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Cities of Commerce: How Can We Test the Hypothesis?

Guillaume Daudin

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Abstract ¹

This paper discusses Gelderblom's hypothesis that urban competition (including a large number of competing cities, footloose foreign traders and municipal autonomy) was central to the rise of inclusive trade institutions in Europe. The first part discusses the precise behaviour of traders, town authorities and sovereigns underlying Gelderblom's explanatory framework. The second part presents some challenges to the generalisation of the book's thesis to the history of Europe, including Italy and Britain. The last part advances a short econometric exercise to check this generalisation. Urban competition combined with starting institutional quality does not emerge as a positive factor for the growth of European cities in general: this is interpreted as a call for more research rather a decisive counter-argument.

Oscar Gelderblom's book addresses a very important question for the formation of the modern world: the source of inclusive trade institutions². This is one of the many pieces in the jigsaw of the emergence of modern growth in Europe. Even if I am an economist and not a specialist of either the Low Countries or the pre-eighteenth century period, I am very happy to take part on the conversation of the subject, and I hope that by writing from the point of view of Sirius (with as many graphs and regression tables

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I thank to the guest editors Jessica Dijkman, Jeroen Puttevils and Wouter Rychosch for their comments. All errors remain mine.

² Oscar Gelderblom, *Cities of Commerce: The institutional foundations of international trade in the Low Countries, 1250-1650* (Princeton 2013)

as I could put in this short contribution), I will be able to contribute. The book has many qualities. These cannot be revealed to the reader in a collection of review essays, even if most are more interesting than mine. There is only one way to fully appreciate Oscar Gelderblom's book: to read it. This is the most useful piece of advice I can give here.

Its thesis, as I understand it, is that the source of inclusive trade institutions in early modern Europe was neither centred around limiting the power of the central state as discussed in works by Douglass North, Daron Acemoglu and their co-authors, nor around private order solutions discussed by Avner Greif³. It was rather urban competition, conditional on three elements. First, there must have been a large number of cities vying for foreign traders. Second, these foreign traders had to be footloose and ready to move between cities. Third, municipal governments had to enjoy political autonomy.

Gelderblom is quite convincing that the combination of these three factors induced the creation of successful trade institutions in Bruges, Antwerp and Amsterdam from 1250 to 1650. Yet, one might wonder if that alchemy was *sui generis* to this particular place and time. Can these conclusions be generalized outside the Low Countries? The world would be a better place than it is if we had such high-quality monographs on every urban experience in Europe. Instead of waiting for historians to transform themselves into the necessary cornucopia, economists are bound to try for shortcuts. The ambition of this short paper is to suggest one.

This paper is organized around three parts. The first part discusses how precisely some of the mechanisms underlying Gelderblom's explanatory framework actually worked. Most of the time, it is simply because I have enjoyed the book so much that I am asking for more. The second part presents some challenges to the generalisation of the book's thesis. The last part advances a short econometric exercise to check this generalisation.

3 Douglass Cecil North, John Joseph Wallis, and Barry R. Weingast, *Violence and social orders: A conceptual framework for interpreting recorded human history* (Cambridge 2009); Daron Acemoglu and James Robinson, *Why nations fail: the origins of power, prosperity and poverty* (London 2012); Avner Greif, *Institutions and the path to the modern economy: lessons from Medieval trade* (Cambridge 2006)

1 Merchants, authorities and sovereigns

Gelderblom's explores a long period of the history of three cities in a relatively short book. He cannot present every mechanism at work in a way that would preclude all questions. Accordingly, I was left with some regarding the behaviour of traders, town authorities and sovereigns.

