

# A Tale of Three Cities

*How to Generalize from Here?*

Leo Lucassen

TSEG 11 (4): 75–87

DOI: 10.1557/10.5117/TSEG2014.4.LUCA

## Abstract

*Cities of Commerce* is an important study that has serious repercussions for a number of core debates within the field of institutional economic history. Future research will have to make clear what the outcomes of concrete interactions between newcomers and natives were and how these are determined by the interplay between the characteristics of the migrants and the local membership regime. Extending the geographical unit of analysis even further, the book is an excellent starting point to make broader, global comparisons with cities in Asia, Northern Africa and the Middle East, in order to understand better under what conditions gateway cities developed similar evolutionary adaptive processes as in Europe's Northwest.

*Cities of Commerce* is a well-written and -structured study that adds greatly to our understanding how 'inclusive commercial institutional regimes' came about in early modern North Western Europe. With this book Oscar Gelderblom positions himself in larger debates on long term economic institutional change, breaking a lance with top scholars like Douglas North, Barry Weingast, Daron Acemoglu, James Robinson, Avner Greif and Sheilagh Ogilvie. He does so convincingly, by showing that competition between cities with a certain degree of political autonomy was the driving force behind the convergence of inclusive 'open access' regimes, which offered merchants protective and enforceable legal structures with low transaction costs. These regimes go beyond what he calls 'private order solutions', that are mostly associated with ethno-religious groups with high

trust and Granovetterian strong ties.<sup>1</sup> As Gelderblom convincingly shows, however, private order solutions are always embedded in a more general socio-legal urban framework, that over time leads to a merging of commercial institutional practices which transcend specific social groups and kin networks. According to Gelderblom this process of convergence in the Low Countries became the template for a much wider international region. Although, remarkably, he excludes the trade with Asia and the America's, with the – not very convincing – argument that here state led trade monopolies tended to dominate.<sup>2</sup> Crucial in his argument is his critique on North's assumption that institutional commercial change which led to 'open access' is inextricably bound up with strong centralist territorial states. And this is why the three core cities discussed in *Cities of Commerce* (Bruges, Antwerp and Amsterdam) are excellent counter examples, being prototypical strong local polities in decentralized states.

In this contribution I discuss the strengths, weaknesses and agenda setting dimensions of the book from two thematic areas that interest me most, migration and urban history, both in a global comparative perspective. This may, admittedly, not be the fairest approach to an important book that so consciously positions itself in a key economic historical institutional debate, but I hope it can nevertheless be useful to test the more general causal connections that are suggested so forcefully in *Cities of Commerce*.

## 1 Cities of commerce and the Great Divergence

Although the name of Max Weber is not mentioned, the book clearly stands in a Weberian tradition that suggests a causal link between institutional developments and economic and political power. A line of thought that is reproduced by a number of scholars mentioned earlier, in particular North, and Acemoglu and Robinson.<sup>3</sup> Weber puts great value on the importance of independent cities, which developed as an ideal type in Europe, starting in Northern Italy, and then following the 'blue banana' curve

1 Mark Granovetter, 'The strength of weak ties', *American Journal of Sociology*, 78:6 (1973) 1360-1380.

2 As work on Sephardic merchant networks shows, the private and state networks partly overlapped: Jessica Roitman, *The same but different? Inter-cultural trade and the Sephardim 1595-1640* (Leiden and Boston 2011).

