentine urban neighborhoods located sufficiently faraway from Legnaia. We refer to these distant clusters of streets as the “donor pools”. The potential outcome for Talenti St. in case the light rail was located elsewhere, but still in the Legnaia neighborhood, is estimated as weighted average of the other streets belonging to the same Florentine urban neighborhood of Legnaia.

**Donor pools.** Our data-set includes information on 38 thoroughfares and streets of Florence, clustered in 10 peripheral urban neighborhoods sufficiently faraway from the neighborhood of Legnaia (see Section 2 and Figure 2). We view this set of 38 streets as reservoir of control streets from which selecting a subset of control streets, the donor pool, for each one of the streets considered in Legnaia. Specifically, for each street $i \in \{1\} \cup \mathcal{N}_1$, we resort to matching methods to select a donor pool with background characteristics similar to unit $i$ in the two pre-treatment years (2004 and 2005).

We implement matching considering all the pre-treatment outcomes (number of purveyors of non-durables/durables every 500 meters; median of the local sales distribution of purveyors of non-durables/durables), and the following covariates, which further describe the retail sector located on the streets prior to the intervention: the inter-quartile range of the local sales distribution of purveyors of non-durables/durables, and the number of different types of stores that are present. From all the previous street $\times$ time specific variables, we construct neighborhood-level variables for each time point, $t$. Donor pools are selected using all these street-level and neighborhood-level pre-treatment variables, that is, using $Y_{it} = [Y_{i1t}, Y_{i2t}, Y_{i3t}, Y_{i4t}]$, $C_{it} = [C_{i1t}, C_{i2t}, C_{i3t}]$, and their neighborhood-level averages $Y_{N_{it}} = [Y_{N_{1it}}, Y_{N_{2it}}, Y_{N_{3it}}, Y_{N_{4it}}]$, $C_{N_{it}} = [C_{N_{1it}}, C_{N_{2it}}, C_{N_{3it}}]$, for $i = 1, \ldots, 1+38$ and $t = 1, 2,$
where time $t = 1$ and time $t = 2$ refer to year 2004 and year 2005, respectively. For each street $i$, let $D_{it} = [Y_{it}, Y_{N_{it}}, C_{it}, C_{N_{it}}]$, be the $2 \times 4 + 2 \times (K = 3) = 14$-dimensional vector including information on the matching variables at time $t$, $t = 1, \ldots, T_0$, $T_0 = 2$.

As measure of the similarity between a street $i \in \{1\} \cup N_1$ and a street $i' \notin \{1\} \cup N_1$ we use the Mahalanobis distance. For each street $i \in \{1\} \cup N_1$, we calculate the Mahalanobis distance between street $i$ and each street $i' \notin \{1\} \cup N_1$ for each pre-treatment time $t = 1, 2$:

$$d_{ii'}^{(t)} = (D_{it} - D_{i't})' \Sigma_t (D_{it} - D_{i't}),$$

where $\Sigma_t$ is the variance-covariance matrix of $D_t = \{D_{it}\}_{i=1}^{1+N}$ at time $t$. Then, street $i' \notin \{1\} \cup N_1$ is included in the donor pool for street $i \in \{1\} \cup N_1$ if the Mahalanobis distance between $i$ and $i'$ is not greater than 5 points in each pre-treatment time: street $i' \notin \{1\} \cup N_1$ belongs to street $i$’s donor pool, $i \in \{1\} \cup N_1$, if $d_{ii'}^{(1)} \leq 5$ and $d_{ii'}^{(2)} \leq 5$. This inclusion rule was defined through a data-driven approach, striving to find a good compromise between quality of matched controls and number of potential controls for each street in Legnaia. Specifically, for each street $i$ in the urban neighborhood of Legnaia, $i \in \{1\} \cup N_1$, we discard from its donor pool control streets with very different characteristics to $i$, trying to avoid to end up with a donor pool that comprises a too small number of units. The resulting number of matched control units is: 24 for Talenti St., Pollaiolo St. and Pisana St.; 22 for Scandicci St.; and 14 for Magnolie St. (see Table A1 in Web Supplementary Material).