I did not understand some of the economic behaviour of traders. I will give three examples. First, how could the genuine brokers coexist with the hostellers-cum-brokers in Bruges (p. 48, note 29) if they were more expensive and non-resident merchants needed the hostellers' services? Second, the book makes clear that the system based on hostellers-cum-brokers worked as long as the merchants were non-resident (p. 50). But how and why did the merchants become resident? Was it because the scale of merchant activity changed in such a way to force them to become resident? Was it some kind of dynamic process that made the hostellers-cum-brokers system self-defeating through the increase in the scale of merchant activity? Was it an external shock? Third, the book insists on the importance of the footloose character of foreign merchants. And yet, in the central case of Hans Thijs, the book, exploring the mechanisms allowing trust to be possible, underlines that (p. 64): "With [...] no other place to find gems so easily, it would be foolish for the jeweller not to meet this obligation". That does not seem very footloose to me. Why could the merchant not just take the jewels and run? Maybe what was really important was for the merchant group as a whole to be footloose, not for the merchants individually. This "school of fish"-like organisation of merchants would need to be explored in more detail. How did it work? Did individuals have a bond to the group, but not to the place?

The way individual behaviours coalesce into collective behaviour is interesting for municipal authorities as well. They are on the whole presented as institutional innovators benevolent to foreign traders, keen to adopt new institutions if they could attract more footloose merchants to their towns. What were the political economy and the intellectual mechanisms that explained that? This benevolence of town elders was not present in every region in Europe. Town elders in Northern Italian towns seem to have been much less willing to accommodate the wishes of foreigners. Why did they display more destructive behaviour? Was it because of specific local political economies? Were urban workers more docile to local elites in the Low Countries than in Italy, changing the nature of the competition for municipal power and making town elders stationary bandits confident they would reap the benefits from happy foreign merchants

through higher estate prices and tax collection? Was there a difference in the nature of town elders' mercantile activities? Where did they get their ideas? Did the merchants ever explicitly state what they were looking for? Was there any "theoretical" debate on what should be done to encourage trade? Who participated in this debate? Or did new institutional concepts circulate by word of mouth or example between geographically concentrated towns?

One source of institutional innovation might be law. The book discusses the interplay between Roman law and the existence (and incorporation) of customs (pages 120, 133, 136...). Yet, it does not enter the debate around La Porta and co-authors' thesis that the difference between common law and Roman law explains many cross-country economic dynamism differences.⁴ It is not clear if the book defends the idea that the law system of these cities should be seen as a common law system or the idea that the distinction does not make any sense (as the top of page 138 seems to suggest).

Looking further into urban political economy and institutional innovation, the book might have benefited from a closer mobilisation of Mediterranean sources and literature. First, as we have just seen, contrasting explicitly the political economies of the Low Countries and Italy would be worthwhile. Actually, developing a more systematic comparative approach would be useful. Because the book is focused on the history of Bruges, Antwerp and Amsterdam, it sometime fails to provide comparative figures from other towns that would allow the reader to evaluate if a number is small or large. The book insists on the importance of notaries, yet fifteen in Antwerp in 1585 does not seem like a lot (page 92). What is the total number of acts compared with other merchant towns, or compared to the possible total number of agreements, formal and informal, between merchants? In the same way, 0.34 percent chance of appearing in front of the Grand Conseil de Malines between 1450 and 1550 seems high to me, even though the book presents it as a small number (page 129). *A fortiori*, 2 percent chances of appearing each year in front of the Court of Holland to settle a commercial dispute seems very high (page 131), especially as this probably only concerns the gravest disputes.

The second reason why a closer mobilisation of Mediterranean sources and literature would have been useful is because Mediterranean traders were important actors in the Low Countries and may have played a role in the processes of institutional exchange and diffusion. Certainly, the traders of different nationality had contrasted experiences. For example the

4 Rafael La Porta et al, 'Law and finance', *Journal of Political Economy* 106:6 (1998) 1113-1155.

French never seem to obtain any privilege, though they were pretty active in Bruges (page 117) and played a sizeable role in the Grand Conseil de Malines (page 128).

Finally, the explanation of the behaviour of the sovereigns and their motivations would also warrant more exploration. The book affirms on page 153: “By 1360 [...] The Count of Flanders needed the foreign merchants [...] not because they brought in large tax revenues [...] or because they acted as bankers for the count [...] but because their presence stimulated the local economy”. Page 169 suggests foreign merchants stimulated the local economy because they were “unique suppliers of spices, woollens and grain”. That seems plausible enough, but why was that important for the Count of Flanders if he did not benefit either through easier access to capital or increased income? Was he actually conducting an “economic policy” in the fourteenth century believing, for example, that increased prosperity would make his political power more secure?