3 D. Acemoglu & J.A. Robinson, *Why nations fail. The origins of power, prosperity and poverty* (London 2012).

to end in North-Western Europe.<sup>4</sup> Such Weberian cities make up the core of Gelderblom's book. All the key characteristics of successful economic institutions, such as property rights, enforceable contracts, freedom of trade, embedded in a framework of political autonomy and civil society, are to be found in the cities discussed in *Cities of Commerce*. Gelderblom obviously shares this point of view and therefore paints a highly positive picture of the great fruits that societies reap from intense level playing field competition. That may be so if we look at the city as a whole, but his almost neo-liberal praise for this inter-city 'race to the bottom' ignores the negative effects that such behaviour might have for the long term economic strategies and social policies of cities.<sup>5</sup>

In a more general sense his attention to competition links up with the seminal work by Charles Tilly, Wim Blockmans and Geoffrey Parker, who have argued that interstate competition in Europe since the Middle Ages has not only led to massacres and great emotional and material damage, but also to innovation in (military) technology and weaponry,<sup>6</sup> and more in general to efficient logistic and organizational (Weber's stress on impersonal bureaucracies) practices. Together these two developments (autonomous cities and interstate competition) make up the lion's share of what made 'Europe', or at least North Western Europe, distinctive from other parts of the world and which would explain why this part of the world rose to global power in the last two centuries or so. Although this Weberian perspective has been criticized for being Eurocentric and biased,<sup>7</sup> it is far from dead as the ongoing 'Great Divergence' debate on the 'rise of the West' shows.<sup>8</sup>

Notwithstanding the references to the work of Kenneth Pomeranz, Jean-Laurent Rosenthal and Roy Bin Wong, it is not entirely clear where

4 K. Davids & J. Lucassen (eds.), *A miracle mirrored: the Dutch Republic in European perspective* (Cambridge 1995).

5 Apart from that, the overly optimistic interpretation of institutionalists also hides the 'harsh realities' for producers of the commodities the merchants trade in and the urban workers who were involved in processing raw materials. See in this respect Peer Vries, 'Bringing labour back in', *Tijdschrift voor Sociale en Economische Geschiedenis*, 11:1 (2014) 127-140, specifically 133.

6 G. Parker, *The military revolution. Military innovation and the rise of the West, 1500-1800* (Cambridge 1988).

7 K. Pomeranz, *The great divergence. China, Europe, and the making of the modern world economy* (Princeton, 2000); R. Bin Wong, *China transformed: historical change and the limits of European experience* (Ithaca 2000); Jean-Laurent Rosenthal & R. Bin Wong, *Before and beyond divergence. The politics of economic change in China and Europe* (Cambridge Mass.2011).

8 For a recent contribution see P.H.H. Vries, *Escaping poverty. The origins of modern economic growth* (Vienna 2013).

Gelderblom stands and how we should position his book within the Great Divergence debate. He writes in his introduction that in the long run “*these institutional adaptations allowed Europe to catch up with the Middle East and China, where contracting institutions in long-distance trade were already highly developed and widely shared at the end of the first Millennium*”<sup>9</sup>, and adds that through centuries of inter urban competition these institutions reached a high level of sophistication whereas institutional change stagnated in the Middle East and China,<sup>10</sup> but he remains somewhat implicit about their causal role in the rise of the west. Given the lack of a global comparative framework in this book, it is understandable that he avoids this debate, but considering the broad theoretical claims of the book, it is also a missed opportunity. If we want to ascertain whether his general proposition is valid, a much more thorough comparative design is needed which allows us to compare the specific role of Bruges, Antwerp en Amsterdam, all gateways in more or less the same estuary and sharing the same hinterland, with similar socio-spatial configurations elsewhere. Brief references to macro regions such as ‘China’ and ‘The Middle East’ are not very useful here and add little to the Great Divergence debate. Gelderblom’s book, nevertheless, is a good starting point, because it already formulates the criteria that must be fulfilled when we want to select similar cases. As the author makes clear Bruges, Antwerp and Amsterdam meet three conditions: 1) they are positioned in an area with a large number of cities with direct access to tradable surpluses; 2) there are multiple gateways to the hinterland en 3) these cities enjoy a relative political autonomy.<sup>11</sup> For future comparative research these three conditions, or their opposite, are useful analytical tools and will help to test his theoretical claims in a systematic way.

