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Identification issues

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The empirical content of spatial spillovers: Identification issues

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Abstract

The notion of spatial spillovers has been widely used in applied spatial econometrics. In this paper, we consider their empirical content under the lens of identification in both structural and causal reduced-form models. Discussing the various threats to identification in structural models, we point out that the typical estimation framework proposed in the applied spatial econometric literature boils down to considering spillovers as a side-effect of a data-driven chosen specification, which strongly hampers their empirical content. We also discuss the limits of using interaction matrices purely based on geography to identify the source and content of spillovers. Then, we present reduced forms impact evaluation models for spatial data and show that the current spatial versions of usual impact evaluation models are not fully satisfactory when considering the identification issue. Finally, a set of recommendations for applied papers aimed at identifying spatial spillovers is proposed.

JEL Classification: *C18, C21*
Keywords: spatial spillovers; causal inference; interference; structural and reduced form identification.

1 Introduction

Spatial spillovers, which we loosely define here as geographical interactions and/or externalities between neighboring observations, are at the heart of applied spatial econometrics, as typically used in regional/political science, among others. Building upon Tobler's first law in geography, countless empirical papers have estimated spatial spillover effects by specifying a geographically-based connectivity matrix[1] and using one of the classical spatial econometric specifications, such as the spatial Durbin model, the spatial lag model, etc. In essence, a typical paper in applied spatial econometrics aims at analyzing the “impact” (quotation marks for purpose) of some explanatory variable(s) on an explained variable based on a model estimated on georeferenced data and accounting for cross-sectional dependence in various forms. Following a long-standing tradition in spatial econometrics, the analysis is often based on specific-to-general or general-to-specific specification search strategies, and is generally followed by a discussion of interpretations in terms of *implied* spatial spillovers.[2]

However, this practice has been largely criticized for various reasons. The most heavy criticism probably arose from Gibbons & Overman (2012) who pointed out major identification problems, when spatial econometric specifications are used for an explanatory purpose, with the more or less explicit aim of being able to draw causal interpretations of the estimated parameters. Their conclusion best illustrates their arguments: “Identification problems bedevil applied spatial economic research. Spatial econometrics usually solves these problems by deriving estimators assuming that functional forms are known and by using model comparison techniques to let the data choose between competing specifications. We argue that in many situations of interest this achieves, at best, only very weak identification. Worse, in many cases, such an approach will be uninformative about the causal economic processes at work, rendering much applied spatial econometric research “pointless,” unless the main aim is a description of the data.” Partridge et al. (2012) also point out that standard spatial econometrics is not capable of ”differentiating when outcomes in nearby areas are spatially correlated [...] versus spatial causality” while McMillen (2012) argues that standard spatial econometric models are falsely used as a quick fix for any model misspecification. As mentioned by Mur (2013), this lack of attention of spatial econometrics to identification and causality is probably related to its history, as its evolution initially mimicked that of the time series literature.

As a result, one can only observe that, while the standard spatial econometric toolbox is widely used in empirical papers in regional science and related disciplines, the papers in top field or top 5 economic journals that make use of “spatial econometric” models are now almost exclusively anchored in the network econometrics literature. In the latter, the connections between individuals are modeled along a social network and the main focus of interest is to derive identification strategies to disentangle the endogenous effects from the exogenous and correlated effects, following the terminology of Manski (1993) with the final aim of deriving a causal interpretation of these effects. The fiscal federalism literature is representative of this trend. Spatial lag or spatial Durbin models were widely used in the early 2000s to assess the existence and extent of fiscal competition among neighboring local jurisdictions. These models were

[1]This matrix is also called a spatial weights matrix or interaction matrix. In the literature on social networks, it is referred to as the adjacency matrix. However, we prefer the term connectivity or interaction to spatial weights for reasons that will become apparent later.

[2]Figure 1 in Elhorst (2010) illustrates these nested relationships between spatial models in the cross-sectional case.

labeled first-generation models by Agrawal et al. (2022). Yet, they were widely criticized on various grounds related to identification: the exclusion restrictions are never discussed, the instruments are weak, and these models are unable to discriminate between underlying mechanisms (fiscal competition, yardstick competition, Tiebout sorting, etc.). All these pitfalls led the recent literature on fiscal federalism to heavily rely on quasi-experiments to obtain more credible instruments (see among others Parchet 2019, Lyytikäinen 2012).

At the heart of these criticisms is the notion of *identification*. In the twentieth century, identification was mainly discussed within the framework of simultaneous equation models (demand and supply), in the spirit of the Cowles Foundation. This approach, called structural, has been at the center of the economics literature for a long period and is based on explicitly stating modeling and behavioral assumptions. However, following the credibility revolution of Angrist & Pischke (2010), much effort has been made to design identification strategies aimed to be as close as possible to a random experiment: the experimentalist paradigm. This second approach, also called reduced form, treatment effect, impact evaluation of public policy, or causal inference, aims at abstracting from behavioral assumptions typically made in the structural approach to assess the impact of a policy on an outcome, but at the cost of eluding the question of transmission channels. Although in practice, the separation between structural and reduced forms might not be so clear-cut, we use this distinction as the starting point for clarity purposes.

Our first aim in this paper is to reconsider the notion of spatial spillovers in light of this fundamental econometric question of identification. Although the econometric literature has developed many different concepts of identification, our paper only considers its most widely used notion, namely point identification. According to Lewbel (2019, p.842) the parameter θ is point identified (often just called identified) if there exist no pairs of possible values θ and $\widetilde{\theta}$ that are different but observationally equivalent. In this paper, we consider the (point) identification of spatial spillovers in both structural and causal reduced-form models. In the first case, i.e. structural models, spatial spillovers should be the main parameters of interest, as endogenous effects are the main parameters of interest in network econometrics. Yet, we point out that the typical statistical strategies (specific-to-general or general-to-specific) proposed in the literature boil down to considering spatial spillovers as a side-effect of the data-driven chosen specification, which strongly hampers their empirical content. Additionally, the other threats to identification traditionally considered in the network econometrics literature, such as unobserved heterogeneity and endogenous sorting, are typically not discussed. We consider all of these issues. In particular, in line with Neumayer & Plumper (2016), we discuss how the use of interaction matrices based solely on geographical criteria as a proxy of numerous transmission mechanisms cannot allow identification of associated spillovers.

In causal-reduced models, interest typically relies on assessing the causal impact of a treatment on an outcome. Among other assumptions, most papers since Rosenbaum & Rubin (1983) assume a cross-unit no-interference assumption: the Stable Unit Value Assumption (SUTVA). However, for the past 20 years, there has been an explosion of work relaxing the no-interference assumption.[3] Initially, the articles aimed to assess the bias of the causal effects estimated under SUTVA and to propose designs of experiments that avoid interference. Then, the focus has progressively switched to a substantive identification of these spillovers, both in terms of estimands of interests and design of experiments, as they are a crucial component in understanding the full impact of an intervention.[4] Therefore, the notion of spillovers and related concepts has become central in program evaluation (see Angelucci & Di Maro 2016). Most discussions in this literature focus on spillovers along networks, yet, a limited statistical literature exists with respect to spatial spillovers. We discuss this emerging literature and also show that the current spatial versions of usual econometric impact evaluation models, such as difference-in-differences models including a spatial lag of the endogenous variable, are not satisfactory when considered in the light of identification.

These attempts illustrate the fact that the question of identification in a spatial context has recently witnessed an upsurge of interest. Our second aim is then to make a statement on the current state of affairs. Some reviews of this emerging literature are available in Gibbons et al. (2015) mainly for the structural approach, while Kolak & Anselin (2020), Akbari et al. (2023) and Gao et al. (2022) focus more on the causal inference framework. For example, Kolak & Anselin (2020) review how spatial effects (namely spatial dependence and spatial heterogeneity) lead to the violation of the SUTVA assumption and provide an overview of existing attempts, within the spatial econometrics/regional science literature, to extend impact the common evaluation identification strategies and models (difference-in-differences, propensity score matching, regression discontinuity, and instrumental variables) in the presence of spatial effects. They focus their presentation on the counterfactual framework and discuss how spatial effects affect the assignment to treatment, the potential sources of variation in the treatment variables, and the estimands.

Our approach goes one step further in that we consider the notion of identification in general: not only do we also discuss this notion in both structural and reduced forms, but we are also able to provide bridges between these approaches together with a comprehensive view of identification in the presence of spatial heterogeneity that we apprehend both in terms of (spatial) varying treatment effect and spatial confounding. We also consider a range of other identification threats. Conversely, we do not document the concept of Granger causality in a spatial context, nor do we discuss the empirical dynamic modeling or convergent-cross mapping (CCM) frameworks, as they are more rooted in the time series approach to causality. In addition, we do not consider graphical models or path analysis models as we only focus on the counterfactual and

[3]See among others VanderWeele (2015), Hong (2015), Angelucci & Di Maro (2016), Reich et al. (2021), Aronow, Eckles, Samii & Zonszein (2021).