2 Can the hypothesis be generalised?

All the preceding points might simply be linked to my lack of understanding of the mechanisms explored in the book. There is however a more fundamental question: can the book’s hypothesis on the source of trade institutions be generalized? It is not obvious one can make such an important point on European history while looking at only three cities and one region.

Geographically, if the crux of the argument is the importance of inter-urban competition, maybe the ideal place to look for it is Italy? Map 1 shows Italian political fragmentation in 1300. It was still quite intense, despite a process of consolidation that started in the 1230s.⁵ *Prima facie*, the particular elements mentioned by the book seem to have been present in medieval Italy. Certainly Italy was not excluded from the European institution building process. Presenting Northern Italy as another example validating the hypothesis defended by the book would reinforce its thesis. However, the subsequent relative decline of Italy suggests that maybe competition between city-states was too intense in late medieval Italy and led to the demise of the whole system.

5 Stephan R. Epstein, ‘The rise and fall of Italian city-states’, in: Mogens Herman Hansen (ed), *A Comparative study of thirty City-State cultures : an Investigation* (Copenhagen 2000)



Map 1. Italy in 1494

Source: http://en.wikipedia.org/wiki/File:Italy_1494.svg under the Creative Commons Attribution-Share Alike 3.0 Unported licence

Later in time, it seems that the dominant centre of institutional innovation, London, was not in a situation of municipal competition (the book discusses this point on page 206). One might want to extend the pertinent geographical range of municipal competition so much that London is actually seen as being in competition with all other North Sea cities. Yet, maybe the lack of urban competition inside Britain can be seen as bringing the interplay between the city and the sovereign to the forefront. That

would take us back to a story *à la* North. Another reason to push in that direction is that one suspects that the relations between the “town elders” in London and the English parliament were much more about double capture than about autonomy, especially after the decline of the importance of guilds linked with regulated international trading in the city during the second half of the seventeenth century.⁶

Looking at medieval Italy and mercantilist Britain suggests that maybe the three cities studied are at a “sweet spot” for the type of urban competition the book describes. That was not the way merchant activity was encouraged before or after, and the hypothesis it defends is valid “only” for the Low Countries between 1250 and 1650. This “only” is obviously slightly ironic, because finding a proper way of understanding the rise of merchant institutions during this crucial period is by itself an important task.

That is the direction suggested by Larry Epstein’s argument about the evolving optimal level of state sovereignty.⁷ In his view, institutional transaction costs, and hence economic activity are a function of the prevalence of prisoners’ dilemmas and coordination failures. Competition between sovereign entities encouraged emulation, the development of economic activity, and the enlargement of its geographic scale. At some point, however, economic activity was so extended that sovereign competition prevented coordination on a large enough geographical scale and made the preceding political structure inefficient, leading the way to a new political structure (see Figure 1).

6 David Ormrod, *The rise of commercial empires: England and the Netherlands in the age of mercantilism, 1650-1770* (Cambridge 2003)