## 2 Open access? What open access?

The lack of rigorous comparisons beyond the three cities and the very different units of analysis (China, the Middle East), bring me to another point that I think needs clarification. This regards Gelderblom’s contention that the ‘open access’ regime in the three core port cities in the Low Countries was not confined to strong cities and weak central states, but

9 Gelderblom, *Cities of Commerce*, 12.

10 Gelderblom, *Cities of Commerce*, 4.

11 Gelderblom, *Cities of Commerce*, 200-202.

“affected every town with commercial aspirations even in absolute states like France and Spain”.<sup>12</sup> It seems to me that there are several problems with this position. First of all, the claim may be true, but due to the vague formulation not so easy to test. To do so, we would need to know what ‘affected’ means. In what ways, and to what extent? The book offers, alas, few clues, as it concentrates on three cities in the Low Countries. Was the example of Amsterdam in the 17<sup>th</sup> century, for example, so strong that all cities with some commercial functions were somehow forced to emulate its template? If so, that would be hugely surprising and interesting, as it implies that the role of strong, centralist (and mercantilist) rulers, both in France and Eastern Europe, was of subordinate importance. The mere existence of a nearby ‘open access’ city with a key position in international trade as a shining example would suffice.

To arrive at such a conclusion, however, we need a much more precise definition of what variables we need to measure and what their threshold is to determine whether ‘open access institutions’ really did spread over Europe, irrespective of the prevailing state system. But even on the basis of general knowledge there seem to be a number of problems that make such an evolution improbable. First of all, in many parts of Europe certain groups of merchants were unwanted or excluded, as was the case with Jews outside the Ottoman empire, England, Northern German towns and the Low countries, not by coincidence regions where they fled to after the Reconquista. The same is true for Protestants in Catholic regions and vice versa, again with the exception of tolerant regions in the North West and South East of Europe.<sup>13</sup> In more or less mono-religious territorial states, as they emerged after the Augsburg Settlement of 1555, and after the ousting of Huguenots after 1685 in France, trading minorities were not completely banished, but were subjected to severe limitations as to where and in what products and services they could do business.

Gelderblom’s open access optimism is also not in tune with the overall rather restrictive citizenship regimes in most cities and countries in the Hinterland of the three shining cities of commerce. Already within the Low

<sup>12</sup> Gelderblom, *Cities of Commerce*, 4.

<sup>13</sup> C.H. Parker, ‘Paying for the privilege: the management of public order and religious pluralism in two early modern societies’, *The Journal of World History* 17 (2006) 267-296.

Countries,<sup>14</sup> the further East one travelled in the 17<sup>th</sup> and 18<sup>th</sup> century, the less open access citizenship/membership regimes became, as has been systematically mapped in earlier research.<sup>15</sup> The innate social and religious discrimination does not necessarily constitute a problem for Gelderblom's core argument, because he is only interested in the commercial class, often elites. As he states repeatedly they did not even need political power (through citizenship) in order to persuade urban (or central) rulers to bend the rules in their favour and thus leading to a Europe-wide convergence. As the author himself admits, some cities did exclude specific groups of traders, whereas elites and guilds often followed monopolistic strategies and other form of rent seeking and regulatory captive. Not so open access therefore. Gelderblom knows this, but argues that in the long run the ongoing inter urban competition made the open access advantages simply too attractive to ignore: hence the European wide convergence. Again, this may be true, but is far from proven and given the research on other forms of limited and segmented access in various European regions, I expect that a much less linear and much more layered and fragmented picture will arise once we start doing the rigorous comparative research that *Cities of Commerce* invites us to do.

