Bipartite network projections of multi-locational corporations: realising the potential

Abstract

In this commentary, I evaluate the potential of bipartite network projections of multi-locational corporations in quantitative-structural analyses of world/global cities. My starting point is that Pazitka et al.'s (2020) paper provides a number of useful critical observations in this regard, but their analysis is also incomplete and at points potentially misleading. I make three points. First, in this literature the notion of 'construct validity' is vague because there is no such thing as a comprehensive world city network: we are dealing with abstractions of a complex and multi-layered reality at best, and bipartite network projections are no more than generic tools that can help capturing the multiple globalizations of specific sets of cities. Second, there is no reason to assume that a spatial-interaction based benchmark model can be used to assess the 'structural validity' of these bipartite network projections. And third, rather than replacing bipartite network projections of multi-locational corporations with bipartite network projections of project ecologies, this literature could above all benefit from geographers no longer borrowing projections from the social networks literature. Instead, we should try and come up with bipartite network projections that more adequately capture the geographical nature of how multi-locational corporations/projects operate/unfold.

1. Introduction

In this commentary, I evaluate the potential of bipartite network projections in quantitative-structural analyses of world/global cities. In their paper, Pazitka et al. (2020) provide us with a number of useful considerations in this regard. However, in my view, their discussion is also incomplete and at points potentially misleading. I will therefore refer to their line of argumentation to develop a more balanced appraisal of this literature.

Pazitka et al.'s (2020) paper has two main objectives. First, it presents a critical review of the specific strand within the global/world cities literature that seeks to specify, measure and analyse business connections between the world's major cities. Within this literature, there has been an increasingly diverse research agenda that uses bipartite network projections of corporate location data to model these business connections. A bipartite projection function essentially specifies how the 'co-behavior' of actors (in this case the office locations of a firm in different cities) is reflected in connections between these actors (in this case connections between these urban office locations). For example, Taylor's (2001) often-cited projection assumes that (1) a shared presence of a producer services firm in a pair of cities opens up the potential for inter-city connections, with (2) the extent of these connections depending on the importance, size, and operational capabilities of the offices involved. Different bipartite projections have a different algorithmic structure and reflect different sets of assumptions of how the co-behavior of actors gives way to connections (Neal, 2014a). As a corollary, different bipartite network projections applied to the same corporate location dataset will produce different inter-city connections that are only pertinent to the degree that the underlying assumptions hold. Pazitka et al.'s (2020) starting point is therefore justifiable: they rightly note that the accuracy of these assumptions in what they term office location approaches (OLAs) is only seldom critically examined. This combination of a lack of scrutiny and the repeated use of these models may render OLAs self-referential, and purposefully-critical interventions are therefore important for this literature to retain its relevance and vibrancy (see also Nordlund, 2004; Neal, 2012; Liu and Derudder, 2012, 2013; Derudder and Parnreiter, 2014).

The second objective emanates from Pazitka et al. (2020) having access to a dataset detailing what they call 'directly observable inter-city business flows', in this case underwriting syndicates for primary issues of equity and debt securities. However, in my view this 'directly observable' is an overstretched framing, as their 'inter-organisational project approach' (IOPA) *also* draws on a bipartite network projection used for *proxy-ing* unobserved inter-city connections. Pazitka et al. (2020) repeat some of Taylor's (2001) equations in their paper (Eq. 1-2), but overlook that if these equations were to be put in matrix algebra format they would look exactly the same as their own model (Eq. 4, see Neal, 2012, p. 163; Neal, 2020). Indeed, both Taylor's (2001) OLA and Pazitka et al.'s (2020) IOPA create inter-city connections by multiplying the input bipartite dataset with its transpose. There are therefore much more similarities between OLAs and IOPAs than Pazitka et al. (2020) acknowledge, making their distinction between direct/indirect connections exaggerated and their assessment of the different approaches' 'construct validity' less straightforward than it appears.

