Volkswirtschaftliche Schriften

Heft 518

City Growth in Europe

By

Volker Nitsch

Duncker & Humblot · Berlin

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Volkswirtschaftliche Schriften

Begründet von Prof. Dr. Dr. h. c. J. Broermann †

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Die Deutsche Bibliothek – CIP-Einheitsaufnahme

Nitsch, Volker:
City growth in Europe / Volker Nitsch. –
Berlin : Duncker und Humblot, 2001
(Volkswirtschaftliche Schriften ; H. 518)
Zugl.: Berlin, Humboldt-Univ., Diss., 2000
ISBN 3-428-10499-4

Fremddatenübernahme: Klaus-Dieter Voigt, Berlin
Druck: Werner Hildebrand, Berlin
Printed in Germany

ISSN 0505-9372
ISBN 3-428-10499-4

Gedruckt auf alterungsbeständigem (säurefreiem) Papier
entsprechend ISO 9706 ♾

Acknowledgements

To write a dissertation while working full-time as an economist in a private bank is a daunting task. That I was nonetheless able to realize this project and even had fun in doing so is mainly the result of generous support from a number of people who have contributed in one way or another to the completion of this thesis and to whom I owe special debt.

First and foremost, I am grateful to my thesis advisors, Dalia Marin and Michael Burda. They generated in me excitement to think about regional issues in economics and helped to shape my thought in the area in important ways. Beyond their intellectual guidance, however, they provided the personal encouragement and support I needed to complete this work. Finally, I am most grateful for the perspective and understanding they offered me on this non-standard career path.

I also owe special thanks to Dietrich Beier, the former chief economist of Bankgesellschaft Berlin, who supported this project from its beginning. Although the content of this work is completely unrelated to my assignments in the bank, he continuously spent motivation by asking how the project is evolving. Also his successor as chief economist, Heinz Grimm, aided this work in many different ways: from giving me full scope in organizing my bank duties to funding several of my research activities.

While working on this dissertation, it was an interesting experience to discover that another German economist, Karsten Junius, was completing his own doctoral thesis on related topics at the Kiel Institute of World Economics. All parts of the work have been improved by his comments.

Also, this work benefitted from the discussion of seminar participants at Humboldt University, the University of Munich, and the 1996 and 1998 EEA meetings.

Lastly, but most importantly, I thank my family for their incredible support. In particular, I am indebted to my wife Coretta for being indulgent when I was working on this project at home or spending countless hours in libraries. This thesis could not have been completed without her help.

Berlin, February 2001 *Volker Nitsch*

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DOI https://doi.org/10.3790/978-3-428-50499-2

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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

[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 external 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.

Chapter 2

Some Empirics on Zipf's Law for Cities

2.1 Introduction

One of the most striking empirical regularities in urban economics, if not in economics or the social sciences in general, is the rank-size rule which states that the (log of the) population of a city tends to be inversely proportional to (the log of) its rank in a country's urban system. That is, if one sorts the cities within a country by population, it turns out that the number 2 city has about one-half of the population of the largest city, the number 3 city about one-third that population, and so on. More generally, the number of cities whose population exceeds some size S is approximately proportional to $1/S$ or, mathematically, $Prob(Size > S) = bS^{-a}$, with $a \cong 1$. Thus, the size distribution of cities appears to follow a power law with exponent 1, which is also often referred to as the rank-size rule or Zipf's law.[1]

The finding of a log-linear relationship between the rank and the population size of a city (quite apart from the slope of –1) is surprising already for itself since there is nothing in the data causing them to generate this pattern automatically. However, Zipf's law is particularly striking with two respects. On the one hand, the empirical fact appears to be extremely robust. Dobkins and Ioannides (1999), for example, show that the rank-size rule is valid for more than a century of U.S. data. Also in a broad cross-section of countries, the power law seems to describe the size distribution of cities fairly well. Examining a sample of 44 countries, Rosen and Resnick (1980) estimate a mean exponent of 1.136.[2] Given that empirical results in economics are often fragile or subject to qualifications, this almost

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[1] The law is named after George Zipf (1949), who collected a number of empirical regularities in the social sciences. However, the observation of a rank-size relationship in national city-size distributions dates at least back to Auerbach (1913).

[2] Therefore, outside the U.S., the exponent tends to be somewhat larger than the value of 1 implied by the rank-size rule, indicating that the populations in most countries are more evenly distributed. Rosen and Resnick (1980), however, also show that the estimated exponent is quite sensitive to the definition of the city. When applying data for metropolitan areas instead of city data (for a small subsample for which both data were available), they find that the estimated exponent gets closer to 1.

DOI https://doi.org/10.3790/978-3-428-50499-2

universal applicability of the rank-size rule is astonishing or, as Paul Krugman (1996b, p. 40) puts it, "spooky".

On the other hand, the regularity is apparently hard to explain. In fact, although the existence of a power law in national city size distributions is a stylized fact for more than 80 years now, economic models of urban systems have largely ignored this issue or, more precisely, are unable to reproduce this result. The most promising approaches to explain Zipf's law for cities, then, refrain from economic analysis and model the emergence of a power law distribution as the result of random growth processes. An early example for this line of research is Simon (1955), and also Xavier Gabaix's (1999) recent explanation is based on a stochastic growth model.

These explanations, however, begin to shift the focus back to the empirical side again. In fact, even though the good fit of Zipf's law seems to be an established fact now, an (arguably even growing) number of issues has to be investigated empirically. A first set of questions, for example, refers to the proposed theoretical explanations. Do cities really grow randomly across a certain range of city sizes so that there is no observable difference in the growth rate between large, medium and small cities? Furthermore, focusing on Gabaix's (1999) approach, is also the variance of the growth rate independent of city size?

A second set of questions arises from the almost universal validity of the rank-size rule. If Zipf's law holds for countries with very different economic structures and histories, what explains deviations from an exponent of 1? Apart from the above mentioned measurement problems, the standard explanation seems to be the existence of large central cities, with their size possibly distorted by special factors. But, does the exclusion of a country's largest city improve the empirical fit of the rank-size rule? Another possible candidate is that the cut-off point of cities used in the sample is too large and that the gradual inclusion of smaller cities would decrease the Zipf parameter (see Gabaix [1999, pp. 756–758]). But, do smaller cities really have a lower Zipf exponent?

This chapter deals with some of these issues, exploring data from 11 European countries. To preview the main results, national city size distributions in the sample generally follow Zipf's law with reasonable precision. However, there is basically no evidence for some of the proposed explanations for empirical deviations from an exponent of 1. Also the evidence for both random growth and equal variances across city sizes is not convincing. In sum, there is considerable variation in national experiences.

The chapter is in six parts. To provide some motivation for the empirical analysis, section 2 starts with a short presentation of some theoretical attempts to explain the rank-size rule using models of random growth. Section 3 then explores the rank-size relationship for a number of European countries, covering a time span of more than 100 years. Section 4 examines the distribution dynamics in the national urban systems in more detail. Section 5 argues that the break-up of the Austro-Hungarian Empire in 1918 provides some sort of a "natural experiment" to analyze a city size distribution which can be assumed to converge to its new steady state, and section 6 concludes.

2.2 Theoretical Explanations

Given the striking empirical robustness of Zipf's law for cities, it is not surprising that there have been a number of attempts to explain this regularity. Basically, two lines of research can be distinguished. On the one hand, there is some work which seeks to provide an economic rationale for this pattern. The basic aim is to build a model which generates a city size distribution that obeys Zipf's law.[3] However, while it may be possible to show that an urban hierarchy model produces a Zipf distribution for certain specific parameter assumptions, the main difficulty of these attempts is that they do not explain the actual key feature of the rank-size rule: the robustness for a wide range of countries and time periods and, in effect, economic conditions.

The probably more promising approach is therefore to derive Zipf's law as the result of random growth processes. Fascinated by innovations in other fields of sciences, especially Paul Krugman (1996b, 1996c) has promoted this line of research which partly borrows from physics. The basic underlying idea is that there is scale invariance in the growth rates of cities. Because cities display on average the same pattern of growth at all scales, size would be more or less irrelevant. If this assumption is correct, however, then it is possible to show that the resulting distribution of cities is also scale-invariant and can be described by a power law.

Consider, for example, the thought experiment by Herbert Simon (1955).[4] Suppose that a country's urban population grows over time and that new migrants locate at a previously unpopulated place (i.e., form a new small city) with some probability π and go to an existing city with probability $1 - \pi$, with the probability that they choose to locate in any particular city proportional to its population. After some algebra, it can be shown that the resulting size distribution of cities indeed follows a power law, with expo-

[3] Actually, this work is rather rare. Instead of forceful attempts to model Zipf's law as the outcome of economic interactions, theoretical work in urban economics has largely neglected Zipf's law and, thus, often yields unrealistic results.

[4] See also Krugman (1996b) and Fujita, Krugman and Venables (1999) for detailed expositions of Simon's (1955) model.

nent $1/(1-\pi)$. However, while Simon's model represents a remarkable break-through, it is not the whole story. In fact, there are at least two problems, both related to the empirical finding of a Zipf exponent of 1. First, at $\pi = 0$ (which is needed to get an exponent of 1), there is a degeneracy problem. As Krugman (1996b, pp. 96–97) shows, a value of π close to 0 implies that it requires a very large (at the limit infinitely) increase in population to produce a smooth power law distribution. Second, Gabaix (1999) notes that a Zipf exponent of 1 requires in Simon's model that the growth rate of the number of cities is equal to the growth rate of the population of existing cities and argues that this is a highly unrealistic and actually counterfactual proposition.

In view of these difficulties, there have been some other attempts to use a stochastic growth model to explain Zipf's law. An interesting recent contribution is Gabaix (1999). Departing from Gibrat's law which states that a growth process is independent of size and additionally assuming that also the variance of the growth rate is uncorrelated with size, he demonstrates that the limit distribution will converge to Zipf's law. The basic idea can be illustrated as follows. Let S^i $(i = 1 \ldots N)$ denote the normalized size of city i so that $\Sigma^N S^i = 1$. Assume then that cities grow at some common mean rate, but with some idiosyncratic shocks γ^i. Hence, the growth process can be described by $S^i_{t+1} = \gamma^i_{t+1} S^i_t$, where the γ^i_{t+1}'s are independently and identically distributed random variables. Further, since the average normalized size must be constant, $E(\gamma) = 1$ and the mean normalized growth rate is 0. If one denotes the tail distribution of city sizes by $G_t(S) := Pr(S_t > S)$, the equation of motion is $G_{t+1} = E[G_t(S/\gamma_{t+1})]$, or in steady state $G = E[G_t(S/\gamma)]$. Finally, Gabaix proves that a Zipf law distribution of the form $G(S) = a/S$ is the only steady-state distribution, which satisfies this equation.

In sum, Gabaix (1999) shows that if all cities over some range of sizes follow a proportional growth process with the same expected growth rate and the same standard variation, the limit distribution will converge to Zipf's law. The main question then is whether these two assumptions are reasonable. Preliminary empirical evidence (e. g., Eaton and Eckstein [1997]) is supportive but shaky. Therefore, in the next sections, I will attempt to verify these propositions.

2.3 Evidence on Zipf's Law in Europe

Although there is already a rich empirical literature on Zipf's law, I will examine in a first step the robustness of this regularity for the countries in my sample. The aim is threefold. First, before it is possible to examine whether conditions for the emergence of Zipf's law are fulfilled, it is of course necessary to make sure that the data itself display a Zipf distribution. Second, as a perfect fit of Zipf's law for all countries and time periods in the sample is rather unlikely, I am also able to explore some of the proposed explanations for empirical deviations from an exponent of 1. Finally, an examination of national city size distributions also provides some intuition for particular characteristics of the data.

The sample comprises data from ten European countries: Belgium, Denmark, Finland, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, and Switzerland.[5] The main advantage of this focus on a limited set of countries is the availability of fairly reliable historical data. In fact, for several countries, the data series on city population reach back to 1870. Given mostly moderate changes in city size distributions over time, the analysis often proceeds in 20-year-intervals.

The plan for the remainder of this section is as follows. The next subsection describes the data sources in detail. Then, the basic results on the validity of Zipf's law for the countries in the sample are presented, and a concluding subsection examines empirical deviations from an exponent of 1.

2.3.1 Data

Apparently, the most critical issue in analyzing empirical city size distributions is the problem of data selection and data transformation. In fact, previous studies have shown that small variations in the definition of the data set can have a large impact on the results. For example, Dobkins and Ioannides (1999) and Black and Henderson (1998) both provide estimates for the U.S. city size distribution from 1900 to 1990. Their estimated Zipf exponents, however, vary considerably both in absolute size and changes over time. While Dobkins and Ioannides (1999) report a gradual decline in the exponent from 1.04 in 1900 to about 0.95 in 1990, Black and Henderson (1998) find a relatively stable parameter value of about 0.85.

In compiling the data, then, at least three sets of problems can be distinguished. First, the city data should refer to the widest possible definition of a metropolitan area. Since workers often reside in surrounding areas, census data based on tightly defined city boundaries are likely to capture only a fraction of the population of the integrated economic unit. If this fraction is roughly constant across cities and, thus, all cities are affected alike, this problem would only marginally affect the results. However, as the existence of suburbs is mainly a phenomenon of large cities, a shift in convention from proper city limits to metropolitan areas can generally be expected

[5] Section 2.5 additionally explores data from Austria.

DOI https://doi.org/10.3790/978-3-428-50499-2

to lead to larger differences across city sizes. Indeed, Rosen and Resnick (1980) present a comparison of national size distributions for cities and urban agglomerations, using a subsample of six countries for which both data were available. As expected, they find that the estimated Zipf parameter is consistently lower for metropolitan areas, indicating a more uneven distribution of population.

Second, when analyzing the growth of cities, it is necessary to define these entities consistently over time. Otherwise, redefinitions of city boundaries will lead to discontinuous changes in population which would distort the observed growth rates. In a few cases, it is easily possible to overcome this difficulty. Some national statistical offices do not only report the results of the latest census but also provide historical population data based on time-consistent (usually the most recent) definitions of city limits, directly allowing a reliable analysis of growth dynamics. An example is the decennial publication *Wohnbevölkerung der Gemeinden* by the Swiss Federal Statistical Bureau. It is, however, also possible to deal with this problem when official data are not directly available. Donald Bogue (1953), for example, reconstructs U.S. city and county data from 1900 to 1940 to get numbers which are compatible with the 1950 concept of Standard Metropolitan Statistical Area.

Third, there has to be some convention to determine when an area actually enters the sample as a city. Even if one decides to include *all* officially named cities in a country at a particular point in time, the choice of cut-off is subjective. There will be probably some villages (which are not included in the sample because of their lack of city status) with a population larger than that of the smallest cities (which are, by definition, included in the sample). The problem is compounded when deciding – for the purpose of practicability – to limit the size of the sample and to consider only the upper tail of the distribution. Then, there are basically three ways to define a cut-off point. A first method is to adhere to the procedure often used by statistical offices which report urban areas with a population above a fixed absolute size. The U.S. Bureau of the Census, for example, defines metropolitan areas as having more than 50,000 inhabitants. The main difficulty of this fixed absolute size cut-off is, however, that the number of observations is likely to change each year. More specifically, given long-run processes of rising total population and growing urbanization, the sample size is likely to increase over time which could possibly distort the results. Gabaix (1999), for example, argues that Dobkins and Ioannides' (1999) finding of a gradual decline in the estimated Zipf exponent for U.S. metropolitan areas over a century is simply due to the fact that their sample contains more relatively small cities in later decades, with their sample size growing from 112 in 1900 to 334 in 1990.

DOI https://doi.org/10.3790/978-3-428-50499-2

To avoid this distorting effect, it might be preferable to define a relative cut-off point. Black and Henderson (1998), for instance, calculate the ratio of the minimum to the mean metropolitan area population in 1990 and then apply this ratio as the selection criterion to earlier decades. While this procedure does not rule out changes in sample size, it allows at least the analysis of comparable ranges of city sizes over time.

Finally, it is possible to fix the number of cities included in the sample. The main advantage of this approach is that it captures approximately the same portion of the overall national city size distribution across countries so that it may be particularly useful when comparing the results internationally. In fact, Rosen and Resnick (1980) use this method in their analysis of the urban structure in 44 countries. Eaton and Eckstein (1997) even go one step further. Examining the size distribution of cities in France and Japan over a period of up to 100 years, they do not only fix the number of observations in their sample but also use a constant set of cities, arguing that there would have been only marginal variations if they had allowed for changes in the composition of the sample. Interestingly, Rosen and Resnick (1980) also provide a comparison of Zipf estimates for different sample sizes. However, if they depart from their standard selection criterion of using a country's 50 largest cities and extend their sample to comprise all cities with populations greater than 100,000 inhabitants, they find that both the direction and the magnitude of changes in the results vary widely across countries.

In view of this obvious sensitivity of the results to data definitions, it is important to describe the construction of the data set in detail and to discuss explicitly how it is dealt with the problems. Unfortunately, often the lack of appropriate data already severely restricts the opportunities to address possible effects of alternative data definitions. Therefore, it is also necessary to emphasize the limits of the empirical analysis in this chapter.

I begin, then, with a description of the data sources. The population figures are mostly taken from national statistical yearbooks. Specifically, the data is compiled by hand from historical issues of national yearbooks, with one exception. For Portugal, Nunes (1996) already provides a comparable compilation of city data so that it was not necessary to consult yearbooks. Most notably, the Portuguese population figures also refer to city boundaries at the time of the census.[6]

Whenever possible, this information is cross-checked with special statistical releases on the results of population censuses. In the cases of Italy and Spain where yearbooks sometimes provide only population figures for

[6] The references provide a detailed listing of the data sources.

county capitals, these sources are used to extend the sample to cover the full range (and not only a selective set) of large cities in both countries. Moreover, as those publications often contain data for earlier decades revised to conform with the latest definitions of city boundaries, it is also possible to compare reconstructed figures with initial releases. However, only in a very few cases, I find minor differences between both sources, suggesting that the impact of redefinitions of city sizes is rather limited. Therefore, I have decided to stick to the original yearbook data, favoring a year-specifically correct analysis over consistency over time.

As a standard selection criterion, I have included all cities with a population of more than 50,000. However, the data set also comprises a number of small countries with very few cities above this absolute cut-off point. In those cases, the sample is extended to cover at least the 20 largest cities in a country to guarantee a minimum sample size.[7]

The most obvious difficulty arising from these data definitions then is the exclusive use of national sources. As the data are not harmonized and, thus, are likely to display some country-specific characteristics, they might provide a correct snapshot of a country's urban structure at a particular point in time, but are probably not fully compatible across countries. Therefore, the results, especially differences between countries, should be generally interpreted with care. There are also a number of other potential data problems. As these issues, however, are often closely related to the question at hand, they are discussed in the respective subsections.

2.3.2 Basic Results

In a first step, I examine the empirical fit of the rank-size rule for the ten European countries in my sample. Figures 2.1a–2.1e present simple Zipf plots. Apart from the general observation that the distributions tend to shift to the right over time reflecting an increase in mean city sizes, there are also considerable differences across individual countries. Belgium, for example, is characterized by four *almost equally large* cities (Brussels, Antwerp, Gent and Liege). Similarly, the Dutch urban structure is dominated by three, the Portuguese and Spanish structures by two cities which are

[7] Even with 20 observations, the sample may appear a bit small. However, a comparison of the results with Rosen and Resnick's (1980) estimates for the 50 largest cities in a country does not indicate that a larger sample necessarily yields further insights. Appendix table A.1 shows that the differences, if there are any, are rather small. Moreover, it should be noted that for some countries and years a further extension of the sample is almost impossible. For example, the #20 city in Finland in 1870 has a population of only 1,121, a size which can only hardly be associated with an urban agglomeration.

DOI https://doi.org/10.3790/978-3-428-50499-2

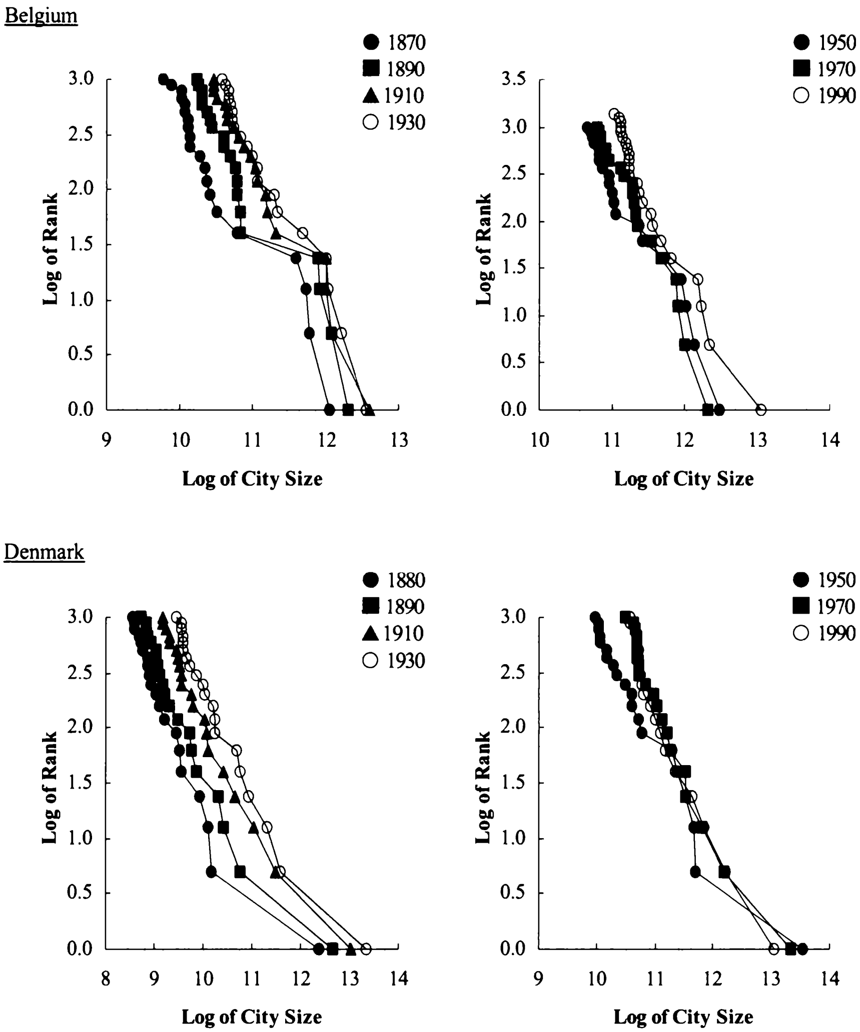
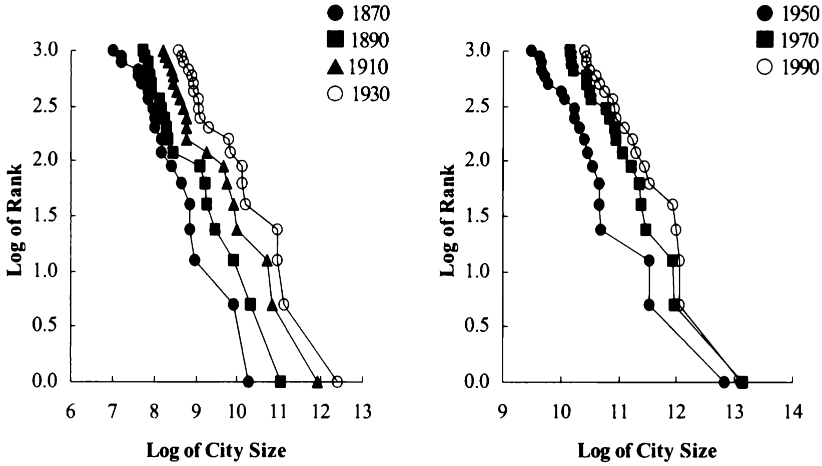


Figure 2.1a: Zipf Plots for Belgium and Denmark

considerably larger than the rest of the distribution. In Denmark and Finland, the population is obviously too heavily concentrated in overdimensioned central cities while the plots for Norway, Sweden and Switzerland display fairly smooth city size distributions. Another general observation is, however, that those differences get smaller over time and national urban structures tend to converge to a distribution which appears to obey Zipf's law.