### 3 Footloose

The reason commercial cities were so forthcoming is exactly because they feared that otherwise merchants would move to a competitor and to avoid that, they were willing to lower transaction costs, offer protection and refrain from clamping down on merchants. Due to the social and religious discrimination in many early modern cities in Europe, however, many merchants who constituted the elites in the three cities, were restricted in their options where to settle. That was most true for the Sephardic Jews, who – as far as they did not migrate to Istanbul and other Ottoman cities – did not end up in Amsterdam by coincidence. But also protestant refugees from Antwerp and other Southern Netherlandish cities, were restricted to a

14 L. Lucassen, 'Population and migration', in: Peter Clark (ed.), *The Oxford handbook of cities in world history* (Oxford 2013) 664-682

15 P. Lourens & J. Lucassen "'Zunftlandschaften" in den Niederlanden und im benachbarten Deutschland', in: W. Reininghaus (ed.), *Zunftlandschaften in Deutschland und den Niederlanden im Vergleich* (Münster 2000) 11-44. On the notion of membership regimes see U. Bosma, G. Kessler & L. Lucassen (eds.), *Migration and membership regimes in global and historical perspective* (Leiden and Boston 2013).

relative small number of cities that met the three conditions mentioned in the previous paragraph. Moreover, Gelderblom seems to underestimate religious and other cultural motives of these merchants to stay or move and thereby shape their networks. Many of them invested heavily in their local social capital and did aspire upward social (and thereby) political mobility in the cities of arrival. Many of them became members of religious communities, in which they could share cultural and diaspora values and memories.<sup>16</sup> The idea that they would move as soon as their demands were not answered in a satisfactory way, therefore seems unlikely. As Gelderblom shows, and did earlier in his dissertation,<sup>17</sup> there was indeed much mobility between commercial cities, but mostly within kin networks and with strategic objectives to cement existing trading networks, not primarily to put pressure on specific urban authorities. These considerations make one doubt about the extent of ‘footlooseness’ that plays such a crucial role in Gelderblom’s argument.

These personal and cultural motives for choosing a particular city should be understood in a wider commercial setting, which brings me to a possible alternative explanation for the sequence in the three commercial cities that Gelderblom seeks to understand and explain. Although I find his stress on institutional competition important, his decision to focus only on these three port cities might prove to be somewhat limited, a myopia which is further strengthened by his decision to leave out inter-continental trade. As specialists on the Atlantic trade have shown, what we see happening in the late 16<sup>th</sup> century is not so much a constant shift of merchants to cities that offer the best institutional setting, but a process of centralization in a number of hubs, such as Amsterdam, Lisbon, Bordeaux, Rouen, London, Seville, Hamburg and Bristol that functioned well alongside each other. In these cities merchants ‘benefited from the convergence of services and conditions that helped trade run’.<sup>18</sup> From there junior partners and agents served as ‘spokes’ and were sent to distant markets and commercial cities to connect different markets. This coexistence of different hubs in close proximity does not necessarily reject Gelderblom’s claims of institutions adaptation, but nuances it as it introduces a second layer in his model which allows for market segmentation and national (mercanti-

16 As argued in the forthcoming thesis of Johannes Müller, ‘Exile memories and the Dutch Revolt. The narrated Diaspora, 1550-1750’ (Institute for History, Leiden University, 2014).

17 Oscar Gelderblom, *Zuid-Nederlandse kooplieden en de opkomst van de Amsterdamse stapelmarkt (1578-1630)* (Hilversum 2000).

18 Daviken Studnicki-Gizbert, *A nation upon the Ocean Sea. Portugal’s Atlantic diaspora and the crisis of the Spanish Empire, 1492-1640* (Oxford 2007) 94-95.

list) interests. Furthermore, studies on the Portuguese trade in the 16<sup>th</sup> and 17<sup>th</sup> centuries stress that apart from this ‘concentration model’, intercontinental trade (which was always interlinked with intra-European trade), dominated by the Portuguese, developed a different, multiple, strategy which resembled not the hub-and-spoke, but a web, linking many hubs with colonial markets.<sup>19</sup> Cities like Lisbon and Amsterdam, with their respective colonial networks,<sup>20</sup> were the proverbial spiders, which could not so easily be replaced by competitors with even more attractive institutions. Once in place these cities profited from both overarching political protection driven by state interests and from that fact that transaction costs that a large scale move would involve were prohibitive.