Pazitka et al. (2020) also argue that the inter-city connections derived from their IOPA offer – contra OLAs – an empirical model of the world city network that is 'structurally valid'. This claim is made in the final part of their paper, which features a series of analyses where both

various OLAs and their IOPA are correlated with a benchmark model. This benchmark model assumes that the strength of global inter-city business connections should mainly shaped by three dimensions: according to Pazitka et al. (2020), the strength of these connections has to decline with geographical distance, be lower for cities separated by political borders, and higher for large cities. They find that correlations are much higher for their IOPA than for the various OLA alternatives, which they subsequently take to be “an empirical red flag” for OLAs at large. Their overall conclusion is that, in this literature, IOPAs hold great potential and should be expanded, while corporate location data can be useful on a number of fronts but probably not in their present usage as the basis for conjecturing inter-city business connections by means of OLAs.

Pazitka et al.’s (2020) conclusions offer a damning assessment of the OLA literature which now consists of myriad papers, and which comprises methodological refinements such as other bipartite network projection functions (e.g. Rozenblat and Pumain, 2007; Neal, 2014b), applications to other sets of location data (Hoyler and Watson, 2013; Sigler and Martinus 2017), interpretations at other scales of analysis (Taylor et al., 2008; Burger et al., 2014), and substantive theoretical analyses using OLA results as their formative starting point (Wójcik, 2013; Taylor and Derudder, 2016). In other words: if OLAs at large are built on scientific quicksand, as Pazitka et al. (2020) claim, what do we have to make of this literature specifically, and its use in literatures that take their cue from it more generally? In the remainder of this is commentary, I will argue that Pazitka et al.’s (2020) line of argumentation does not justify their explicit fault-finding. And, as a result, it is far too soon to abandon the allegedly flawed OLAs. To this end, the remainder of this commentary is organised in three main sections. First, I discuss the rise of OLAs in the literature on inter-city business connections in the broader literature, emphasizing that these are developed as generic tools to capture the multiple globalizations of cities rather than attempts to reveal a comprehensive world city network. Second, I critically discuss the idea behind, and the practical implementation of their benchmark model for assessing the alleged structural validity of the different approaches. And third and finally, acknowledging that a critical appraisal of OLAs is indeed needed, I use some of the more relevant observations in Pazitka et al. (2020) to spell out a research agenda that would result in OLA research that better captures actually existing inter-city business connections.

2. Multiple world city networks

Even though contemporary OLA research exhibits some parallels with conceptual (Pred, 1977) and methodological research (Nystuen and Dacey, 1961) published a long time ago, it is now most commonly framed in the context of the world/global cities literature. The exact borders of the latter literature are difficult to locate, because the praxis of doing research prevails over a rigid definition or conceptualization of what world/global cities are (Acuto, 2011). Nonetheless, as van Meeteren et al. (2016a) argue, a key thread in this literature is understanding how the geographical behaviour of economic actors operating on a global scale leads to “the formation, at least incipient, of transnational urban systems” (Sassen, 1991, p. xxi).

The rising focus on networks, flows, and connectivity¹ in the world/global cities literature initially had theoretical underpinnings, most clearly embodied in Castells’ (1996) *The Rise of the Network Society*. Although Castells’ (1996, chapter 6, p. 415) now well-known treatise of the space of flows presents it as an overarching socio-spatial logic, he identifies world/global cities as its “most direct illustration”. He thus argues that our understanding of world/global cities should be centred on how they connect flows of people, ideas, knowledge, information, and capital. Throughout the 1990s, thinking in terms of networks, flows and connectivity gradually became at the heart of how we came to understand world/global cities, and this to the detriment of other approaches that are territorial, scalar or place-based (Derudder, 2020). This in itself warrants a critical appraisal (Van Meeteren and Bassens, 2016), but for the sake of this commentary the key point is that, at least in the Anglophone literature², this theoretical-conceptual focus on networks has found a methodological-empirical translation (e.g. Smith and Timberlake, 2001; Taylor, 2001).