[4]If beneficial spillovers exist, a lower percentage of the population might need treatment. Furthermore, information on the nature and extent of spillover effects may allow targeting specific individuals or groups of individuals to increase the overall impact of the intervention.

potential outcomes approach.[5] In a spatial context, these three approaches are discussed in Akbari et al. (2023), Gao et al. (2022).[6]

The paper is structured as follows. Section 2 presents the structural approaches that have been developed to identify spatial spillovers as well as the different threats to identification that are often overlooked in the applied spatial econometrics literature. Section 3 describes the causal inference methods that account for interference (spillovers) and discusses how they have been extended so far in a spatial context. Section 4 develops recommendations for applied papers aiming to identify spatial spillovers: definition of parameters of interest, structural versus reduced-form approaches, unit of analysis, choice of interaction matrix, spatial heterogeneity and structure of error terms. Section 5 concludes.

2 Structural approach to (spatial) spillovers

2.1 Related literature

The structural modeling approach is based on the development of a structural model, where the identification focuses on the associated (deep) parameters, giving them a *ceteris paribus* interpretation (Clarke & Windmeijer 2012). Some papers in structural spatial and network economics have developed several models to account for spillovers between observations, which can be decision units (microeconomic agents for instance) or more aggregate units (countries, regions).

Starting with the case of structural models involving aggregate units, in the field of economic growth, Lopez-Bazo et al. (2004) model externalities of production between regions. Ertur & Koch (2007) develop a spatially augmented Solow model where interactions between economies are motivated by technological interdependencies. Egger & Pfaffermayr (2006) follow their approach to study the consequences of spatial dependence on convergence, while Pfaffermayr (2009) contrasts the spatial Solow model with the Verdoon's model and demonstrates that the speed of convergence in both models depends on the remoteness and the income gaps of all regions. In a subsequent paper, Ertur & Koch (2011) integrate technological interdependencies in a Schumpeterian growth model, allowing to cast both models in an integrated theoretical and methodological framework. Turning to the empirical applications of the models, Lopez-Bazo et al. (2004), Egger & Pfaffermayr (2006), Ertur & Koch (2007), Pfaffermayr (2009) assume a geographically-based interaction scheme. In contrast, Ertur & Koch (2011) consider an interaction scheme based on geographic proximity or bilateral trade. However, they point out that this interaction matrix should ideally be theory-based as the implementation of spatial methods requires accurate identification of their localization (Ertur & Koch 2011, p.236). A step towards this direction is due to Behrens et al. (2012) who derive a quantity-based structural gravity

[5] See Ogburn & VanderWeele (2014) for some insight in specifying causal diagrams with interference.

[6] See also Mur (2013), Herrera et al. (2016) for an extension of the Granger concept of causality for spatial data.

equation in which both trade flows and error terms are cross-sectionally correlated. To the best of our knowledge, this is the only paper in which the channel through which interactions occur and the precise functional form of the interaction scheme, constructed from the population shares, are completely derived from economic theory.

At the microeconomic level, Schone et al. (2013) develop a theoretical model to analyze the determinants of cities' growth control decisions, showing that it is the political struggle between different groups of voters and lobbies that is the source of these decisions. Furthermore, by accounting for the residential mobility of renters in the city, their model implies spatially interdependent local growth control policies so that cities engage in strategic interactions. In the context of social networks, the literature has also developed models to theoretically justify the presence of peer effects. These models, derived from game theory with strategic interactions, mainly explain spillovers by conformity or strategic complementarities between individuals.

2.2 Applied spatial econometrics

Gibbons et al. (2015) propose a general framework that captures almost all channels through which spatial effects may be included in a regression model. In the cross-sectional case, this model is shown in (1).

$$y_i = \mathbf{x}_i\boldsymbol{\beta} + \lambda \sum_{j=1}^{n} w_{ij}^{y} y_j + \sum_{j=1}^{n} w_{ij}^{x} \mathbf{x}_j \boldsymbol{\gamma} + \sum_{j=1}^{n} w_{ij}^{z} \mathbf{z}_j \boldsymbol{\theta} + \sum_{j=1}^{n} w_{ij}^{v} \mathbf{v}_j \boldsymbol{\kappa} + \varepsilon_i, \quad i = 1, \cdots, n \tag{1}$$

where y_i is the outcome for individual i, $\sum_{j=1}^{n} w_{ij}^{y} y_j$ is the aggregate[7] value of the neighboring outcomes of i, $\mathbf{x}_i$ is a vector of individual characteristics and $\sum_{j=1}^{n} w_{ij}^{x} \mathbf{x}_j$ represents the aggregate value of neighborhood's characteristics, $\sum_{j=1}^{n} w_{ij}^{z} \mathbf{z}_j$ is a vector of observed common factors and $\sum_{j=1}^{n} w_{ij}^{v} \mathbf{v}_j$ represents the spatial aggregate of unobservables. This last term may represent interactions between observations in unobserved dimensions or spatially autocorrelated errors. Finally, ε_i is the idiosyncratic error term. In the social interactions literature, $\sum_{j=1}^{n} w_{ij}^{y} y_j$ refers to *endogenous effects*, $\sum_{j=1}^{n} w_{ij}^{x} \mathbf{x}_j$ is named *contextual effects* while $\sum_{j=1}^{n} w_{ij}^{v} v_j$ represents *correlated effects* (Manski 1993).

This model allows different connectivity schemes for y, $\mathbf{x}$, $\mathbf{z}$ and v. However, in the vast majority of works, these matrices are the same, notably due to the difficulty of justifying different interaction schemes. When individuals interact in separate groups of the same sizes (for example, farmers who interact with all other farmers in the same villages, but not with farmers in other villages, with villages displaying the same number of farmers), Manski (1993) shows that it is impossible to distinguish between endogenous and contextual effects even in the absence of correlated effects. This is known as the reflection problem. However, as soon as we depart from this specific interaction scheme, either by

[7] W is not row-standardized, this point is discussed below.

assuming different group sizes (Lee 2007) or considering interactions along networks (Bramoullé et al. 2009), it is possible to distinguish between these two different types of spillovers if correlated effects are absent. Therefore, considering the possibility of correlated effects is of the greatest importance for the identification of spillover effects.

In applied spatial econometrics, two approaches dominate the selection of the econometric specification. The first one, a general-to-specific approach, consists in estimating a Spatial Durbin model, shown in (2), which already constitutes a constrained version of model (1 where the vectors $\boldsymbol{\theta}$ and $\boldsymbol{\kappa}$ are set to $\mathbf{0}$.[8]

$$y_i = \mathbf{x}_i\boldsymbol{\beta} + \lambda \sum_{j=1}^{n} w_{ij} y_j + \sum_{j=1}^{n} w_{ij}\mathbf{x}_j\boldsymbol{\gamma} + \varepsilon_i, \quad i = 1, \cdots, n, \tag{2}$$

As such, the SDM assumes away correlated effects and observed common factors, which are prevalent in applied work. We return to this issue in Section 2.3.3. Moreover, most of the time, this SDM specification is not driven by economic arguments, but by statistical properties. LeSage & Pace (2009) argue that the SDM generalizes the spatial autoregressive model to account for possible omitted variables that are spatially autocorrelated (i.e neighborhood characteristics).[9] Although this problem is indeed pervasive in applied economics, other threats to identification, presented in the next section, need to be tackled to identify spillovers.

In a large majority of papers, once this model is estimated, using quasi-maximum likelihood, Bayesian methods, two-stages least squares[10], or generalized method of moments, the reduced form is computed and marginal effects are calculated (LeSage & Pace 2009). These marginal effects are interpreted as the direct, indirect, or total effect of a change in each determinant on the outcome of interest, accounting for the presence of spillovers between observations, as shown in (3) when the reduced form of model (2) is written in matrix form:

$$\frac{\partial \mathbb{E}(\mathbf{y} \mid \mathbf{X}, \mathbf{W})}{\partial \mathbf{X}_k} = (I_n - \lambda \mathbf{W})^{-1}(\mathbf{I}_n \beta_k + \mathbf{W}\gamma_k), \tag{3}$$

where $\mathbf{I}_n$ stands for the identity matrix of dimension n.

The second approach used in applied spatial econometrics papers is the specific-to-general strategy. It consists of starting with a linear model (with or without neighborhood's characteristics) and relies on specification tests (typically of the Lagrange multiplier or Wald type) to find the most relevant spatial model (namely the one that best fits the data). The objective of this approach

[8] The SDM further encompasses the spatial lag model (SAR) when $\boldsymbol{\gamma} = 0$, the spatial X model (SLX) when $\lambda = 0$ and the spatial error model (SEM) when $\boldsymbol{\gamma} = -\lambda\boldsymbol{\beta}$. Note that in the tax competition literature, the spatial lag model is used as a point of departure, assuming that contextual effects play the role of the excluded instruments for the endogenous effect.

[9] The inclusion of neighborhood characteristics also implies more heterogeneity in the total impacts (see LeSage & Pace 2009, p.40)

[10] In the SDM model, internal instruments for the 2SLS may be weak since only higher order contextual effects can be used, the latter being potentially highly correlated with the contextual effects.

is to assess whether cross-sectional dependence should be modeled in the dependent variable (SAR model), in the explanatory variables (SLX model) and/or in the error term (spatial lag in the errors or spatial moving average errors). Once the "best" specification is selected, the "impact" measures (average direct, indirect, and total effects) are reported and interpreted as the impact change in a given explanatory variable on the outcome. This model selection procedure borrows from the Box-Jenkins approach developed for time-series data. As already pointed by Gibbons & Overman (2012), this specific-to-general model selection procedure, initially motivated by computational reasons[11], is strongly hampered by the fact that it is not linked to any consideration on the economic reasons for the presence of spillovers and their transmission channels.