Given the mercantilist and other more centralist interests mentioned above, I finally was not entirely convinced by the idea that what distinguishes Bruges, Antwerp, and especially Amsterdam, is their local autonomy. As Gelderblom shows, Amsterdam could only be so successful because it was part of a, admittedly decentralized, Dutch Republic, which most importantly had a well-functioning army and fleet, which was necessary to defend its commercial realm. It is true that in the Estates General, in which all cities and provinces were represented, Amsterdam’s voice could outweigh that of other representatives, but at the same time it shows that at critical moments broader superseding interests were at stake. In this sense North’s argument that territorial states mattered seems less debatable. Here the role of London, and more broadly of England as a territorial state, is of interest. London did after all fish in the same pond as Bruges, Antwerp and Amsterdam. Gelderblom might argue that this proves his point, at least from the end of the 17th century when Jews and later Catholics were given more freedom to trade. The rise of chartered and livery companies, however, shows also considerable differences and highlights the shift to a new phase in which the “cities of commerce” model is replaced by the national model,<sup>21</sup> quite similar to the shift in membership regimes.<sup>22</sup> In short, the Gelderblom model seems limited in time.

19 Studnick-Gizbert, *A nation upon the Ocean Sea*. See also Xabier Lamikiz, *Trade and Trust in the Eighteenth-Century Atlantic World. Spanish Merchants and their Overseas Networks* (London 2010) 5-6.

20 Cátia Antunes, *Globalisation in the Early Modern Period. the economic relationship between Amsterdam and Lisbon, 1640-1705* (Amsterdam 2004).

21 Davids & Lucassen, *A miracle mirrored*, 411-459.

22 Bosma, Kessler & Lucassen, *Migration and membership regimes*.

## 4 Cross-cultural migration

Apart from invitations to draw up a research agenda that puts the claims of this book to a rigorous test, which is one of the greatest values of this study, *Cities of Commerce* also has something to offer to the developing ideas about the effects of cross-cultural migration on social (and institutional) change. Although migration as such is not thematized, let alone treated as an *explanans*, the book is an excellent starting point to develop and think more fundamentally about the role of migrants in (commercial) cities. People who moved to cities in Early modern Europe, and other parts of the world, did cross a cultural boundary, and the same is true for urban dwellers – as many merchants were – who ended up in cities with different cultural, social and political opportunity structures.<sup>23</sup> Following Manning,<sup>24</sup> I assume that such migrations forged social change, because they imported different practices, cultural norms and human capital which – in interaction with local customs and depending on the ‘openness’ of the prevailing membership regime – could lead to all kind of innovations, including the type of commercial institutions that are at the center of Gelderblom’s book. It seems that migration as an *explanans* is partly hidden in what the author terms ‘competition’. In essence this competition boils down a rivalry to attract migrants with specific cultural and social capital. At a more concrete level, therefore, migrants and everything they bring with them were responsible for change, albeit always in interaction with locals. The outcome of such interactions depends very much on 1) the cultural and social capital of the migrants and the natives and 2) the prevailing membership regime that set the rules for these two groups to interact and mingle. This may vary from complete openness and equality, as was to some extent the case in Amsterdam (although not for Jews and Catholics), to highly segmented and labelled interactions or outright exclusion in most territorial states.

Although *Cities of Commerce* underplays the role of migration as an independent variable, the book is nevertheless important because it forces us to think harder about the function of migration as a causal factor in forging social and institutional change.<sup>25</sup> To some extent the author does allude to this when he talks about the emergence of ‘cosmopolitan’ urban

23 J. Lucassen & L. Lucassen, ‘The mobility transition revisited, 1500-1900: what the case of Europe can offer to global history’, *The Journal of Global History* 4:4 (2009) 347-377.