**Synthetic control weights.** Once we have selected a donor pool for each street in the urban neighborhood of Legnaia, we move to the calculation of the weights defining the corresponding synthetic control street. Recall that a synthetic street for street $i$ in Legnaia is constructed as weighted average of potential control streets belonging to street $i$’s donor pool, with weights chosen so that the resulting synthetic street best reproduces the values of the outcome of interest in street $i$ and in its neighborhood before the light rail line was constructed in Talenti St. Specifically, for each outcome of interest, we seek for weights that minimize the mean squared prediction error (MSPE) over the pre-treatment period, i.e., they minimize the squared deviations between the outcome for the unit of interest and the synthetic control unit summed over all pre-intervention periods. The estimated weights are reported in Tables A1 and A2 in Web Supplementary Material.
Table 1 shows the differences between the individual- and cluster-level pre-intervention outcomes for each street, \(i\), in Legnaia, \(i \in \{1\} \cup \mathcal{N}_1\), and its synthetic control (columns 1-5). The differences in terms of number of stores are always extremely small. Also in terms of sales, which are expressed in Euros, unbalances are acceptably small. Therefore we can argue that the estimated weights work well, by leading to construct individual- and cluster-level synthetic controls’ pre-intervention outcomes that are approximately equivalent to those observed for the corresponding streets in Legnaia. The last column in Table 1 shows the differences between the pre-intervention outcomes of Talenti St. and its \((0, e_{\mathcal{N}_1}^{(*)})\) synthetic control, constructed as weighted average of the pre-intervention outcomes of its neighbors. Again, these differences suggest that the synthetic \((0, e_{\mathcal{N}_1}^{(*)})\)-Talenti St. well mimics Talenti St. in the pre-treatment years.

| Year = 2003 | Talenti (direct effect) | Pisana | Pollaiolo | Magnolie | Scandicci | Talenti (unrealized spillover) |
|---|---|---|---|---|---|---|
| Pre-treatment outcomes | Street | Neighbors | Street | Neighbors | Street | Neighbors | Street | Neighbors | Street | Neighbors | Street |
| Number of stores selling durables | 0.060 | -0.147 | -0.024 | -0.087 | 0.017 | -0.099 | -0.072 | -0.020 | -0.099 | -0.970 | -0.557 | 0.376 |
| Number of stores selling non-durables | 0.014 | 0.011 | 0.009 | 0.042 | 0.038 | 0.067 | -0.015 | -0.006 | -0.065 | -0.124 | -0.025 | 0.256 |
| Median sales of stores selling durables | -152.42 | -359.03 | -232.44 | -366.15 | -101.77 | -152.68 | -679.93 | -477.66 | -1841.59 | -7437.49 | 3205.00 |
| Median sales of stores selling non-durables | -319.93 | -2161.49 | -196.33 | -2544.59 | 1335.36 | 2151.12 | -502.37 | -12741.38 | -1149.67 | -2793.88 | -3842.25 |
| Year = 2004 | Talenti (direct effect) | Pisana | Pollaiolo | Magnolie | Scandicci | Talenti (unrealized spillover) |
| Pre-treatment outcomes | Street | Neighbors | Street | Neighbors | Street | Neighbors | Street | Neighbors | Street | Neighbors | Street |
| Number of stores selling durables | -0.008 | 0.141 | 0.065 | 0.018 | 0.066 | 0.017 | 0.066 | -0.343 | 0.402 | -0.387 |
| Number of stores selling non-durables | 0.013 | 0.012 | 0.068 | 0.010 | 0.045 | 0.039 | -0.032 | -0.007 | -0.006 | -0.280 |
| Median sales of stores selling durables | 1541.51 | -3722.64 | 207.09 | 132.17 | 1056.45 | 29.06 | -3544.05 | 2001.05 | -38061.05 | 2716.88 | 2070.00 |
| Median sales of stores selling non-durables | -796.98 | -4922.75 | -4981.32 | -9828.75 | -1366.58 | -1781.65 | -2594.19 | -11115.77 | -4223.18 | -9250.31 | 1613.5 |

Table 1: Differences between the observed and the SCM-imputed values of the outcomes in the pre-intervention period for Talenti (direct effect and unrealized spillover), Pisana, Pollaiolo, Magnolie and Scandicci.