Finland



Italy

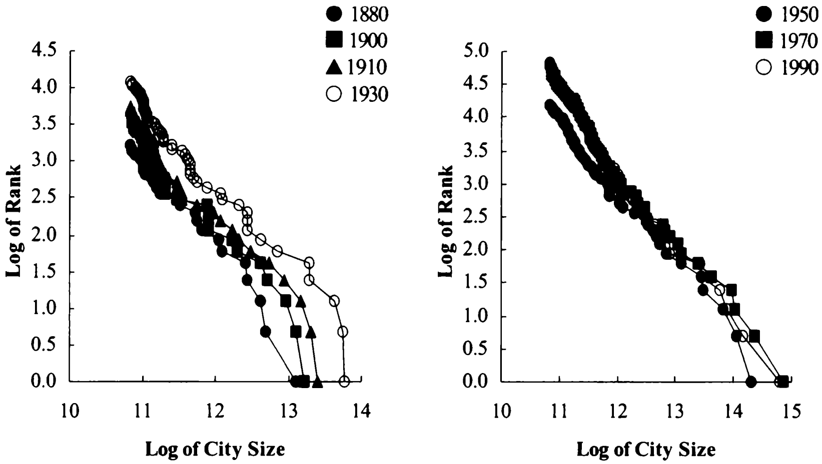


Figure 2.1b: Zipf Plots for Finland and Italy

To analyze the empirical fit of the rank-size rule in more detail, tables 2.1a–2.1d report estimated Zipf exponents. The fit of the regressions is excellent, with an adjusted R^2 generally above 0.90 and the coefficients being statistically highly significant at the 1 % level. An interpretation of the estimated parameter values is somewhat harder. At first sight, the figures vary considerably, ranging from slightly below 0.7 (Portugal 1910, 1930) to about 1.8 (Belgium 1970). Given the possible distorting effects of data defi-

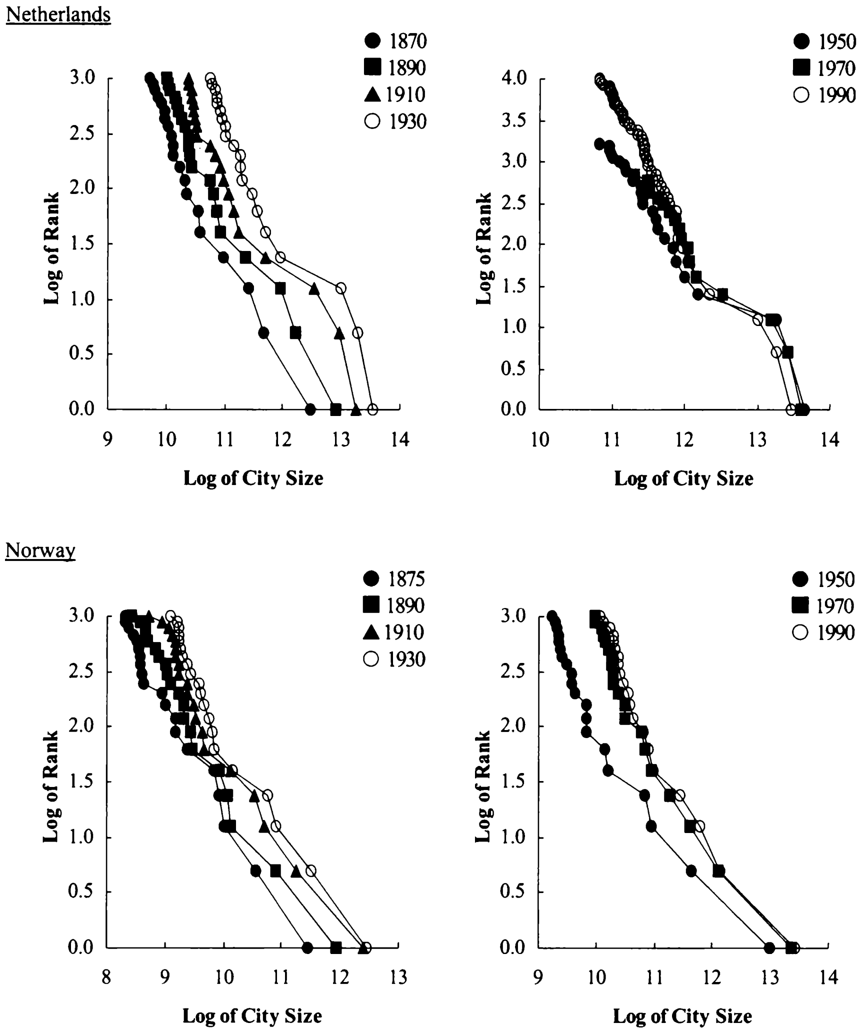


Figure 2.1c: Zipf Plots for the Netherlands and Norway

nitions, however, the observed differences in the estimated Zipf exponents should probably not be overemphasized. In fact, discussing the often only imperfect fit of Zipf's law, Krugman (1996b, p. 40) rightly notes: "The picture is not perfect, but if you had any reason to believe that there was a fundamental reason to expect a straight line with a slope of –1, you would find this picture compelling evidence in favor of your theory!" Moreover, the results are generally not far away from the expected value of 1.

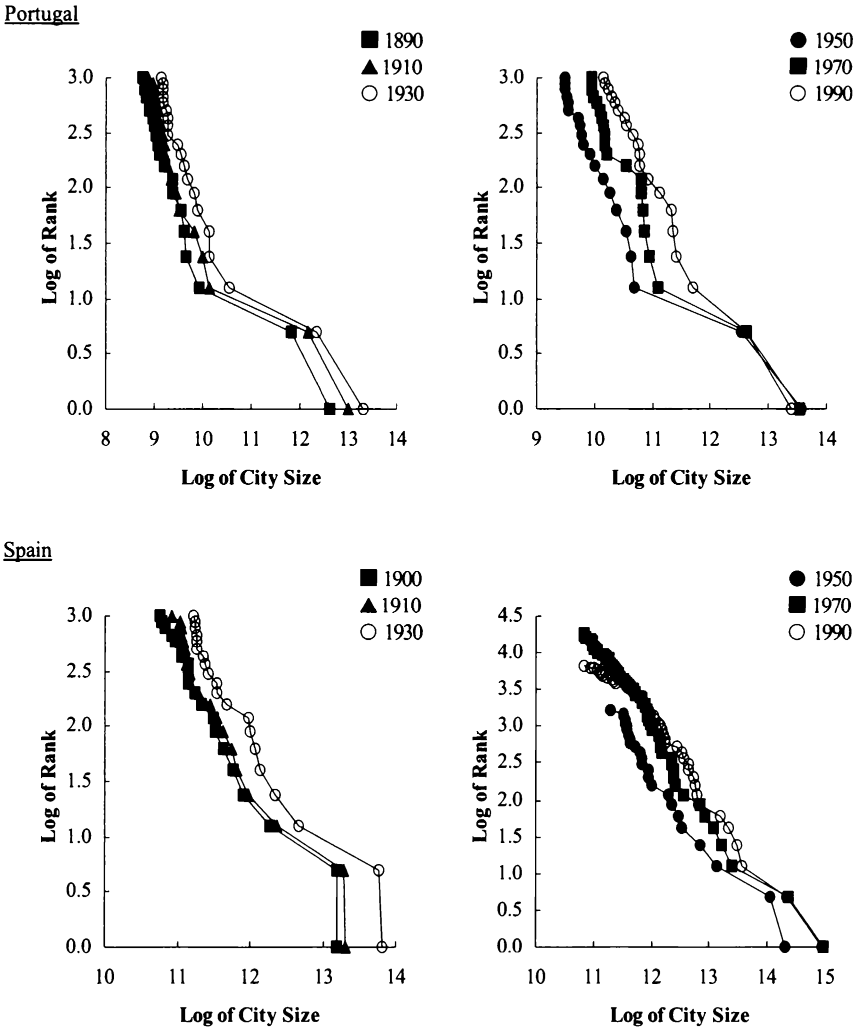


Figure 2.1d: Zipf Plots for Portugal and Spain

Figure 2.2 presents a histogram of estimated Zipf exponents, illustrating a remarkable concentration of the results between 0.95 and 1.05.

It should also be noted, however, that there is no general tendency of convergence towards a log-linear city size distribution with exponent 1. More generally, there is even no clear pattern of changes in the estimated Zipf exponent over time. In most countries, the parameter tends to increase over the course of a century. However, in only a few cases, this increase

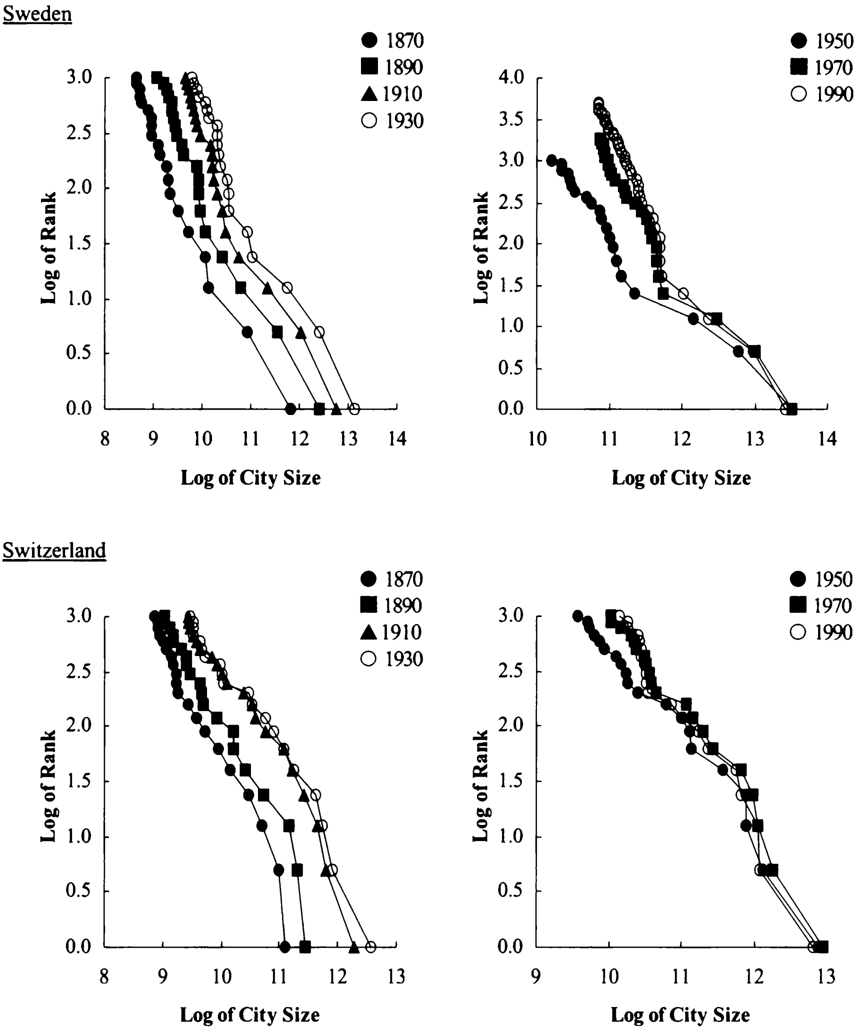


Figure 2.1e: Zipf Plots for Sweden and Switzerland

starts from values below 1 and then approaches this target value, suggesting that there is indeed a process of convergence to Zipf's law at work (Portugal). In others, it surpasses the benchmark (Sweden) or even already starts from a value *above* 1 (Belgium) and, thus, rather indicates divergence from a Zipf distribution. Moreover, there are examples with fairly stable coefficients, also irrespective of whether the initial value is below (Norway) or above 1 (Italy).

Table 2.1a: **Estimated Zipf Exponents**

Year	Summary Statistics				Full Sample			Without Largest City			Constant number of cities		
	Number of cities	Largest city	Smallest city	Medium city size	Zipf exponent		Adj. R^2	Zipf exponent		Adj. R^2	Zipf exponent		Adj. R^2
Belgium													
1870	20	174,273	17,690	49,012	–1.128	(0.075)	0.922	–1.277	(0.094)	0.911			
1890	20	224,012	28,250	65,824	–1.188	(0.082)	0.916	–1.345	(0.103)	0.903			
1910	20	301,766	35,024	82,061	–1.280	(0.053)	0.968	–1.494	(0.085)	0.945			
1930	20	284,373	39,594	87,487	–1.326	(0.056)	0.968	–1.515	(0.075)	0.958			
1950	20	261,412	42,618	87,997	–1.449	(0.067)	0.961	–1.649	(0.093)	0.946			
1970	20	224,543	49,002	91,092	–1.787	(0.079)	0.964	–2.008	(0.125)	0.935			
1990	27	467,875	52,825	107,514	–1.612	(0.055)	0.971	–2.036	(0.075)	0.967	–1.549	(0.064)	0.968
Denmark													
1880	20	234,850	5,106	21,889	–0.876	(0.067)	0.900	–1.526	(0.068)	0.965			
1890	20	312,859	6,087	29,467	–0.844	(0.051)	0.935	–1.341	(0.042)	0.982			
1910	20	462,161	9,494	46,165	–0.834	(0.040)	0.958	–1.235	(0.023)	0.994			
1930	20	617,069	12,473	60,904	–0.837	(0.042)	0.955	–1.244	(0.043)	0.979			
1950	20	768,105	21,522	85,455	–0.902	(0.051)	0.942	–1.304	(0.086)	0.927			
1970	20	622,612	36,154	97,027	–1.157	(0.061)	0.949	–1.734	(0.050)	0.985			
1990	20	466,723	39,719	87,592	–1.245	(0.064)	0.952	–1.750	(0.069)	0.973			
Finland													
1870	20	28,519	1,121	5,565	–0.950	(0.040)	0.967	–1.109	(0.061)	0.948			
1890	20	61,530	2,302	9,764	–0.855	(0.026)	0.982	–1.023	(0.042)	0.971			
1910	20	147,218	3,740	19,421	–0.789	(0.027)	0.979	–0.962	(0.047)	0.958			
1930	20	243,560	5,196	30,769	–0.780	(0.028)	0.976	–0.949	(0.057)	0.939			
1950	20	369,380	13,244	51,819	–0.987	(0.049)	0.954	–1.332	(0.084)	0.934			
1970	20	510,352	25,577	84,248	–1.066	(0.041)	0.973	–1.375	(0.083)	0.938			
1990	20	490,629	33,041	99,648	–1.117	(0.048)	0.966	–1.337	(0.101)	0.906			

Notes: The regressions use OLS to estimate equations of the form $log(Rank_i) = \beta + \alpha\ log(City\ size_i) + \varepsilon_i$, where α is the estimated Zipf exponent. To save space, the results for β are not reported. Standard errors are in parentheses.

Table 2.1b: **Estimated Zipf Exponents**

Year	Summary Statistics				Full Sample			Without Largest City			Constant number of cities		
	Number of cities	Largest city	Smallest city	Medium city size	Zipf exponent		Adj. R^2	Zipf exponent		Adj. R^2	Zipf exponent		Adj. R^2
Italy													
1880	25	494,314	50,651	133,591	−1.209	(0.048)	0.963	−1.329	(0.067)	0.944			
1900	35	547,503	51,264	133,637	−1.185	(0.044)	0.956	−1.286	(0.045)	0.961	−1.120	(0.055)	0.945
1910	42	668,633	50,051	138,848	−1.177	(0.035)	0.965	−1.278	(0.034)	0.972	−1.083	(0.049)	0.953
1930	60	960,729	50,047	157,476	−1.126	(0.025)	0.972	−1.205	(0.023)	0.980	−1.054	(0.052)	0.945
1950	65	1,657,588	50,441	191,186	−1.074	(0.015)	0.987	−1.162	(0.016)	0.989	−1.019	(0.034)	0.974
1970	110	2,799,836	50,342	182,963	−1.167	(0.013)	0.987	−1.265	(0.012)	0.991	−1.000	(0.023)	0.988
1990	123	2,693,383	50,163	158,481	−1.240	(0.012)	0.989	−1.349	(0.010)	0.993	−1.065	(0.023)	0.989
Netherlands													
1870	20	264,694	16,422	46,521	−1.113	(0.046)	0.969	−1.476	(0.053)	0.977			
1890	20	406,316	21,967	69,612	−0.999	(0.041)	0.968	−1.261	(0.053)	0.969			
1910	20	573,983	31,792	106,914	−0.904	(0.050)	0.944	−1.081	(0.060)	0.947			
1930	20	759,286	46,505	152,882	−0.935	(0.055)	0.939	−1.107	(0.066)	0.940			
1950	25	845,266	50,010	168,411	−1.066	(0.052)	0.946	−1.240	(0.062)	0.946	−1.037	(0.065)	0.931
1970	21	820,406	65,981	205,747	−1.100	(0.048)	0.963	−1.261	(0.054)	0.966	−1.102	(0.051)	0.961
1990	54	702,444	50,072	120,166	−1.536	(0.035)	0.974	−1.734	(0.037)	0.976	−1.379	(0.080)	0.940
Norway													
1875	20	94,869	4,102	14,540	−0.952	(0.028)	0.984	−1.206	(0.046)	0.974			
1890	20	151,239	4,578	19,974	−0.931	(0.034)	0.975	−1.254	(0.043)	0.979			
1910	20	240,908	6,113	29,358	−0.886	(0.043)	0.956	−1.218	(0.053)	0.968			
1930	20	253,124	8,899	33,657	−0.904	(0.043)	0.959	−1.208	(0.052)	0.967			
1950	20	434,047	10,311	44,981	−0.831	(0.048)	0.941	−1.211	(0.049)	0.971			
1970	20	645,413	21,228	77,117	−0.937	(0.050)	0.948	−1.381	(0.042)	0.984			
1990	20	685,530	23,759	83,950	−0.956	(0.054)	0.943	−1.415	(0.051)	0.977			

Notes: The regressions use OLS to estimate equations of the form $log(Rank_i) = \beta + \alpha\ log(City\ size_i) + \varepsilon_i$, where α is the estimated Zipf exponent. To save space, the results for β are not reported. Standard errors are in parentheses.

Table 2.1c: **Estimated Zipf Exponents**

Year	Summary Statistics				Full Sample			Without Largest City			Constant number of cities		
	Number of cities	Largest city	Smallest city	Medium city size	Zipf exponent		Adj. R^2	Zipf exponent		Adj. R^2	Zipf exponent		Adj. R^2
Portugal													
1890	20	301,206	6,371	31,055	–0.746	(0.071)	0.851	–1.055	(0.110)	0.835			
1910	20	435,359	7,133	41,984	–0.696	(0.069)	0.841	–0.982	(0.107)	0.823			
1930	20	594,390	9,444	55,059	–0.696	(0.060)	0.874	–0.994	(0.089)	0.874			
1950	20	783,226	13,114	73,446	–0.728	(0.059)	0.889	–1.053	(0.085)	0.894			
1970	20	769,044	20,651	85,049	–0.825	(0.064)	0.897	–1.169	(0.094)	0.895			
1990	20	663,315	25,929	95,359	–0.959	(0.038)	0.970	–1.275	(0.046)	0.977			
Spain													
1900	20	539,835	47,544	131,436	–1.123	(0.054)	0.958	–1.334	(0.056)	0.969			
1910	22	599,807	53,269	135,881	–1.153	(0.060)	0.946	–1.380	(0.067)	0.954	–1.129	(0.063)	0.944
1930	26	1,005,565	53,977	177,889	–1.068	(0.048)	0.951	–1.274	(0.054)	0.959	–1.011	(0.053)	0.949
1950	52	1,618,435	51,975	163,143	–1.148	(0.025)	0.977	–1.323	(0.022)	0.986	–1.011	(0.044)	0.966
1970	71	3,146,071	50,051	208,083	–1.129	(0.020)	0.979	–1.282	(0.023)	0.978	–1.009	(0.049)	0.958
1990	46	3,084,673	50,085	307,589	–0.991	(0.027)	0.968	–1.105	(0.040)	0.946	–1.091	(0.038)	0.977
Sweden													
1870	20	136,016	5,716	19,237	–0.977	(0.041)	0.968	–1.332	(0.038)	0.986			
1890	20	246,454	8,716	33,491	–0.947	(0.047)	0.954	–1.300	(0.052)	0.972			
1910	20	342,323	15,535	50,510	–0.972	(0.054)	0.945	–1.307	(0.065)	0.957			
1930	20	502,213	18,007	70,556	–0.907	(0.048)	0.948	–1.193	(0.061)	0.955			
1950	20	744,143	27,087	106,198	–0.931	(0.052)	0.944	–1.233	(0.069)	0.947			
1970	26	740,486	52,774	126,302	–1.234	(0.061)	0.943	–1.571	(0.076)	0.947	–1.163	(0.068)	0.939
1990	40	674,452	51,047	107,117	–1.545	(0.055)	0.952	–1.929	(0.056)	0.969	–1.355	(0.088)	0.926

Notes: The regressions use OLS to estimate equations of the form $log(Rank_i) = \beta + \alpha\ log(City\ size_i) + \varepsilon_i$, where α is the estimated Zipf exponent. To save space, the results for β are not reported. Standard errors are in parentheses.

Table 2.1d

Estimated Zipf Exponents

Year	Summary Statistics				Full Sample			Without Largest City			Constant number of cities	
	Number of cities	Largest city	Smallest city	Medium city size	Zipf exponent		Adj. R^2	Zipf exponent		Adj. R^2	Zipf exponent	Adj. R^2
Switzerland												
1870	20	65,606	7,008	19,567	–1.106	(0.049)	0.965	–1.236	(0.041)	0.981		
1890	20	94,129	8,412	27,125	–1.031	(0.045)	0.964	–1.143	(0.039)	0.979		
1910	20	215,488	12,707	50,854	–0.901	(0.043)	0.958	–0.992	(0.059)	0.939		
1930	20	290,937	13,036	57,820	–0.846	(0.038)	0.963	–0.944	(0.059)	0.934		
1950	20	390,020	14,488	73,127	–0.841	(0.035)	0.968	–0.948	(0.059)	0.935		
1970	20	422,640	22,705	88,816	–0.956	(0.038)	0.971	–1.087	(0.065)	0.940		
1990	20	365,043	25,407	81,016	–1.047	(0.045)	0.966	–1.208	(0.076)	0.934		

Notes: The regressions use OLS to estimate equations of the form $log(Rank_i) = \beta + \alpha\ log(City\ size_i) + \varepsilon_i$, where α is the estimated Zipf exponent. To save space, the results for β are not reported. Standard errors are in parentheses.

DOI https://doi.org/10.3790/978-3-428-50499-2

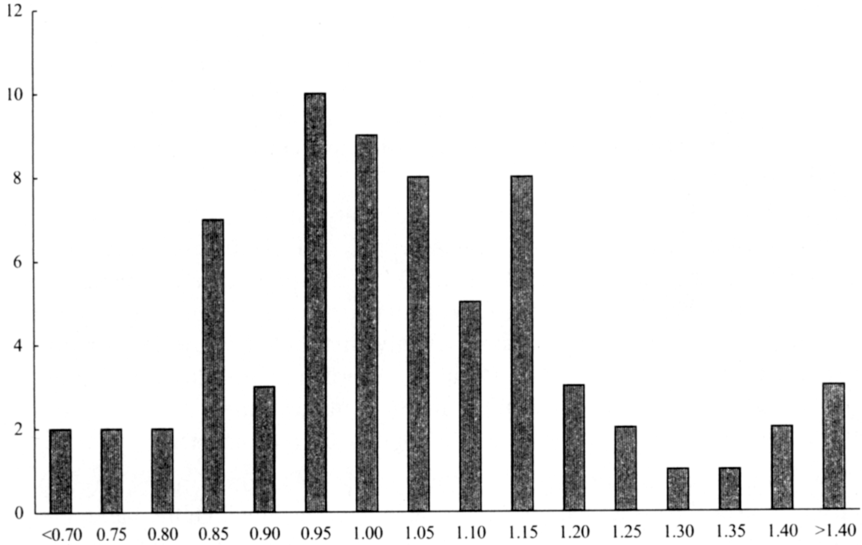


Figure 2.2: Histogram of Estimated Zipf Exponents

2.3.3 Examining Explanations for Deviations from an Exponent of 1

Although the empirical fit of Zipf's law for cities is generally remarkable, it might also be interesting to analyze why there are some obvious deviations from an exponent of 1. More specifically, if there is really a universal characteristic of city growth processes which leads to size distributions satisfying the rank-size rule, it becomes important to explain why it works apparently particularly well in some countries and less so in others.

The most obvious explanation for differences in estimated Zipf exponents are differences in data quality. In view of the observed sensitivity of the estimates to data definitions, small cross-country variations in, say, sample size, the definition of an urban unit, or the precision of the census clearly have the potential to affect the results. Citing the study by Rosen and Resnick (1980), Fujita et al. (1999, p. 217) even argue somewhat ambitiously "that the exponent gets closer to 1 the more carefully the metropolitan areas are defined".

Apart from data issues, however, there are also two fundamental arguments. A first explanation focuses on the existence of large central metropolises. Krugman (1996b, p. 41), for example, notes:

> "Zipf's law is not quite as neat in other countries as it is in the United States, but it still seems to hold in most places, if you make one modification: many countries, for example, France and the United Kingdom, have a single "primate city" that is much larger than a line drawn through the distribution of other cities would lead you to expect. These primate cities are typically political capitals; it is easy to imagine that they are essentially different creatures from the rest of the urban sample."