24 P. Manning, *Migration in world history. Second edition* (Abingdon and New York 2013).

25 This argument is developed in J. Lucassen & L. Lucassen (eds.), *Globalising Migration History. The Eurasian experience (16th-21st centuries)* (Leiden and Boston 2014).

cultures, which are the result of prolonged interactions.<sup>26</sup> This is exactly where migration as an independent variable can be studied, with as a logical follow up question, under what conditions such cosmopolitanism (or the opposite: highly segmented, strong tie groups) could develop and contribute to the convergence of shared commercial institutions?

A better insight in the 'transfer' of knowledge and institutional practices by migrants would also deepen our understanding in the causality in the rise and fall of commercial cities. Were urban governments the leading actors and prime movers, with 'footloose merchants' following? Or should we see it as a much more interactive model in which merchants as migrants through intensive cross cultural contacts contributed to the development of new urban institutions, and thus anchoring innovation? Here the extensive literature on creative industries in past and present, as well as guilds, could be fruitful, in which certain economic segments in cities are largely dependent on new ideas provided by newcomers with different cultural backgrounds.<sup>27</sup>

Gelderblom's book provides ample examples of the transfer of trading practices that started out as 'private order solutions', which then gradually received official legal recognition by urban authorities.<sup>28</sup> Most of these, as the author shows, originated in Southern Europe, especially in cities like Venice, Genoa and Lucca and were later introduced in Northern European cities like Bruges, Antwerp and Amsterdam. Others were further developed in Germany, such as the 'bilateral commenda'.<sup>29</sup> *Cities of Commerce* makes clear that Italian, Spanish, Portuguese en German traders, as cross-cultural migrants, functioned as innovators in the North and that through their day to day trading practices, from the *commenda*, to the systematic dissemination of information, *compagnia* and insurance policies, were pivotal in introducing these innovations. Less clear, however, is how cities like Antwerp en Amsterdam adapted these practices, or in other words, what the result was of the cross-cultural interactions between newcomers with a relative high status and power and local merchants and city officials who

26 Gelderblom, *Cities of Commerce*, 13.

27 Michael Deinema and Robert Kloosterman, 'The city and the art of earning. Cultural industries in the twentieth-century Netherlands', in: Leo Lucassen and Wim Willems (eds.), *Living in the City. Urban Institutions in the Low Countries, 1200-2010* (New York 2012) 168-191. On the relationship between migration and innovation through guilds see: S.R. Epstein, 'Craft guilds, apprenticeship, and technological change in pre-industrial Europe', *The Journal of Economic History* 58:3 (1989) 684-713, there p. 702.

28 Gelderblom, *Cities of Commerce*, chapter 4.

29 Gelderblom, *Cities of Commerce*, 76-83.

entered the game much later. Although Gelderblom does not pose this question, the information he provides seems to suggest that in most cases cities in Europe's North West simply copied existing (Mediterranean) institutional arrangements, such as double-entry bookkeeping, and then fine-tuned these to reach a competitive advantage.

Finally, the role of merchants-as-migrants become visible in the choices certain groups of merchants make in developing and sustaining their networks. These decisions are closely linked to the centrality of trust in early modern trade. Although Gelderblom is right that legal improvements facilitate the growth of trust, institutional arrangements are not the only factor. As argued by Xabier Lamikiz, for Spanish merchants the quality and extensity of information was very important and depended on the presence of co-ethnics in commercial cities.<sup>30</sup> Although he admits that local institutions can entice merchants to develop such trading posts, these decisions were caused by other (cultural) factors than institutions alone. In other words, trust is not entirely the result of rational calculations, but also of ethnic and normative habits of the merchants involved. This shift of perspective from the city as an independent actor to migrants as innovators and culturally embedded agents might offer a more balanced picture and nuances the institutional and urban determinism that characterizes Gelderblom's study.