**Statistical inference.** We apply the in-space placebo studies described in Section 3 to assess the statistical significance of the estimated effects. In the construction of these placebo studies, we exclude control units for which the RMSPE was extremely high; the values of RMSPE for each control unit involved in the placebo studies are reported in Tables A3 and A4 in Web Supplementary Material. Specifically, we exclude control units with RMSPE < 1 in the placebo studies for the effect of the number of stores, and control units with RMSPE < 5000 in the placebo studies for the effect on the median sales (in Euros).
4.2 Estimated direct and average spillover effects

Figure 3 shows the estimated direct effect of the new light rail on Talenti St. We find that local retailers selling durables suffered from a marked reduction of median sales during the initial couple of years of light rail construction, and that such negative effects tend to be rather extreme compared to placebos. There is some evidence that the new light rail increases the median sales of stores selling durables on Talenti St. during the first two years of operation, but the estimated effects are small and statistically negligible. We find no direct effect the new light rail on median sales of stores selling non-durables on Talenti St.

As for the number of retailers, the light rail has no effect during the construction stage on the number of purveyors of non-durables, while it causes a rather ephemeral increase in the number of purveyors of durables. The positive effects of the new light rail on the number of stores selling non-durables further increase once the tramway is operational, presumably in response to increased flows of passers-by at stations. These results are quite in line with previous empirical literature, which highlights weak (if any) signs of commercial
revitalization close to transit stations located in urban areas Credit (2018), Schuetz (2015).

The average spillover effects on the other streets in the urban neighborhood of Legnaia are shown in Figure 4. We find a clearly positive spillover on the number of purveyors of non-durables during the construction period, though this comes at the price of a negative spillover effect on median sales. After the inauguration of the tramway, the spillovers vanish, suggesting that the streets return to business as usual. A possible interpretation of these results is that new purveyors of non-durables settle in these streets in the attempt to "steal" some of the customers that used to patronize stores on Talenti St., assuming that these customers would have been willing to escape from the construction site to do their daily shopping within walking reach.

We find a positive spillover on the median sales of stores selling durables, while there is no spillover on their number. Since durables are subject to less frequent purchase, the attraction of new entrants in the area is weak. However, incumbent purveyors of durables on
these streets are able to attract consumers from Talenti St. especially during construction, although they continue to benefit from positive spillovers afterwards, presumably because of the unchanged car accessibility of their locations.

4.3 Estimated unrealized spillover effect

Figure 5 shows the unrealized spillover effects on Talenti St., that is, the cost avoided or the benefit forgone by Talenti St. if the tramway had been constructed in some other street belonging to its same urban neighborhood. Figure 5 highlights no major unrealized spillovers on the outcomes related to the number of stores. In terms of sales, Talenti St. misses both negative and positive spillovers, though of moderate size, during light rail construction, and mostly positive spillovers afterwards.

![Graphs showing the number of stores selling durables and non-durables, and median sales of stores selling durables and non-durables.]

Figure 5: Estimated unrealized spillover effect on Talenti St. and placebo tests

It is also worthwhile to look at the difference between the direct effect and the unrealized spillover effect on Talenti St. Figure 6 reports such difference, which quantifies – given the choice of locating a light rail in the urban neighborhood of Legnaia – the “net”
advantage/disadvantage connected to a situation of immediate proximity to tracks and stations, relative to a situation where the light rail is slightly more distant. From Figure 6, we gather that Talenti St. does gain some new shops, but no positive effect on sales derives from hosting the tramway rather than having it close by.

Figure 6: Difference between the estimated direct effect and unrealized spillover effect for Talenti St. and placebo tests

5 Concluding Remarks

The SCG has been hailed as “...the most important innovation in the policy evaluation literature in the last 15 years” Athey and Imbens (2017) and the ideas initially put forward in Abadie and Gardeazabal (2003), Abadie et al. (2010, 2015) have sparked avenues of methodological research. This paper has met the challenge of extending the SCG method to settings where the assumption of interference is untenable. This is a nascent stream of research in the SCG literature, which our study contributes to inaugurate. The implications
for applied economic research may be very relevant: SCG methods are often applied to estimate causal effects on a single treated unit at the meso- and macro-economic level, and the plausibility of the no-interference assumption is debatable in many studies. In the presence of interference, the direct effect on the treated unit may no longer be the only one of interest, but spillovers may also become an important component of the overall change induced by the intervention.

In this paper, building on recent methodological works on causal inference with interference in the potential outcomes framework, we have first formally defined unit-level direct effects and average spillover causal effects under a partial interference assumption. We have also introduced a new spillover effect, the “unrealized spillover,” which is the spillover that would have taken place on the currently treated unit if another unit had been assigned to the intervention. We believe that these three quantities may be relevant for a comprehensive evaluation of interventions at the meso- and macro-economic level.