An implication of both approaches is that spillover effects, under the form of endogenous and/or contextual effects, are relegated as side-products of the specification. Indeed, interpretations are based on marginal effects, which compute the effect of a change in the (exogenous) determinants on the outcome, accounting for spillovers. However, the spillovers *per se* are not of interest. Yet, given the structural constraints inherent to these specifications, for instance, the motivation for spillovers, the construction of the interaction scheme (selection of the relevant interaction space, the functional form of the assumed links) and its properties (possible evolution over time, endogeneity, mismeasurements, etc.), the proper identification of these spillovers should be at the center of interest and not considered as a data-driven consequence of the specification.

2.3 Spatial threats to identification

As the previous section has made clear, the presence of a spatial lag, the *endogenous effect*, should be closely related to behavioral assumptions. The empirical modeling of interactions thus requires questioning the unit of decision and its possible discrepancy with the unit of observation, something that has been mostly overlooked in applied spatial econometrics. From an economic point of view, units of decisions might be individuals, firms, local jurisdictions, governments, etc. that typically optimize objective functions under constraints. On the contrary, the unit of observation is the unit on which the empirical analysis is performed (typically depending on data availability). The discrepancy between the unit of decision and the unit of observations raises classical caveats largely documented in the spatial statistical literature: the ecological fallacy and the change of support problem. The ecology fallacy arises when conclusions obtained for aggregated data do not reflect the reality of individuals belonging to this aggregation. Also called aggregation bias, it has received lots of attention in regional and political sciences as it is common to have data at aggregated spatial levels while the meaningful mechanisms are at a lower spatial level. The change of support problem refers to issues related to the combination of spatial data

[11]Spatial models were traditionally estimated by maximum likelihood method, which required the computation of the Jacobian of the transformation, computationally costly.

observed at various scales and support.[12] The consequences of the discrepancy between units of decision and of observations are further amplified in studies of spatial spillovers as the threats to their identification are even more acute. We now discuss these threats, that can be gathered into three main categories: i) Nature and construction of the interaction matrix; ii) Use of proxy variables for interaction; iii) Presence of spatial heterogeneity.

2.3.1 Role of the interaction matrix

The interaction matrix plays a crucial role in spatial econometric models from the identification, estimation, or interpretation perspectives. With respect to identification, in the context of model (2) and assuming exogenous explanatory variables, Bramoullé et al. (2009) show that the interaction scheme should include intransitive triads, i.e triads such as "peers of my peers are not my peers" to identify endogenous effects.[13] Developed for social networks, this seminal paper spells out the conditions originally developed by Kelejian & Prucha (1998, 1999) in the context of SAR models. Turning to estimation, the (quasi-) maximum likelihood approach requires a correct definition of the functional form and of the interaction matrix to provide unbiased estimators. Finally, the interpretation of the model is mainly based on marginal effects, which explicitly depend on $\mathbf{W}$. As such, papers have studied the consequences of a misspecified interaction matrix (Florax & Rey 1995, Paez et al. 2008, Vande Kamp 2019). Further, in the context of non-stochastic exogenous connectivity matrices, several statistical approaches have been developed to help the researcher to use relevant connectivity schemes. Kelejian (2008) has initiated a series of papers that use the J test of Davidson & MacKinnon (1981) to select the most relevant interaction scheme, among a finite number of candidates. Also, Jin & Lee (2013) develop Cox-type tests to choose between two competing interaction matrices, while Bayesian model averaging procedures have also been derived (LeSage & Pace 2009, Zhang & Yu 2018). In addition, models that simultaneously introduce several interaction matrices in the (SAR) model have been developed (see among others Lee & Liu 2010, Badinger & Egger 2011, Han et al. 2017, Hazir et al. 2018, Debarsy & LeSage 2022).

Nevertheless, the large majority of papers in applied spatial econometrics specify the interaction scheme between observations in terms of geographic proximity. For instance, they consider k nearest neighbors, contiguity or other decreasing functions of distance (distance threshold, inverse distance, etc.). They further include a robustness section with respect to the choice of the interaction matrix, typically considering alternative functional forms of distance or different numbers of neighbors.[14]

[12] Gotway & Young (2002), Wakefield & Salway (2001), among others, provides a complete overview of the various statistical issues related to these topics.

[13] We assume here that individuals do not interact in groups. For the latter case, Lee (2007) shows that as soon as the size of the group changes, the identification of endogenous effects is warranted.

[14] LeSage & Pace (2014) argue that this fine-tuning of the connectivity matrix is unnecessary and constitutes the biggest myth in spatial econometrics.

The first invoked reason to rely on a geographically-based connectivity matrix is its exogeneity (and non-stochasticity). This assumption allows to greatly simplify identification and estimation, but the cost of this exogeneity is nevertheless very high. Indeed, it generally prevents the modeling of relationships between aggregate units from socio-economic indicators, which generally lies at the heart of interactions. Furthermore, it prevents one from accounting for changes in the connectivity structure following an (exogenous) shock in some of the determinants.[15]

Alternatively, Corrado & Fingleton (2012) advocate the use of economically-based interaction matrices, as was already done in Case et al. (1993). This might complicate the estimation strategy as the exogeneity is not guaranteed, but several methods have been developed to account for potentially endogenous interaction schemes. Kelejian & Piras (2014) propose an IV procedure that directly instruments the elements of $\mathbf{W}$, in the context of panel data models. This procedure has been applied by Agrawal et al. (2020) who use syndicate membership as instruments for endogenous inter-municipal cooperation between jurisdictions. Qu & Lee (2015) assume that the endogeneity in $\mathbf{W}$ originates from the variables used for its construction. This methodology has been extended to dyadic data (Qu et al. 2021), to panel data models (Qu et al. 2017*a*, Shi & Lee 2018) and to endogenous social networks (Johnsson & Moon 2021). In the context of social networks, Jochmans (2023) develops an IV estimator that exploits restrictions in $\mathbf{W}$ to construct instruments from leave-own-out networks.

The second reason which motivates the use of geographically-based connectivity matrices is that it may act as a proxy of many economic phenomena (mobility of firms or consumers, transport costs, traded goods, capital movements, etc.). Following Neumayer & Plumper (2016), we argue that $\mathbf{W}$ must capture the *causal* mechanism of spillovers and thus reflect connectivity. In other words, $\mathbf{W}$ should define the transmission channel through which interactions occur.[16] Using geographic proximity as a proxy for connectivity thus prevents drawing sound conclusions with respect to interactions for at least two reasons. Firstly, it says nothing with respect to the causal mechanism of spatial dependence and thus cannot help distinguish alternative theories justifying cross-sectional dependence. Secondly, a-theoretical geographical proximity is at best a mismeasurement of connectivity, leading to misspecification problems discussed above, and at worst, completely unrelated to the true channels driving interactions, leading to unreliable conclusions with respect to spatial dependence.

However, in the context of microeconomic models, geographical space may play an important role in understanding spillovers between units. We already mentioned the work of Schone et al. (2013) on local growth control decisions, who show that closer (in a spatial sense) cities interact more than cities located

[15] Boucher & Fortin (2016) develop and discuss a three-case categorization of the connectivity matrices depending on their exogeneity and randomness.

[16] Neumayer & Plumper (2016) discuss crucial specification choices of the connectivity matrix, which are most of the time overlooked in the applied spatial econometrics literature. In our paper, we focus on the geographical nature of proximity, but refer the reader to Neumayer & Plumper (2016) for further details.

far away from each other. Glaeser et al. (1996) explain the high variance of crime rate across cities using a model where agents' propensity to engage in criminal activities partly depends on the propensity of other agents in the neighborhood. By contrast, in the context of network effects in education, Del Bello et al. (2015) examine the relative importance of spatial proximity versus peer effects in education outcomes. They show that once a precise definition of spatial neighbors is used, and when one accounts for neighbors' choices, there is no evidence of spatial proximity effects on education. Furthermore, they show that peer effects are important in education and operate mainly in schools. Finally, Kim et al. (2020) develop a new theory of social-tie formation where individuals care about the geographical location of other individuals, to account for the transport cost inherent to social interactions.[17]

This discussion clearly shows the consequences of the discrepancy between units of decision and units of observation. By specifying a model in which the origin and nature of interactions are clearly specified, we know at which aggregation level the model should be estimated and which connectivity matrix will accurately reflect the causal mechanism of spillovers. In contrast, estimating a model with spillovers on some (geographically) aggregated data not linked to decision units leads to a loss of modeling of relationships between these aggregate data, which prevents sound interpretations of the results.

The last point we make concerns the normalization of the connectivity matrix. In the vast majority of papers, the connectivity matrix is row-normalized, to interpret the spatial lag as the average value of the neighbors. The problem with this normalization is that it is not neutral and creates misspecifications if not derived from theory. The reason is that there is not a one-to-one correspondence between the row-normalized model and the original one due to a different normalizing factor for each row. In addition to this problem, row-normalization also changes the informational content of the connectivity matrix by converting absolute distances (geographic or not) into relative ones. For example, if the connectivity between two observations should represent transport costs, then it is the absolute distance that matters, not the relative one. On the contrary, focusing on juvenile delinquency, Patacchini & Zenou (2012) develop a model where they show that conformism to a social norm deters criminal activities, with conformity modeled through a row-normalized connectivity matrix. As such, without theoretical justification, the row-normalization should not be used. Kelejian & Prucha (2010) propose alternative matrix norms that do not alter the model specifications, and which consist in dividing all elements of the connectivity matrix by the same factor (the spectral radius, or the minimum between the maximum of the row and column sums).