## 5 Conclusion

*Cities of Commerce* is much more than a tale of three cities. Oscar Gelderblom has written an important study that has a clear central argument with serious repercussions for a number of core debates within the field of institutional economic history. As all excellent books do, it suggests new and promising roads for further research along the lines set out by the author, as well as by choosing different perspectives, as suggested in this article. Roads that in the first place lead to rigorous comparative research in time and space, but at the same time it urges us to think harder about other factors and contexts. I was not entirely convinced that institutional factors necessarily trump cultural ones. Cultural factors, including the role of migrants and the interactions with natives in urban settings (by spreading new ideas and trading techniques), may prove to be at least as fruitful as that of 'competition', although they might offer insights at a different

<sup>30</sup> Lambikiz, *Trade and trust*, 182-183.

level of analysis. But even at the general level one might argue whether it are the urban governments that play a pivotal role, or the foreign merchants themselves, who through their power to shift their operational base forced cities to lower transactions costs.

Whatever perspective one takes, however, it is clear that ‘competition’ does an excellent job at explaining the inter-urban dynamics in the early modern period, but I would urge the author not to consider the role of urban governments exclusively as an independent factor, but also as reliant on the decision taken by merchants. And thus the cross-cultural migration perspective may have an important added value. Future research will have to make clear what the outcomes of such concrete interactions between newcomers and natives were and how these were determined by the interplay between the characteristics of the migrants and the local membership regime.

As for the broader context, *Cities of Commerce* shows how within the general shift in the European commercial center of gravity from the Mediterranean to the Atlantic competition between cities to attract foreign merchants, and thus to develop open access institutions, played an important role. The author shows that merchants and city officials in Bruges, Antwerp and Amsterdam did their utmost best to offer the best conditions to (foreign) traders. Autonomous urban polities that had access to domestic and foreign markets and that were confronted with footloose merchants, brought about ‘an evolutionary process in which merchants and cities selected [...] the most fit to organize commercial and financial transactions’.<sup>31</sup> At the same time, however, the local dimension of these polities, was also their drawback. Their success depended not only on the Braudelian shift from the Mediterranean to the Atlantic, caused by the first round of globalization, but also on larger power conflicts between territorial states, which put Antwerp (until its harbor was closed due to political factors in 1585),<sup>32</sup> Amsterdam and London in the driving seat as centers of trade between Europe, Asia and the Americas. In the end, global shifts, in this case the ‘discovery’ of the Americas and the trade with Asia, in combination with the advantageous position in the North-South trade routes cannot be ignored. To test this competing geographic and geo-political explanation, a systematic comparison between Amsterdam and rivals with a similar geographical advantage, such as Bordeaux or Lisbon, seems an obvious route to explore and thus would extend the theoretical range of

<sup>31</sup> Gelderblom, *Cities of Commerce*, 202.

<sup>32</sup> Gelderblom, *Cities of Commerce*, 142.

*Cities of Commerce*. Extending the geographical unit of analysis even further, the book is an excellent starting point to make broader, global comparisons with cities in Asia, Northern Africa and the Middle East, in order to understand better under what conditions gateway cities developed similar evolutionary adaptive processes as in Europe's Northwest.

Oscar Gelderblom's focus on the local, urban, level is a healthy antidote to theories of institutional change that privilege the role of territorial states in the early modern period. The institutional arrangements of urban governments, as developed in Bruges, Antwerp and Amsterdam (and earlier in Genoa and Venice), were indeed superior in an era of weak states and his book stresses once again the importance of Tilly's 'capital intensive' path. At the same time, however, *Cities of Commerce* also shows the limited storage life of city states and thus of his model, which lost out once national states, with London as its crown, took over in the course of the 18<sup>th</sup> century.<sup>33</sup>

## About the author

**Leo Lucassen** (1959) is Research Director of the International Institute of Social History (IISH) in Amsterdam and part time professor of Global Labour and Migration History at the Institute of History of Leiden University. He specializes in migration history, urban history, state formation, eugenics and other socio-political developments in modern states.

E-mail: [leo.lucassen@iisg.nl](mailto:leo.lucassen@iisg.nl)

33 In that sense, one could argue that the argument developed in this book owes a greater debt to the work of Charles Tilly than that of Douglas North.