7 Stephan R. Epstein, *Freedom and growth: The rise of states and markets in Europe, 1300-1750* (London 2000).

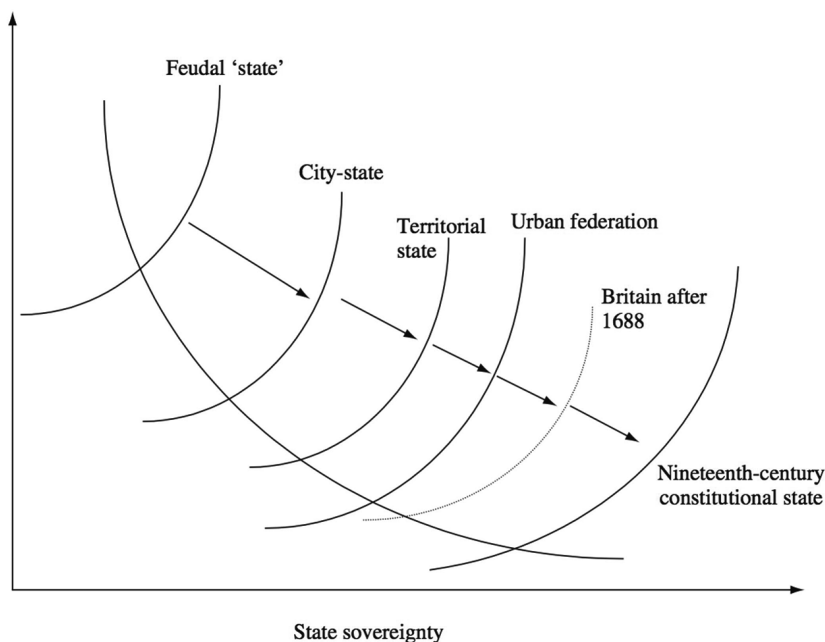


Figure 1. Political structure and institutional transaction costs (vertical axis)

Source: Stephan R. Epstein, 'The rise of the West' in John A. Hall and Ralph Schroeder (eds), *An anatomy of power: The Social Theory of Michael Mann* (Cambridge 2006) 254

In that perspective, the territorial state and the urban federation that are under examination in the book are only particular stages in European development. Maybe Avner Greif's point of view about the crucial ingredients for institutional building was true before (or elsewhere), and North and Acemoglu's point of view was true after (or elsewhere).

3 How can we test the hypothesis?

Would it be possible to devise a falsification test to verify how much the hypothesis in the book explains the European experience in general and the experience of the three cities in particular? Though it is well beyond the ambition of this short paper to do that, I would like to illustrate what could be done and how it could be done. I will do that with a shameless self-publicity plug. I am currently exploring the results of Daron Acemoglu, Simon Johnson, and James A. Robinson on the interplay between Atlantic

trade, institutions and growth in Europe.⁸ One of their main points is that Atlantic trade had an effect on growth, conditional on good starting institutions. However, they have not been interested in looking at whether the positive effect of Atlantic trade was really national or rather local. The logic of their argument implies that the effect was national, as it goes through the reinforcement of good national institutions. If the nation is not the right geographical entity to study early modern Europe, and if the effect goes through local capital markets or local institutions (as ‘Cities of Commerce’ implies), it might be that the effect is actually local rather than national.

This study necessitates a measure of economic success at the local level. That would also be the case for a test of the ‘Cities of Commerce’ hypothesis. The most natural one is the size of cities. Thanks to the databases gathered first by Paul Bairoch, such data are available for Europe in the long run.⁹ Maarten Bosker, Eltjo Buringh and Jan Luiten van Zanden have corrected the database.¹⁰ Thanks to the editors, I have integrated the latest estimates for the population of Amsterdam and Antwerpen.¹¹ Using city size as a measure of success is not ideal. It does not take a large population to be a successful trading hub. Yet, success should encourage in-migration and urban growth. For example, Bruges, despite its commercial success, experienced population decline every century between 1300 and 1600. The fourteenth century decline can be attributed to the Black Death, but Bruges was not recovering as fast as the rest of the Low Countries and Europe from 1400. In contrast, the growth of Antwerp in the fifteenth century and the growth of Amsterdam in the sixteenth and seventeenth century do seem to reflect a specific urban dynamism, even when compared to the rest of the Low Countries or Western Europe.