But, does excluding a country's largest city improve the results on the rank-size rule? Tables 2.1a–2.1d also report Zipf estimates based on samples without the largest city. Three findings are particularly notable. First, in each case, the estimated coefficient increases in absolute size, indicating that the population is indeed more evenly distributed if the largest city is excluded. This result, however, is hardly surprising since dropping the maximum value reduces concentration in the sample almost by definition as there are less large cities relative to a slightly decreasing mean.

Second, even more important for the purpose at hand, dropping the largest city increases the estimated coefficient also in cases in which the parameter is already above 1 and, thus, leads to an even larger deviation from the expected value. In Belgium, for example, the estimated Zipf exponent for the national urban system in 1990 rises from about 1.6 to more than 2.0 if the largest city, Antwerp, is excluded from the sample. Thus, there is no convincing empirical evidence for the claim that deviations from an exponent of 1 are mainly due to large central cities and excluding them would improve the result.

Finally, it should also be noted that excluding the largest city does not necessarily improve the empirical fit of the log-linear relationship. In fact, in a number of cases, the adjusted R^2 even falls considerably if the central city is dropped from the sample. This result is also supported by figures 2.1a–2.1e which illustrate that, perhaps with the exception of Denmark and Finland, the largest cities in the countries in the sample are hardly outliers dominating the national urban structure.

A second explanation for empirical deviations from the expected Zipf exponent of 1 has been proposed by Gabaix (1999). Puzzled by Dobkins and Ioannides' (1999) finding that the estimated coefficient for U.S. cities decreases over time, he argues that the Zipf exponent is generally lower for small and medium sized cities. Extending then the sample over time (for example, by using a fixed cut-off of 50,000 inhabitants) changes the composition of the sample and thereby lowers the aggregate coefficient automatically.

While a natural test for this hypothesis would be to calculate separate Zipf parameters for different ranges of city sizes, the small number of observations in my sample does not allow a meaningful application of this strategy. Tables 2.1a–2.1d, however, also present estimates, holding the sample size constant over time. As this fixed sample then contains less small cities, the estimated Zipf exponent should be larger than for the full sample, if Gabaix's (1999) reasoning is correct. It turns out, however, that the estimated Zipf coefficient for the sample with a constant number of observations is, if anything, below the comparable value for a larger sample. Thus, there is also no evidence for Gabaix's claim that smaller cities tend to have a lower Zipf exponent.

In sum, both proposed explanations for some obvious empirical deviations from a Zipf exponent of 1 fail to meet the European data in my sample. While occasionally a country provides some empirical support for one of the hypotheses (e. g., fixing the sample in the case of Italy), there is at least an equal number of failures, suggesting two conclusions. First, more analytical work is needed to explain differences in the empirical fit of Zipf's law for cities. Given considerable variation in city size distributions across countries, explanations based on only a single country's experiences are likely to be insufficient. Second, it probably has to be acknowledged that there is no perfect regularity – a finding which is not surprising in social sciences. If, by historical accident or any other reason, one or a few of the largest cities display an "anomalous" size, the result will be strongly affected. Deviations from the rank-size rule are then possibly magnified by differences in data quality.

2.4 Distribution Dynamics

Having analyzed the rank-size distribution of cities, I examine in a next step the empirical validity of proposed explanations for this pattern. As the most promising suggestions for the emergence of Zipf's law are based on stochastic growth processes, it is of particular interest to explore the growth pattern of cities inside their national urban structure. More specifically, the analysis focuses on two issues. First, does the data really display the assumed random growth of cities across different sizes? Here, Eaton and Eckstein (1997) already provide some support. However, their analysis is based on only two countries' experiences, Japan and France. Also similar findings by Glaeser, Scheinkman and Shleifer (1995) for the U.S. are not entirely convincing as their analyzed time period of only 30 years is a bit short.

Second, is there evidence for the claim that also the variances of growth rates are independent of city size? Obviously, this issue is far more controversial. While Gabaix (1999, fn. 10) provides some rough calculations based on Eaton and Eckstein's data supporting this claim, Fujita et al. (1999, p. 224) suspect that the variance of the growth rate declines with city size simply as a matter of industrial diversification.

2.4.1 Kernel Density

A first way to examine growth processes in a national urban system is to explore the evolution of the city size distribution over time.[8] The basic idea is to seek for a law of motion in the long-run behavior of the distribution. If, for example, the distribution tends to concentrate at a single point mass, there is a process of convergence at work in which all cities are likely to end up with an equal size. If, on the other hand, the distribution displays tendencies towards limits with other properties, e.g., a continual spreading apart, those too would be observable from the law of motion and, thus, would provide interesting insights.

Figures 2.3a–2.3e then plot for each country a sequence of kernel estimates of the density of relative city sizes.[9] At a first look, the density plots appear to be quite similar. Even though there are in a few cases changes over time, the shapes of the national city size distributions remain often fairly constant, perhaps apart from the particular location of the country's largest city. Nonetheless, there are also some noticeable differences in detail.

A general observation is, for example, that for the majority of countries the peak of the distribution slightly moves to the right over time. Specifically in Belgium, Denmark, the Netherlands, Portugal and Sweden, the mass gets closer to the mean; a behavior which suggests that there is some catching-up of smaller cities. This conclusion, however, becomes less convincing if one takes another general observation into consideration, namely that the peak of the distribution in 1990 is often below the peak of earlier years so that the distributions tend to get flatter. As national urban systems display a wider variety of city sizes, there are also probably differences in individual growth experiences of cities. The emergence of a more diversified urban structure is particularly visible for Portugal.

The density plots also display a few other notable features. First, there are considerable differences in the dominance of the country's largest city, ranging from about 11 times the average of the 20 largest cities (Copenhagen in Denmark, Lisbon in Portugal) to about factor 4 (Belgium). This result is comparable to the observed discrepancies in estimated Zipf expo-

[8] This technique has recently become very popular in empirical research on patterns of cross-country economic growth. See Durlauf and Quah (1998) for a detailed description.

[9] This normalization of the data by dividing city sizes by the sample average in each decade provides two advantages. First, it allows a better comparison of changes over time because otherwise the distributions would shift to the right as mean city sizes increase. Second, it also allows a comparison of distributions between countries as it provides a consistent scale on the horizontal axis, with a value of 1 indicating the national average.

DOI https://doi.org/10.3790/978-3-428-50499-2

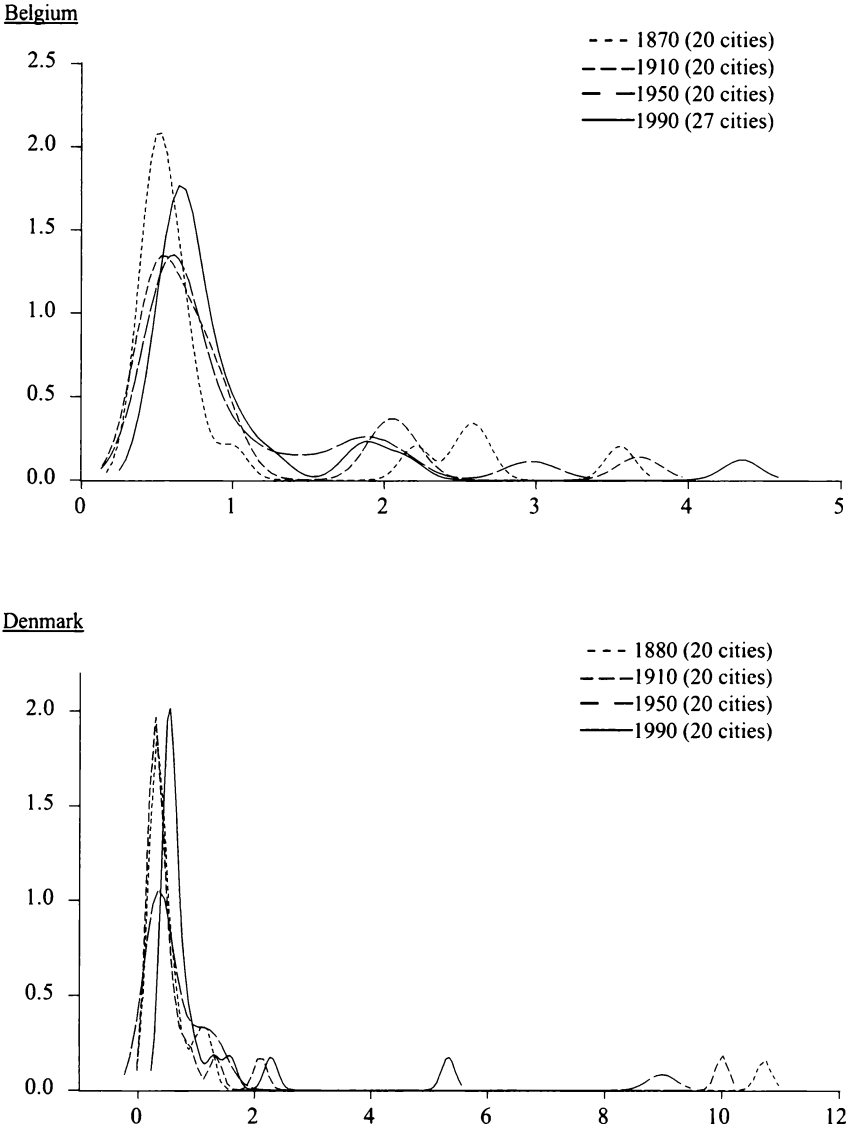
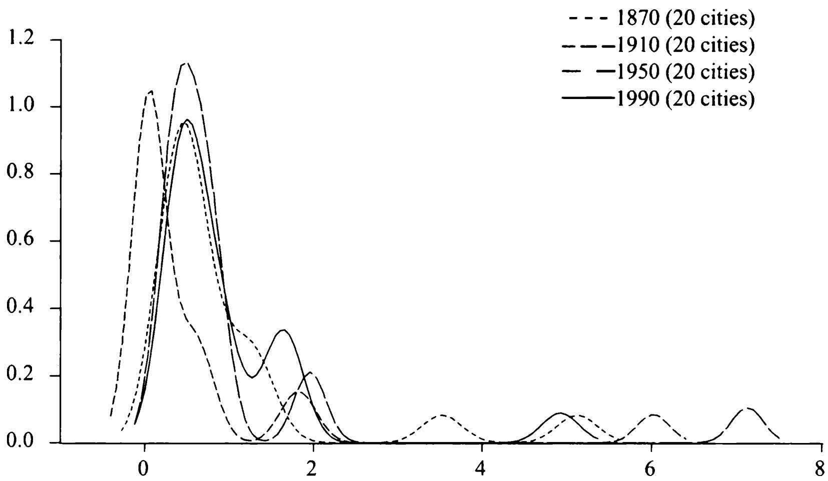


Figure 2.3a: Density Plots of Relative City Sizes for Belgium and Denmark

Finland



Italy

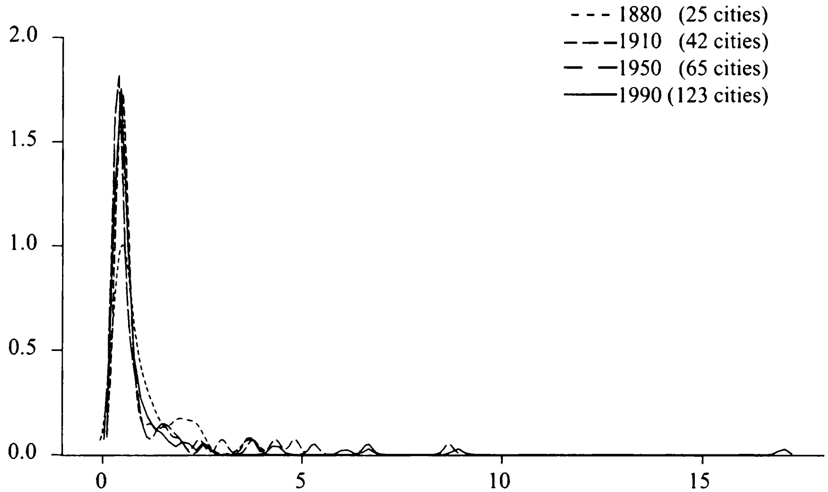
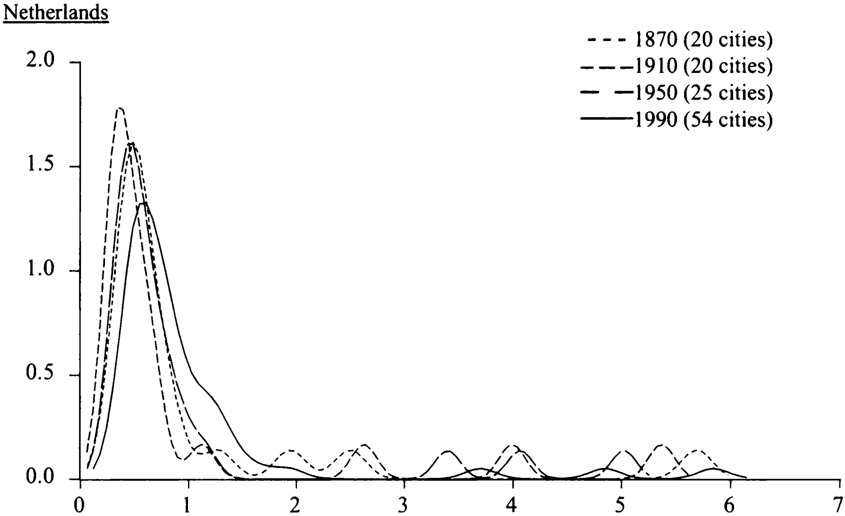


Figure 2.3b: Density Plots of Relative City Sizes
for Finland and Italy



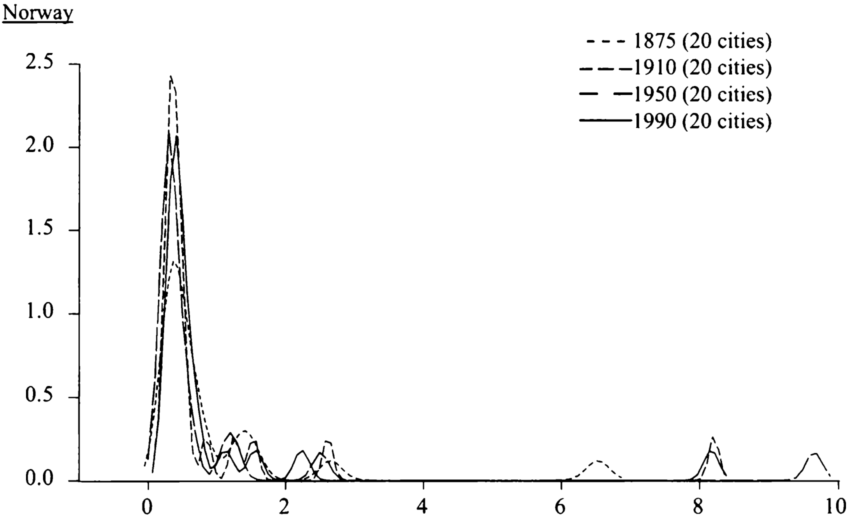
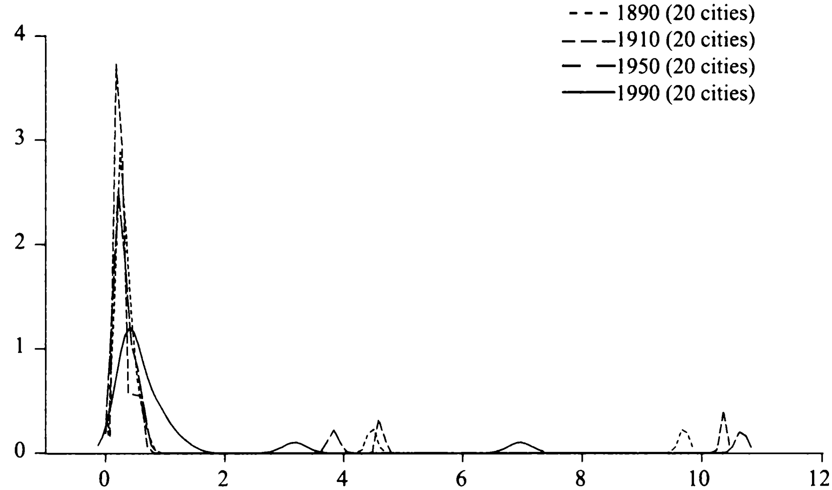


Figure 2.3c: Density Plots of Relative City Sizes for the Netherlands and Norway

DOI https://doi.org/10.3790/978-3-428-50499-2

Portugal



Spain

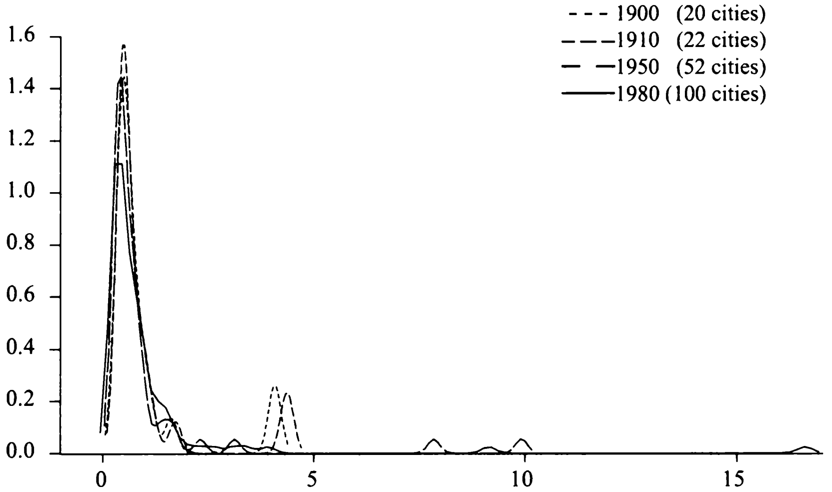


Figure 2.3d: Density Plots of Relative City Sizes
for Portugal and Spain

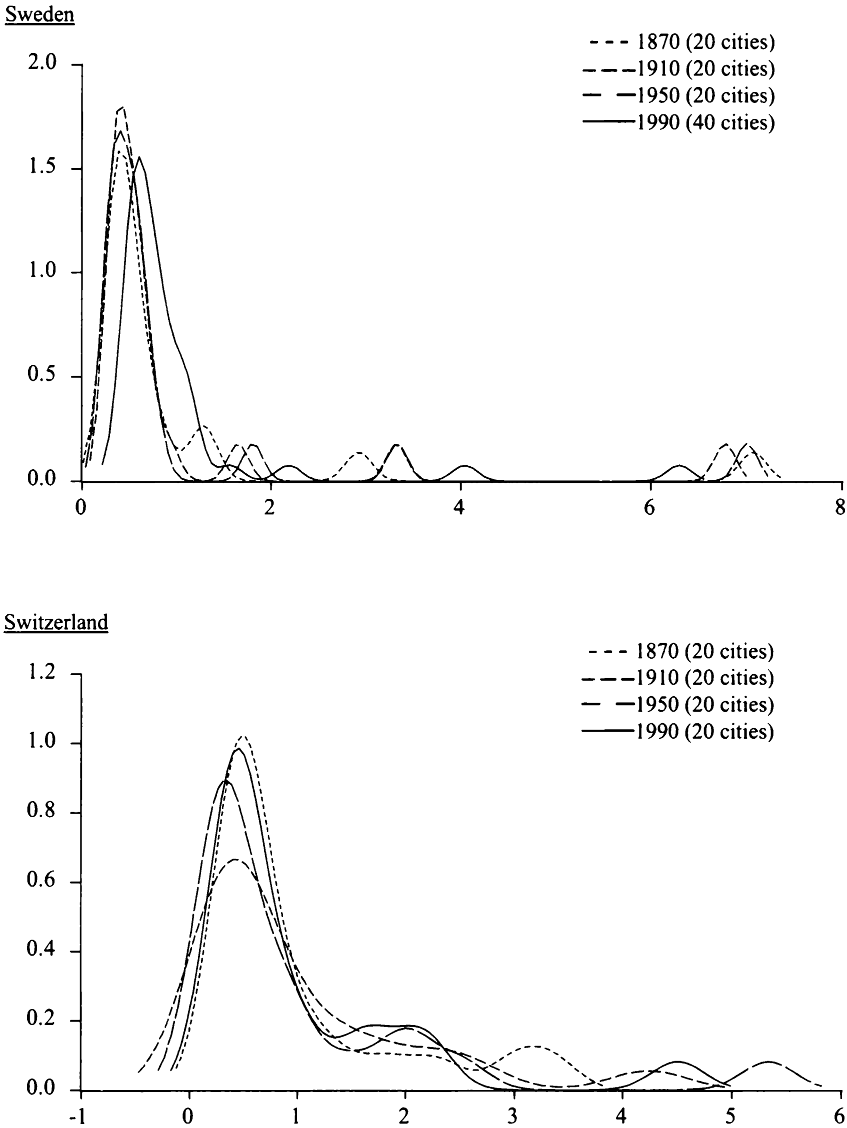


Figure 2.3e: Density Plots of Relative City Sizes
for Sweden and Switzerland

nents. Countries with relatively large central cities have a regression line with a flat slope and, thus, have smaller Zipf parameters.

Second, the dominance of the largest cities tends to decline, at least during the last 40-year-interval. Note that this pattern is partly masked by the decision to allow for an increase in sample size over time (motivated by the desire to analyze a sample as large as possible). For example, the remarkable increase in urban primacy of Rome (Italy) and Madrid (Spain) is mainly the result of a larger number of cities used in the calculation of the average city size (thereby lowering the average).

Finally, with the peak of their distributions below 1, Finland and Switzerland display the most evenly distributed urban system.

2.4.2 Transition Matrices

While the density plots are illustrative about changes in the entire size distribution of a national urban system, they provide no information about the actual behavior of individual cities. Thus, they do not allow, for example, to answer the question whether the obvious decrease in polarisation is the result of intermediate cities gradually catching-up with the big ones or some small cities growing very rapidly and becoming large cities themselves or even a different process.

A by now very common technique[10] to track the evolution of each city's relative size over time is the construction of transition probability matrices. Starting from an initial distribution of cities sorted into different size groups, the matrix describes how cities are redistributed among these size categories over a given period of time. Specifically, each cell of the matrix gives the proportion of cities which start with a given size in an initial year (rows) and move to a particular size group in the final distribution (columns). Thus, each row, for example, shows the probability that a city remains in its size group or transits to any other group. To observe a transition pattern of parallel growth then requires that the mass of the distribution is concentrated in the diagonal, indicating that cities are staying in the same category as in the previous period.

In practice, the construction of a transition probability matrix requires at least three, fairly crucial, decisions. First, the time period has to be defined. As I have data for up to 120 years, I construct, whenever possible, separate matrices for three 40-year-intervals: 1870 (or the first year for which data are available) –1910, 1910–1950 and 1950–1990. Second, there has to be a choice of cities included in the sample. As a general rule, the sample com-

[10] See De Vries (1984, pp. 123–136) for an early application.

prises all cities with a population of more than 50,000 inhabitants. If the resulting sample size is smaller than 20 observations, however, the sample is extended to comprise at least the country's 20 largest cities. To avoid Galton's fallacy[11], I initially applied these criteria only to city size distributions in the base year. Later I realized, however, that this procedure would underestimate the true growth dynamics, because small, fast-growing cities would be excluded from the sample. Therefore, I also include cities which belong to the top 20 in the final year, further increasing sample size. Third, if the variable of interest is continuous (such as relative city sizes), a discretisation of the space of possible outcomes is necessary. After some experimentation, it turns out that Eaton and Eckstein's (1997) classification based on 0.30, 0.50, 0.75, 1.00, and 2.00 times the sample mean is quite practicable.[12] Dobkins and Ioannides (1999) also provide a comparison with alternative results where groups are based on deciles, finding – besides more detail – not much difference.

Tables 2.2a–2.2e then present three transition matrices for each of the ten countries in my sample. Interestingly, the matrices share several similarities. In almost all cases, for example, very large cities (i.e., cities with a size of at least twice the average) retain their relative position over time. This finding may in part result from the low cut-off point of the largest quintile which does not allow to observe when a city falls from about, say, ten times the average to only three times. Nonetheless, this pattern suggests very strong persistence at the top end of the distribution. If a city has reached a certain size, the probability of decline is virtually zero. In fact, supporting Eaton and Eckstein's results for France and Japan, *all* cities which had a dominant position in 1870 are also dominant in 1990.