The most-cited OLA introduced in Taylor (2001) was derived from the social network analysis literature (the interlocking network model put forward in Knoke and Kuklinksi (1982)), and applied to a specific set of corporate location data (producer services firms as put forward in Sassen (1991)). Following this publication, other OLAs have been introduced, and these have also been applied to other sets of location data. In addition to this diversity within OLA research, there is even more diversity because the literature on inter-city connections also continues to feature research on infrastructural patterns such as airline networks (Grubestic and Matisziw, 2011), maritime networks (Ducruet et al., 2018), and Internet backbone networks (Malecki and Wei, 2009).

In spite of a shared objective – specifying, measuring and analysing inter-city connections – it is important to stress that a ‘world city network’, just like any other ‘urban system’, is a theoretical abstraction: it is a representation and simplification of a complex and multi-

¹ Ducruet and Beauguitte (2014) point out that in contemporary geographical research concepts such as flows, networks, and connectivity are often used metaphorically rather than formally/structurally (e.g. in actor-network theory and assemblage thinking), but here these terms are used in their graph-theoretical guise: the mathematical study of graphs as a representation of symmetric or asymmetric relations between discrete objects (i.e. cities).

² There are also longstanding non-Anglophone traditions marshalling network approaches to urban systems, especially in France (e.g. Pumain et al., 1987).

layered reality. Attempts to abstract complex and multi-layered urban realities in a network, system or model help to get a handle on this complexity. However, they also carry the risk of reification, as Van Meeteren (2019) and Neal (2020) point out, and it may also unwantedly feed the notion that this abstraction can or should simply correspond with comprehensive observable realities on the ground. It is therefore important to point out that, say, neither Alderson and Beckfield (2004) nor Taylor and Derudder (2016) analyse 'the' world city network, but specific abstractions of specific processes within contemporary globalization. The idea, of course, is that the analysis of such an abstraction can shed some light on certain aspects of the role of world/global cities (e.g. Bassens and Van Meeteren, 2015; Taylor et al., 2014), but it is clearly a specific subset of processes and places that leaves out many other places and processes (Robinson, 2002; Phelps, 2014). In Pazitka et al. (2020), however, different models and data sources are presented and treated as wholesale approaches designed to reveal a comprehensive and tangible world city network. Although questions around the validity of different models and data sources are of course always pertinent, it is not clear to me how the validity of models and data sources can be *universally* benchmarked given that 'the' world city network is elusive at best. In fact, much OLA research explicitly uses the observation of the world city network being *multiplex* to emphasize the need for using different data sources and models.

For example, in a recent paper, Derudder and Taylor (2018) apply the interlocking network model not only to producer services data, but also include applications to maritime produces services firms, global media firms and non-governmental-services. They find that a small number of important world/global cities – particularly London and New York, but also Singapore and Paris – can be found atop most rankings, while other cities are on and off the map insofar they have specific needs, expertise or competencies on offer. More recently, Derudder and Taylor (2020) revealed different subnets within the overall structure produced by the interlocking network model, eschewing any notion of the research being about 'the' world city network. The sometimes radically different geographies of inter-city business connections that thus emerge more broadly capture what Krätke (2014) has called the multiple globalizations of cities. This seems to be a more apt description of the global urban condition than asserting there being a comprehensive world city network. Moreover, just like the choice of which actors are producing networks is not a mere question of 'construct validity', so too the specific OLA being adopted is not. As a consequence, Pazitka et al.'s (2020) "advice (...) to verify construct validity of empirical measures regardless of the specific context" does not strike me as appropriate.

When Neal (2013, p. 1286) presented the first of his alternatives for Taylor's (2001) interlocking network model, he argued that this should not be framed as a simple matter of preference or choice: "(The different approaches) provide different and potentially complementary understandings of how cities might come to participate in the network and of how the network is structured. Viewing these two perspectives, and the networks they imply, in relation to one another can serve to draw on the strengths of each." Van Meeteren et al.'s (2016b, p. 68) conclusions in their discussion of the potential relevance of the concept of 'network externalities' for understanding cities are therefore also relevant when appraising the construct validity of data sources and model specifications in the world city network literature:

“That two different phenomena can be meaningfully represented in a network does not mean they automatically refer to the same object. The question thus remains to what extent inter-governmental collaborations, inter-firm networks, airline and maritime networks, etc. add up to a generalized ‘urban networks’ concept.”