8 Daron Acemoglu, Simon Johnson, and James A. Robinson, ‘The rise of Europe: Atlantic trade, institutional change and economic growth’, *American Economic Review* 95:3 (2005) 546-579

9 Paul Bairoch, Jean Batou, and Pierre Chèvre, *La Population des villes européennes : Banque de données et analyse sommaire des résultats 800-1850 / The Population of European cities: Data bank and short summary of results* (Genève 1988)

10 Maarten Bosker, Eltjo Buringh, and Jan Luiten van Zanden, ‘From Baghdad to London: Unraveling urban development in Europe, the Middle East, and North Africa, 800–1800’, *Review of Economics and Statistics* 95:4 (2013) 1418-1437

11 Jan Van Roey, ‘De bevolking’, in: Walter Couvreur (ed), *Antwerpen in de XVIde eeuw*, (Antwerpen 1975) 95-108; Piet Lourens and Jan Lucassen, *Inwonertallen van Nederlandse steden, ca. 1300-1800* (Amsterdam 1997) 55-57.

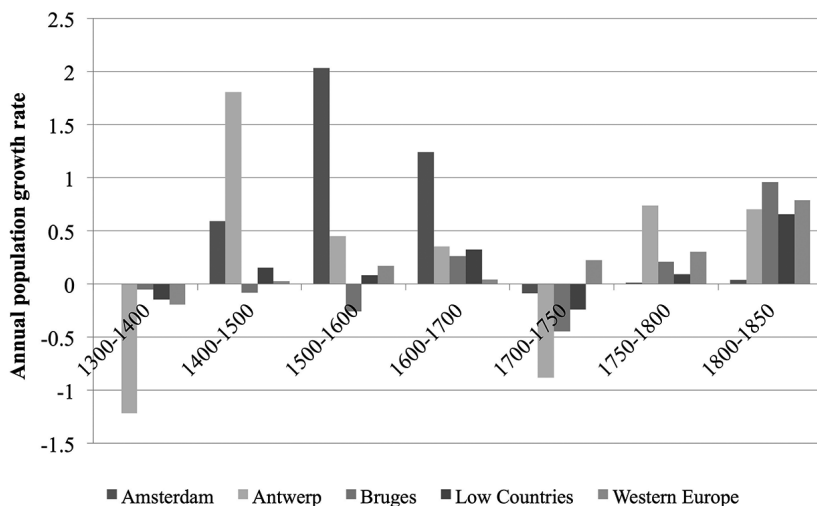


Figure 2. Annual population growth, 1300-1850

Source: Bairoch, Batou, and Chèvre, *La Population des villes européennes*.

The seminal paper studying the dynamics of city growth using this database has been the one by Maarten Bosker, Eltjo Buringh and Jan Luiten van Zanden on the dynamics of urban development in Europe, the Middle East and North Africa.¹² Taking their cue from economic geography, they use an “urban potential” variable to measure the interdependence of city growth. Is computed, for each city, as the sum of the population of all other cities inversely weighted by distance. This variable can also be understood as a measure of urban competition, and should play a central role in a test of the ‘Cities of Commerce’ hypothesis.

Intense urban competition is only one of three conditions ‘Cities of Commerce’ puts forward for the design of merchant institutions. The second one is municipal autonomy. Measuring the actual autonomy of more than a thousand towns would be problematic. Staying as close as possible to the original paper by Acemoglu and his co-authors, I use their “starting institutions” variable. It measures the strength of institutions that constrained the power of the monarchy and allied groups. In late medieval Europe, that could be taken as a proxy of municipal autonomy, as cities were one of the main alternative rule-makers to sovereigns.

The last condition is the presence of footloose traders. This is impossible to measure in a systematic way across all of Europe for so long a time. A

12 Bosker, Buringh, and van Zanden, ‘From Baghdad to London’.

close proxy might be participation in long-distance trade. This is not available either. Following again Acemoglu and his co-authors, I use direct participation to Atlantic trade (defined as trade with Sub-Saharan Africa, America and Asia). This can at least be quantified at the country level throughout the period. It is problematic as well, as current Belgium did not participate directly in Atlantic trade such defined, excluding Antwerp and Bruges from that qualification. Furthermore, Atlantic trade was not the important moving factor at the beginning of the period under study. I could not think of any other simple way to introduce this hypothesis, however. I am sure the reader will keep this caveat in mind while examining the results of the exercise.