Furthermore, there is a notable break in the transition pattern over time. Most countries exhibit a fairly stable size distribution until 1950 and display a far more dynamic behavior afterwards. Indeed, the most dramatic changes occur between 1950 and 1990, with some cities even moving upwards by more than three categories. The explanations behind those explosions in city size are probably quite diverse. In Portugal, for example, rapid city growth largely reflects the emergence of a more developed urban system. Also redefinitions of the boundaries of urban agglomerations and city mergers may play a role (e.g., in Belgium and the Netherlands). Moreover, some cities with a rapidly expanding population size are located in

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[11] See Christopher Bliss (1999) for an extensive discussion of Galton's fallacy.

[12] Actually, Eaton and Eckstein (1997) also define a sixth category which comprises cities larger than 20 times the mean. As my sample, however, is considerably smaller and, moreover, focuses only on the very upper tail of the distribution so that there is hardly a city which is 20 times larger than the sample mean, I drop this category and stick to quintiles.

Table 2.2a

Transition Matrices

Belgium

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
1	[0–0.3)	0.00	1.00	0.00	0.00	0.00	0.00
6	[0.3–0.5)	0.00	0.33	0.33	0.33	0.00	0.00
10	[0.5–0.75)	0.00	0.50	0.20	0.00	0.30	0.00
3	[0.75–1)	0.00	0.00	0.67	0.33	0.00	0.00
1	[1–2)	0.00	0.00	1.00	0.00	0.00	0.00
4	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
25							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
1	[0–0.3)	0.00	0.00	1.00	0.00	0.00	0.00
4	[0.3–0.5)	0.00	0.00	1.00	0.00	0.00	0.00
9	[0.5–0.75)	0.00	0.22	0.78	0.00	0.00	0.00
4	[0.75–1)	0.00	0.00	0.25	0.50	0.25	0.00
3	[1–2)	0.00	0.00	0.00	0.33	0.67	0.00
4	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
25							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
0	[0–0.3)	0.00	0.00	0.00	0.00	0.00	0.00
7	[0.3–0.5)	0.00	0.14	0.43	0.29	0.14	0.00
8	[0.5–0.75)	0.00	0.25	0.63	0.13	0.00	0.00
5	[0.75–1)	0.00	0.00	0.80	0.00	0.20	0.00
3	[1–2)	0.00	0.00	0.33	0.67	0.00	0.00
4	[2–∞)	0.00	0.00	0.00	0.00	0.50	0.50
27							

Denmark

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
6	[0–0.3)	0.67	0.17	0.17	0.00	0.00	0.00
9	[0.3–0.5)	0.44	0.56	0.00	0.00	0.00	0.00
3	[0.5–0.75)	0.00	0.00	0.67	0.33	0.00	0.00
0	[0.75–1)	0.00	0.00	0.00	0.00	0.00	0.00
3	[1–2)	0.00	0.00	0.00	0.33	0.33	0.33
1	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
22							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
10	[0–0.3)	0.40	0.40	0.20	0.00	0.00	0.00
7	[0.3–0.5)	0.29	0.57	0.14	0.00	0.00	0.00
3	[0.5–0.75)	0.00	0.33	0.33	0.00	0.33	0.00
1	[0.75–1)	0.00	0.00	0.00	0.00	1.00	0.00
2	[1–2)	0.00	0.00	0.00	0.00	1.00	0.00
2	[2–∞)	0.00	0.00	0.00	0.00	0.50	0.50
25							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
5	[0–0.3)	0.00	0.20	0.80	0.00	0.00	0.00
9	[0.3–0.5)	0.00	0.33	0.67	0.00	0.00	0.00
4	[0.5–0.75)	0.00	0.00	0.50	0.50	0.00	0.00
0	[0.75–1)	0.00	0.00	0.00	0.00	0.00	0.00
5	[1–2)	0.00	0.00	0.00	0.20	0.60	0.20
1	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
24							

Table 2.2b

Transition Matrices

Finland

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
7	[0–0.3)	0.43	0.43	0.14	0.00	0.00	0.00
4	[0.3–0.5)	0.75	0.25	0.00	0.00	0.00	0.00
6	[0.5–0.75)	0.67	0.33	0.00	0.00	0.00	0.00
2	[0.75–1)	0.50	0.50	0.00	0.00	0.00	0.00
5	[1–2)	0.00	0.00	0.00	0.00	0.80	0.20
2	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
26							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
11	[0–0.3)	0.27	0.36	0.27	0.09	0.00	0.00
9	[0.3–0.5)	0.44	0.33	0.11	0.00	0.11	0.00
1	[0.5–0.75)	0.00	0.00	0.00	0.00	1.00	0.00
0	[0.75–1)	0.00	0.00	0.00	0.00	0.00	0.00
4	[1–2)	0.00	0.00	0.00	0.75	0.25	0.00
3	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
28							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
4	[0–0.3)	0.00	0.25	0.25	0.00	0.25	0.25
8	[0.3–0.5)	0.13	0.88	0.00	0.00	0.00	0.00
5	[0.5–0.75)	0.00	0.40	0.40	0.20	0.00	0.00
3	[0.75–1)	0.00	0.00	0.33	0.33	0.33	0.00
3	[1–2)	0.00	0.00	0.33	0.33	0.33	0.00
3	[2–∞)	0.00	0.00	0.00	0.00	0.33	0.67
26							

Italy

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
1	[0–0.3)	0.00	1.00	0.00	0.00	0.00	0.00
15	[0.3–0.5)	0.00	0.87	0.13	0.00	0.00	0.00
12	[0.5–0.75)	0.00	0.25	0.75	0.00	0.00	0.00
2	[0.75–1)	0.00	0.00	1.00	0.00	0.00	0.00
7	[1–2)	0.00	0.00	0.14	0.14	0.71	0.00
5	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
42							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
5	[0–0.3)	0.40	0.60	0.00	0.00	0.00	0.00
23	[0.3–0.5)	0.22	0.74	0.00	0.04	0.00	0.00
21	[0.5–0.75)	0.00	0.62	0.29	0.10	0.00	0.00
6	[0.75–1)	0.00	0.17	0.33	0.50	0.00	0.00
6	[1–2)	0.00	0.00	0.00	0.17	0.83	0.00
8	[2–∞)	0.00	0.00	0.00	0.00	0.13	0.88
69							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
20	[0–0.3)	0.00	0.90	0.10	0.00	0.00	0.00
40	[0.3–0.5)	0.00	0.90	0.10	0.00	0.00	0.00
33	[0.5–0.75)	0.00	0.21	0.67	0.09	0.03	0.00
8	[0.75–1)	0.00	0.00	0.25	0.63	0.13	0.00
10	[1–2)	0.00	0.00	0.00	0.10	0.90	0.00
12	[2–∞)	0.00	0.00	0.00	0.00	0.17	0.83
123							

Table 2.2c

Transition Matrices

Netherlands

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
2	[0–0.3)	0.00	1.00	0.00	0.00	0.00	0.00
6	[0.3–0.5)	0.50	0.50	0.00	0.00	0.00	0.00
8	[0.5–0.75)	0.00	0.63	0.38	0.00	0.00	0.00
3	[0.75–1)	0.00	0.00	0.67	0.33	0.00	0.00
1	[1–2)	0.00	0.00	0.00	0.00	1.00	0.00
3	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
23							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
4	[0–0.3)	0.00	0.75	0.00	0.25	0.00	0.00
11	[0.3–0.5)	0.00	0.64	0.36	0.00	0.00	0.00
5	[0.5–0.75)	0.00	0.20	0.80	0.00	0.00	0.00
2	[0.75–1)	0.00	0.00	0.00	1.00	0.00	0.00
1	[1–2)	0.00	0.00	0.00	0.00	1.00	0.00
3	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
26							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
13	[0–0.3)	0.00	0.54	0.39	0.08	0.00	0.00
12	[0.3–0.5)	0.00	0.17	0.50	0.25	0.08	0.00
10	[0.5–0.75)	0.00	0.00	0.50	0.50	0.00	0.00
8	[0.75–1)	0.00	0.00	0.38	0.38	0.25	0.00
7	[1–2)	0.00	0.00	0.00	0.00	1.00	0.00
4	[2–∞)	0.00	0.00	0.00	0.00	0.25	0.75
54							

Norway

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
2	[0–0.3)	0.50	0.50	0.00	0.00	0.00	0.00
10	[0.3–0.5)	0.30	0.60	0.10	0.00	0.00	0.00
4	[0.5–0.75)	0.00	0.75	0.25	0.00	0.00	0.00
1	[0.75–1)	0.00	0.00	1.00	0.00	0.00	0.00
3	[1–2)	0.00	0.00	0.00	0.25	0.75	0.00
2	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
22							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
3	[0–0.3)	1.00	0.00	0.00	0.00	0.00	0.00
11	[0.3–0.5)	0.55	0.45	0.00	0.00	0.00	0.00
2	[0.5–0.75)	0.00	0.50	0.50	0.00	0.00	0.00
1	[0.75–1)	0.00	0.00	1.00	0.00	0.00	0.00
2	[1–2)	0.00	0.00	0.00	0.00	1.00	0.00
2	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
21							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
9	[0–0.3)	0.33	0.56	0.11	0.00	0.00	0.00
9	[0.3–0.5)	0.11	0.56	0.33	0.00	0.00	0.00
2	[0.5–0.75)	0.00	0.00	0.50	0.50	0.00	0.00
0	[0.75–1)	0.00	0.00	0.00	0.00	0.00	0.00
2	[1–2)	0.00	0.00	0.00	0.00	1.00	0.00
2	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
24							

Table 2.2d

Transition Matrices

Portugal

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
14	[0–0.3)	1.00	0.00	0.00	0.00	0.00	0.00
6	[0.3–0.5)	0.17	0.67	0.17	0.00	0.00	0.00
1	[0.5–0.75)	0.00	0.00	1.00	0.00	0.00	0.00
0	[0.75–1)	0.00	0.00	0.00	0.00	0.00	0.00
0	[1–2)	0.00	0.00	0.00	0.00	0.00	0.00
2	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
23							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
16	[0–0.3)	0.81	0.13	0.06	0.00	0.00	0.00
3	[0.3–0.5)	0.00	0.67	0.33	0.00	0.00	0.00
2	[0.5–0.75)	0.00	0.50	0.50	0.00	0.00	0.00
0	[0.75–1)	0.00	0.00	0.00	0.00	0.00	0.00
0	[1–2)	0.00	0.00	0.00	0.00	0.00	0.00
2	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
23							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
17	[0–0.3)	0.18	0.53	0.24	0.00	0.06	0.00
5	[0.3–0.5)	0.20	0.20	0.60	0.00	0.00	0.00
4	[0.5–0.75)	0.00	0.00	0.00	0.25	0.75	0.00
0	[0.75–1)	0.00	0.00	0.00	0.00	0.00	0.00
0	[1–2)	0.00	0.00	0.00	0.00	0.00	0.00
2	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
28							

Spain							
n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
0	[0–0.3)	0.00	0.00	0.00	0.00	0.00	0.00
7	[0.3–0.5)	0.00	1.00	0.00	0.00	0.00	0.00
7	[0.5–0.75)	0.00	0.29	0.71	0.00	0.00	0.00
3	[0.75–1)	0.00	0.00	0.00	1.00	0.00	0.00
3	[1–2)	0.00	0.00	0.00	0.00	1.00	0.00
2	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
22							
n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
5	[0–0.3)	0.00	1.00	0.00	0.00	0.00	0.00
21	[0.3–0.5)	0.00	1.00	0.00	0.00	0.00	0.00
7	[0.5–0.75)	0.00	0.14	0.57	0.29	0.00	0.00
9	[0.75–1)	0.00	0.11	0.44	0.33	0.11	0.00
6	[1–2)	0.00	0.00	0.17	0.17	0.67	0.00
4	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
52							
n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
32	[0–0.3)	0.47	0.34	0.13	0.06	0.00	0.00
16	[0.3–0.5)	0.13	0.63	0.25	0.00	0.00	0.00
20	[0.5–0.75)	0.15	0.25	0.35	0.15	0.10	0.00
8	[0.75–1)	0.00	0.13	0.38	0.38	0.13	0.00
16	[1–2)	0.00	0.00	0.00	0.44	0.56	0.00
8	[2–∞)	0.00	0.00	0.00	0.00	0.13	0.88
100							

DOI https://doi.org/10.3790/978-3-428-50499-2

Table 2.2e

Transition Matrices

Sweden

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
1	[0–0.3)	$\boxed{0.00}$	1.00	0.00	0.00	0.00	0.00
11	[0.3–0.5)	0.27	$\boxed{0.55}$	0.18	0.00	0.00	0.00
5	[0.5–0.75)	0.00	0.40	$\boxed{0.60}$	0.00	0.00	0.00
2	[0.75–1)	0.00	0.00	0.50	$\boxed{0.50}$	0.00	0.00
2	[1–2)	0.00	0.00	0.00	0.00	$\boxed{1.00}$	0.00
2	[2–∞)	0.00	0.00	0.00	0.00	0.00	$\boxed{1.00}$
23							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
0	[0–0.3)	$\boxed{0.00}$	0.00	0.00	0.00	0.00	0.00
11	[0.3–0.5)	0.27	$\boxed{0.45}$	0.27	0.00	0.00	0.00
7	[0.5–0.75)	0.00	0.43	$\boxed{0.57}$	0.00	0.00	0.00
1	[0.75–1)	0.00	0.00	0.00	$\boxed{1.00}$	0.00	0.00
1	[1–2)	0.00	0.00	0.00	0.00	$\boxed{1.00}$	0.00
2	[2–∞)	0.00	0.00	0.00	0.00	0.00	$\boxed{1.00}$
22							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
10	[0–0.3)	$\boxed{0.00}$	0.00	0.90	0.10	0.00	0.00
12	[0.3–0.5)	0.00	$\boxed{0.25}$	0.58	0.17	0.00	0.00
7	[0.5–0.75)	0.00	0.29	$\boxed{0.29}$	0.29	0.14	0.00
5	[0.75–1)	0.00	0.00	0.00	$\boxed{0.60}$	0.40	0.00
4	[1–2)	0.00	0.00	0.00	0.00	$\boxed{1.00}$	0.00
3	[2–∞)	0.00	0.00	0.00	0.00	0.00	$\boxed{1.00}$
41							

Switzerland

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
1	[0–0.3)	1.00	0.00	0.00	0.00	0.00	0.00
8	[0.3–0.5)	0.50	0.38	0.13	0.00	0.00	0.00
6	[0.5–0.75)	0.17	0.50	0.17	0.00	0.17	0.00
2	[0.75–1)	0.00	0.00	0.00	0.50	0.50	0.00
2	[1–2)	0.00	0.00	0.00	0.50	0.50	0.00
4	[2–∞)	0.00	0.00	0.00	0.00	0.25	0.75
23							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
7	[0–0.3)	0.71	0.29	0.00	0.00	0.00	0.00
6	[0.3–0.5)	0.50	0.50	0.00	0.00	0.00	0.00
1	[0.5–0.75)	0.00	1.00	0.00	0.00	0.00	0.00
3	[0.75–1)	0.00	0.00	0.33	0.67	0.00	0.00
3	[1–2)	0.00	0.00	0.00	0.00	1.00	0.00
4	[2–∞)	0.00	0.00	0.00	0.00	0.00	1.00
24							

n		[0–0.3)	[0.3–0.5)	[0.5–0.75)	[0.75–1)	[1–2)	[2–∞)
8	[0–0.3)	0.25	0.75	0.00	0.00	0.00	0.00
6	[0.3–0.5)	0.00	0.50	0.50	0.00	0.00	0.00
1	[0.5–0.75)	0.00	0.00	1.00	0.00	0.00	0.00
2	[0.75–1)	0.00	0.50	0.00	0.50	0.00	0.00
3	[1–2)	0.00	0.00	0.00	0.00	1.00	0.00
4	[2–∞)	0.00	0.00	0.00	0.00	0.25	0.75
24							

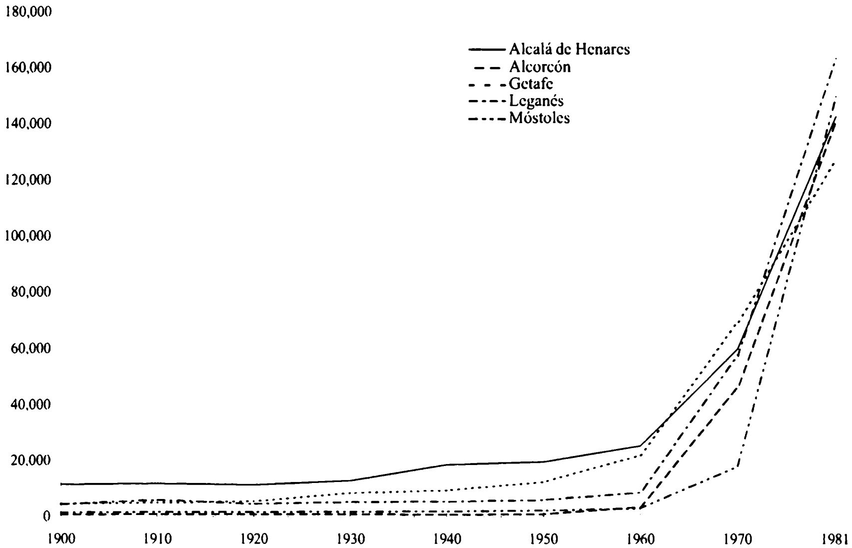

Figure 2.4: Fast Growing Cities in the Madrid Region

the neighborhood of large agglomerations and, thus, may benefit from suburbanization. Figure 2.4 illustrates an example of fast growing Spanish municipalities in the Madrid region. Finally, a further extreme is observable in Finland where Espoo, a city officially founded in 1972, has become the country's second largest city by 1990.

A number of transition matrices also exhibit interesting individual characteristics. Belgium, for example, shows a strong pattern of convergence in the period from 1950 to 1990. Only 3 of the 12 cities with an initial size above 0.75 times the average keep or improve their relative position while all others decline to a lower category. In contrast, Italy, the country with the largest sample of cities, shows strong persistence. In all categories, except the smallest, at least 63% of the cities remain in the quintile they started from. The relative distribution in Switzerland seems to be similarly stable. Finally, the matrices for Portugal nicely illustrate the duopolist urban structure, with the national system of cities dominated by Lisbon and Oporto and all other cities being smaller than 0.75 times the average.

In sum, the transition matrices display interesting features of city growth processes in Europe. Apart from large central cities, however, they provide no convincing evidence of parallel growth across cities. Rather, some (relatively) small cities appear to catch up, in some cases even very quickly, while a number of previously intermediate cities apparently fall behind.

2.4.3 Which Cities Grow?

This section complements the results from nonparametric techniques with a number of parametric specifications. In particular, the analysis focuses on the two crucial assumptions behind Gabaix's (1999) approach to explain Zipf's law: random city growth across different sizes and the variance of growth rates being independent from city size.

In motivating his theoretical explanation based on Gibrat's law[13], Gabaix (1999) particularly cites empirical evidence from Eaton and Eckstein (1997). In a recent study, these authors analyze the long-run behavior of city size distributions in France and Japan and find that there is no relationship between the growth rate of cities and their initial size. Specifically, running simple growth regressions, they show that the coefficient on the initial level of population is statistically not significant.

Table 2.3 then presents comparable estimates for the countries in my sample. In contrast to Eaton and Eckstein's set up, however, I do not explore changes about the entire time period of 120 years for which I have data. Rather, I split up the time period into three 40-year-intervals. This procedure provides two advantages. For one thing, it allows to take a fuller account of the actual dynamics of the national urban systems in the sample. In addition, it delivers results which are comparable to findings in previous sections. As before then, the sample comprises all cities with a population of more than 50,000 and, if necessary, it is extended to cover at least a country's 20 largest cities, with these selection criteria applied exclusively to the base year to avoid the problem of Galton's fallacy.

The results are interesting. If Eaton and Eckstein's claim is correct that cities grow randomly across a wide range of sizes, one would expect that the coefficient on initial city size is zero in each of the analyzed time periods. Only then, the starting level would be consistently irrelevant for city growth. It turns out, however, that in a number of cases initial conditions *do* affect subsequent growth rates, with the sign of the relationship changing over time. Indeed, a clear pattern emerges. For the first period, the growth rate of a city is, if anything, positively correlated with its initial size. For the Netherlands (statistically significant at the 1% level) and, in somewhat weaker form, Finland and Italy (significant at the 10% level), there is convincing evidence that large cities tend to grow faster than the rest of the distribution. Thus, existing differences in city size increase further between 1870 and 1910.

In later years, however, the pattern of divergence appears to fade out. While the sign of the relationship between initial size and subsequent popu-

[13] See John Sutton (1997) for an extensive discussion of Gibrat's law.

DOI https://doi.org/10.3790/978-3-428-50499-2

Table 2.3

Convergence, Divergence, or Parallel Growth of Cities? (Part 1)

Country	Period	Coefficient	Stand. Dev.	R^2	# of Obs.
Belgium	1870–1910	–0.074	(0.108)	0.026	20
Belgium	1910–1950	–0.036	(0.063)	0.019	19
Belgium	1950–1990	–0.175	(0.151)	0.026	14
Denmark	1880–1910	0.055	(0.082)	0.026	19
Denmark	1910–1950	–0.025	(0.065)	0.008	20
Denmark	1950–1990	–0.232**	(0.064)	0.422	20
Finland	1870–1910	0.238#	(0.118)	0.184	20
Finland	1910–1950	0.044	(0.121)	0.007	20
Finland	1950–1990	–0.073	(0.071)	0.055	20
Italy	1880–1910	0.097#	(0.050)	0.144	25
Italy	1910–1950	0.142*	(0.054)	0.155	40
Italy	1950–1990	–0.052#	(0.027)	0.057	64
Netherlands	1870–1910	0.243**	(0.073)	0.379	20
Netherlands	1910–1950	–0.124*	(0.051)	0.259	19
Netherlands	1950–1990	–0.236**	(0.040)	0.597	25
Norway	1875–1910	0.062	(0.074)	0.038	20
Norway	1910–1950	0.015	(0.062)	0.004	19
Norway	1950–1990	–0.076	(0.082)	0.046	20
Portugal	1890–1910	0.079	(0.037)	0.196	20
Portugal	1910–1950	–0.012	(0.048)	0.004	20
Portugal	1950–1990	–0.138#	(0.066)	0.151	20
Spain	1900–1910	0.003	(0.018)	0.002	20
Spain	1910–1950	0.130#	(0.075)	0.131	22
Spain	1950–1990	–0.034	(0.061)	0.006	52
Sweden	1870–1910	0.002	(0.093)	0.000	20
Sweden	1910–1950	0.029	(0.074)	0.009	20
Sweden	1950–1990	–0.233**	(0.062)	0.442	20
Switzerland	1870–1910	0.199	(0.133)	0.110	20
Switzerland	1910–1950	0.125*	(0.056)	0.224	20
Switzerland	1950–1990	–0.121**	(0.040)	0.339	20

Notes: The regressions use OLS to estimate equations of the form log(City growth$_{\text{Period}}$) = α + β log(Initial city size), where only the results for β are reported to save space. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

lation growth is ambiguous for the period from 1910 to 1950, the results become unequivocal in the final 40-year-period. In all countries, the coefficient on the initial level of population is negative, suggesting that the size distribution of cities is getting more equal over time. The finding of convergence is statistically robust for six of the ten countries in the sample: Denmark, Italy, the Netherlands, Portugal, Sweden and Switzerland.

I have also experimented with alternative specifications, but the findings were qualitatively unchanged. Table 2.4, for example, reports the results of regressing the growth rate of a city on its rank in the base year. As expected, the estimated coefficients take here exactly the opposite signs. Cities on the top of the distribution and thus with a lower rank first grow faster (negative correlation) and then slower (positive correlation) than the rest of the sample.

The explanation for this pattern of city growth is quite intuitive. In the initial stages of industrial development (dating back in Europe to the late 19th century), especially large cities benefit from growing urbanization. Later on, improvements in transportation infrastructure allow to develop a more balanced urban system, gradually reducing urban primacy. In fact, this inverted U-curve relationship between economic development and urban concentration is by now well established.[14] Taken together over time, however, these diverging tendencies may compensate for each other and, in effect, may probably suggest mistakenly that initial size is irrelevant for city growth.

Unfortunately, I am unable to test this hypothesis directly with the data at hand since there is too much variation in the sample. In most countries, only about one half of the initially large cities survive. An useful alternative, then, might be to explore Eaton and Eckstein's (1997) data set on France and Japan in more detail. In particular, the aim would be to analyze whether their results for the full period also hold for shorter periods of time. It turns out, however, that – contrary to Eaton and Eckstein's claim – there is *no* clear evidence that cities grow independently of initial size. Results for the whole period then tend to hold over time. Appendix B provides a detailed discussion of the results.