Well before the current wave of network-focused research, Friedmann (1995, p. 23) pointed to “the lack of unambiguous criteria for assigning particular cities to a specific place in the global system” so that “establishing such a hierarchy once and for all may.... be a futile undertaking.” This not only implies that there is no comprehensive world city network ‘out there’, but also that an assessment of the relevance of the degree of convergence between the measurable networks produced by different models and datasets risks being moot. Seeking out the degree of convergence between different networks may most certainly be of interest for a number of reasons (Liu et al., 2013; Choi et al., 2006) but not in the sense that these networks can be conceived as being proxy-representations of a tangible and all-encompassing urban network superstructure.

3. Which structural validity?

The above discussion implies that a critical appraisal of the issue of construct validity in world city network research as suggested by Pazitka et al. (2020) may be pertinent, but only if it systematically considers *which* conceptualisation of the world city network a particular dataset/model is speaking to. Things become even murkier when considering their proposed assessment of the 'structural validity' of different models and data sources: with what structure are different specifications and measurements of inter-city business connections actually supposed to converge? And, perhaps even more pertinent: if we know with what urban network superstructure these inter-city business connections are supposed to align with, why even bother specifying and measuring them in the first place? Pazitka et al. (2020)'s answer to the first question is surprisingly simple: inter-city business connections at the global scale, they assert, are shaped by three parameters commonly found in basic spatial interaction models (geographical distance and political borders impeding flows, and city size facilitating flows). The larger the empirical parallels between inter-city business connections derived from any of the network specifications and these three parameters, they argue, the more structurally valid the network specification. However, the rationale for adopting spatial interaction-type parameters as a benchmark is circumscribed at best: we have to make do with the assertion that these parameters represent "axiom-like tendencies widely documented in urban and economic geography (...) as well as international economics."

Although there are of course plenty of analogues in geography that may make the idea of there being some 'classical' patterns of spatial interaction in world city networks ostensibly logic, calling these 'axiom-like' is problematic, especially in a paper that laments other approaches for making unqualified assumptions. But simply calling one's own assumptions 'axiom-like' is not enough to put them beyond scrutiny. Even a basic appraisal of these axiom-like tendencies makes me wonder how a full alignment would imply that a network specification is "entirely valid", as Pazitka et al. (2020) claim. For example, even if we accept that political borders matter for inter-city business connections, are these the same for the Brussels-Paris connection and for the Delhi-Karachi connection? And, even if respecting the relevance of Tobler's first law of geography as a basis for geographical analysis (Miller, 2004), will distance play a similar role for the Kinshasa-Lubumbashi connection and for the New York-Chicago connection (these city-pairs cover roughly the same distance)? The answer to such questions, it would appear, is negative: in an analysis of inter-city airline connections, for example, Zook and Brunn (2005, p. 203) conclude that the "cost and time of air travel (...) do not adequately illuminate the specific contours of this system vis-à-vis historical and cultural connections."

Furthermore, even if one accepts that these parameters can serve as a benchmark, questions regarding the mathematical nature of the spatial interaction remain unanswered in Pazitka et al. (2020). It is well known that a spatial interaction model needs to be calibrated (Fotheringham and O'Kelly, 1989), but it is unclear how such a calibration might work in this context – is it even possible to benchmark a benchmark model? There is no mention of a calibration, and Pazitka et al. (2020) state that "natural logarithms (are used) to allow for non-linear relationships in the data." However, this is neither a calibration nor a well-defined mathematical specification of this relationship per se. Non-linear relationships may of course mean all sorts of things, and there is therefore neither a conceptual nor an empirical

justification for using logarithms. Unless this, too, is 'axiom-like'. Taken together, then, the structural validity test is not only an ambiguous exercise in its own right, in practice its operationalisation also seems to combine far-fetched and unspecified judgement calls.