Our study has been motivated by the evaluation of the direct and unrealized spillover effects of a new light rail line built in Florence, Italy, on the retail environment of the street where it was built, and the spillover of the light rail on a number of streets close by. Our approach is very original also with respect to the field literature, where causal studies are still scarce and scholars usually conduct their analyses by aggregating all streets within a given radius (usually half mile) from the new infrastructure. Although the modest retail revitalization effects of light rail we find in our study are rather in line with previous empirical research, our approach, where each street in the radius acts as a distinct unit, has the advantage of portraying a highly detailed causal picture.

We have explicitly defined the assumptions under which we can identify the direct and spillover causal effects of interest. Our identification strategy relies on the presence of clusters of units where no unit is exposed to the treatment. Then we have showed how the causal effects of interest can be estimated using the SCG method originally proposed by Abadie and Gardeazabal (2003), Abadie et al. (2010, 2015).

There are several valuable topics for future research that are ongoing. Extensions to causal studies with general forms of interference are important. The assumption of partial interference is plausible in our application study, as it is in many other causal
studies (e.g., Papadogeorgou et al. 2018, Forastiere et al. 2019, Huber and Steinmayr 2019). Nevertheless, we are aware that some studies might require a more general structure of interference (e.g., Forastiere et al. 2018,b). Also, the generalization of the estimation methods recently proposed by Doudchenko and Imbens (2017) and Athey et al. (2018) to causal panel data studies with interference is also at the top of our research agenda. It is worth noting that these methods have been developed to estimate causal effects in panel studies with typically a large number of untreated periods. Therefore, they may not work well in application studies like ours, where limited information on pre-treatment periods is available.
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Web Supplementary Material
Synthetic Control Group Methods in the Presence of Interference: The Direct and Spillover Effects of Light Rail on Neighborhood Retail Activity

Giulio Grossi\textsuperscript{1}, Patrizia Lattarulo\textsuperscript{2}, Marco Mariani\textsuperscript{2}, Alessandra Mattei\textsuperscript{1,3}, and Özge Öner\textsuperscript{4,5}

\textsuperscript{1}Department of Statistics, Computer Science, Applications, University of Florence
\textsuperscript{2}IRPET - Regional Institute for Economic Planning of Tuscany, Florence, Italy
\textsuperscript{3}Florence Center for Data Science, Florence, Italy
\textsuperscript{4}Department of Land Economy, Cambridge University
\textsuperscript{5}Research Institute of Industrial Economics, Stockholm, Sweden
## A Weights

Table A1: Direct and spillover effects: Weights through which the synthetic control values of the outcome variables \( Y_{1t}(0, 0_{|N_1|}) \), and \( Y_{it}(0, 0_{|N_1|}) \) are calculated.