To examine the claim that the variance of city growth is independent of city size, I basically follow Gabaix's (1999) approach. Specifically, I sort the cities in the sample by population in the base year, divide the sample into two halves, and then calculate separate variances of the growth rate for the two subsamples. Finally, the equality of the variances is evaluated by an F-test.

[14] See Karsten Junius (1999) for a recent discussion.

Table 2.4

Convergence, Divergence, or Parallel Growth of Cities? (Part 2)

Country	Period	Coefficient	Stand. Dev.	R^2	# of Obs.
Belgium	1870–1910	0.004	(0.013)	0.007	20
Belgium	1910–1950	–0.001	(0.007)	0.000	19
Belgium	1950–1990	0.026#	(0.014)	0.238	14
Denmark	1880–1910	–0.006	(0.012)	0.014	19
Denmark	1910–1950	–0.004	(0.011)	0.009	20
Denmark	1950–1990	0.027*	(0.011)	0.268	20
Finland	1870–1910	–0.029**	(0.017)	0.139	20
Finland	1910–1950	–0.013	(0.020)	0.022	20
Finland	1950–1990	0.007	(0.010)	0.027	20
Italy	1880–1910	–0.008	(0.005)	0.107	25
Italy	1910–1950	–0.007*	(0.003)	0.105	40
Italy	1950–1990	0.003*	(0.001)	0.069	64
Netherlands	1870–1910	–0.028**	(0.009)	0.341	20
Netherlands	1910–1950	0.022**	(0.007)	0.352	19
Netherlands	1950–1990	0.022**	(0.005)	0.486	25
Norway	1875–1910	–0.001	(0.011)	0.001	20
Norway	1910–1950	0.011	(0.009)	0.082	19
Norway	1950–1990	0.013	(0.013)	0.052	20
Portugal	1890–1910	–0.010	(0.007)	0.100	20
Portugal	1910–1950	0.001	(0.009)	0.000	20
Portugal	1950–1990	0.009	(0.013)	0.028	20
Spain	1900–1910	0.001	(0.002)	0.017	20
Spain	1910–1950	–0.008	(0.008)	0.045	22
Spain	1950–1990	0.001	(0.003)	0.001	52
Sweden	1870–1910	0.009	(0.013)	0.026	20
Sweden	1910–1950	–0.001	(0.010)	0.000	20
Sweden	1950–1990	0.024*	(0.010)	0.233	20
Switzerland	1870–1910	–0.026	(0.016)	0.126	20
Switzerland	1910–1950	–0.019*	(0.008)	0.241	20
Switzerland	1950–1990	0.018*	(0.007)	0.302	20

Notes: The regressions use OLS to estimate equations of the form $\log(\text{City growth}_{\text{Period}}) = \alpha + \beta \log(\text{Rank}_i)$, where only the results for β are reported to save space. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

The results are shown in table 2.5, with columns (5) and (6) reporting the variables of interest. In column (5) I check for the sign of the difference in the calculated variances for the two subsamples, marking the expected outcome of large cities displaying the smaller variance with a ✓. Column (6) then reports the F-statistic on the difference.

Two points are particularly noteworthy. First, in a few cases the F-value is significant at conventional levels of confidence, indicating that the null hypothesis that the variances are equal is rejected. This result suggests, however, that Gabaix's findings for France and Japan are not necessarily robust for other countries or time periods. Hence, equality of variances of growth rates across different size ranges can not be assumed to be a general feature of city growth processes.

Second, there is at first sight no evidence for Fujita et al.'s (1999, p. 224) intuitive claim that the variance of the growth rate declines with city size. In fact, in almost exactly one-half (13 out of 30) of the cases the variances of the log growth rates are actually *higher* for the upper half of the sample than for the small cities. This confirms Gabaix's analysis who obtains equally puzzling results for France and Japan. It is also necessary, however, to put this finding into perspective. If the difference between variances is statistically significant, then there is indeed a good chance that the growth rates of small cities display a larger variance. Except for Denmark 1950–90 and Italy 1880–1910, *sizeable* differences in variances are characterized by a negative correlation between the variation in growth rates and initial city size.

In sum, the results of parametric specifications raise some serious questions about the empirical validity of the assumptions underlying Gabaix's (1999) approach to explain Zipf's law for cities. There is neither consisting evidence that the growth rate of a city is fully independent of its initial size nor that the variance of growth rates is always constant across a wide range of city sizes. More work, however, is needed to examine the possible impact of these deviations from the theoretical pattern on the actual size distributions of cities.

2.5 The Austrian Experience

One of the most interesting empirical issues related to Zipf's law for cities is the actual process of convergence to a power law distribution. This aspect is of particular relevance for two reasons. First, it is apparently hard to model a framework which produces a smooth power law distribution within a fairly reasonable period of time. Krugman (1996b, pp. 96–97), for example, notes that it is one of the problems of Simon's (1955) model that it requires an unrealistically large increase in total urban population (by far more than factor 100) to generate a city size distribution which roughly

Table 2.5

Variances of Growth Rates Across Different City Sizes

Country	Period	$Variance_{large}$	$Variance_{small}$	$V_s > V_l$	F-Test	# of Obs.
Belgium	1870–1910	0.088	0.120	✓	1.359	20
Belgium	1910–1950	0.036	0.019		1.927	19
Belgium	1950–1990	0.109	0.062		1.768	14
Denmark	1880–1910	0.069	0.129	✓	1.868	19
Denmark	1910–1950	0.097	0.046		2.114	20
Denmark	1950–1990	0.137	0.038		3.663#	20
Finland	1870–1910	0.271	0.115		2.362	20
Finland	1910–1950	0.123	0.455	✓	3.707#	20
Finland	1950–1990	0.058	0.071	✓	1.219	20
Italy	1880–1910	0.048	0.012		4.204*	25
Italy	1910–1950	0.075	0.059		1.275	40
Italy	1950–1990	0.033	0.034	✓	1.030	64
Netherlands	1870–1910	0.100	0.050		2.001	20
Netherlands	1910–1950	0.030	0.053	✓	1.749	19
Netherlands	1950–1990	0.046	0.043		1.081	25
Norway	1875–1910	0.058	0.091	✓	1.588	20
Norway	1910–1950	0.046	0.068	✓	1.485	19
Norway	1950–1990	0.031	0.187	✓	6.035*	20
Portugal	1890–1910	0.051	0.016		3.174	20
Portugal	1910–1950	0.020	0.083	✓	4.183*	20
Portugal	1950–1990	0.153	0.070		2.197	20
Spain	1900–1910	0.001	0.005	✓	4.786*	20
Spain	1910–1950	0.060	0.064	✓	1.070	22
Spain	1950–1990	0.034	0.193	✓	5.675**	52
Sweden	1870–1910	0.054	0.153	✓	2.837	20
Sweden	1910–1950	0.056	0.072	✓	1.292	20
Sweden	1950–1990	0.103	0.067		1.536	20
Switzerland	1870–1910	0.102	0.244	✓	2.388	20
Switzerland	1910–1950	0.060	0.041		1.468	20
Switzerland	1950–1990	0.012	0.041	✓	3.325#	20

Notes: Each sample is sorted by initial city size and divided into two halves. This table then reports calculated variances of growth rates in these two sub-samples. A ✓ indicates that the lower half displays a higher variance, and an F-test evaluates the equality of the variances. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

satisfies Zipf's law. In contrast, Gabaix (1999, p. 745) emphasizes that his approach produces a power law distribution with an exponent close to 1 within seven decades.

Second, as Zipf's law seems to hold for most places and time periods, there is hardly any process of convergence empirically observable. Indeed, historical studies show that even very young urban systems such as the U.S. in 1790, Argentina in 1860 or India in 1911[15], already follow a power law distribution with an exponent of 1 with remarkable precision.[16] Thus, the basic obstacle to examine the mechanism of convergence to Zipf's law in detail is apparently to find an urban system which does not yet obey the rank-size rule.

The optimal framework then would be an initial distribution of fairly equally-sized cities. However, there is also an useful alternative. If, for example, a "shock" pushes an existing urban system outside of its steady-state distribution, it is possible to analyze the following adjustment process in which the city-size distribution is expected to converge to its new steady state. While there are a number of events thinkable which might have the potential to affect city growth processes, probably the most dramatic shock to a city-size distribution is the sudden disintegration of a nation which is likely to disrupt the existing urban network.

An interesting natural experiment might then be provided by the break-up of the Austro-Hungarian Empire in 1918.[17] In fact, the collapse of the Habsburg Monarchy at the end of World War I was accompanied by a massive territorial redistribution. The newly established Republic of Austria, for instance, comprised less than one third of the territory of the former Austrian part of the Habsburg Empire. Moreover, since the corresponding population size fell from 29 million to 7 million, this was in no way only a loss of less important peripheral territories, but a major shock to the exist-

[15] These examples are, among others, cited in Gabaix (1999). See this paper for references to original sources.

[16] A possible exception is provided by De Vries (1984, pp. 94–96). Analyzing rank-size distributions for Europe as a whole from 1300 to 1979, he observes that the urban system before 1600 can hardly be described by Zipf's law. For cities below rank 20, the slope coefficient (–0.63) is already significantly smaller than 1. The inclusion of the largest cities then would have reduced this coefficient even further, as all these cities are below the regression line formed by the small cities. Possible explanations for this pattern in which the largest cities are too small range from a low level of integration (e.g., primitive transportation infrastructure, relatively autarchic urban systems) to social conditions (e.g., rapid spread of diseases and fires).

[17] Another example would be the division of Germany after World War II. Quite surprisingly, however, the urban structure in former East Germany displays a rank-size distribution already at the time of the dissolution.

DOI https://doi.org/10.3790/978-3-428-50499-2

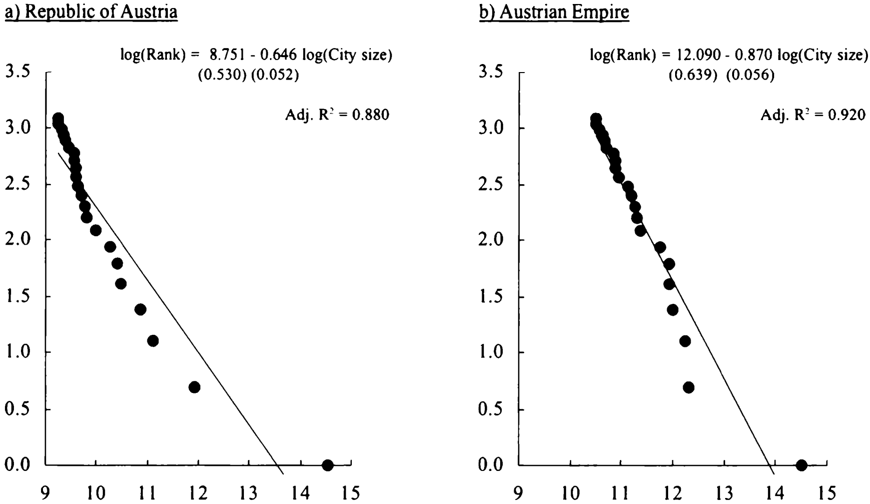

Figure 2.5: Zipf Plots for Austria, 1910

ing system of cities. Hence, it appears to be particularly worth to examine the evolution of the national urban structure in the Republic of Austria.

I start with an analysis of the city size distribution before the disintegration of Austria-Hungary. The last census for the Austrian Empire is from 31 December 1910. For this year, the statistical yearbook contains all cities with more than 10,000 inhabitants. However, only 22 of these cities are located on the territory of the later Austrian Republic, limiting the sample to 22 observations. Plotting the rank-size distribution of these cities, the left graph of figure 2.5 shows that this urban system hardly satisfies Zipf's law. The distribution is dominated by a too large central city, Vienna. This result, however, is not surprising since Vienna is at this time the capital of the Habsburg Empire. Further down the distribution, the medium-sized cities on Austrian territory are too small, while the cities from rank 14 downward are above the regression line again. In sum, the estimated Zipf exponent (–0.65) is considerably below 1 and the fit of the regression (0.89) is weaker than for other countries (see tables 2.1a–2.1d).

In comparison, the size distribution of the 22 largest cities of the fairly established urban system of the Austrian Empire can reasonably described by the rank-size rule. As shown in the graph on the right hand side of figure 2.5, the basic pattern of a still too dominant Vienna is unchanged. However, the slope of the regression line (–0.87) is steeper, and the empirical fit (0.92) is improved.

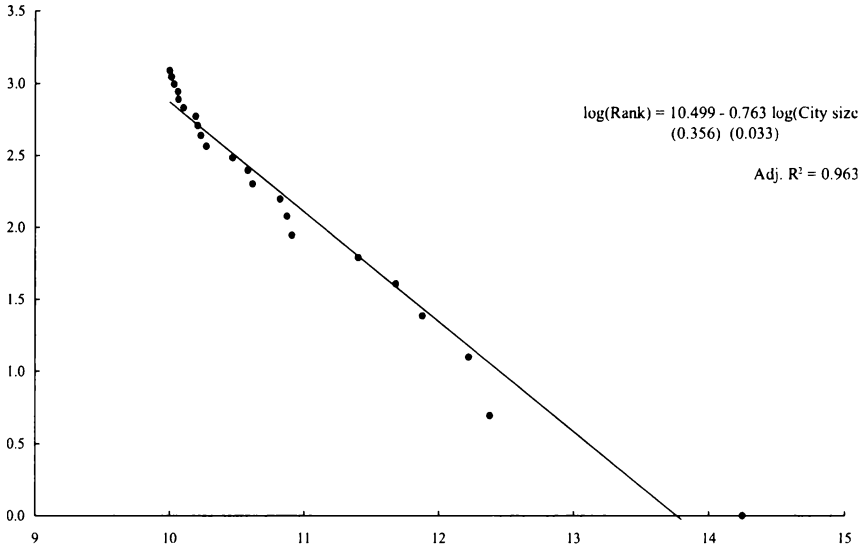

Figure 2.6: Zipf Plot for Austria, 1990

In a next step, I examine changes in the city-size distribution after the proclamation of the Austrian Republic. In particular, the aim is to analyze whether there is a process of adjustment in which the randomly defined urban structure in Austria converges to a new steady state which, furthermore, possibly exhibits Zipf's law. Therefore, I calculate Zipf exponents for the 22 largest Austrian cities for all years for which I have data.

In the first few decades, there is little variation in the estimated coefficients. The parameter value slightly increases to 0.68 in 1934 and 0.69 in 1948. Thus, if anything, the adjustment process is very slow, even though the total population increases by about 7.4%. After World War II, then, changes in city size processes slightly accelerate. Now the distribution appears to converge gradually but still slowly to Zipf's law, with the exponent rising from 0.73 in 1960 to 0.74 a decade later and 0.76 in 1980. However, figure 2.6 illustrates that in 1990 Vienna still dominates the Austrian urban structure. In fact, the unchanged Zipf parameter of 0.76 is mainly the result of Vienna's oversize and, at the same time, the less-than-proportionate size of Austria's second largest city, Graz. Indeed, when both cities are dropped from the sample, the Zipf exponent jumps to 1.19.

Finally, it is worth to analyze the growth pattern of cities. Here, it turns out that it is one of the advantages of the Austrian sample that the relative position of cities inside the urban system is astonishingly stable. Similar to

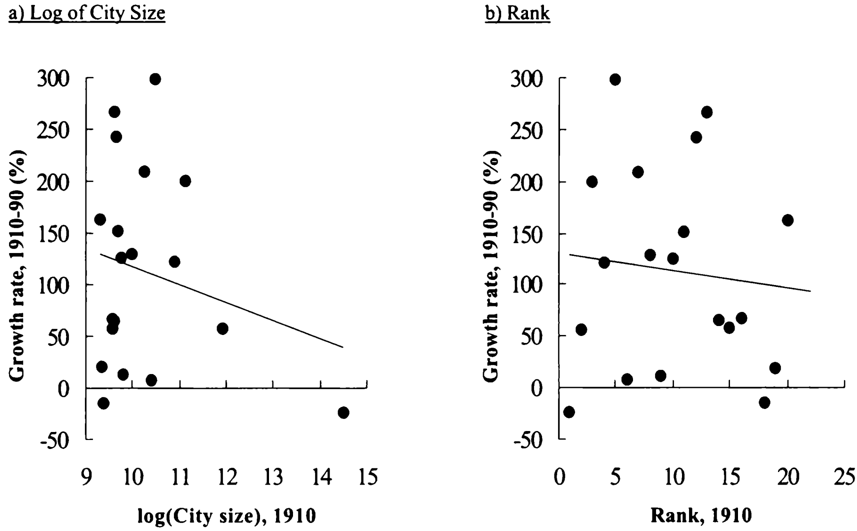

Figure 2.7: City Growth in Austria, 1910–90

Eaton and Eckstein's (1997) findings for France and Japan, there is little variation in the upper tail of the distribution. In fact, 16 of the 22 largest Austrian cities in 1910 still belong to the top 22 in 1990.[18] This stability, however, clearly limits the extent to which possible distortions introduced by sample bias may affect the results.

The left graph of figure 2.7 then displays the growth rates of the 19 surviving cities from 1910 to 1990 and the log of city size in 1910. Similarly, the graph on the right hand side plots the growth rates against the rank in 1910. In both cases, there is no obvious correlation between initial conditions and later growth rates. Reporting formal regression results, table 2.6 shows that the coefficient on initial size is not statistically different from zero. Thus, the growth rate of cities appears to be unaffected from city size so that there is neither evidence for a catching-up of smaller cities nor for a process of divergence in which larger cities grow consistently faster than smaller ones. This result supports Eaton and Eckstein's (1997) finding of parallel growth of cities.

[18] Of the six cities which do not belong to the 22 largest cities anymore, three have ceased to exist. Atzgersdorf has merged with Vienna, Eggenberg has become a part of Graz, and Urfahr is now a suburb of Linz. The other three cities are Mödling (rank #24 in 1990), Stockerau (#36), and Neunkirchen (#65). These cities are replaced in the top 22 by Wolfsberg (#14), Bregenz (#15), Feldkirch (#16), Kapfenberg (#19), Traun (#21), and Amstetten (#22).

Table 2.6

City Growth in Austria

Period	# of Obs.	Ln(City size$_{\text{Initial year}}$)			Rank$_{\text{Initial year}}$			Variance$_{\text{large}}$	Variance$_{\text{small}}$	F-Statistic
		Constant	Coefficient	R^2	Constant	Coefficient	R^2			
1910–90	19	1.895# (0.955)	–0.122 (0.092)	0.092	0.700** (0.239)	–0.005 (0.020)	0.004	0.285	0.234	1.218
1910–50	19	1.179 (0.836)	–0.069 (0.081)	0.041	0.507* (0.204)	–0.003 (0.017)	0.002	0.196	0.183	1.072
1950–90	22	0.771# (0.436)	–0.055 (0.041)	0.084	0.121 (0.005)	0.005 (0.007)	0.026	0.034	0.060	1.739

Notes: The table reports the results of a simple growth regression of the form: $ln(City\ growth_{Period}) = \alpha + \beta\ Variable$. Standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

To examine Gabaix's (1999) hypothesis of growth variances being independent from city size, the sample is sorted by size in the initial year and split into two halves. Table 2.6 then also reports variances of calculated log-growth rates for these two subsamples. Even though the variances vary a bit between both samples, an F-test indicates that these differences are statistically not significant. In contrast to Gabaix's findings for France and Japan, there is also no consistent pattern of large cities displaying always a higher variance. For 1950–90, the upper half of the distribution confirms the theoretical reasoning that large cities have smaller variances.

In sum, the Austrian experience basically supports Gabaix's (1999) explanation for the emergence of Zipf's law. After a shock has disrupted the existing urban network, the national system of cities in Austria appears to converge to a new Zipf distribution, while both of Gabaix's necessary conditions – random growth and equal variances across different city sizes – are met. The only remaining problem then is the surprisingly slow speed of convergence in Austria which probably can be explained by the relatively small variances in city growth rates.

2.6 Summary

This chapter examines the evolution of national city size distributions in a number of European countries over a period of up to 120 years. Inspired by recent attempts to explain the emergence of a Zipf distribution theoretically, the analysis focuses particularly on some of the underlying assumptions necessary to make these explanations work. Notably, the chapter examines empirically whether there is random growth across cities and whether also the variances of that growth rate are independent of city size. The results can be summarized as follows:

1. I find that the national city size distributions in the sample mostly follow Zipf's law with reasonable precision. Even though there is some variation in the estimated Zipf exponents, they cluster around the expected value of 1.

2. There is basically no evidence for some of the proposed explanations for deviations from an exponent of 1. Neither excluding the country's largest city nor holding the sample size constant over time consistently improves the results.

3. The results do not support the claim that the growth rate of cities is independent of city size. An examination of the growth dynamics in 40-year-intervals rather suggests that the relationship between city size and subsequent growth has changed over time. While large cities grow

on average faster than smaller cities in an early development stage, there is convergence in later years.

4. Also the hypothesis that the variance of growth rates is constant across different city sizes is not robust. If variances differ significantly across size groups, often smaller cities display larger variances.
5. The natural experiment of the evolution of the Austrian city size distribution after the break-up of the Austro-Hungarian Empire in 1918 shows convergence towards a new Zipf distribution, while there is both random growth and equal variances across different city sizes. This national example supports the explanation by Gabaix (1999).

Chapter 3

Krugman and Livas Elizondo Revisited: Is There a Link Between Trade Policy and Urban Concentration?

3.1 Introduction

A very interesting contribution of the new economic geography literature is the recent claim that large urban metropolises are – at least in part – the result of protectionist trade policies. Inspired by the case of Mexico City and São Paulo, Paul Krugman has developed a simple theoretical model which shows that the opening of trade leads to a less concentrated urban system. The reasoning is that while in autarky firms have a strong incentive to choose production sites with good access to both inputs and consumers, there is only little advantage for a firm to be located in a country's largest city when an economy is open to international trade.

In both presentations (Krugman and Livas Elizondo 1996, Krugman 1996a), however, Krugman's model is only very roughly sketched. In fact, there are two basic deficiencies. On the one hand, the model incorporates a number of simplifying (and, of course, unrealistic) ad hoc assumptions. Vernon Henderson (1996), for example, notes that Krugman's results depend crucially on the assumption that domestic cities are equidistant from international markets. On the other hand, as the model is basically solved through simulations, it is surprising that no sensitivity analysis is provided. Krugman only varies his protection parameter and shows how this affects the population concentration in the model without checking the robustness of his results.

This chapter deals with both criticisms. In particular, I attempt to develop a variant of Krugman's basic theoretical model that allows to analyze the following extensions. First, I divide overall transaction costs for shipping goods between home locations and the "rest of the world" into transportation costs and tariffs. Basically, this has the advantage that it is possible to introduce a redistribution of tariff revenues, i.e., a factor which is likely to speak in favor of protectionist policies (and, in fact, often has done so, especially in developing countries). On this point, Krugman and Livas Elizondo (1996, p. 144) plainly note "... we simply imagine that any potential revenue is somehow dissipated in waste of real resources – not too unrealistic a view, if the rent-seeking story is to be believed."

Even more important is, however, that a separate treatment of natural transport costs and artificial trade barriers allows to discuss the critical range of protection. Krugman and Livas Elizondo (hereafter, K-LE) simply assume a parameter value for the costs of transacting between domestic locations and then calculate the critical range for the protection parameter. Being fully confined on the mechanics of the model, they discuss neither their assumption nor their results.

Second, with respect to Henderson's (1996) critique, I allow for different transportation costs between domestic cities and the "rest of the world". In fact, Henderson's point is quite intuitive. Suppose a country has only two locations, where one city is located near the border (e.g., at the coast) and the other is at an interior central site. If in autarky all production is concentrated at one of these sites, the introduction of trade will not affect urban concentration. More specifically, if all production is at the border, it will remain there, while if it is in the center it will either remain there (if the locational advantage of the border location is not large enough to induce a shift) or it will all shift to the periphery. In either case, the degree of urban centralization remains unchanged.

Third, I address the effects of trade liberalization on urban concentration for different parameter values. This exercise is a robustness check which aims to analyze whether Krugman's results are indeed valid for the full range of plausible parameter values.

The chapter is organized in the following way. Section 2 presents the theoretical model. Section 3 discusses the results of the simulations which show that K-LE's findings are not robust and largely depend on unreasonable parameter assumptions. Section 4 provides some concluding remarks.

3.2 The Model

The following model is a slightly modified and extended version of a new economic geography model developed by K-LE. Both the notation and the description will closely follow their presentation to assure comparability. Instead of purely focusing on the mechanics of the basic model, however, the extensions aim to provide a more realistic set-up and, effectively, to allow a discussion of the critical parameter values.