In principle, the above would be enough for OLA researchers (and those who use their findings) to discard Pazitka et al.'s (2020) punitive assessment of (the starting point of) their work. But, even if one is willing to buy into the argument that *any* specification of inter-city connections needs to correlate with their version of spatial interaction parameters for the former to be structurally valid, it would appear that the empirical exercise disfavors OLAs. Throughout their paper, Pazitka et al. (2020) focus on a number of OLAs, which include versions of Taylor's (2001) interlocking network model alongside some of Neal's (2014b) alternatives. This implies that some OLAs are not included in their analysis. This may seem warranted for reasons of clarity, but of course: if results are presented as a collective 'red flag' for OLAs, then using an unspecified sample of OLAs does not suffice. Exactly why these OLAs were chosen, why others were left out, and how representative these are for 'the' OLA at large remains unclear. This is not mere nit-picking, because I find it remarkable that the OLA introduced in Hennemann and Derudder (2014) is not part of their structural validity analysis, especially given that Pazitka et al. (2020) cite this paper for another reason. The reason why ignoring the bipartite network projection put forward in Hennemann and Derudder (2014) is a problem becomes clear when briefly spelling out its major tenets.

Hennemann and Derudder's (2014) OLA specifies inter-city business connections as a spatially constrained system of expertise and information exchanges in a firm's multi-locational office network. Their bipartite network projection makes two main assumptions when projecting corporate location data into inter-city business connections:

(1) All other things being equal, it is more probable that an office in city x will seek expertise in (and therefore makes a connection with) an office in city y that has a more important presence of that company: a larger office or an office with extra functions or expertise. So, if the Antwerp office of an accountancy firm does not have the expertise to deal with a specific query, it will likely turn to a city with an office that has more resources, personnel, expertise, etc. In short, Hennemann and Derudder's (2014) OLA assumes that *city size/importance matters* in inter-city business connections.

(2) All other things being equal, it is more probable that expertise will be sought in (and therefore a connection made with) a nearby city. If the Antwerp office of an accountancy firm does not have the expertise to deal with a specific query, it will not simply turn to any city with an office that has more resources, personnel, expertise, etc., but probably to the Brussels office. This 'proximity' can obviously be defined in Euclidean terms, but also in functional-regional terms. Hennemann and Derudder (2014) operationalise proximity as a combination of both functional and Euclidean distance: connections are first and foremost made with cities in the same country, and in case there are multiple options the connection is made with the most proximate city³. In short, Hennemann and Derudder's (2014) OLA assumes that *political borders* and *geographical distance* matter in inter-city business connections.

³ In practice the operational definition is even more refined: if there is no higher-level office in the same country, then connection is assumed to be with the highest-level office within a city's 'world region' (9 such regions are identified). And, if there is no such higher-level city within a city's own world region, a connection is assumed with the global headquarters.

In short, the ignored Hennemann and Derudder (2014) OLA would produces what Pazitka et al. (2020) assert is a structurally valid world city network: size matters, political border matters, and distance matters as per the alleged axiom-like tendencies. This not only begs the question why it is not included in their OLA assessment exercise (it would likely have a very high R^2), but also shows that it *is* possible to devise OLAs that can be tailored to specific geographical requirements and assumptions.

4. The future of OLAs in world city network research

Pazitka et al. (2020) suggest that OLAs should no longer be used in world/global cities research, at least not in their present guise. They advocate substituting it with other approaches, and argue that IOPAs hold great potential in this regard (see also Knight and Wójcik, 2017; Pan et al., 2019)⁴. I agree that IOPAs in general and their dataset in particular are interesting, and I look forward to reading substantive empirical analyses. However, I do not agree that their analysis credibly makes the case for IOPAs *replacing* OLAs: extraordinary claims require extraordinary evidence, and in this commentary I have argued that this extraordinary evidence is not presented in their paper. Furthermore, I am afraid that the at times assertive and argumentative tone in their paper risks derailing rather than facilitating debate – this happens too often in this literature (cf. Van Meeteren et al., 2016a).