|                  | \( Y_{1t}(0, 0_{|N_1|}) \) | \( Y_{it}(0, 0_{|N_1|}) \) |
|------------------|-----------------------------|-----------------------------|
| \( t = \) Talenti St. | A B C D                     | A B C D                     |
| Alderotti        | 0.031 0 0 0                | 0.013 0 0 0                |
| Giuliani         | 0.016 0 0 0.014 0.017       | 0.031 0 0 0                |
| Morgagni         | 0.117 0 0 0.011            | 0.027 0 0 0                |
| Panche           | 0.011 0 0 0.270 0.042       | 0.056 0 0 0.078            |
| Cornici          | - - - -                    | - - - -                    |
| Macchi           | 0.021 0 0 0.341 0.013       | 0.464 0.028 0 0.047 0 0    |
| Ponte di Mezzo   | - - - -                    | - - - -                    |
| Romito           | 0.240 0.013 0.561 0.186 0.018 | 0.239 0.031 0 0.823 0 0 0 |
| Toranti          | 0.013 0 0 0.016            | 0.030 0 0 0                |
| Vittorio Emanuele| 0.658 0.054 0 0.460 0.011 | 0.027 0 0 0.052 0 0 0 0    |
| Baracca          | 0.024 0 0.024 0.014 0.136 | 0.031 0 0 0                |
| Guidoni          | - - - -                    | - - - -                    |
| Novoli           | 0.018 0 0.728             | - - - -                    |
| Europa           | 0.004 0.541 0 0 0.012 0 0 | 0.03 0 0 0 0                |
| Datini           | 0.007 0 0 0 0.160          | 0.067 0 0 0 0.044 0 0 0    |
| Ripoli           | 0.005 0 0 0.059           | 0.188 0 0.041 0.208 0.423 |
| Villamagna       | 0.110 0 0.248 0.025 0.109 | 0.035 0 0 0                |
| Centrostelle     | - - - -                    | - - - -                    |
| Mille            | 0.008 0.006 0 0 0 0        | - - - -                    |
| Volta            | - - - -                    | - - - -                    |
| Masaccio         | 0.050 0 0 0 0.010          | - - - -                    |
| Affrico          | 0.023 0.088 0 0 0.016 0.555 | 0.032 0.364 0 0 0 0 0 0 0 0 0.022 0.901 |
| D’Ammanzo        | 0.028 0.351 0 0 0.013 0.394 | 0.030 0.372 0.074 0 0 0.027 0 0 0 0.016 0 0 |
| Rondinella       | 0.011 0 0 0 0.031          | 0.042 0 0 0 0.973 0 0 0.012 0 0 |
| Arezzina         | - - - -                    | - - - -                    |
| De Santis        | - - - -                    | - - - -                    |
| Piagentina       | - - - -                    | - - - -                    |
| Galliano         | 0.025 0 0 0 0.018          | 0.031 0 0 0 0 0 0 0.011 0 0 |
| Maragliano       | 0.014 0 0 0 0.012          | 0.029 0 0 0 0.363 0 0 0.003 0 0 |
| Ponte alle Mose  | 0.008 0 0 0 0.199          | 0.043 0 0.311 - - - - - 0 0.006 0.099 0 0 |
| Redi             | 0.019 0 0 0 0.023          | 0.033 0 0 0 0 0 0 0 0.022 0 0 |
| Toselli          | 0.024 0 0 0 0.018 0.009    | 0.031 0 0 0 0 0 0 0.577 0.567 0.018 0 0 |
| Peretola         | - - - -                    | - - - -                    |
| Pastorese        | - - - -                    | - - - -                    |
| Pratese          | - - - -                    | - - - -                    |
| Caracciolo       | - - - -                    | - - - -                    |
| Faentina         | - - - -                    | - - - -                    |
| Maffei           | - - - -                    | - - - -                    |

**Legend:** A = number of stores selling durables; B = number of stores selling non-durables; C = median sales of stores selling durables; D = median sales of stores selling non-durables. The symbol − means the street was not matched to the one under scrutiny and, therefore, no weight is computed.
Table A2: Unrealized spillover effects on Talenti St.: Weights through which the synthetic control values of the outcome variable $Y_{1t}(0, e^{(i)}_{|N_1})$ are calculated.

|       | A    | B    | C  | D  |
|-------|------|------|----|----|
| Pisana| 0    | 0    | 0  | 0  |
| Pollaiolo| 0 | 0 | 1  | 0  |
| Magnolie| 0.594 | 0.389 | 0  | 1  |
| Scandicci| 0.406 | 0.611 | 0  | 0  |

Legend: A = number of stores selling durables; B= number of stores selling non-durables; C= median sales of stores selling durables; D = median sales of stores selling non-durables.

B Root mean squared prediction errors

For direct and spillover effects, for each street $i \in \{1\} \cup N_1$, the pre-intervention root mean squared prediction error is defined as

$$\sqrt{\frac{1}{T_0} \sum_{t=1}^{T_0} \left[ Y_{i,t} - \sum_{i' \notin \{1\} \cup N_1, i' \in M_i} \hat{\lambda}_i^{(i)} Y_{i',t} \right]^2}$$

Similarly, for each street $i' \notin \{1\} \cup N_1$, but $i' \in M_i$ for some $i \in \{1\} \cup N_1$, the pre-intervention root mean squared prediction error is defined as

$$\sqrt{\frac{1}{T_0} \sum_{t=1}^{T_0} \left[ Y_{i',t} - \sum_{i'' \notin \{1\} \cup N_1 \cup \{i\} \cup N_1} \hat{\lambda}_{i''}^{(i')} Y_{i'',t} \right]^2}$$