3.2.1 Stylized World Geography

In order to use the simplest possible framework for modeling both urban concentration in a country and international trade, I consider an economy which consists of only three locations. Two locations (labeled 1 and 2) are assumed to be domestic sites so that the relative size of these two cities measures the degree of urban centralization. The third location (labeled 0) represents the "rest of the world". Figure 3.1 illustrates the basic architecture of the stylized world geography.

There are two ways for interactions between these three locations. First, labor, which is the only factor of production, is freely mobile within a country but not across countries. Workers can move at no cost between locations 1 and 2 but cannot cross borders. Therefore, given that the total national supply of labor (L) is fixed, we have $\bar{L} = L_1 + L_2$.

Second, goods can be shipped between locations. Transportation of goods, however, is costly. In particular, departing from K-LE, I divide overall transaction costs into natural transport costs (e. g., costs of the physical transportation of goods, costs of communications) and artificial trade barriers (e. g., tariffs).

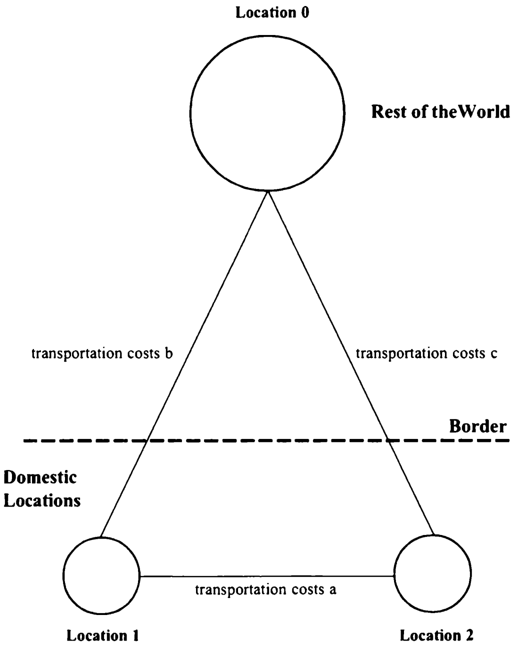

Figure 3.1: Stylized World Geography

DOI https://doi.org/10.3790/978-3-428-50499-2

Following conventional practice, transport costs are assumed to be of Samuelson's "iceberg" type: For every unit of goods that is shipped, only $1-\phi$ unit of goods arrives, while the rest (ϕ of the good) melts on the way. Specifically, the cost of domestic transport will be represented as a, while that of cross-border transport between domestic locations 1 and 2 and the "rest of the world" is given by b and c, respectively, where $0 \leq a, b, c \leq 1$. While K-LE always assume that domestic locations are of equal distance from international markets ($b = c$), it is one of the contributions of this extended model that it allows for the case where cities are of different distances from the border. Then, without loss of generality, I assume that location 1 is the site on the country's periphery. This means that $b \leq c$.

Also, tariffs will be treated in the standard way. The government levies an ad-valorem tariff t on foreign goods. Specifically, it is assumed that the tariff is levied as a proportion of the value of the good expressed in c.i.f. terms, i.e., including transportation costs.[1]

In sum, for every unit of a good consumed, but not produced at home, consumers in location 1 will have to pay

$$P_{0.1} = \frac{(1+t)P_{0.0}}{1-b}, \tag{3.1}$$

$$P_{2.1} = \frac{P_{2.2}}{1-a} \tag{3.2}$$

for a good imported from location 0 and shipped from location 2, respectively. Similar expressions can be derived for the consumers at each of the other locations.

3.2.2 Wage Structure

Following K-LE, I assume that each location is a linear city. In particular, it is supposed that workers live on a fixed unit of land and are effectively spread along a line. Production, however, takes place at a single central point (e.g., a central business district) so that workers must commute to work. If we, then, assume that the living space which each worker needs is one unit of land, the distance the last worker in location j must commute is[2]

[1] Recent studies in the trading blocs literature suggest that the results are not qualitatively different if instead the probably less realistic f.o.b.-based assumption is used (see Frankel, Stein and Wei [1993, 1995]).

[2] Here the basic advantage of our assumption of a linear city becomes obvious as the commuting distance of the last worker is simply proportional to a location's

DOI https://doi.org/10.3790/978-3-428-50499-2

(3.3) $$d_j = \frac{L_j}{2}.$$

Commuting costs are incurred in labor or, more specifically, in workers' time so that time spent commuting is time not spent working. In particular, suppose a worker is endowed with one unit of labor. If he then must commute a distance d, he arrives at the central business district with a net amount of labor to sell of only

(3.4) $$S = 1 - 2\gamma d,$$

where γ is a parameter capturing the constant marginal costs of commuting, with $0 \leq \gamma \leq 1$. Accordingly, the last worker in location j can sell an amount of labor net of commuting of $S = 1 - \gamma L_j$.

While workers who live close to the city center save time on commuting and are effectively able to offer a higher net amount of labor (and, thus, earn more money), they must pay an offsetting land rent. In particular, it is assumed that the land rents always exactly offset the locational advantage of being closer to the center. On the extremes, the last worker who lives on the edge of the city pays no land rent (as there is no locational advantage), but has the longest commuting distance, while the worker who lives inside the business district has no commuting costs, but has to pay a land rent which is identical to the commuting costs of the last worker. Therefore, all workers receive the same wage net of both commuting and land rents. Figure 3.2 sketches the overall picture of the wage structure in a location.

In order to determine this net wage, suppose that w_j is the wage rate per unit of labor paid in the business district. At the outskirts of the town, the last worker then receives a net wage of only $(1 - \gamma L_j)\ w_j$ because of the time spent in commuting. As the land rent always compensates for this locational disadvantage, this is also exactly the wage net of both commuting and land rents for all workers. Therefore, as a city becomes larger, the commuting distance of the last worker increases and the net wage of all workers declines (for positive costs of commuting) as illustrated in figure 3.3.

By multiplying the labor input per worker[3] with the labor force L_j, the total labor input of a location is

(3.5) $$Z_j = L_j(1 - 0.5\gamma L_j).$$

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population while in a disk-shaped city it would depend on the square root of the population (Krugman [1996a, p. 14]).

[3] The labor input per worker in a location is simply the average of the labor input of the worker living in the center and the worker living at the edge of the city.

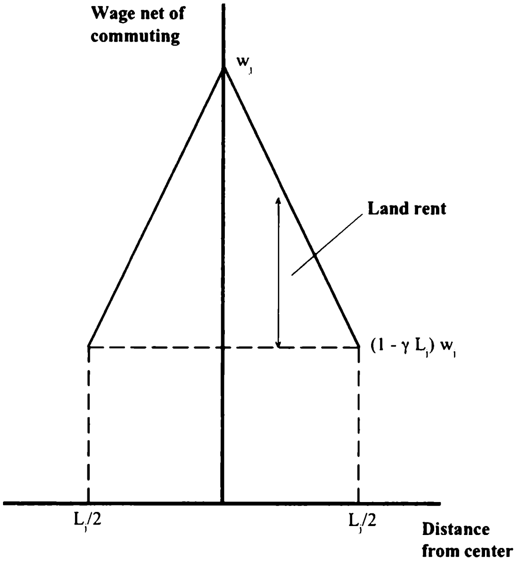
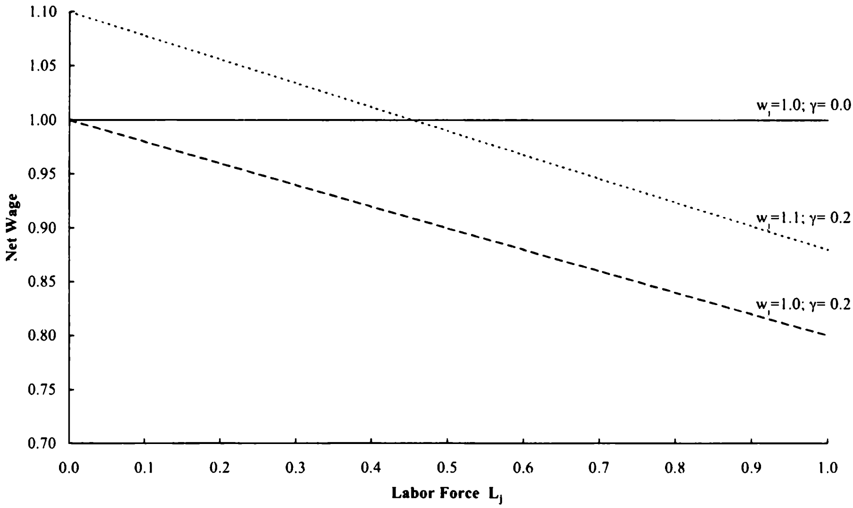
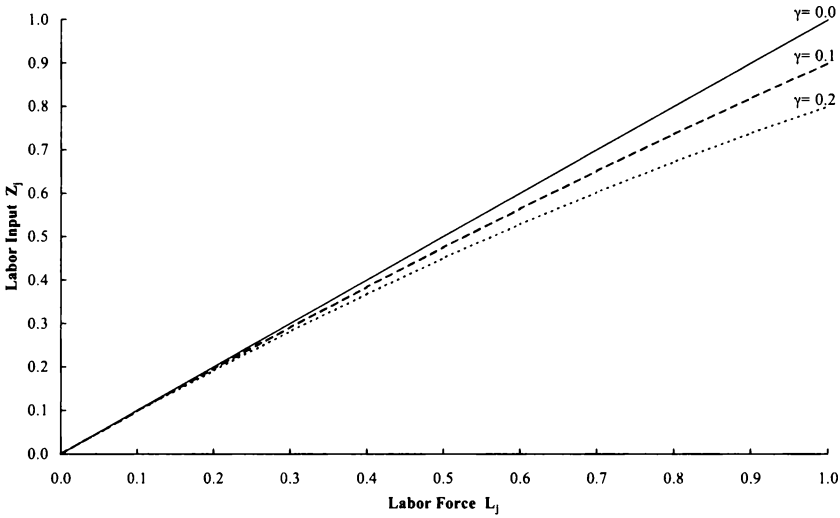


Figure 3.2: Wage Structure



Notes: The wage net of both commuting and land rents is defined as $(1 - \gamma L_j)w_j$.

Figure 3.3: The Relationship Between Labor Force and the Wage Level Net of Commuting and Land Rents



Notes: Labor input is defined as $Z_j = L_j(1 - 0.5\gamma L_j)$.

Figure 3.4: The Relationship Between Labor Force and Labor Input

Figure 3.4 plots the labor input as a function of the labor force. It is shown that the labor input exactly matches the labor force as long as commuting involves no costs ($\gamma = 0$) (see also equation 3.4). The higher the overall commuting costs[4], the lower is the labor input Z_j and the larger is the deviation from the 45°-line.

Given the total labor input of a location, the location's total income, including the income of landowners but without an explicit redistribution of tariffs, is

$$Y_j = w_j Z_j. \tag{3.6}$$

3.2.3 Consumer's Problem

Suppose that there is a large number of symmetric goods or varieties being produced in the economy, and there is a much larger number of potential products. A typical consumer, then, chooses from the menu of avail-

[4] Note that the overall commuting costs are equally determined by the cost coefficient γ and city size L_j.

DOI https://doi.org/10.3790/978-3-428-50499-2

able goods (indexed by i) to maximize the following constant elasticity of substitution utility function

$$U = \left[\Sigma_i C_i^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}}, \quad \sigma > 1 \tag{3.7}$$

where C_i is the consumption of the ith good and σ is the elasticity of substitution indicating the consumer's love for variety.

Even though all goods enter symmetrically into the consumer's utility function, he or she will be paying different prices for the consumption of varieties produced at home and shipped from other locations and, therefore, will consume them in different quantities. Specifically, the first-order conditions for the consumer's problem in a domestic location (say location 1) yield the following relative consumption of a product imported from location 0 and shipped from location 2, respectively:

$$C_{0.1} = C_{1.1}\left(\frac{P_{0.1}}{P_{1.1}}\right)^{-\sigma} \tag{3.8}$$

$$C_{2.1} = C_{1.1}\left(\frac{P_{2.1}}{P_{1.1}}\right)^{-\sigma}, \tag{3.9}$$

where $C_{j,k}$ is the consumption of a typical good from location j at location k. Similar expressions can be derived for all other combinations of relative consumption pairs.

3.2.4 Producer's Problem

While commuting costs and land rents are diseconomies of agglomeration working in our model as centrifugal forces, the production side provides advantages of concentration and, therefore, introduces a compensating centripetal force. In particular, building on the familiar features of Dixit-Stiglitz-type models it is assumed that agglomeration benefits arise from economies of scale which lead to imperfect competition.

Each producer is a profit-maximizing monopolistic competitor. Free entry, however, drives profits to zero implying that every producer chooses to specialize in producing a different variety from other producers. Thus, a large concentration of population produces a large variety of differentiated products.

Assume, then, that the production of any good i at location j involves a fixed cost α and a constant marginal cost β, so that the production technology exhibits increasing returns to scale:

(3.10) $$Z_{ij} = \alpha + \beta Q_{ij},$$

where Q_{ij} is total output and Z_{ij} is the amount of labor used in producing that variety.

Firms set the price to maximize the profit function $\Pi_{ij} = P_{ij}Q_{ij} - (\alpha + \beta Q_{ij})\, w_j$ where Π_{ij} is the profit of the ith producer and P_{ij} is the price of the ith differentiated product. Given the demand structure, where all goods enter symmetrically into demand, the profit-maximizing price is

(3.11) $$P_{ij} = \frac{\sigma}{\sigma - 1}\beta w_j.$$

Note that the price of each variety produced at location j will be the same since the parameters β, w_j, and σ are the same for all varieties produced at location j.

Assuming free entry and free exit of firms, profits are driven to zero ($\Pi = 0$) and output per variety is given by

(3.12) $$Q = \frac{\alpha(\sigma - 1)}{\beta}.$$

Thus, in equilibrium the production of each variety depends only on the cost parameters α and β and on the elasticity of substitution σ, which have been assumed to be identical for all goods. Given that the scale of output of each variety is constant, it is the number of goods produced at each location n_j that depends on the size of the location in terms of labor (after commuting):

(3.13) $$n_j = \frac{Z_j}{\alpha \sigma}$$

To make matters easier, assume that – without loss of generality – the fixed costs of setting up production of a new good is given by $\alpha = \frac{1}{\sigma}$. Equation (3.13), then, simplifies to

(3.14) $$n_j = Z_j$$

which basically implies that good bundles are defined in a way which make them equal to the net amount of labor supplied in any location.

Also, marginal production costs are assumed to depend on the substitution parameter taking the specific form $\beta = \frac{\sigma - 1}{\sigma}$. Accordingly, equation (3.11) can be reduced to

(3.15) $$P_j = w_j$$

so that the f.o.b. price of goods produced at any location is equal to the wage rate at the location's center.

3.2.5 Government's Problem

Extending K-LE's analysis, I allow for a government sector in the economy and, specifically, for a redistribution of tariffs. I abstract, however, from modeling the process through which governments choose their tariff level and simply assume that a uniform tariff rate t is imposed on the c.i.f. value of all imported goods.

Thus, the total tariff revenue in a domestic location j ($j = 1, 2$) is

(3.16) $$R_j = \frac{t}{1+t} n_0 P_{0.j} C_{0.j}.$$

The redistribution of the tariff revenue is assumed to take the form of a transfer to domestic consumers in lump-sum fashion so that revenues are simply added to a location's total income. Equation (3.6) then becomes

(3.17) $$Y_j = w_j Z_j + R_j.$$

3.2.6 Equilibrium

Having laid out the basic model, the aim now is to determine the equilibrium allocation of labor between the two domestic locations. As workers are freely mobile between locations 1 and 2, the equilibrium requires that all workers receive the same net real wage. In fact, it should be obvious that if one of the locations offers a higher net real wage this provides for workers an incentive to move.

To solve the model for real wages, it is first necessary to define consumer price indices for goods consumed in each location. To simplify notation, it is useful to start with two definitions. First, the number of goods produced in any location as a share of world production is given by

(3.18) $$\lambda_j = \frac{n_j}{\Sigma_k n_k} = \frac{Z_j}{\Sigma_k Z_k}.$$

Second, K is defined as the total number of goods produced in the world raised to the power of $1/(1-\sigma)$ so that

(3.19) $$K = (n_0 + n_1 + n_2)^{\frac{1}{1-\sigma}}.$$

Then, the price indices can be defined as

(3.20) $$T_0 = K\left[\lambda_0 w_0^{1-\sigma} + \lambda_1 w_1^{1-\sigma} + \lambda_2 w_2^{1-\sigma}\right]^{\frac{1}{1-\sigma}},$$

(3.21) $$T_1 = K\left[\lambda_0\left(\frac{1+t}{1-b}w_0\right)^{1-\sigma} + \lambda_1 w_1^{1-\sigma} + \lambda_2\left(\frac{1}{1-a}w_2\right)^{1-\sigma}\right]^{\frac{1}{1-\sigma}}$$

(3.22) $$T_2 = K\left[\lambda_0\left(\frac{1+t}{1-c}w_0\right)^{1-\sigma} + \lambda_1\left(\frac{1}{1-a}w_1\right)^{1-\sigma} + \lambda_2 w_2^{1-\sigma}\right]^{\frac{1}{1-\sigma}},$$

where I follow K-LE in assuming that exports to location 0 are not affected by any barriers to trade.[5]

Accordingly, the net real wage in location j can be defined as

(3.23) $$\omega_j = \frac{(1 - \gamma L_j)w_j}{T_j},$$

so that the adjustment mechanism towards an equilibrium allocation of domestic labor between the two home locations can be assumed to take the form

(3.24) $$\frac{\mathrm{d}L_1}{\mathrm{d}t} = -\frac{\mathrm{d}L_2}{\mathrm{d}t} = \delta(\omega_1 - \omega_2),$$

where any positive net real wage differential provides an incentive for workers to move from location 2 to location 1 and vice versa.

Having defined the price indices, we can now solve the model for the nominal wage rates at domestic city centers. Consider, then, how consumers in a location, say location 0, spend their income:

(3.25) $$Y_0 = n_0 P_{0.0} C_{0.0} + n_1 P_{1.0} C_{1.0} + n_2 P_{2.0} C_{2.0}$$

Substituting relative consumption pairs as derived in equations (3.8) and (3.9) and the price index in location 0, we can derive the total demand of consumers at 0 for a typical good from location 1:

[5] As the analysis in this chapter exclusively focuses on the equilibrium allocation of labor between the two domestic locations and takes the "rest of the world" as given, this assumption has no qualitative impact on the results.

$$P_{1.0}C_{1.0} = Y_0 \left[\frac{P_{1.0}}{T_0}\right]^{1-\sigma}, \tag{3.26}$$

and similar expressions for the expenditure of each location's consumers on goods either imported or produced at home.

As the total income of a location is simply the global expenditure on goods produced at this location (leaving tariff revenues aside for a moment), we have

$$w_1 Z_1 = n_1 \left[Y_0 \left(\frac{w_1,}{T_0}\right)^{1-\sigma} + Y_1 \left(\frac{w_1,}{T_1}\right)^{1-\sigma} Y_2 \left(\frac{1}{1-a}\frac{w_1}{T_2}\right)^{1-\sigma} \right] \tag{3.27}$$

or, after some algebra,

$$w_1 = \left[Y_0\, T_0^{\sigma-1} + Y_1\, T_1^{\sigma-1} + Y_2([1-a]T_2)^{\sigma-1}\right]^{\frac{1}{\sigma}}. \tag{3.28}$$

Similarly, we can derive an expression for the net wage rate paid at the central business district of location 2:

$$w_2 = \left[Y_0\, T_0^{\sigma-1} + Y_1([1-a]T_1)^{\sigma-1} + Y_2\, T_2^{\sigma-1}\right]^{\frac{1}{\sigma}}. \tag{3.29}$$

As the above system of equilibrium conditions is nonlinear, it cannot be solved explicitly. So, following K-LE, the model is solved through numerical simulations. In particular, for any given allocation of labor between domestic locations 1 and 2, the total number of products produced at each location can be determined and the equations for incomes ([3.6]), price indices ([3.20]–[3.22]) and wage rates ([3.28] and [3.29]) are then solved simultaneously. Finally, using (3.23), the real wage rates are obtained and the real wage differential can be calculated.

3.3 Simulation Results

While K-LE have demonstrated the basic mechanics of the model, the intention of this chapter is to check the robustness of their results and to allow for some extensions. In particular, the aim is to analyze whether the model really yields a strong positive linkage between the openness of an economy and the degree of population concentration as K-LE suggest.

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DOI https://doi.org/10.3790/978-3-428-50499-2

3.3.1 Replicating Krugman and Livas Elizondo (1996)

In a first application, I check that the slightly modified version of K-LE's model which now incorporates transport costs and tariffs separately does in fact encompass their results. To allow comparability, I use exactly the same parameter values without discussing their plausibility.

Assume, then, that $L = 1$, $\sigma = 4$, $\gamma = 0.2$, $Z_0 = 10$ and $a = b = c = 0.286$ (which is comparable with K-LE's assumption of $\tau = 1.4$[6]). Further, an initial degree of protection of $t = 0.307$ (again identical with K-LE's figure of $\rho = 1.83$[7]) is assumed.

Following K-LE, I calculate the real wage differential $\omega_1 - \omega_2$ for any combination of the distribution of labor between the two domestic sites. Plotting this wage differential against the labor force in one of the domestic locations, say location 1, then nicely illustrates the equilibrium distribution of workers and, therefore, the degree of urban centralization.

In particular, an equilibrium requires that the wage differential between the two cities is zero so that workers have no incentive to move. Such an equilibrium is only stable, however, if the schedule is downward sloping so that any deviation from that equilibrium allocation implies that there are incentives to move back to that equilibrium. A positive wage differential, for example, induces workers to move to location 1 and, therefore, increases the size of location 1 and reduces the wage premium. If the wage differential is zero and the schedule is upward sloping, the equilibrium is unstable. Then, any incremental shift away from that equilibrium allocation of labor would, according to our dynamics, provide incentives for other workers to move and, thus, drive the distribution further away from that equilibrium.

Finally, there may also be corner equilibria. If, for example, all workers are concentrated in location 1 and there is also a positive wage differential, i.e., location 1 offers a higher wage rate than location 2, there is no incentive to move and workers stay in location 1.

Figure 3.5 then plots the real wage differential for the full range of possible allocations of labor in the domestic economy using the different "critical" protection parameter values as suggested by K-LE. Contrary to K-LE,

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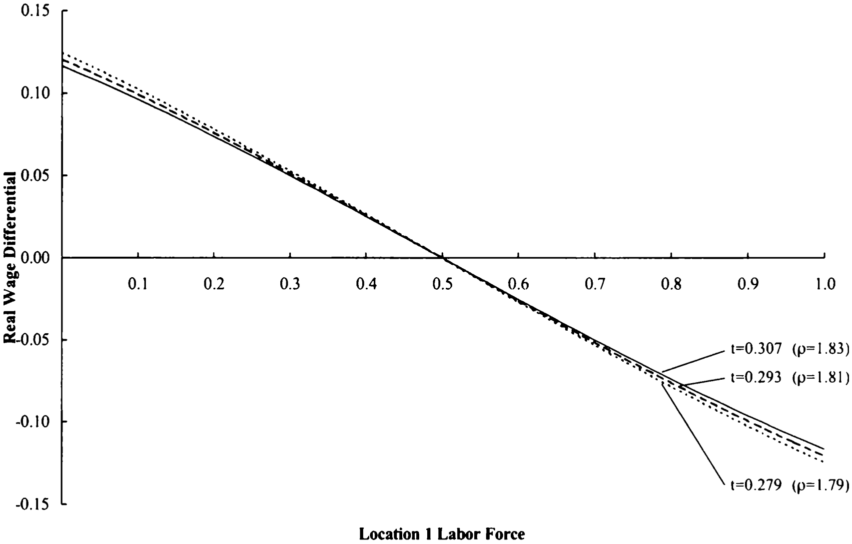
[6] Krugman and Livas Elizondo (1996) define transport costs in a way so that the c.i.f. price of a good shipped is τ times its f.o.b. price. Therefore, comparing with the formulation in the model laid out above, we have $\tau = \dfrac{1}{1-a}$.

[7] In particular, we have $\rho = \dfrac{1+t}{1-b}$ or, for identical transport costs between domestic and foreign locations ($a = b$), $\rho = (1+t)\tau$.



Notes: The following parameters were used in the simulations: $L = 1$; $\sigma = 4$; $\gamma = 0.2$; $Z_0 = 10$; $a = b = c = 0.286$ ($\tau = 1.4$).