To summarize this section, we plead for giving up the blind use of a-theoretical geographically-based connectivity matrices on the ground that it is exogenous, deterministic, and acts as a proxy for many channels. Geographically-based

[17]The interested reader may consult Topa & Zenou (2015) for an extensive literature review on the modeling of spatial spillover effects.

proximity, unless theoretically grounded such as in Schone et al. (2013), jeopardizes identification of the causal mechanism of spillovers and further prevents the underlying economic reasons leading observations to interact, as should ideally do structural models, which underpin spatial econometrics models. Furthermore, the row-normalization of the connectivity matrix should be avoided unless theoretical motivations, such as those in Liu et al. (2014) exist. As stated by Neumayer & Plumper (2016, p.177), "Specification choices [of $\mathbf{W}$] should follow theory rather than convention."

2.3.2 Proxy variables to model interactions

As seen above, the workhorse model for applied spatial econometrics is the SDM model as it accounts for omitted variables that are spatially autocorrelated, when all other threats to identification are addressed. In addition, a constrained version of the SDM, shown in (5) corresponds to the rewriting of the model with spatially correlated errors (SEM), presented in (4).

$$\begin{aligned}\mathbf{y} &= \mathbf{X}\boldsymbol{\beta} + (\mathbf{I}_n - \lambda\mathbf{W})^{-1}\mathbf{u}, \quad \mathbf{u} \sim iid(\mathbf{0}, \sigma^2\mathbf{I}_n) &(4)\\ &= \lambda\mathbf{W}\mathbf{y} + \mathbf{X}\boldsymbol{\beta} + \mathbf{W}\mathbf{X}\boldsymbol{\gamma} + \mathbf{u} &(5)\end{aligned}$$

where $\boldsymbol{\gamma} = -\lambda\boldsymbol{\beta}$. While the link between a constrained spatial Durbin model and a spatial error model is a well-known issue since Burridge (1981), it is fundamental to distinguish between a true SDM and a SEM as the *economic* implications are different. In the SDM (2), the behavior of an (statistical) unit is affected by its characteristics and the behavior and characteristics of its neighbors, leading to endogenous and exogenous spillovers between observed variables. In contrast, in the SEM, interactions between units come from unobserved variables, leading to completely different economic interpretations. As noted by Boucher & Fortin (2016), the researcher does not always observe the "true" variable of interest and use proxies. For example, in terms of obesity, papers use Body Mass Index (BMI) to proxy effort to reduce weight (see among others Christakis & Fowler 2007, O'Malley et al. 2014). Boucher & Fortin (2016) show that the apparent contagion effect of BMI may come from the unobserved effort the individual makes to reduce her weight, and which depends on her peers' effort. Hence, the derived economic model is similar to a SEM model. If the SDM is estimated instead, one might wrongly conclude to the presence of contagion effects of BMI.

In light of this discussion, the common factor statistic, derived by Burridge (1981), which tests whether the SDM model can be reduced to a SEM specification, should be a fundamental part of the spatial empirical toolbox.

2.3.3 Spatial heterogeneity

In the applied spatial econometrics literature, spatial heterogeneity has often been presented as a secondary econometric issue, with respect to spatial autocorrelation. For instance, Anselin (2001, p. 311) states that spatial heterogeneity is "simply structural instability, either in the form of non-constant variances in a regression model (heteroskedasticity) or in the form of variable regression coefficients. Most methodological issues related to spatial heterogeneity can be tackled by means of the standard econometric toolbox". Typically, discrete spatial heterogeneity can be handled with dummy variables reflecting the spatial regimes or spatial switching regressions, while continuous spatial heterogeneity might be tackled with the inclusion of coordinates. Although this may have somewhat evolved with the advent of geographically weighted regression or spatial semi-parametric or nonparametric approaches as a way to tackle flexible forms of spatial heterogeneity (see, among others McMillen 2010, Basile et al. 2014, Osland et al. 2016, Géniaux & Martinetti 2018), there is still a view that spatial heterogeneity does not involve as many conceptual complexities as spatial dependence. Yet, whenever looked at from the identification point of view, we contend that spatial heterogeneity is a major threat to identification that is generally overlooked, while conversely, the issue of unobserved heterogeneity is at the heart of the identification strategy in network econometrics.

First, it is well known that in a cross-section, there is observational equivalence between spatial heterogeneity and spatial dependence in cross-sectional settings (Anselin & Bera 1998). In other words, when looking at a map showing some clustering, it is impossible to know whether the underlying GDP is that of spatial regimes or one that includes spatial interactions. For instance, we might observe spatial clustering of firms and the underlying causes might be spatially varying characteristics of the locations in terms of access to population, amenities, etc. or because there are direct interactions between firms. This issue is also known in epidemiology as "true" versus "apparent" contagion (Messner & Anselin 2004). Related to this, numerous authors have pointed out that spatial interactions might be the result of unmodelled spatial heterogeneity under the form of spatially varying coefficients. For example, McMillen (2003) shows that spatial autocorrelation is also often the result of incorrect functional forms and spatially autocorrelated omitted variables in space. Overall, this means that a lack of proper handling of spatial heterogeneity will result in an under- or overestimation of the spatial lag coefficient, and possibly spurious spatial autocorrelation, and hence spurious assessment of spatial spillovers.

Second, common factors are frequent whenever spatial units are affected by unobserved shocks or events occurring at a higher spatial scale. While distinguishing between endogenous (direct spatial interactions) and exogenous (group-average) effects is already challenging, the possible occurrence of such unobserved common shocks adds additional complexity. If there are valid arguments that these unobservables factors are uncorrelated with the included control variables, robust inference can be applied. In this respect, we follow Conley & Molinari (2007) and Kelejian & Prucha (2007) whose approach is preferable to specifying a strong parametric form for the error terms. However, most of the time, these unobserved common factors are likely to be correlated with the determinants. Bai & Li (2021) develop a spatial panel model where unobserved heterogeneity is modeled with interactive fixed effects rather than with additive fixed effects, to account for more flexible unobserved patterns. In the context of tax competition, Chirinko & Wilson (2017) estimate the factor model of Pesaran (2006) and found that heterogeneous responses to common shocks, as well as delayed reaction, are crucial for understanding spillovers between US States. These authors show that States' tax rate follow a "ride on a seesaw" rather than a race to the bottom, as usually found in the literature.

Third, the impact of spatial sorting is not always considered. Spatial sorting refers to the situation where heterogeneous individuals self-select themselves into various locations following observed but also unobserved characteristics. When the analysis refers to units of decisions, spatial sorting directly affects the structure of $\mathbf{W}$ which may be endogenous (see above). Yet, spatial sorting might also be an issue regarding the control variables, which then correlate with the error terms, whether the observations are units of decision or for aggregated spatial data. If individual unobserved characteristics are correlated with both the outcome and location, then spatial fixed effects (spatial differencing) is of little help to restore identification. For instance, when the control reflects characteristics of the population (structure by age, professional category, etc.) that are directly linked to the location decisions and hence to the resulting spatial sorting, they will be endogenous. The network econometrics literature has largely approached this issue by specifying a structural model of network formation in addition to the outcome model. Otherwise, one might think to use instrumental variables. A large range of papers have in effect considered the estimation of spatial models with endogeneity coming from both the endogenous spatial lag and additional endogenous variables. These papers are reviewed extensively in Le Gallo & Fingleton (2019) for both cross-sectional and panel data models and for single-equation and multi-equation models. They essentially consist in using versions of instrumental variables/GMM with an extended set of instrumental variables, where, in addition to the traditional powers of spatial lags of the explanatory variables to instrument for the endogenous spatial lags, other instruments are added. Discussions are then about the asymptotic properties of the IV/GMM estimators. Another route, and if one is not willing to put even more structure in the model and at the same time, prevent identification and interpretation complications whenever several endogenous variables are present is to avoid the inclusion of bad controls altogether.

2.4 Structural identification strategies

From an identification perspective, understanding the causes of non-random spatial distribution of observations necessitates discriminating between all these various possibilities (endogenous effects, exogenous effects, spatial heterogeneity, common shocks, spatial sorting) while mitigating the impact of omitted variables. The identification strategies developed in the literature on peer effects may constitute food for thought for applied spatial interactions. Gibbons et al. (2015) and Bramoullé et al. (2020) discuss several identification strategies, based on exogenous sources of variations (random peers, random shocks), nature of data (panel), or structural models (network formation or modeling of structured omitted variables). A substantive part of identification strategies developed in the peer effects literature have been derived to solve the complexities induced by endogenous modeling of the interaction scheme, constructed from socioeconomic indicators (interpersonal links, for instance). Nevertheless, some problems related to the definition of the interaction matrix remain. For example, partial network knowledge only has started to receive some attention (see Boucher & Houndetoungan 2023, Lewbel et al. 2023, for further details), while time-varying networks are only at their early stages, a topic for which methods developed in the already extensive theoretical spatial econometric literature might be useful (see, among others Yu et al. 2008, Kelejian & Piras 2014, Qu et al. 2016, 2017*b*, Shi & Lee 2017).