But not having that debate would be a shame because Pazitka et al.'s (2020) observation that the assumptions associated with OLAs are only rarely critically examined is pertinent. Instead, I think it would be more useful to recast such a critical appraisal of both OLAs and IOPAs into an appraisal of the relevance of the bipartite network projections they *both* invoke. Indeed, the overarching issue with the network projections adopted in this literature is that their specification of 'co-behavior' is not based on pertinent geographical scholarship on either corporate organisation (OLAs) or project ecologies (IOLAs). Hennemann and Derudder (2014) do attempt to be more explicitly geographical, but their choices are ad hoc and cursory at best. As Neal (2014a) points out, the literature on bipartite network projections include forms of 'co-behavior' as diverse as co-authorship, co-sponsorship, co-attendance and co-location. It is unlikely that each of these co-behaviors is guided by the same logic. This is, of course, why in the social networks literature – and in the OLA literature! – there is a blossoming debate on the structure of bipartite network projections: different sets of actors and different sets of co-behaviors require different specifications for the resulting inter-city connections to be meaningful (or 'valid'). The real issue, therefore, is that in the quantitative-structural strands of the world/global cities literature there has been very little consideration of research spelling out how firms *actually* network across geographical locations or – in the case of Pazitka et al. (2020) – how actors *actually* network as they engage in a project.

Based on qualitative research into the nature of managerial control in supposedly globalized business service industries, Jones (2002) speaks of the myth of global management. Likewise, based on qualitative research on the Munich software and the London advertising ecologies, Grabher (2004) points to there being diffuse geographical associations in projects that extend beyond and beneath actual collaboration ties. Both insights would, respectively, challenge some of the assumptions in many of the network projections adopted in OLAs and IOPAs. But, more usefully: such qualitative research could actually form the basis for network projections or calibrations thereof that are better adjusted to how multi-locational corporations/projects *actually* operate in a geographical sense. So, rather than us geographers uncritically borrowing projections from the social networks literature, we could try and come up with projections that adequately capture the *geographical nature* of the 'co-behavior' we are dealing with (see also Glückler and Doreian, 2016). This is, of course, what Pazitka et al. (2020)

⁴ There are also parallels with research using client-producer interactions across space, which could probably be advanced as a third set of approaches (see Hanssens et al., 2014; Rossi et al., 2007).

implicitly do as they posit allegedly axiomatic geographical patterns, but here too the better option would have been to replace these crude guesstimates with insights derived from relevant qualitative research on multi-locational corporate/project operations.

As in any large-scale structural-quantitative research exercise, some degree of simplification and uncalled-for homogenization will be inevitable when specifying and implementing a bipartite network projection. And, given diverse spatial logics, looking for the 'perfect' projection is probably a useless endeavour altogether. Nonetheless, OLA and IOPA research could be collectively improved by aligning their network projections with insights emanating from qualitative geographical research (for an excellent example of this in a different context, see Sohn et al., 2020). This was already implicitly argued by Watson and Beaverstock (2014), who recently identified a 'theoretical impasse' in quantitative-structural world/global cities research: they argue that this research shows very little regard for research on how firms are managed across borders (Beaverstock, 2004) and/or how debates about the way stretched relational spaces and global communities of practice connect individuals, firms, and cities (Faulconbridge, 2010). Rather than simply trading one quantitative-structural approach (OLA) for another (IOPA), I therefore believe it would be better to retain both approaches, with each of them starting to explore how their bipartite network projections could better speak to insights into how corporations and projects geographically operate. Paraphrasing Neal (2013), it could be said that both OLAs and IOPAs, and the networks they produce, can then serve to draw on the strengths of each other. The end result will be a more robust methodological basis for exploring world city networks.

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