Figure 3.5: Replicating Krugman and Livas Elizondo (1996)
Using Their Parameter Values

however, I get for all parameters qualitatively identical results. There is one stable equilibrium in which population is evenly distributed between the two locations. In appendix C, I show in a step-by-step calculation that this finding also results from K-LE's original model.[8]

This finding is particularly striking for two reasons. First, using exactly K-LE's parameter values, I surprisingly observe results which are both quantitatively and qualitatively different from that of K-LE. Second, and even more important, the simulation results suggest that K-LE's claim that there is a linkage between trade policy and urban concentration is not robust. In the example above, the modification of the protection parameter has no effect on urban concentration. For all different degrees of protection, the model yields identical equilibria in which a country's population is equally divided between two locations.

[8] I have also tried a number of simulations varying the other parameters to allow for the possibility of typing errors. I was unable, however, to replicate Krugman and Livas Elizondo's (1996) results using variants of their parameter values.

Only for significantly higher values of the protection parameter, I am able to replicate K-LE's results (at least qualitatively). Assume, then, that we have $t = 1.14$ (comparable to $\rho = 3.0$ in K-LE's original formulation). As shown in graph (a) of figure 3.6, the only stable equilibria are those allocations in which the country's population is concentrated in one of the two locations. The equilibrium in which each location accommodates half of the country's population is unstable.

In a second step, the degree of protection is slightly reduced so that the economy becomes more open. As the graph in the middle of figure 3.6 illustrates, the equilibrium in which the population is evenly divided between the two locations is now stable. But, also the two corner equilibria in which the total population is centralized in either location 1 or location 2 are still stable. Moreover, between those stable allocations there are two unstable equilibria.

Finally, graph (c) of figure 3.6 shows what happens if the economy opens up further ($t = 0.857$). Now, the equal-division allocation is the only stable equilibrium.

In sum, for certain parameter values, I observe K-LE's pattern in which the internal geography of an economy depends on its degree of openness. Figure 3.7 illustrates this result, showing how the equilibrium allocation of domestic labor varies with the rate of protection where solid (dotted) lines represent stable (unstable) equilibria. In a completely open economy with zero tariffs, the only stable equilibrium is with production evenly distributed between the two domestic sites. On the other hand, when protection is high, the only stable equilibria are with workers completely concentrated in one of the two locations. Between those two outcomes, there is a narrow range of protection in which both kinds of urban structures (i.e., dispersion and complete centralization) are possible.

3.3.2 Sensitivity Analysis

While K-LE's approach to explain the existence of urban metropolises as a consequence of protectionist trade policies is both innovative and surprising, it is particularly disappointing that they neither discuss their assumed parameter values nor provide a robustness check of their results. This, however, comes uncomfortably close to what T. N. Srinivasan (1993, p. 85) once has called "theory without relevance".

Therefore, in a second application, I analyze the plausibility of K-LE's parameter choices and examine whether their results are also valid for more realistic parameter values. While it is useful to normalize the total domestic labor force L to be one and it is also fairly conventional to

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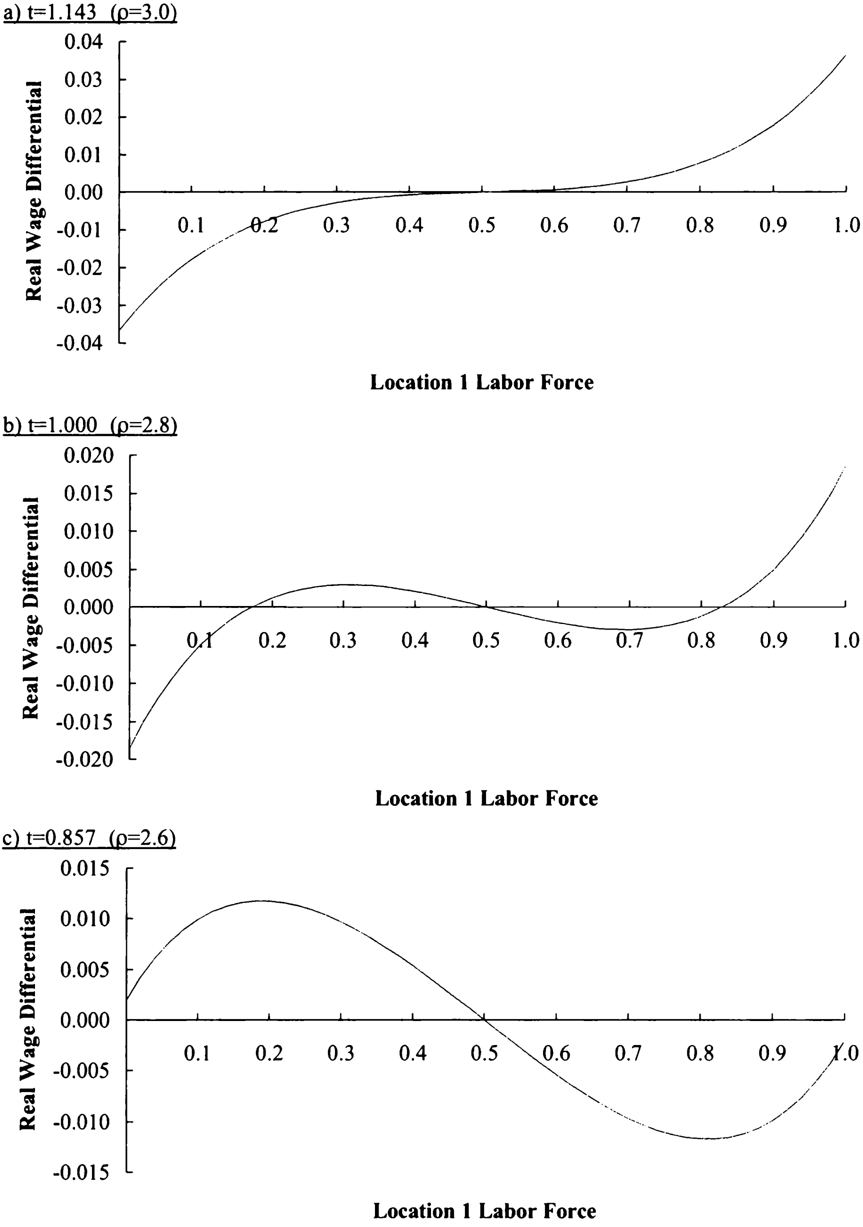

Notes: The following parameters were used in the simulations: $L = 1$; $\sigma = 4$; $\gamma = 0.2$; $Z_0 = 10$; $a = b = c = 0.286$ ($\tau = 1.4$).

Figure 3.6: Replicating the Results of Krugman and Livas Elizondo (1996)

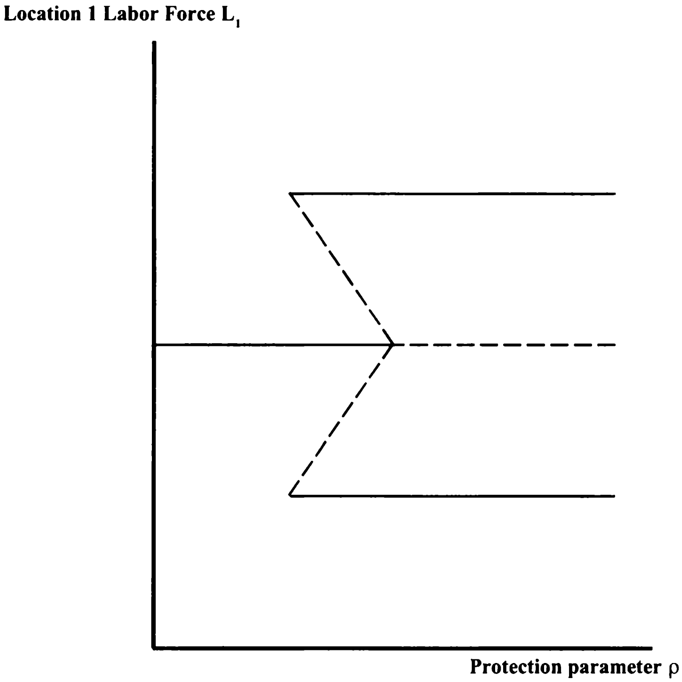

Figure 3.7: Equilibrium Allocation of Labor as a Function of the Rate of Protection

assume a substitution parameter $\frac{\sigma - 1}{\sigma}$ of 0.75[9], I will pay special attention to the marginal costs of commuting γ, the net labor input in the "rest of the world" Z_0 and to transaction costs. In particular, it is one of the contributions of the model presented in this chapter to allow for a separate treatment of natural and artificial barriers to trade.

Let us start then with the marginal costs of commuting. To get an idea about a reasonable magnitude of this parameter remember that commuting costs are incurred in labor. Using equation (3.5), a value of 0.2 implies that workers in large metropolises spend on average about one fifth of their total working time commuting (see also figure 3.4 for an illustration). Assuming a net working day of 8 hours, this would imply an average one-way commuting time of one hour in the largest cities. As a rough estimate, this

[9] See, for example, the discussion in Krugman (1991a, p. 19) and Frankel (1997, p. 167 and appendix D).

DOI https://doi.org/10.3790/978-3-428-50499-2

seems to be not too unrealistic so that I will keep K-LE's assumption of $\gamma = 0.2$ in all following simulations.

In a next step, it is necessary to get an idea about the magnitude of transaction costs. Here, K-LE simply assume that for shipments between the two domestic locations the ratio of import values, including insurance and freight, to export values is about 1.4 without providing any comment whether their assumption is reasonable. This ad hoc procedure is even more surprising as there have been recently considerable attempts to get realistic estimates of the magnitude of transport costs in international trade. Jeffrey Frankel (1997) and Frankel, Stein and Wei (1995), for example, provide a thorough empirical analysis of costs to doing business at a distance. Although there is generally wide variation in shipping costs across commodities and across countries[10], they report as a first crude measure a total worldwide c.i.f./f.o.b. ratio of about 1.06. This is, however, considerably lower than the value K-LE have assumed for a country's *internal* trade. Given that in Frankel, Stein and Wei's (1995, p. 90) sample the mean distance between countries on the same continent (different continents) is 2,896 (11,776) kilometers, and the average distance within countries can be assumed to be considerably smaller[11], the true intra-national shipping costs will probably be only a fraction of the worldwide average.[12]

Specifically, Frankel (1997, pp. 198–200) uses the estimate of worldwide shipping costs of about 6 percent and attempts to separate roughly between inter-continental and intra-continental transport costs, obtaining an estimate of about 9 to 10 percent for the former and about 2.5 percent for the latter. As it is likely, however, that the c.i.f./f.o.b. ratio understates the costs of trade by focusing solely on the cost of physical transport, I assume a value of $a = 0.05$ (for internal trade) and $b = c = 0.15$ (for international trade).[13]

[10] David Hummels (1998, 1999) provides a detailed discussion of rare data on international shipping costs.

[11] Recently, there has been an intensifying discussion in the trade literature about the "correct" method of estimating average distances for doing trade within countries (see, for instance, Wei [1996] and Nitsch [2000]).

[12] Admittedly, it is possible that due to the poor condition of the transportation infrastructure in developing countries, the costs for shipping goods within those countries could be higher than a simple comparison with the worldwide average suggests. Recent research by Amjadi and Yeats (1995), however, shows that international transport costs for developing countries do not differ much from that of developed countries with the exception of Africa.

[13] A number of studies in the trading blocs literature argue that transportation costs within continents are so small that they can be neglected for the purpose of simplicity. This practice, however, has been criticized by Nitsch (1996).

DOI https://doi.org/10.3790/978-3-428-50499-2

Finally, considering the net labor input outside of the home country, the premise should be to assume that the domestic economy is a small country compared to the "rest of the world". Given that the total domestic supply of labor is normalized to be one and, therefore, the total net labor input in the home economy can be very close to one[14], K-LE's assumption of $Z_0 = 10$ implies that the share of the home economy in world production is near one-tenth. As K-LE, however, particularly claim to analyze the linkage between urban metropolises and trade policies in developing countries, this assumption is quite unreasonable. Perhaps with the exception of China and India, all developing countries have populations less than one-tenth of the world population. Moreover, as in our model the number of varieties produced in an economy is given by its net labor input, it has to be noted that there is no developing country which produces different varieties that even come close to one-tenth of the varieties available in the world. Therefore, in the following simulation, I will use a parameter value of $Z_0 = 100$.[15]

Having discussed the range of reasonable parameter values, it is now possible to analyze whether K-LE's results are robust in this parameter space or whether their findings are sensitive to their crude and highly unrealistic assumptions.

Figure 3.8 presents the results of this simulation plotting again the real wage differential for all possible labor allocations in the domestic economy. It is easily observable that the results do not qualitatively differ for various rates of protection. In particular, I have varied the tariff rate from 0% (i.e., free trade) to an extreme of 1,000%. In each case, there is only one stable equilibrium in which workers are evenly distributed between the two sites. The rate of protection only determines the speed of adjustment towards this equilibrium.

This shows, however, that K-LE's theoretical claim that urban metropolises are an unintended by-product of protectionist trade policies is – besides of being not robust – not valid for reasonable parameter values. Specifically, K-LE's simulation results depend crucially on their assumption of extremely high values of transportation costs. Accordingly, Krugman's model is an interesting theoretical exercise but falls short of explaining the giant size of primate cities in developing countries.

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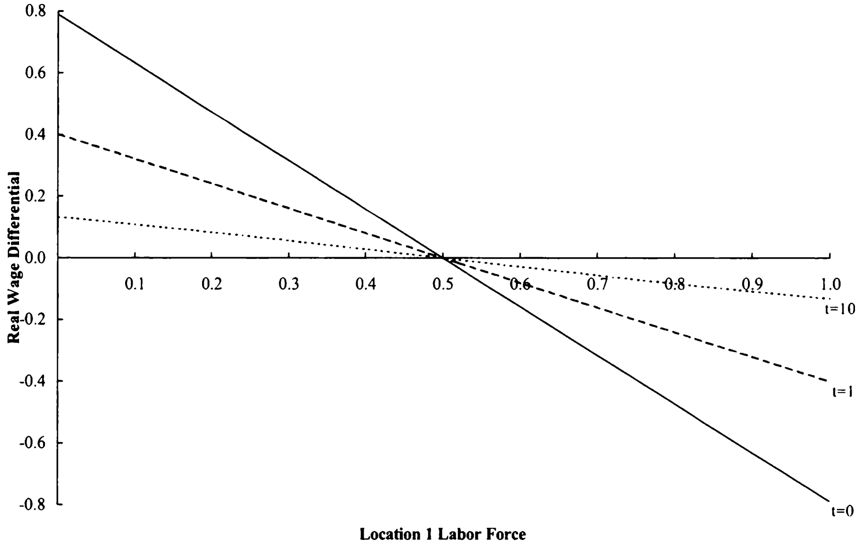
[14] For example, when the fixed supply of domestic labor $L = 1$ is evenly distributed between the two sites, the net labor input in the economy is $Z = 0.95$.

[15] In simulations not reported here, it turns out, however, that this assumption has no qualitative impact on the results.



Notes: The following parameters were used in the simulations: $L = 1$; $\sigma = 4$; $\gamma = 0.2$; $Z_0 = 100$; $a = 0.05$; $b = c = 0.15$.

Figure 3.8: Real Wage Differentials Using More Plausible Parameter Values

3.3.3 Allowing for Different Distances Between Domestic Locations and the ROW

In this application, I will analyze Vernon Henderson's (1996) critique that Krugman's (1996a) results depend on the assumption that domestic cities are equidistant from international markets. In particular, Henderson claims that the opening of trade has no impact on the degree of urban concentration as long as a country's population is fully centralized.

To allow for comparability with K-LE's original results, I will use their unreasonable parameter values as benchmark case. Further, different distances between domestic sites and the "rest of the world" are introduced by slightly lowering the transportation costs for shipments between locations 0 and 1 (and, accordingly, between domestic locations 1 and 2[16]). Specifi-

[16] This is due to the implicit assumption that intra-national trade should be at least as cost-attractive as any cross-border trade. In fact, evidence from John McCallum (1995) suggests that international borders matter a great deal. Even though the border between Canada and the U.S. is commonly assumed to be one of the most easily passable lines in the world and, therefore, to have relatively little

cally, I set $a = b = 0.25$ and keep the original value of $c = 0.28$ (= 0.4/1.4) for shipments between the interior central site 2 and the "rest of the world".

Figure 3.9 shows the results. In the top panel, the real wage differential is plotted for a very high tariff rate of 200%. As before for protectionist trade policies, there are two stable corner equilibria in which domestic labor is either concentrated in location 1 or in location 2. We also observe, however, that both sites are not equally attractive anymore. While in the benchmark case, the larger city offered in autarky a real wage premium, now site 1 which is closer to international markets has a locational advantage and, therefore, offers a higher real wage even if it is initially smaller than location 2. In our specific parameter space, the interior location will only attract workers if it initially comprises at least 70% of the total domestic population.

In graph (b), protection is lowered to a tariff rate of 100%. Supporting Henderson's (1996) intuition, we find that there is a positive wage differential for the full range of possible labor allocations so that location 1 always offers a higher wage rate than location 2. Even if all production was previously concentrated in location 2, it will all shift to the border location. This increased attractiveness of the outside location is due to the growing importance of international trade as trade barriers are lowered.

Finally, the tariff rate t is reduced to 50%. As illustrated in the lower panel of figure 3.9, the model now yields a stable equilibrium in which the domestic population is divided between the two sites, with location 1 having a larger share of the total population than location 2. This implies, however, that – contrary to Henderson's intuition – the locational advantage has become less important. As in K-LE's original model, the opening of an economy to international trade weakens the centripetal forces which create and support a single large metropolis. Offsetting centrifugal forces provide incentives for the creation of new cities so that lower rates of protection lead to a less concentrated urban system.

In sum, then, a tariff reduction first increases and then lowers the locational advantage of the border location. Even if domestic cities have different distances to international markets, K-LE's result that the opening of trade reduces the degree of urban concentration remains valid – given their unrealistic parameter space. Thus, Henderson's general claim that trade has no impact on urban concentration in Krugman's model if domestic cities are not equidistant from international markets is wrong.

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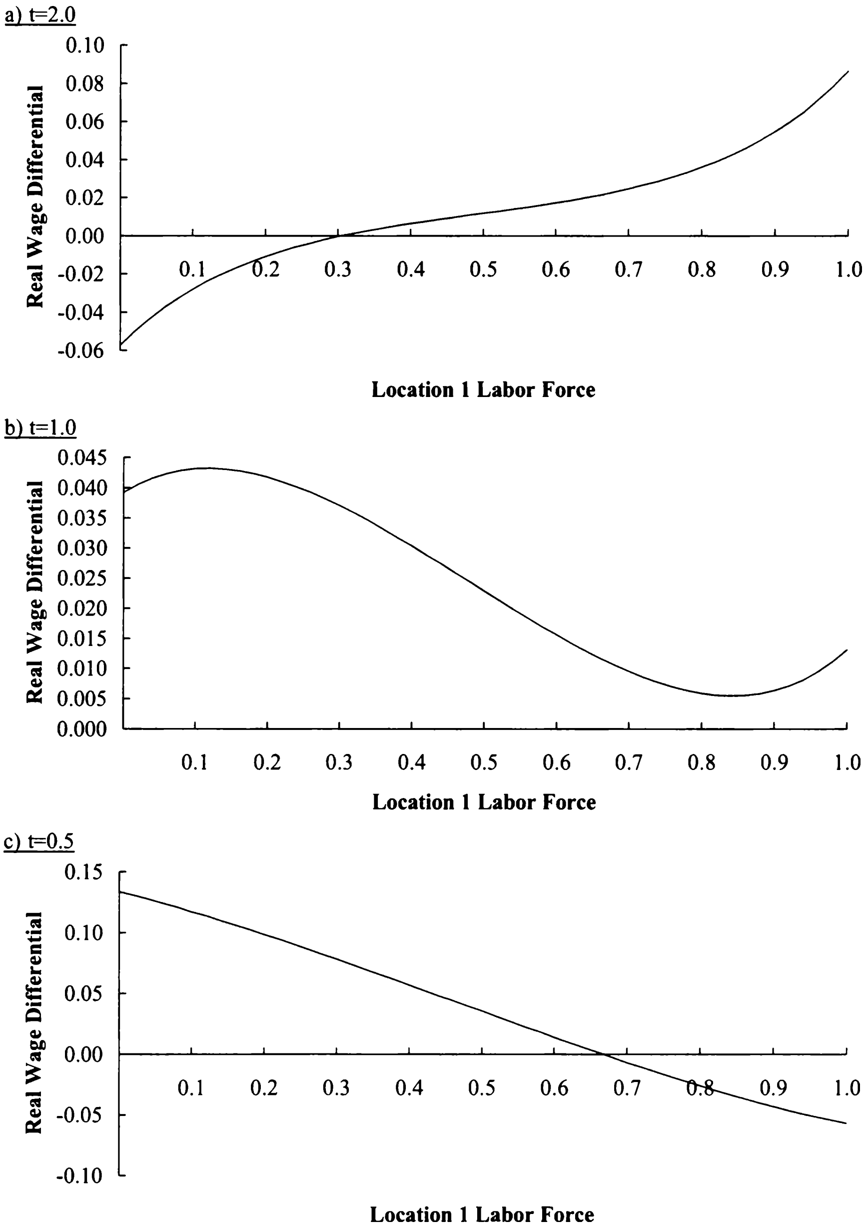
effect on trade, McCallum (1995) and Helliwell (1996, 1997) find that Canadian provinces trade about twenty times more with each other than they do with U.S. states of similar economic size and proximity.



Notes: The following parameters were used in the simulations: $L = 1$; $\sigma = 4$; $\gamma = 0.2$; $Z_0 = 10$; $a = b = 0.25$; $c = 0.286$.

Figure 3.9: Assuming Different Distances Between Domestic Locations and the Rest of the World

3.3.4 Allowing for a Redistribution of Tariff Revenues

As in the model presented in this chapter transaction costs are divided into natural and artificial barriers to trade, I will now examine the robustness of the results to a redistribution of tariff revenues. Intuitively, the impact should be limited as tariff revenues, which are simply introduced in the model as some additional income (see equation [3.17]), make up only a small share of total income. Moreover, returning to the baseline assumption of cities being equidistant from international markets, workers in both domestic locations benefit equally from revenues depending on their consumption share of imported goods.

It should also be noted, however, that tariff revenues provide an incentive for protectionist trade policies. Therefore, our intuition suggests that as a country gets more open to international trade, a redistribution of tariff revenues tends to support the initial status quo of a closed economy, i. e., in K-LE's case a monocentric regional structure.