Partly to solve these missing variables issue, we use city fixed effects in the following regression exercises. If we consider the presence of traders in the Low Countries to be a permanent characteristic, they should control for that. Their main limitation is that they do not allow us to study the interplay of the three aspect of the hypothesis. In other words we are not able to measure the combined effect of urban competition, municipal autonomy and a large number of traders.

Table 1 shows the result of a panel regression with fixed effect relating city growth with various explanatory variables. Four variants are given: including only cities more than 5,000 from 1300 to 1850 ("Balanced panel"), including all cities ("Unbalanced panel"), including only urban competition and starting institutions variables (columns 1 and 3), including also Atlantic-trade related variables (columns 2 and 4). Atlantic-trade related variable includes an Atlantic-trade potential, which is a measure of urban competition restricted only to Atlantic trade cities and computed the same way as urban potential.

Table 1. Explaining city growth in Europe (1300-1800)

City and year dummies everywhere Western Europe dummies everywhere Weighted by city populations	Balanced panel (1) Without trade variables	Balanced panel (2) With trade variables	Summary statistics Unweighted	Unbalanced panel (4) Without trade variables	Unbalanced panel (3) With trade variables	Summary statistics
Explained variable: annual urban population growth			0.21 (0.56)			0.41 (0.69)
Log(city population)	-0.38*** (0.04)	-0.49*** (0.04)	3.21 (0.83)	-0.51*** (0.03)	-0.60*** (0.03)	2.56 (0.74)
Log(country-specific volume of Atlantic trade) x being an Atlantic port		0.05 (0.08)	0.16 (0.80)		0.06 (0.06)	0.16 (0.82)
Log(country-specific volume of Atlantic trade) x being part of a country participating to Atlantic trade		-0.21*** (0.05)	0.18 (1.06)		-0.19*** (0.03)	0.20 (1.14)
Log(country-specific volume of Atlantic trade) x being an Atlantic portx starting institutions		0.05 (0.07)	0.84 (1.60)		0.04 (0.05)	1.35 (1.99)
Log(country-specific volume of Atlantic trade) x starting institutions		0.19*** (0.05)	0.81 (1.86)		0.17*** (0.03)	1.42 (2.58)
Log(Urban potential)	0.30 (0.18)	0.16 (0.24)	2.63 (0.60)	0.61*** (0.12)	0.39*** (0.16)	2.87 (0.61)
Log(Urban potential) x starting institutions	-0.08* (0.05)	-0.17** (0.09)	3.15 (2.41)	-0.07** (0.03)	-0.09 (0.06)	3.28 (2.58)
Log(country-specific volume of Atlantic trade) x Log(1+ Atlantic trade potential)		0.32 (0.25)	0.28 (0.37)		0.27* (0.16)	0.45 (0.44)
Log(country-specific volume of Atlantic trade) x Log(1+ Atlantic trade potential)x starting institutions		-0.05 (0.14)	0.33 (0.58)		-0.14 (0.09)	0.51 (0.72)
Constant	0.99** (0.36)	1.93*** (0.41)		0.52** (0.24)	1.24*** (0.27)	
Observations	924	924		3,372	3,372	
R-squared	0.53	0.57		0.66	0.68	

Source: author's computations. Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. In the descriptive statistics, the top number is the mean and the bottom one the standard deviation. Reading explanations: see text

Explaining variables are introduced in logs. This is a standard procedure that allows approximately to interpret the coefficients in terms of semi-elasticities, *id est* the association between changes of the explained variable in percentage points and changes in the explaining variables in percentage (not percentage points). For example, the first specification suggests that *ceteris paribus*, increasing city size by 10% is associated with a decreased of annual city population growth by 0.036 percentage point ($-0.036 = \log(1.1) * (-0.38)$). To put that result in perspective, mean annual city growth is 0.21 percent in the sample. So a city with the mean growth rate would see it reduced to 0.174 if its size were to increase by 10%.

