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City Growth in Europe

By

Volker Nitsch

Duncker & Humblot · Berlin

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Berlin, February 2001 *Volker Nitsch*

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Chapter 1

Introduction

In the 1990s, there has been a broad revival of interest in economic geography, the location of economic activity in space. According to *Econlit*, the share of articles with JEL classification code R covering "urban, rural, and regional economics" in four top economics journals[1] has risen considerably over the past decade, almost doubling from an average of about 1.6% in the period from 1986 to 1988 to 2.9% ten years later. Also in the policy context, issues in urban economics have attracted greater attention. The World Bank, for instance, has recently devoted two complete chapters of its *World Development Report 1999/2000* to cities.

One of the most notable features of this "new economic geography" is a close association between theoretical and empirical work. In contrast to earlier research, theoretical studies appear to be far more strongly focused on real-world phenomena. Recent examples include the role of natural advantages in the making of major cities (e.g., Fujita and Mori [1996]), the potential impact of trade liberalization on peripheral regions (e.g., Krugman and Venables [1990]), and the evolution of hierarchical urban systems (e.g., Fujita, Krugman and Mori [1999]). Moreover, new modeling techniques also allow to address complex issues in greater detail.

At the same time, empirical work is often much more closely tied to theoretical models. Instead of purely detecting possible stylized facts, considerable efforts have been made to test for the relevance of theoretical results. Donald Davis and David Weinstein (1996, 1999), for instance, have analyzed in a series of papers the empirical importance of the home market effect, as suggested by Krugman (1980). Another example is Gordon Hanson (1998) who provides an interesting attempt to estimate a market potential function, implied by new geography models.

A major shortcoming of recent empirical work in urban economics is, however, the startling concentration on basically only two estimation strategies. Probably driven by the limited availability of data, most of the analyses are either cross-country studies which usually seek to explore a data

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[1] The examined journals are the *American Economic Review*, the *Quarterly Journal of Economics*, the *Journal of Political Economy*, and the *Review of Economics and Statistics*.

set as rich as possible or the studies examine single country data and then often focus on U.S. experiences.

This thesis aims to provide a new – European – perspective. The basic idea is that a focus on European cities, apart from being interesting for itself, allows to combine the advantages of both previous approaches. In particular, there is considerable cross-country variation while, in addition, also reliable historical data is available. Therefore, it is one of the contributions of this thesis to compile a new data set of European cities which covers 13 countries and ranges from 1870 to 1990.

This data set is then applied to explore several hypotheses which have been recently proposed in the literature. In fact, as the field of urban economics is emergent and dynamic, there are a number of interesting and innovative suggestions which virtually cry out for further examination. On the theoretical side, it is often necessary to sort through which of the intriguing possibilities indicated by economic models are truly relevant or need to be elaborated further. On the empirical side, evidence is often only informative or not convincingly robust and therefore has to be investigated in far more detail, examining different contexts and applying alternative econometric methods. Inspired by the spirit of the new economic geography then and thus closely connecting theoretical and empirical aspects, three sets of issues are discussed in this thesis: the growth pattern of cities and their implications for Zipf's law, the relationship between trade openness and urban concentration, and the role of history for city growth.

Chapter 2 begins with an analysis of Zipf's law, the striking empirical regularity that the number of cities with a population larger than S tends to be proportional to $1/S$. Surprisingly, there is still no convincing explanation for this astonishingly stable pattern in the size distribution of cities, even though the empirical regularity is known for at least 80 years now. The best available answer then is a model of random growth of cities – an idea which has been recently formalized by Xavier Gabaix (1999) who shows that a scale-invariant growth process produces a final distribution that follows a power law. The analysis in chapter 2, however, raises some doubts whether there is really random growth across cities. The results rather suggest that there *is* an empirical relationship between city size and subsequent growth, but with a changing sign over time. Nonetheless, Zipf's law seems to hold for the European countries in the sample with reasonable precision.

Chapters 3 and 4 explore another interesting recent hypothesis which can be appropriately analyzed with the data at hand. In a provocative paper, Krugman and Livas Elizondo (1996) have suggested that protectionist trade policies are a major cause of large central cities. Based on anecdotal evidence from Mexico, they develop a simple theoretical model in which ex-

ternal trade liberalization promotes spatial deconcentration. As the model is basically solved through simulations, however, chapter 3 provides a detailed sensitivity analysis and allows for several extensions, showing that the theoretical results are not robust. Specifically, it is shown that, for a particular range of plausible parameter values, trade does not affect urban concentration.

Chapter 4 then turns to the empirical analysis. Looking at a long time series from 1870 to 1990, the results are not convincing. While there is indeed a negative association between openness and the size of a country's largest city in the last few decades, confirming earlier findings for this time period (e.g., Ades and Glaeser [1995]), the results become insignificant for earlier years and alternative measures of urban concentration. Thus, the empirical evidence for an association between external trade and internal geography turns out to be shaky, at best.

Chapter 5, finally, examines the impact of history on city growth. Here, it is argued that the dissolution of the Austro-Hungarian Empire in 1918 provides a natural experiment to analyze the existence of path dependence. Specifically, if history matters, one would expect that the dramatic reduction in the country's population and territory has no measurable effect on the subsequent development of the largest city, Vienna. The Austrian experience, then, is in favor of lock-in effects. While Vienna's urban dominance declines relative to other European capitals in the sample immediately after the break-up, this effect quickly runs out. Despite its overdimension, Vienna's primacy even starts to *increase* again a half century after the dissolution of the Habsburg Empire, indicating that there is a strong pattern of path dependence in city growth.

In conclusion, the three examples in this thesis nicely illustrate the variety of interesting challenges for empirical work in urban economics and the extent to which a new data set can be used to address these seemingly disparate issues. The European experience then provides a rich laboratory of real-world data which still waits to be explored.