For unrealized spillover effects on Talenti St., the pre-intervention root mean squared prediction errors are defined as

$$\sqrt{\frac{1}{T_0} \sum_{t=1}^{T_0} \left[ Y_{1,t} - \sum_{i \in N_1} \hat{\lambda}_i^{(1)} Y_{it} \right]^2} \quad \text{and} \quad \sqrt{\frac{1}{T_0} \sum_{t=1}^{T_0} \left[ Y_{i,t} - \sum_{i' \in N_1 \setminus \{i\}} \hat{\lambda}_i^{(i)} Y_{i',t} \right]^2} \quad i \in N_1$$
Table A3: Direct and spillover effects: pre-intervention root mean squared prediction errors.

| Talenti | Pisana | Pollaiolo | Magolie | Scandicci |
|---------|--------|-----------|---------|-----------|
| A | B | C | D | A | B | C | D | A | B | C | D | A | B | C | D | A | B | C | D | A | B | C | D | A | B | C | D |
| Talenti | 0.10 | 0.00 | 1576.68 | 3179.69 | Pisana | 0.05 | 0.00 | 249.88 | 5191.85 |
| Pollaiolo | 0.05 | 0.00 | 1086.26 | 1253.52 |
| Magolie | 0.07 | 0.01 | 3141.20 | 8933.61 |
| Scandicci | 0.62 | 0.00 | 18315.66 | 5250.69 |
| Alderotti | 0.69 | 0.12 | 13047.95 | 3179.09 |
| Giuliani | 1.17 | 0.28 | 248.98 | 5191.85 |
| Morbegno | 0.12 | 0.00 | 3145.12 | 2487.43 |
| Panico | 0.02 | 0.66 | 5703.42 | 3912.44 |
| Mariani | 0.45 | 0.00 | 3165.75 | 4165.95 |
| Romito | 0.23 | 0.01 | 275.76 | 2700.70 |
| Terenzi | 0.00 | 0.00 | 1530.00 | 0.00 |
| Vittorio Emanuele | 0.03 | 0.20 | 2322.60 | 0.00 |
| Baracca | 0.00 | 0.00 | 4837.17 | 2562.70 |
| Guidoni | − | − | − | − |
| Novelli | 0.00 | 0.00 | 6252.95 | 4196.07 |
| Europa | 0.56 | 1.39 | 7591.18 | 0.00 |
| Ripoli | 0.00 | 0.08 | 1182.13 | 2784.51 |
| Villamagna | 0.02 | 0.02 | 18120.55 | 16624.49 |
| Centro Stelle | − | − | − | − |
| Mille | 3.85 | 3.50 | 34757.80 | 3.50 |
| Masaccio | 2.35 | 1.92 | 2487.10 | 2.57 |
| Affrico | 0.00 | 0.00 | 2515.60 | 0.00 |
| D'Annunzio | 0.00 | 0.00 | 2515.60 | 0.00 |
| Roncalli | 0.03 | 0.03 | 1804.00 | 0.00 |
| Galliano | 0.00 | 0.00 | 1804.00 | 0.00 |
| Maragliano | 0.00 | 0.00 | 1804.00 | 0.00 |
| Fonte alle Mose | 0.00 | 0.00 | 8007.63 | 3674.65 |
| Redi | 0.00 | 0.00 | 32340.95 | 0.71 |
| Treosti | 0.71 | 0.00 | 32340.95 | 1463.11 |

**Legend:** A = number of stores selling durables; B = number of stores selling non-durables; C = median sales of stores selling durables; D = median sales of stores selling non-durables. The symbol − means that the street was not matched to the one under scrutiny.
Table A4: Unrealized spillover effects on Talenti St.: pre-intervention root mean squared prediction errors.

|       | A   | B   | C       | D       |
|-------|-----|-----|---------|---------|
| Talenti | 0.28| 0.20| 1966.38 | 2147.77 |
| Pisana  | 0.25| 0.00| 117.37  | 10330.77|
| Pollaiolo| 0.00| 0.46| 96.49   | 68.61   |
| Scandicci| 4.91| 1.36| 18822.09| 341.91  |
| Magnolie| 0.80| 0.50| 3383.88 | 0.00    |

**Legend:** A = number of stores selling durables; B = number of stores selling non-durables; C = median sales of stores selling durables; D = median sales of stores selling non-durables.
Figure A1: Estimated causal effects and their standard errors calculated according to Doudchenko and Imbens (2017)