3 Reduced form approach to spillovers

We now turn to reduced-form approaches where the parameter of interest is a treatment effect. This experimentalist paradigm with randomized controlled trials as the gold standard[18] is the core of most applied econometrics. Although this causal inference literature avoids behavioral assumptions, one of the founding blocks of all its methods nevertheless is a behavioral nature: the Stable Unit Treatment Value Assumption (SUTVA) (see Rubin 1974). When imposed, this assumption prevents the presence of general equilibrium effects, social (spatial) interactions, spillovers, or dynamic behavior. In this context, spillovers, named interference in the impact evaluation literature, have been first considered a nuisance that confounds the identification of the parameter of interest. However, in biostatistics and epidemiology, a growing, but very active, and rapid literature starts to deal with this limit by deriving estimators that allow interference, which is considered as a way to enrich the identification of the parameter of interest.[19] This section discusses all these issues together with the way the specific characteristics of spatial data have been considered so far.

3.1 The canonical impact evaluation model

The canonical impact evaluation model of Rubin (1974) starts with the effect of a binary treatment T, $T_i \in \{0, 1\}$, which refers to the action that applies to the units, such as a public policy intervention.[20] Each observation has two potential outcomes: y_{0i} is the outcome that would be observed for unit i in the absence of intervention, $T_i = 0$, and y_{1i} is the outcome that would be observed in its presence, $T_i = 1$. The causal impact of the intervention for unit i is $\Delta_i = y_{1i} - y_{0i}, \forall i = 1, ...N$. For each unit, only one of these two outcomes is observed, the other being the counterfactual. The fundamental problem for estimating Δ_i is then a missing observation problem (Holland 1986).

At the population level, the literature focuses mainly on the Average Treatment Effect (ATE), $E(y_{1i} - y_{0i})$, and the Average Treatment Effect on the

[18]We do not provide an extensive literature review of the potential outcomes approach. The interested reader may consult, among others, Imbens & Rubin (2015), Abadie & Cattaneo (2018), Fougère & Jacquemet (2021) for surveys.

[19]See for instance VanderWeele, Ogburn & Tchetgen Tchetgen (2012), VanderWeele & An (2013), Ogburn et al. (2020), Reich et al. (2021).

[20]We only consider binary treatments.

Treated (ATT), $E(y_{1i} - y_{0i}|T_i = 1)$. At the subgroup level, for values of the pretreatment variables, X, the Conditional Average Treatment Effect (CATE) is also defined as $E(y_{1i}|X = x) - E(y_{0i}|X = x)$ to account for a varying treatment effect along the various strata of X. Importantly, the variables in X should be predetermined relative to the treatment, in the sense that the values of X cannot be changed by active manipulation of the treatment T, such as the characteristics of the units measured before T is known. We will come back later to this important issue.

According to Heckman & Vytlacil (2007), the definition of a causal effect should precede the stages of identification and estimation. A causal effect may be defined for an individual, a subpopulation, or the entire population and always involves one or more counterfactuals. Then, a causal effect defined for a subpopulation or the population is identified only if the counterfactual quantities can be equated in some way with observable population data without introducing selection bias. Identification of this causal effect depends on the assignment mechanism in the treatment. For example, in a randomized controlled experiment, the "prima facie causal effect" (Holland 1986), $E(Y|T = 1) - E(Y|T = 0)$ equals ATE. For observational data, it is generally not equal to a population average causal effect, and identification assumptions must be made:

Assumption 1 (Stable Unit Treatment Value Assumption, SUTVA) *The potential outcomes for any unit do not vary with the treatment assigned to other units, and, for each unit, there are no different forms or versions of each treatment level, leading to different potential outcomes.*

Assumption 2 (Ignorability) *Given X, treatment assignment T is independent to the potential outcomes: $T \perp (y_{0i}, y_{1i})|X$*

Assumption 3 (Positivity) *For any value of X, treatment assignment is not deterministic: $0 < P(T = t|X = x) < 1, \forall t, x$*

Assumption 1 first assumes the absence of interactions between units.[21] It also states that a single version exists for each treatment. Spatial analysis is mainly concerned with the first point as it involves the absence of spatial spillovers.[22] Assumption 2 holds in observational studies when there are no confounding variables that bring dependence between Y and T. Conversely, lack of independence between the treatment and the potential outcomes is referred to as confounding, which can occur due to the self-selection of agents in treatment based on their potential outcomes or when information correlated with potential outcomes is used for treatment assignment, such as place-based policies targeting specific areas.[23] Then omitted variables or spatial heterogeneity are also major threats to identification in this framework.

[21] Manski (2013) calls this first condition the individualistic treatment response assumption.

[22] To the best of our knowledge, the second point covered by SUTVA hasn't yet been studied in spatial applications.

[23] Assumptions 2 and 3 together are called *Strong Ignorability* or *Strong Unconfoundedness.*

3.2 Interference

SUTVA includes within its definition a no-interference assumption. Interference, the term used in the biostatistical literature rather than spillovers, "is said to be present when exposure or treatment received by one individual may affect the outcomes of other individuals" (Tchetgen & VanderWeele 2012). Initially, it was merely considered as a threat to the identification of the main causal estimands with papers assessing the bias of causal effects estimated under SUTVA (Sobel 2006) and proposing designs of experiments that avoid interference or adjusting inference (Rosenbaum 2007, Athey et al. 2018). Then, the focus has progressively switched to a substantive identification of interference, both in terms of estimands of interests and design of experiments.

The literature has first remained agnostic on the source of interference and has proceeded by extending the standard estimands presented in section 3.1 to four key estimands, in the context of a design-based approach at the unit level belonging to some groups. By defining $\mathbf{a}_j = (a_{ji}, \mathbf{a}_{j(-i)})$ the treatment program for all individuals in group j, composed of the treatment for individual i and all other individuals $\mathbf{a}_{j(-i)}$, and $y_{ji}(a_i, \mathbf{a}_{j(-i)})$ as the potential outcome for individual i in group j, subject to treatment $a_{ji} = 0, 1$, and the treatment program for all other individuals established at $\mathbf{a}_{j(-i)}$, Hudgens & Halloran (2008) define:

1. **Direct effect**: $DE_i(\mathbf{a}_{j(-i)}) = y_{ji}(a_{ji} = 1, \mathbf{a}_{j(-i)}) - y_{ji}(a_{ji} = 0, \mathbf{a}_{j(-i)})$, ie. the difference between the potential outcome for individual i given treatment compared to the potential outcome for that individual without treatment, all other things being equal (including the treatment status of the others individuals.

2. **Indirect effect**: $IE_i(\mathbf{a}_{j(-i)}, \mathbf{a}'_{j(-i)}) = y_{ji}(a_{ji} = 0, \mathbf{a}_{j(-i)}) - y_{ji}(a'_{ji} = 0, \mathbf{a}'_{j(-i)})$. This estimand is also called the spillover or peer influence effects as it compares untreated subject i under scenarios where other subjects receive $\mathbf{a}_{j(-i)}$ versus $\mathbf{a}'_{j(-i)}$. It equals 0 if there is no interference. The indirect effect may also be computed for treated individuals ($a_{ji} = 1$)

3. **Total effect**: $TE_i(\mathbf{a}_{j(-i)}, \mathbf{a}'_{j(-i)}) = y_{ji}(a_{ji} = 1, \mathbf{a}_{j(-i)}) - y_{ji}(a'_{ji} = 0, \mathbf{a}'_{j(-i)})$. This estimand reflects the difference in responses that would be seen in i, between the scenarios in which she is treated and the others receive treatment program $\mathbf{a}_{j(-i)}$ and i is not treated and the others receive another treatment program $\mathbf{a}'_{j(-i)}$. Note that total causal effects are not commutative, in general. Lastly, we have $TE_i(\mathbf{a}_{j(-i)}, \mathbf{a}'_{j(-i)}) = DE_i(\mathbf{a}'_{j(-i)}) + IE_i(\mathbf{a}_{j(-i)}, \mathbf{a}'_{j(-i)})$.

4. **Overall effect**: $OE_i(\mathbf{a}_j, \mathbf{a}'_j) = y_{ji}(\mathbf{a}_j) - y_{ji}(\mathbf{a}'_j)$. This effect looks at the overall difference in potential outcomes for unit i between two alternative population treatment programs $\mathbf{a}_j$ and $\mathbf{a}'_j$. This is similar to TE_i, but OE_i allows for individual treatment to be determined by T (whereas TE_i always includes $a_{ji} = 1$, $a'_{ji} = 0$). Once these effects are considered over

more general treatment scenarios, OE_i will correspond to the effect as "averaged over a treatment policy".

These effects are then averaged (Hudgens & Halloran 2008). Allowing for interference makes causal inference challenging, so that in practice researchers usually impose an underlying structure limiting its scope. The first and still the most common relaxation of the no interference assumption is that of *partial interference* or *clustered interference* (Sobel 2006). In this case, it is assumed that individuals can be partitioned into distinct groups, such that interference can occur between individuals in the same group, but there is no interference between individuals in different groups. This assumption should approximately hold if individuals are clustered in space, time, or some other fashion. Second, a growing literature tackles the case where interference arises on a *network*, with observations interfering with others along connected edges. This implies that assumptions about interference are driven by the structure of the network. Most papers studying network interference assumes that the network is known and given *a priori*, as in the applied spatial econometrics literature. Finally, *general interference* has been considered, in which no explicit assumption on interference is made. Such a setting presents major difficulties and, therefore, has rarely been used.