The quantitative exercise suggests the book's hypothesis cannot be easily generalised to the whole European experience. Urban competition is always associated with faster city growth – though the result has more than 10 percent probability of being due to chance in the balanced panel estimates. However, the interacted coefficient between starting institutions (which we are using as proxies of municipal autonomy) and urban potential is negative, suggesting that better urban institutions actually dampened the positive effect of urban competition. The interacted coefficient between starting institutions, Atlantic trade potential and actual country trade is also negative, suggesting – if one is ready to go on a limb, and accept that participation in Atlantic trade is a good indicator of the presence of footloose merchants – that the presence of footloose merchants reduced a bit more the combined effect of urban competition and municipal autonomy.

One should not make too much of these computations (though I doubt any of my readers are tempted to do so). The first reason is that, as we have discussed, urban competition is approximately the only thing that we are measuring with some degree of confidence. We are missing good proxies for municipal autonomy and the presence of footloose merchants. The second reason is that the book does not pertain to explain the sources of city growth, but the sources of merchant institutions: we do not have a good outcome variable to test this result. The case of Bruges suggest that population growth is not a good measure of success. Figure 3, Figure 4 and Figure 5 illustrate the third reason. These figures compare the actual growth of Amsterdam, Antwerp and Bruges with what the model (specifically equation 4) predicts. In the sixteenth and seventeenth century, the growth of Amsterdam and Antwerp was faster than what the model predicts, even if it “explains” a fair share of the “surplus growth” of Antwerp in the fifteenth century and Amsterdam in the sixteenth and seventeenth century. It does not explain the subsequent relative decline. The exercise

barely makes sense for Bruges, and other discrepancies abound. Important time-specific aspects of the experience of these three cities would have to be taken into account to reconcile reality and this quantitative exercise, as currently a large part of the action is happening in the residual.