A popular approach to formalize causal inference under interference has been introduced by Halloran & Struchiner (1995) and Aronow et al. (2017) with the concept of *exposure mapping* that summarizes the impacts from other individuals' treatments into sufficient statistics. A mapping is specified that relates the vector of treatment assignments for the experimental units to a finite set of exposures that can be assigned to them. Causal effects, called 'exposure effects', can then be defined in terms of comparisons of outcomes under different exposures. This framework is flexible because one can use any form of mapping to characterize the interference structure. The link to the connectivity matrix is obvious and, likewise, for the obtained spillover effects to be valid, the specification of the exposure mapping must be justified. If the chosen specification is inappropriate, the resulting causal inference may be misleading (e.g., failure to detect treatment spillovers) with imprecise variance estimation. Sävje et al. (2021), Sävje (2023) are the first to study these questions and provide assumptions under which these problems can be mitigated.

3.3 Causal inference with spatial interference

We now describe the main strategies used when the SUTVA assumption is not verified, focusing our exposition on observational spatial data.[24]

[24]There is an extensive literature on causal inference under interference in experiments with design strategies incorporating network information and controlling treatment assignments to mitigate interference (see among many others Hudgens & Halloran 2008, Liu & Hudgens 2014, Basse & Airoldi 2018, Aronow, Eckles, Samii & Zonszein 2021, for literature reviews). The specific case of spatial experiments with interference is reviewed in Aronow, Samii & Wang (2021), Samii et al. (2023). In particular, they define a quantity called an average marginalized response, which measures how on average outcomes that are a given distance

3.3.1 Spatial interference under unconfoundedness

In the spatial context, Cerulli (2017) makes a first proposal to account for interference under unconfoundedness. His idea is to specify the potential outcome y_{i0} as a parametric function of the potential outcomes y_{j1} of the neighboring observations. His model is as follows:

$$\begin{aligned} y_{0i} &= \mu_0 + \mathbf{x}_i \boldsymbol{\beta}_0 + \gamma \sum_{j=1}^{n_1} w_{ij} y_{1j} + \varepsilon_{0i} \\ y_{1i} &= \mu_1 + \mathbf{x}_i \boldsymbol{\beta}_1 + \varepsilon_{1i} \end{aligned} \tag{6}$$

where μ_0 and μ_1 are scalars, $\boldsymbol{\beta}_0$ and $\boldsymbol{\beta}_1$ are two unknown vectors defining the responses of unit i to the vector of covariates, ε_0 and ε_1 are idiosyncratic error terms, w_{ij} are the elements of a row standardized interaction matrix and n_1 is the number of treated units. He then shows that substituting Eq.(6) into $y_i = y_{0i} + T_i(y_{1i} - y_{0i})$ and assuming that the expectation is conditionally unconfounded ($\mathbb{E}(y_{gi}|T_i, x_i) = \mathbb{E}(y_{gi}|x_i)$ with $g = 0, 1$), yields a consistent OLS estimator of the ATE when y_i is regressed on $(1, w_i, \mathbf{x}_i, w_i(\mathbf{x}_i - \bar{\mathbf{x}}), \mathbf{z}_i)$ with $\mathbf{z}_i = \mathbf{v}_i + w_i(\bar{\mathbf{v}} - \mathbf{v}_i)$ and $\mathbf{v}_i = \sum_{j=1}^{n_1} w_{ij}\mathbf{x}_j$. However, this parametric approach is not the most commonly used in the literature, which rather prefers to rely on the propensity score approach.

The propensity score, $e(x) = Pr(T = 1|X = x)$ is the conditional probability of exposure, given X, where the overlap condition, i.e. $0 < e(x) < 1$ is assumed to hold. $e(x)$ acts as a balancing score when both overlap and unconfoundedness hold (Rosenbaum & Rubin 1983). Propensity scores are typically estimated using a logistic or a probit regression model. Then, to estimate the ATE, various strategies can be used, such as inverse-probability weighted (IPW) estimator, doubly robust estimators, or matching procedures.

Spatially explicit versions of propensity score-based procedures have been developed to deal with spatial confounding, i.e. unobserved spatially structured variables affecting both the outcome and the treatment. Davis et al. (2019) include a conditional autoregressive prior for the random terms of the propensity score model and the outcome models. They show that the inclusion of spatial random effects gives lower bias and lower RMSE. Another route has been taken by Papadogeorgou et al. (2019) who propose to combine the similarity of propensity scores with spatial similarity to perform the matching between the treated and control units to estimate a distance-adjusted propensity score.

With respect to spatial interactions, some rather *ad hoc* proposals exist. For example, in order to estimate the propensity scores in the first stage, Chagas et al. (2012) use a probit model including both a spatial lag term and a spatial error term that they estimate using Monte Carlo Markov Chains. This raises an important issue as the spatial lag term $\mathbf{Wy}$ cannot be considered as a predetermined variable. Indeed, if the outcome $\mathbf{y}$ is determined by the treatment vector

from an intervention site are affected by activating treatment at that site, taking into account ambient effects emanating from other intervention sites. See also Pollmann (2023).

$\mathbf{T}$, then so will be $\mathbf{Wy}$. As noted in Gibbons et al. (2015, p.133), "the endogenous spatial lag, which is an aggregation of the dependent variable, cannot be directly, exogenously manipulated within the population of sample to which the model related. Further, it cannot be changed holding other factors (determinants) constants.". Alternatively, Zigler et al. (2012), in considering the impact of environmental regulations on air quality measures observed at spatial locations throughout the US, includes the spatial information by modeling the two potential outcomes as a function of treatments and a spatially-varying Gaussian process. Giffin et al. (2023) include spatial spillovers in treatment using a Bayesian-spline-based regression model in the first step.

In the statistical/biostatistical literature dealing with interference, the first identification and estimation methods were derived under the assumption of partial interference. Hong & Raudenbush (2006) and Verbitsky-Savitz & Raudenbush (2012) mimic a two-stage experiment and base their identification on a generalization of the standard conditional randomization assumption made at the individual level, which they call a spatial ignorability assumption, and then use a multilevel propensity score stratification that allows for partial interference. Finally, we note that a growing statistical literature derives identification and estimation methods under unconfoundedness when interference occurs on a known network (Liu et al. 2016, Aronow & Samii 2017, Forastiere et al. 2021). Typically, these papers start by defining an exposure mapping function (or equivalent), which specifies how the treatment is propagated to immediate neighbors and then gives propensity-weighted estimates. Spatial extensions of these proposals still need to be developed.

3.3.2 Spatial interference with confounding

A confounding variable is an unobserved variable that influences both the hypothesized treatment and the hypothesized outcome. When not carefully addressed, it can generate an association between treatment and outcome that can be falsely considered as evidence of a causal effect of the former on the latter. Examples include common spatial shocks that affect neighboring observations. When the ignorability assumption cannot be maintained, several identification strategies are possible with various identification assumptions and/or relying on the specific design of the data.

We start with the difference-in-differences (DiD) model, which relies on the availability of data on at least two periods (before and after treatment) for both control and treated groups, and the parallel trend assumption. Formally, suppose that there is a binary treatment and that observations $i = 1, ..., n$ are available at two time periods. The treatment occurs in one group during the second period, and then the standard DiD equation reads as:

$$y_{it} = \alpha_0 + \alpha_1 X_{it} + \alpha_2 D_{it} + \alpha_3 \widetilde{T}_{it} + \alpha_4 D_{it} \widetilde{T}_{it} + \varepsilon_{it} \tag{7}$$

where D_{it} is a dummy indicating treatment status for unit i at time t, and $\widetilde{T}_{it}$ is a dummy indicating period. As is well known, under the assumption of parallel trend and assuming that ε_{it} is an *iid* error term, uncorrelated with D_{it} and $\widetilde{T}_{it}$, the ATE is equal to α_4, so that evidence of a causal effect is present when $H_0 : \alpha_4 = 0$ is rejected.

Several papers have estimated a "spatial" version of the DiD model by including a spatial lag term in this equation and/or a spatial autoregressive process in the error term.[25] The use of an endogenous spatial lag Wy in difference-in-differences models raises more serious issues, similar to our previous discussion of the papers including such a spatial lag in the first step of a propensity score matching procedure. Because of simultaneity, a spatial lag term cannot be considered as a pre-treatment variable. Moreover, the presence of a spatial lag implies that the remaining coefficients cannot be considered marginal effects anymore, and consequently, the parameter α_4 cannot be interpreted as the ATE. At best, average direct/indirect/total effects can be calculated (see LeSage & Pace 2009), but it remains to be demonstrated that the average direct effects (in the spatial econometrics meaning) of the treatment could be interpreted as the ATE. Other proposals have taken a more rigorous approach to identification in DiD models on spatial data.

Delgado & Florax (2015) consider the case where causal spatial spillover effects where the potential outcome of an individual depends on her own treatment, as well as on the treatment status of proximate neighbors. They define $\mathbf{W}_s$ as the (nT, nT) block-diagonal row-standardized interaction matrix containing non-zero elements for neighboring spatial units. This entails the presence of the term $(I + \rho\mathbf{W}_s)\mathbf{D} \circ \widetilde{\mathbf{T}}$ in equation (8), where $\circ$ is the element-by-element multiplication, or Hadamard product, giving the model with spatial interaction in the responses in matrix form:

$$
\begin{aligned}
y &= \alpha_0 + \alpha_1\mathbf{X} + \alpha_2\mathbf{D} + \alpha_3\widetilde{\mathbf{T}} + \alpha_4(\mathbf{I} + \rho\mathbf{W}_s)\mathbf{D} \circ \widetilde{\mathbf{T}} + \varepsilon \qquad (8)\\
&= \alpha_0 + \alpha_1\mathbf{X} + \alpha_2\mathbf{D} + \alpha_3\widetilde{\mathbf{T}} + \alpha_4\mathbf{D} \circ \widetilde{\mathbf{T}} + \alpha_5\mathbf{W}_s\mathbf{D} \circ \widetilde{\mathbf{T}} + \varepsilon \qquad (9)
\end{aligned}
$$

with $\alpha_5 = \rho\alpha_4$ in equation (9), leading to the average treatment effect $ATE = \alpha_4(1 + \rho\overline{\mathbf{WD}})$, where $\overline{\mathbf{WD}}$ is the average proportion of treated neighbors. Consequently, ATE is a function of the magnitude of the direct effect of treatment α_4 and an indirect effect $\alpha_5\overline{\mathbf{WD}}$.[26] This method has been extended to the multivariate case by Bardaka et al. (2019) while Butts (2021*a*) proceeds with the notion of exposure mapping, hence not relying on a linearity assumption of spillovers, and allowing for both local spillovers onto control units and onto treated units. The case of spatially-targeted treatment is investigated in Butts (2023) with the estimation of a treatment effect curve. Huber & Steinmayr (2019) start with a partial interference assumption, where SUTVA is verified at an aggregate level, allowing the identification of an individual effect and a

[25]See among others.Sunak & Madlener (2016), Diao et al. (2017), Dubé et al. (2017), Kaneko et al. (2019), Xu & Liu (2021), Zeng & Bao (2021).

[26]Empirical applications of this approach in empirical spatial econometric papers can be found in Chagas et al. (2016), Han et al. (2018), Feng et al. (2021), Kosfeld et al. (2021), Madeira Triaca et al. (2021), Yu (2021).

within-aggregate spillover effect driven by the treatment of other individuals in the group. As Roth et al. (2022), we expect more development on DiD models with spillovers in the coming years.

Another strategy relying on a specific design of the data is that of the regression discontinuity design. These designs are based on the existence of a known cut-off and are therefore useful in the analysis of public policy characterized by discrete policy-led interventions above or below certain levels. The usual scenario is that all individuals or units receive a score and the treatment is offered to the individuals whose score is above (below) the threshold. If the unit's characteristics do not change abruptly at the cutoff point, then the causal effect of the treatment can be uncovered by the change in treatment outcome as determined by the treatment assignment rule. Of particular interest in a spatial context are RDDs where the thresholds are geographic borders, a case that the literature refers to as a geographic RDD (Keele & Titiunik 2015, 2018, Butts 2021*b*). In this case, the geographic border is considered as a natural experiment. One side of the border is treated (for example, a regional subsidy or other form of policy intervention), while the other side doesn't receive the treatment. However, an obvious issue in these designs is spatial spillovers within units on each side of the border together with spatial spillovers operating across the border, which do not allow us to interpret the usual estimates of RDD as ATEs. Some papers have tackled this issue. For instance, Aronow et al. (2017) show that allowing for interference of arbitrary and unknown nature under a local randomization assumption, the causal effect consisting of the difference in means applied to subjects near the boundary can be interpreted as the Hudgens & Halloran (2008)'s average direct effects for this subpopulation of subjects. In the context of voter turnout during a presidential campaign, Keele & Titiunik (2018) assume, similarly to Verbitsky-Savitz & Raudenbush (2012), that interference occurs only when treated individuals are in close geographic proximity to a sufficiently high number of control individuals. This allows them to define and identify non-parametrically estimands for the direct and indirect effects. Cornwall & Sauley (2021) also consider the question of spatial spillovers in regression discontinuity design. Their approach consists in residualizing the outcome by applying a spatial Durbin model and then proceeding with the RDD usual estimates. The causal estimands at hand are unfortunately not defined clearly and as we mentioned above, using parametric specifications with a spatial lag in a reduced-form context raises important identification issues.

3.3.3 Sources of interferences

An emerging literature in biostatistics focuses on sources of interference. According to Ogburn & VanderWeele (2014), there are three specific mechanisms, not mutually exclusive, that might cause interference. In the case of *direct interference*, the treatment T_i of individual i directly impacts the outcome Y_j of individual j, regardless of the value of Y_i. *Contagion interference* occurs when there is dependence between the outcomes Y_i and Y_j, and therefore treatment T_i can impact Y_j through its relationship to Y_i. Peer effects might be an example. Finally, *allocation interference* refers to the assignment of subjects to groups and Y_i can be impacted by the characteristics of the other subjects in that group. Several mechanisms might be present simultaneously.

This classification is close to the typology of Manski (1993), of, respectively, contextual effects, endogenous effects, and correlated effects. Disentangling the sources of interference requires more structured approaches relying on models and identification issues that are close to the ones detailed in section 3, including the need to control for confounding and homophily (Tchetgen & VanderWeele 2012, VanderWeele, Tchetgen & Halloran 2012, Ogburn & VanderWeele 2017, Liu & Tchetgen Tchetgen 2021, Ogburn & Shpitser 2021). In the tax competition literature, an example of such an approach is Agrawal (2016) who shows how spillovers drive the differences in equilibrium tax rates between neighboring jurisdictions and then uses these differences to identify tax competition.

4 Recommendations for applied spatial econometrics

In this section, we propose some recommendations for good practice in applied spatial econometrics, based on our discussion on identification in spatial structural and reduced forms. These recommendations include a clear discussion on the parameters of interest, the internal logic of using a structural versus a reduced approach, the unit of decision, and, related to this, the choice of interaction, the issue of spatial heterogeneity and spatial confounding, and the nature of inference.

Parameters of interest. The first recommendation is related to the need to clearly define the parameters of interest, i.e. those for which the researcher wishes to provide a causal interpretation. These parameters might be spatial spillovers (endogenous and/or contextual spillovers) or they might be other coefficients related to the impact of some variable on another variable. As we saw in section 2, both are of interest when the analysis of spatial spillovers enriches the impact analysis. While this recommendation might seem trivial or unnecessary, one has to admit that many applied spatial econometric papers are not explicit enough, especially when the identification strategy is blurred with spatial spillovers solely obtained as a side-product of a data-driven chosen specification. When spatial spillovers are the main parameters of interest, a structural approach with special care devoted to the identification conditions and adequate considerations of the spatial threats discussed in section 2.3 is appropriate. Otherwise, a reduced-form approach (where identification and interference are also carefully discussed) might be considered.

Approaches. The necessity of a clear discussion of the parameters of interest leads to our second recommendation, which is to keep in mind the specific features of identification in each of the structural and reduced-form approaches.

On the one hand, in structural models, the mechanisms of underlying interactions should ideally be set up. For instance, in the case of fiscal federalism, one can rely on the various models of local government policy interactions: fiscal competition, bidding for firms, yardstick competition, expenditure spillovers and Tiebout sorting (Agrawal et al. 2022). A purely structural approach is not without difficulties, as these various mechanisms yield the same reduced reaction function. Likewise, in the peer effects literature, it is only very recently that a structural model managed to distinguish between the behavioral sources of interactions (conformism or spillovers) within a linear-in-means model(see Boucher et al. 2024). On the other hand, in causal reduced forms, an important first step consists in precisely defining the causal estimands of interest. As we point out above, some papers introduce a spatial lag term of the exogenous or the endogenous variables *ex-post* in PSM, DiD or RDD designs without defining the causal estimands at hand, making it unclear what is really identified. Moreover, an endogenous spatial lag term ($\mathbf{Wy}$) cannot be included in a causal reduced form model, as it cannot be considered as a pretreatment variable in the sense that it is impossible to exogeneously manipulate it while keeping all other variables constant. This issue can be related to the problem of bad controls, that is, the introduction of control variables that can be affected by the treatment (see for instance Zeldow & Hatfield 2021). The inclusion of neighborhood characteristics variables ($\mathbf{WX}$) might be relevant to mitigate spatial mismatch issues, yet, if the individual characteristics are bad controls, as it is often the case when spatial sorting is present for instance, including the neighborhood characteristics in the model might worsen the problem. If one thinks that spatial spillovers might be an issue in an impact evaluation, the contributions that view them as interferences (see section 3.2) are more relevant.