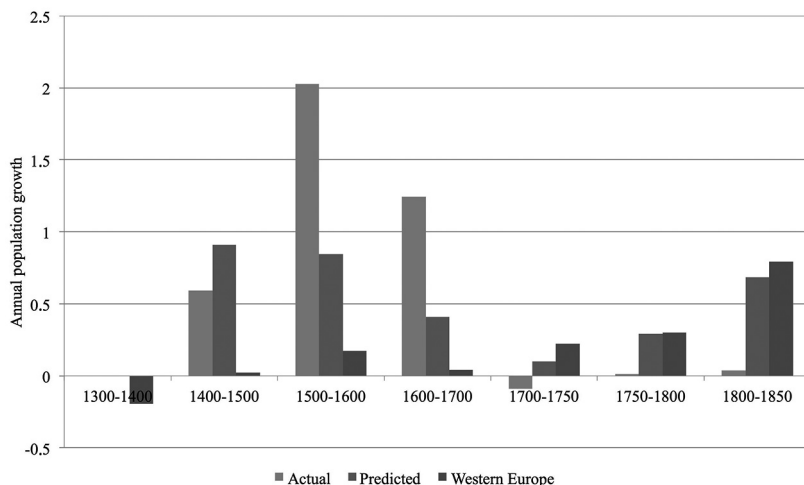


Figure 3. The model and Amsterdam

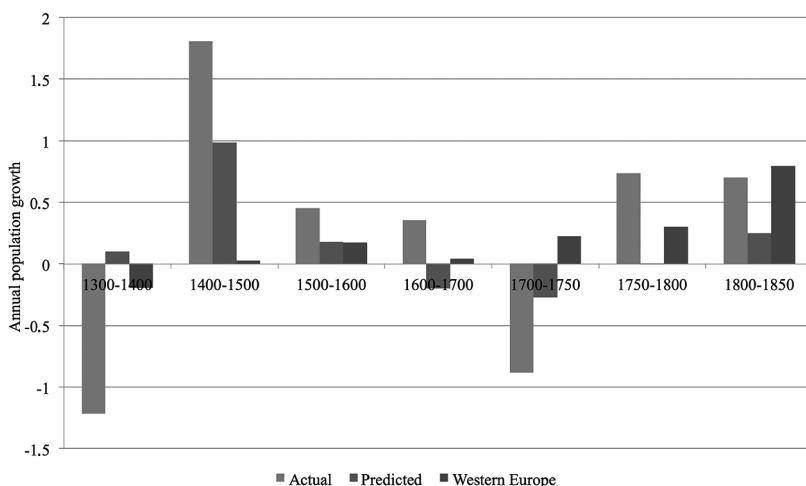


Figure 4. The model and Antwerp

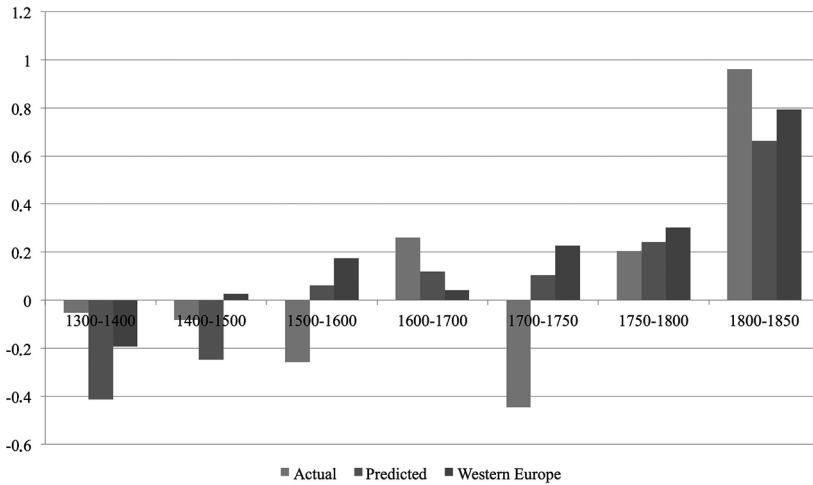


Figure 5. *The model and Bruges*

4 Conclusion

This paper has explored ‘Cities of Commerce’s’ thesis on the source of efficient trade institutions in early modern Europe. After wondering about the behaviour of traders, town authorities and sovereigns, reflecting about generalisation and trying for a quantitative effort to do just that, I realize that my comments are a bit contradictory. On the one hand, I would like to explore the situation more precisely, but, on the other hand, I am also very interested in systematic comparisons that entail numerous simplifications. The appropriate dose of generalization is difficult to find.

In the current state of knowledge, the book’s approach, which could be characterized as deep reflection based on three case studies, is more fruitful than the tentative quantitative approach presented here. This latter approach is confused about how to measure Bruges’s success and, even if Amsterdam and Antwerp success can be partly explained by common factors, it does not capture the whole story. None of this is a surprise. What is more of a surprise is that urban competition combined with starting institutional quality does not emerge as a positive factor for the growth of European cities in general. Yet, getting better data on municipal autonomy and traders’ numbers would be necessary to take this test seriously. This might necessitate reducing the sample, illustrating again the tension between precision and generalisation.

The danger of course would be the absence of dialogue between researchers with different opinions on the ideal position of the dial. I have learned a lot from this book. I hope it will inspire all kind of works on its important subject. Monographs and econometric exercises of the world, unite!

About the author

Guillaume Daudin (1973) is Professeur des Universités in Economics since 2008, in Université Paris-Dauphine. He is also an associate researcher at the OFCE (SciencesPo). He has published on early modern economic history and current globalization: «Who produce for Whom in the World Economy?», with Christine Riffart and Danièle Schweisguth, *Canadian Journal of Economics*, vol. 44, °4, p. 1403-1437, 2011; "Domestic Trade and Market Size in Late 18th century France", *Journal of Economic History* vol. 63, n°3, 2010, *Commerce et prospérité: la France au XVIIIe siècle*, Presses Universitaires de Paris-Sorbonne, 610 p, 2005 (new edition: 2011); « Profitability of slave and long distance trading in context: the case of eighteenth century France », *Journal of Economic History*, vol. 64, n°1, 2004... He is currently heading a research project on the 18th century French international trade (<http://toflit18.hypothese.org>).

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