Linked to this recommendation, we point out two major issues related to the current practice in applied spatial econometrics, which heavily relies on the spatial Durbin model (SDM). First, as mentioned above, the SDM model is widely used in applied spatial econometrics based on the argument that in the presence of spatially autocorrelated omitted variables, this model provides consistent estimates of parameters (LeSage & Pace 2009). However, it should now be apparent that this strategy is only valid if all other threats to identification are adequately considered, which is usually not the case. Furthermore, it assumes that the exogenous variables ($\mathbf{X}$) and their spatial lags ($\mathbf{WX}$) are plausibly exogenous, a major aspect that is rarely discussed. In other words, the standard spatial Durbin model is an adequate answer to identification in very restrictive cases only, characterized by assumptions that are rarely, if ever, met when working with observational data. More fundamentally, as the presence of an endogenous spatial lag term introduces many complexities (in terms of identification, interpretation, and estimation), which is not appropriate in a purely reduced-form model, the spatial Durbin model should ideally be rooted in an underlying theory, as is done in the social network literature for instance.

Second and related to this, the impacts (total, direct, indirect impacts, developed by LeSage & Pace 2009) traditionally computed in SAR or SDM specifications cannot be considered as impacts in the causal sense without having addressed all possible threats to identification. Otherwise, they should only be viewed as marginal changes without causal interpretation.

For all the reasons set above, when the main parameter of interest is not some spillover effect (or if the theory does not clearly state that structural spillovers should be accounted for), reduced forms with appropriate treatment of interference might be more adequate.

Unit of decision vs unit of observation. Once the approach is chosen and the identification strategy adopted, related specifications will have to be estimated. We set forth to precisely consider the implications of the potential discrepancy between the unit of observation of the analysis and what we call the unit of decision. Most of the time, the literature in applied regional science considers aggregate spatial units (municipalities, counties, regions, countries) to perform the quantitative analysis, but without questioning its adequacy with respect the the decision making process. For instance, using regional data to study policies that are set up at a different geographic scale is not particularly relevant. In relation to these, we raise a more fundamental issue related to identification. Indeed, many papers estimate the SAR or the SDM model using aggregate spatial units, such as counties, municipalities, etc. in contexts where these spatial units are not units of decisions; sometimes, they may be only aggregations of units of decision. Following our discussion on structural models, we state that when the unit of observation is not a unit of decision, a specification including an endogenous spatial lag (as in SAR or SDM models) is mostly not relevant, unless there are strong theoretical foundations that imply interdependence at equilibrium (for instance, microfounded gravity models). Otherwise, as discussed above, the spatial lag coefficient should be linked to behavioral assumptions with stringent conditions in terms of identification, and where there should be coincidence between the unit of observation and the unit of decision. However, even for this case, Gibbons et al. (2015) advocate that the scope for including an endogenous spatial lag is more limited than the current practice (at least in the cross-sectional case), pointing out an additional limitation that the simultaneity between decisions is rarely discussed.

Interaction matrix. Another more traditional issue in applied spatial econometrics concerns the definition of the interaction matrices. Our point related to the question of identification is to make sure, in the context of a structural form, that the choice of the interaction matrix allows the identification of spillover effects. This is almost always the case for geographically-based connectivities (since they are not of the group type and include intransitive triads). However, as pointed out by Gibbons et al. (2015), such weights might generate weak instruments if they are not sufficiently sparse. More fundamentally, the connectivity matrix should capture the causal transmission channel of interactions and plays a fundamental role in the identification of spillover effects.

To sum up, it is important to consider the cost of using an a-theoretical geographically-based interaction matrix. Indeed, such a matrix precludes the identification of the causal mechanism of interactions between observations, as it constitutes a catch-all of different interaction theories (see Neumayer & Plumper 2016, Agrawal et al. 2022, for examples). Further, it does not allow to account for the effects of a change in a public policy variable on the structure of interactions, and may therefore miss out on public policy effects. Finally, a purely spatial interaction structure completely ignores the socio-economic sources of interactions. These socio-economic channels contain many econometric challenges, but are much richer in terms of interpreting and understanding spillovers.

Spatial heterogeneity. An important point is that spatial heterogeneity should not be undermined, either in structural forms nor in reduced forms. In the structural framework, heterogeneous effects of spillovers (or other variables of interest) could be considered, using the QML approach developed by Aquaro et al. (2021) or the Bayesian framework of Pace & LeSage (2004) and Cornwall & Parent (2017). In fact, the theoretical models developed by Ertur & Koch (2007) and Behrens et al. (2012) imply heterogeneous parameters for spillovers and determinants across countries. Agrawal (2016) model strategic interactions as a function of distance to the borders between local jurisdictions. Within a reduced form approach, spatially differentiated treatment effects may be obtained by interacting the treatment with a spatial indicator. However, as we have forcefully argued previously, spatial heterogeneity is a crucial issue to be considered in order to obtain estimates that can be interpreted causally, both in structural or in reduced forms as spatial heterogeneity also acts as a confounder of spillovers, notably through sorting or hierarchical structure (common factors). This treatment of spatial heterogeneity is typically overlooked in applied spatial econometrics work, preventing clear interpretations of findings. We recommend that much more attention be paid to the presence and form of spatial heterogeneity. There are several possibilities to mitigate the impact of unobserved spatial heterogeneity, such as using flexible spline functions of latitude-longitude coordinates (Reich et al. 2021). Related to this, we emphasize that the econometric consequences of spatial sorting have received a lot of attention in the urban economics literature, but less so in applied spatial econometrics. This problem might arise whether the analysis pertains to spatially mobile units of decision or whether some control variables are the results of outcomes of spatially mobile decision units, such as the percentage of each activity category in a country and should be viewed as bad controls.

Structure of error terms. Unless theoretical reasons imply the parametrization of spillovers in the error terms, we follow Kelejian (2016) who states p.115: "errors are the *unknown* part of the model; we should not model them!" (Highlights from Kelejian). Indeed, by definition, all variables that are in the error term are, in essence, unobserved. As such, imposing a strong structure on the error term needs to be carefully motivated by theory. For instance, in the context of gravity equation, the model developed by Behrens et al. (2012) leads to an econometric model that includes spatial moving average errors. In addition, in the literature on peer effects in education, Calvò-Armengol et al. (2009) develops a model of peer effects characterized by a spatial autoregressive process for the error terms.

A large battery of tests have been developed in spatial econometrics. However, contrary to the usual approach that consists in using LM-based tests for specification search, we contend that it is preferable to use diffuse tests such as non-parametric tests (López et al. 2010) or scan tests (Kulldorff & Nagarwalla 1995, López et al. 2015, Chasco et al. 2018) for diagnostic purposes. These tests are indeed very powerful in detecting the remaining structure in the residuals. For example, Chasco et al. (2018) using a scan test on the residuals of a hedonic model allows improving its specification by uncovering local spatial clusters of high or low residuals pinpointing to spatial omitted variables. Due to their diffuse alternative hypothesis, these aforementioned tests do not allow us to know what is wrong but should invite to revise the whole estimation strategy. Indeed, cross-sectional dependence in the error term may come from confounding unobserved variables, local common factors, misspecification of the interaction matrix, or several of them at the same time.

In other words, we advocate the use of general spatial specification tests to check that no remaining spatial structure is present in the error terms and second, using some robust inference method (Conley & Molinari 2007, Kelejian & Prucha 2007).

5 Conclusion

Our objective in this paper is to provide insights into the conditions under which the widely used notion of spatial spillovers has a meaningful empirical content, by looking at this question from the side of identification.

First, one should stop systematically considering spillovers as side effects of specifications and see them as the main objective. Indeed, modeling interactions through the use of a matrix imposes a lot of structure on the model, such as explicitly specifying the channel(s) through which interactions occur and the functional form of the links between observations. This also implies that these models necessarily fall into the structural econometrics approach as endogenous spatial lags are not compatible with a causal inference framework.[27] If spillovers are only an effect that might confound the identification of another parameter of interest, then causal reduced forms approaches should be preferred. Second, scaling up the importance of spillover effects in the analysis implies avoiding selecting the econometric model using mechanical specification tests. Indeed, this selection procedure, borrowed from time series analysis, is not compatible with a causal interpretation of spillovers, as the model is selected according to the data and not from the economic model. Third, it is important to pay better attention to the matching between the observational unit and the unit of decision, to provide sound interpretations. Fourth, except if the economic model involves cross-sectional dependence in the error term, we also recommend using diffuse tests to assess whether remaining cross-sectional dependence in the error term is present and use a robust approach to model the non-iid behavior of the

[27]Exogenous spatial lags play a different role as they might be viewed as a particular way of modeling exposure mapping, a topic that needs to be further investigated.

error term rather than parametrizing it. Fifth, the identification of spillovers requires a thorough investigation of the multiple facets of spatial heterogeneity to control for all potential confounding effects and be more confident about the interpretations to be extracted from the estimated model. Last but not least, much more thought should be given to the selection of the interaction matrix and except in some very specific cases, avoid using geographic proximity to construct the links scheme between observations. As discussed above, the costs associated with the use of geographical space far outweigh the benefits obtained in terms of simplified econometric estimation.

To conclude somewhat provocatively, we advocate renunciating the confusing name of *spatial econometrics*. Indeed, this term encompasses both the methodological tool (spillover modeling) and a particular type of spillover (based on locational similarity). We believe that *econometrics of spatial interactions* would be a much better name since it would focus on the challenges inherent to the use of spatial data. This term would further be in line with the econometrics of social networks (or peer effects), which is understood as concentrating on all difficulties raised by interactions between individuals. We also suggest to use spatial data econometrics to refer to all methods specific to spatial data.

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