As noted above, allowing for a redistribution of tariff revenues affects only a location's total income while the income derived from producing goods and the definition of price indices remain unchanged. Equations (3.28) and (3.29) then become

$$w_1 = \left[Y_0 T_0^{\sigma-1} + Y_1 (T_1^{1-\sigma} - D_1)^{-1} + Y_2 (1-a)^{\sigma-1} (T_2^{1-\sigma} - D_2)^{-1}\right]^{\frac{1}{\sigma}} \tag{3.30}$$

and

$$w_2 = \left[Y_0 T_0^{\sigma-1} + Y_1 (1-a)^{\sigma-1} (T_1^{1-\sigma} - D_1)^{-1} + Y_2 (T_2^{1-\sigma} - D_2)^{-1}\right]^{\frac{1}{\sigma}}, \tag{3.31}$$

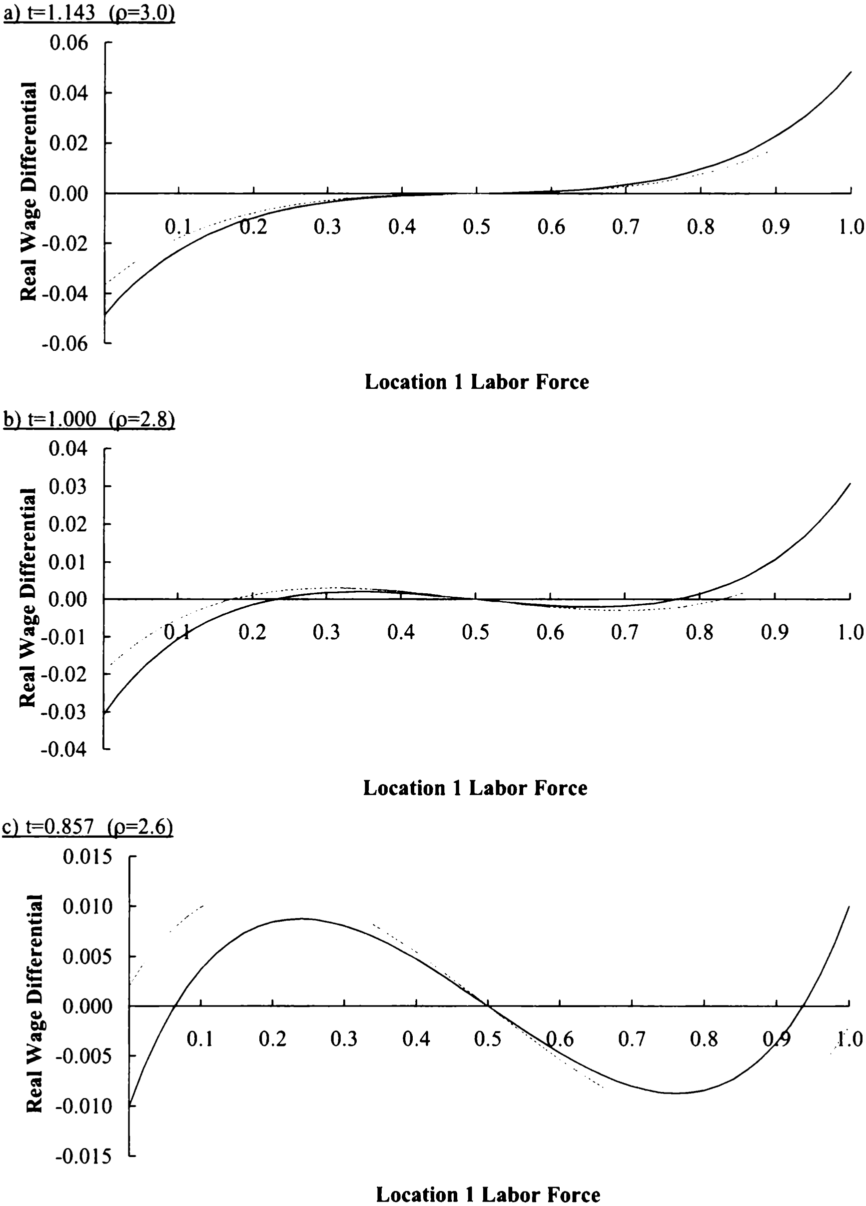
respectively, where

$$D_1 = \frac{t}{1+t} n_0 \left(\frac{1+t}{1-b} w_0\right)^{1-\sigma} \tag{3.32}$$

and

$$D_2 = \frac{t}{1+t} n_0 \left(\frac{1+t}{1-c} w_0\right)^{1-\sigma} \tag{3.33}$$

Figure 3.10 presents the results of this simulation. Again, I use K-LE's unreasonable parameter values to illustrate the mechanics of the model. The solid lines then show the real wage differential with a redistribution of tariffs and, to allow comparability, the dotted lines show the wage differen-



Notes: The following parameters were used in the simulations: $L = 1$; $\sigma = 4$; $\gamma = 0.2$; $Z_0 = 10$; $a = b = c = 0.286$ ($\tau = 1.4$) Solid (dotted) lines show the wage differential with (without) a redistribution of tariff revenues.

Figure 3.10: Allowing for a Redistribution of Tariff Revenues

tial without an explicit redistribution of tariff revenues (taken from figure 3.6). Two features are noteworthy. First, the qualitative results remain basically the same as in the benchmark case. This supports K-LE's approach to simplify the model by assuming a dissipation of revenues. Second, as our intuition has suggested, the incorporation of a redistribution of tariff revenues slightly lowers the critical range of protection. As an economy liberalizes its trade policies, tariff revenues lower the incentives for the creation of new cities and, therefore, a more balanced urban structure.

In sum, however, the additional insights given by an explicit formulation of a tariff revenue redistribution are small. The basic results remain qualitatively unchanged.

3.4 Conclusions

Paul Krugman has recently argued in a series of papers (Krugman and Livas Elizondo 1996, Krugman 1996a) that there is a linkage between trade policy and urban concentration. Setting up a simple theoretical model, he shows that a trade policy that closes off the domestic market can lead to the emergence of a single large city, while a policy of opening an economy to international trade will support a more balanced urban structure in which a metropolis can lose its dominant position. This contribution of the new economic geography is interesting for at least two reasons.

First, it suggests a strong relationship between trade policy and urban economics and, therefore, proposes a new (or at least underemphasized) way to think about differences in urban concentration across countries. In particular, given a long history of import-substituting industrialization policies in developing countries it provides a very interesting and intuitive explanation for the existence of giant urban metropolises in the Third World.

Second, it provides an useful basis for some empirical tests. While there is a huge and old empirical literature trying to explain differences in regional structures across countries, their hypotheses are often based on plain ad hoc reasoning. Moreover, most of these studies incorporate the same standard set of explanatory variables of which openness is only rarely a part.

While there have been recently a number of attempts to examine the empirical side of Krugman's contribution (Ades and Glaeser [1995] and Moomaw and Shatter [1996], for instance, find some mild support), there has been surprisingly little interest in examining his theoretical set-up. This chapter aims to fill this gap. In particular, the aim is to provide a sensitivity analysis for K-LE's simulation results as their analysis is exclusively focused on the mechanics of the model.

Using an extended version of K-LE's model then, several features and variants of the theoretical model are investigated. The results can be summarized as follows.

First, K-LE's claim that in their model trade has an impact on urban concentration is not robust. It is shown that if one starts in a particular region of parameter space, trade does not affect urban concentration.

Second, given their stylized world geography, K-LE's results are not robust for reasonable parameter values. Specifically, the sequence of equilibria K-LE focus on depends on unrealistic high values of transportation costs.

Third, Henderson's (1996) intuitive critique that Krugman's results depend on the assumption that all cities are equidistant from international markets is not supported by the model. Simulations show that even if a location is near the border and comprises in autarky all domestic labor, the opening of trade leads to the emergence of a new city that can be at an interior site, i.e., further away from international markets.

Fourth, extending the model for a redistribution of tariff revenues has no qualitative impact on K-LE's results.

In sum, K-LE's model is a nice theoretical exercise but has only limited value for analyzing real world issues.

Chapter 4

Does Openness Reduce Urban Concentration? Evidence from 120 Years of European Data

4.1 Introduction

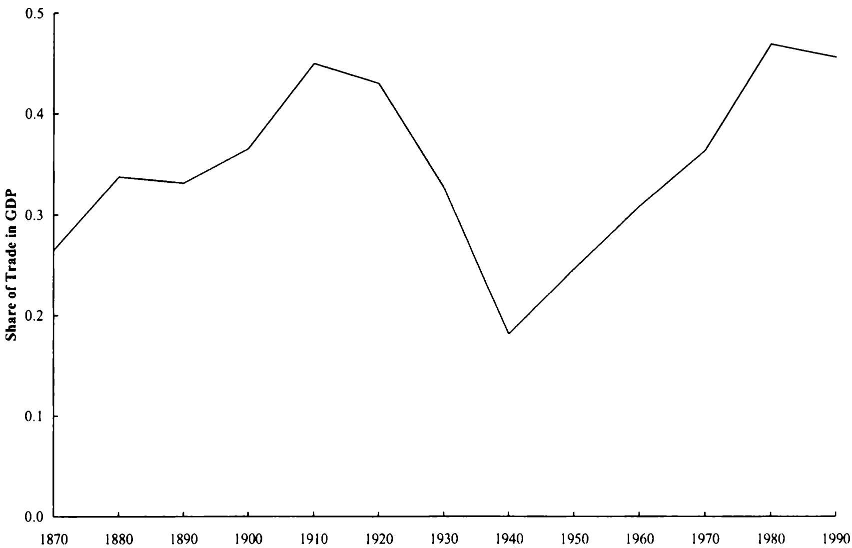
One of the most striking empirical features in urban economics is the large difference in urban concentration across countries. In Austria, for example, about 20 percent of the total population live in the nation's largest city, Vienna, while in neighboring Switzerland, the comparable ratio is considerably smaller, with Zurich containing less than 6 percent of the population. As there is no obvious reason, why extremely different shares of a country's population are concentrated in the main city, a huge and still growing literature seeks to explain these differences in urban primacy.

An interesting recent contribution in the debate is a paper by Paul Krugman and Raul Livas Elizondo (1996). Inspired by the giant size of Mexico City, they develop a simple theoretical model in which protectionist trade policies are a major cause of large central cities. More generally, arguing that "international trade theory and urban economics cannot, ultimately, be regarded as wholly separate disciplines" (p. 150), they suggest that there is a direct linkage between trade policy and urban concentration.

As this insight of Krugman and Livas Elizondo's more or less theoretical exercise is, on the one hand, novel and fascinating and, on the other hand, quite easily testable, it is surprising that, to date, there has been relatively little effort to check this hypothesis empirically. In fact, the only thorough test I know of has been provided by Alberto Ades and Edward Glaeser (1995) who find in a cross-section sample of 85 countries that the share of trade in GDP is indeed negatively related to the size of the largest city, providing mild support for Krugman and Livas Elizondo (1996). Their results, however, are not very robust. The coefficient on openness, for example, loses significance if a Latin America dummy is included in the regressions. Moreover, Ades and Glaeser report some anecdotal evidence in the form of historical case studies where large cities *grew* as a result of trade and commerce, i.e., suggesting exactly the opposite relationship. In sum, the empirical evidence for Krugman and Livas Elizondo's thesis that urban concentration is negatively related to international trade is both rare and far away from being convincing.

This chapter provides a new attempt to examine the relationship between an economy's exposure to foreign trade and urban concentration. In particular, it contributes to the literature along several lines. First, the analysis is explicitly focused on only one region of the world, Europe. As a closer look reveals, this focus offers at least two advantages. On the one hand, there is only a limited loss of information. In fact, European countries offer a wide variety of country and population sizes, population densities, per capita incomes, trade openness', and, as already noted, urban concentrations. On the other hand, if the analysis is focused on only a few countries, often a lot of additional information is available. In particular, the focus on Europe allows to examine (reliable) historical data.

Therefore, the second contribution of this chapter is to analyze the association between trade policy and urban development not only in cross-country fashion but also in time series dimension. As figure 4.1 illustrates there has been considerable variation in the openness ratio of European countries across time. While the aggregated trade-to-GDP ratio has more than doubled in the time period from 1870 to 1913, it collapsed from 1913 to 1945 and then recovered to its pre-World War I level from 1945 to 1990. If Krugman and Livas Elizondo's (1996) theoretical reasoning is correct, one



Notes: The share of trade in GDP is calculated as the weighted average of the trade-to-GDP ratio for individual countries.

Figure 4.1: The Evolution of the Openness Ratio in Europe

should expect that such huge long-term shifts in the openness of European countries have had a considerable impact on the evolution of large central cities in Europe.

Finally, the empirical analysis in this chapter is not confined to only one measure of urban concentration. While Ades and Glaeser (1995) use almost exclusively the absolute size of a country's main city as their dependent variable, this chapter examines the impact of trade policy on several measures of urban centralization in a country.

This chapter, then, is in five parts. Section 2 describes the basic determinants of urban primacy. Section 3 gives a brief description of the data. Section 4 presents the results, and section 5 concludes.

4.2 Potential Causes for Urban Concentration

Generally, the question about discrepancies in urban primacy around the world, i.e., why the size of the largest city relative to total population differs across nations, is only a variant of the more fundamental question why economic activity is not evenly distributed across space. As this problem is at the heart of location theory, one of the oldest fields in economics, a huge literature has evolved over the last century which aims to investigate the factors behind the wide variation in the size of urban metropolises.[1]

Not surprisingly, several explanations have been proposed in the literature. As it seems hard, however, to believe that large central cities are the result of only a single exogenous factor (e.g., a protectionist trade regime), this section explores a number of reasons which can plausibly contribute to an empirical explanation of the observed differences in urban concentration. In particular, the analysis focuses – besides trade policy – on three forces: (i) economic development; (ii) concentration of political power; and (iii) transportation infrastructure.

4.2.1 Economic Development

To urban economists it is very well known that there are close interactions between urbanization and the economy.[2] Examining the sign of the relationship between economic development and urban concentration, how-

[1] The earliest study which Glenn Carroll (1982) cites in his interesting review of empirical studies about city size distributions is a German paper published in 1913.

[2] Krugman and Livas Elizondo (1996) have recently emphasized that the strong link between urban concentration and economic development has been analyzed almost exclusively by urban economists, while development economists have largely ignored this subject. Specifically, they note that "[i]n the development litera-

ever, there are equally convincing arguments for both sides. On the one hand, it is quite obvious that if a large fraction of a country's population is working in sectors which depend on immobile natural resources such as agriculture and mining, the scope for urbanization will be limited. In the extreme case of an economy solely based on agriculture (e.g., in a society of hunters and gatherers), there will be no population concentration at all. This reasoning implies, however, that industrialization, i.e., a shift in economic structure away from the primary sector of an economy (often associated with economic development[3]), will raise the level of urbanization and, accordingly, increase the extent to which an economy can centralize in a single location.

Moreover, agglomeration and urban concentration itself provide economic benefits (agglomeration economies). By concentrating in the same place, firms can benefit from a pooled labor market for specialized workers, a larger variety of inputs in production, and informational spillovers, as has been already noted in the late nineteenth century by Alfred Marshall (1890 [1920]). But if cities have attributes which positively affect the productivity and the growth of the economy, then urban centralization may also be positively correlated with economic development.[4]

On the other hand, there is also an intuitive line of reasoning suggesting exactly the opposite relationship. As an economy industrializes, this tends to increase the size of the domestic market. When local demand thresholds are passed, a growing number of firms will find it attractive to locate away from the center to serve regional markets in order to save transport costs. High income, then, should allow a country to support a network of intermediate-sized cities. In fact, Rosen and Resnick (1980) find that wealthier countries have more evenly distributed populations.

Given this ambiguity about the sign of the linkage, a number of economists have even argued that there is a nonmonotonic inverted U-shape relationship between economic development and urban concentration, i.e., that economic development initially increases and then decreases urban primacy.[5] The empirical evidence on this point, however, has been mixed.

ture [...] urbanization in general and the growth of giant cities in particular are addressed obliquely, if at all" (p. 138).

[3] Ades and Glaeser (1995), for example, report a sample correlation of 0.849 between the share of the labor force outside of agriculture and GDP per capita.

[4] An alternative hypothesis would be that economic development is correlated with urbanization and population density, but not with urban concentration.

[5] Therefore, Krugman's (1996a, p. 12) claim that it is a stylized fact that "per capita income is negatively related to measures of urban concentration" is – to say the least – a bit surprising. In fact, recent studies (e.g., Ades and Glaeser [1995] and Moomaw and Shatter [1996]) have not found significant coefficients on GDP per capita.

While time series analyses often provide support for the inverted U-curve hypothesis, cross section evidence has been less conclusive.[6]

4.2.2 Political Power

It is one of the contributions of recent empirical work in the new economic geography literature to have reemphasized the role of government and politics in determining city size. Bradford De Long and Andrei Shleifer (1993), for example, present evidence showing that absolutist governments have inhibited city growth in preindustrial Europe. Discussing the failure of Zipf's law in many countries due to unproportionately large central cities, Krugman (1996b, p. 41) notes that "[t]hese primate cities are typically political capitals; it is easy to imagine that they are essentially different creatures from the rest of the urban sample." Most explicitly, however, Ades and Glaeser (1995, p. 195) argue that "political forces, *even more than economic factors*, drive urban centralization." (emphasis added)

The basic idea behind an association between politics and urban primacy is that spatial proximity to political power usually increases political influence. In some cases, then, there can be strong incentives to be near the government. Dictatorships, for example, often ignore the needs of the politically weak hinterland so that it may be highly attractive for rural workers to move to the capital. Also weak governments can be expected to raise the size of the central city: By transferring resources to the capital, they try to enhance the probability of their re-election and effectively attract rent-seeking migrants. In sum, undemocratic institutions, political instability and dictatorships should favor urban concentration.

4.2.3 Transportation Infrastructure

The bulk of urban and regional economics explains the demand for cities by the desire to minimize transport costs. When transportation is costly, firms can eliminate distance by locating close to each other. Thus, the incentive for industries to cluster together will, at least in part, depend on internal transport costs. The less efficient the national transport network and, thus, the more expensive the transportation of inputs and final products, the more firms will concentrate in one location. Alternatively, a fall in the costs of moving goods should lead to a more equal distribution of economic activity.[7]

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[6] Junius (1999) provides an useful survey of the empirical literature on the U-curve relationship between urban primacy and economic development.

However, a better infrastructure does not always decrease urban concentration. In fact, there is an equally intuitive argument which suggests that an improvement of the physical infrastructure of a country may also *promote* urban centralization. Only the ability to transport goods over longer distances made agglomerations possible in the first place.[8] More generally, since lower transportation costs allow a city to service a larger hinterland, an efficient transport network may generate larger central cities. Krugman (1991d) formalizes this effect. In his two-regions-model, a fall in transportation costs works in favor of regional divergence (through the possible realization of stronger backward and forward linkages) where, for transport costs below a critical level, complete concentration in one region is an equilibrium. Reviewing the empirical evidence, Krugman (1996a, p. 13) refers to results from Rosen and Resnick (1980) and notes that "[c]ountries in which the capital city has a uniquely central position ... tend, not too surprisingly, to have more populous capitals".

Taken together, these points imply that the association between internal transport costs and concentration is ambiguous. Thus, by controling for a country's transportation infrastructure, I implicitly test for both alternative hypotheses.

4.3 Data

4.3.1 Data Sources

My sample comprises data from 13 European countries[9], covering the time period from 1870 to 1990 in 10-year-intervals. The countries were selected to provide the most complete set of historical data. Nonetheless, I lose several observations mostly in dealing with the countries' population structure.

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[7] This point has been most strongly made by Edward Glaeser. In a recent paper, Glaeser (1998, p. 144-5) notes that "[t]he geographical concentration of manufacturing industries [in the U.S.] has fallen significantly over the past 15 years ... perhaps reflecting the decreased importance of fixed costs and transport costs." He concludes that "[i]f cities' only advantage was eliminating transport costs for manufactured goods, then cities would indeed cease to exist." In addition, Ades and Glaeser (1995) provide empirical evidence indicating that countries with low internal transport costs tend to have less populous capitals.

[8] Discussing the birth of urbanism, economic historian Paul Bairoch (1988, p. 11) points out that "the existence of true urban centers presupposes not only a surplus of agricultural produce, but also the possibility of using this surplus in trade. And the possibilities of trade are directly conditioned by the size of the surplus relative to the amount of ground that has to be covered in transporting it from one place to another, for distance reduces the economic value of the surplus."

[9] The countries are Austria, Belgium, Denmark, Finland, France, (West) Germany, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, and Switzerland.

DOI https://doi.org/10.3790/978-3-428-50499-2

In compiling the data, there were basically two sets of problems. A first issue refers to inconsistencies in the construction of the city population figures which were taken from national statistical yearbooks. In particular, the definitions of metropolitan areas are likely to differ across countries. Sometimes the numbers comprise the whole agglomeration while in some cases they refer only to the central city. Accordingly, the city population data are not directly comparable between countries. As there is no indication, however, that the data in particular countries are consistently biased by explicitly referring to wider agglomerations instead of cities, the impact on the empirical results should be limited.

A related problem is that as agglomerations often grow over time and merge with surrounding cities, the geographical areas referred to by national statistical agencies may differ across time. With the formation of Greater Berlin in 1920, for instance, reporting census data for former cities such as Charlottenburg was ceased, and – according to national statistics – Berlin's population more than doubled within five years, rising from about 1.7 million in 1915 to 3.8 million in 1920. On average, however, this effect is likely to cancel out in the time series dimension. With growing agglomerations, the true impact of the explanatory variables on city size will be first underestimated and then overestimated.

A second difficulty is caused by the numerous changes which have taken place in national boundaries. In principle, it is possible to adjust for those frequent border redrawings (and, thus, abrupt changes in country characteristics such as land area and total population) with no *immediate* impact on the population of a country's largest city by including a persistence variable. In this chapter, however, I basically deal with this problem by taking most of the historical data series (e.g., level of GDP, GDP per capita) from Maddison (1995) who already corrects for territorial change. In particular, Maddison's data refer exclusively to the present territory of the countries.

The flipside of having a consistent basis in terms of geographical area is that at least in some cases urban primacy is not related to actual, but corrected country data. This problem, however, should be of minor importance, largely for two reasons. First, with the exceptions of Austria and Germany, territorial changes in most countries in the sample affected only a small proportion of the total area so that the impact on the overall country data should be negligible. In Spain and Portugal, for example, there were no border changes at all.

Second, whenever feasible, explanatory variables are entered into the regressions not in levels but in ratios or shares. The idea is that territorial changes should have had only a rather limited impact on variables such as a country's overall GDP per capita or on the share of labor force outside of agriculture.

In sum, only in the cases of Austria and Germany I face serious problems in ignoring border changes and using level data based on present-day country size rather than correct territory. The Republic of Austria, for example, was established in 1919 comprising only about one-sixth of the former Austro-Hungarian Empire. Accordingly, explaining Vienna's population before 1919 by focusing on data comparable with Austria's present size would clearly yield distorted results. Also Germany was affected by significant territorial changes. Between 1918 and 1923, for instance, Germany lost about 12 percent of its population (Alsace-Lorraine, Memel, Danzig, Eupen and Malmedy, Saarland, North Schleswig and Eastern Upper Silesia). More importantly, Maddison's (1995) estimates refer to the geographical area of former West Germany and, thus, comprise in some cases less than one-half of the official German territory at that time.[10]

Therefore, to minimize the errors introduced by the basic strategy of using data referring to present boundaries, I depart from the time-consistency approach in compiling the data for the two countries in the sample with the most dramatic territorial changes, Austria and Germany. Specifically, I allow for a marked reduction in country size and, thus, a break in level data (such as area, GDP, and population) in 1920 and 1950, respectively. As Maddison's (1995) adjusted data are then only applicable for the time period after that date, pre-break level data are constructed from Mitchell (1992) who reports actual population figures. Given this population data, GDP numbers comparable to Maddison's estimates are calculated by multiplying population with Maddison's GDP per capita. Thus, I implicitly assume that the frontier change has not affected the country's GDP per capita.[11]

A second modification is chosen for German data. Due to the special geographical and political position of Berlin after World War II, I refrain from using Berlin as Germany's largest city after 1945 and focus instead on the largest city in the contiguous territory of former West Germany, Hamburg.[12]

Having explained the basic procedures to derive the data, appendix D provides a detailed description of the data sources.

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[10] Maddison (1995, p. 131) himself notes that, on the basis of the geographic distribution in 1936, GDP generated within the boundaries initially fixed for the Federal Republic was only 56.9% (excluding Saarland and West Berlin) or 64.3% (if the Saar and West Berlin are incorporated) of that within the 1936 boundaries.

[11] This assumption is justified by Maddison (1995, table H-2) who confronts actual and adjusted estimates of GDP per capita for Germany and reports only minor differences. In 1870, for example, actual German GDP per capita (1821 $) is about 95% of Maddison's estimate for GDP per capita adjusted to 1990 frontiers (1913 $).

[12] None of the results reported are qualitatively affected by this choice.

4.3.2 Alternative Measures of Urban Primacy

The vast empirical literature on the determinants of urban primacy has not only identified a large number of potential explanatory variables showing a statistically robust linkage with urban concentration, but also has occasionally reported ambiguous or even contradictory results on a variety of single explanatory variables. Discussing the relationship between trade openness and urban primacy, Krugman (1996a, p. 13), for example, notes that "[b]efore the Rosen and Resnick (1980) study [finding a negative relationship between openness and concentration] most writing on primacy assumed that export orientation would tend to *increase* primacy. ... One can hardly deny that this effect has existed in some times and places; the evidence that the effect runs the opposite way is not overwhelming. This kind of ambiguity arises in any attempt to summarize the richness of cross-national variation with a short list of explanatory variables."

While the potential candidates for explaining those differences in empirical findings range from the analysis of different time periods to the use of different samples and the application of different estimation methods, it also appears that at least some of the discrepancies in the empirical results can be traced back to differences in defining measures of urban concentration. In fact, as there is no generally agreed upon measure of urban primacy, most empirical papers, while often shortly discussing different ways to define urban centralization, concentrate the analysis on a single measure of urban primacy which is obviously preferred by the respective author(s). Ades and Glaeser (1995), for example, focus exclusively on the absolute population of the largest city. Moomaw and Shatter (1996) define primacy as the largest city's share of a country's urbanized population. Other authors construct even more complex variables largely based on sophisticated distribution measures such as Herfindahl indices or coefficients on the Pareto distribution.

Given this variety of primacy measures, it is one of the contributions of this chapter to allow for different indices of urban concentration as dependent variables. Specifically, four measures of urban primacy are used to examine the relationship between trade openness and urban concentration, each having its own advantages and shortcomings and throwing light on slightly different aspects of population concentration.

The first measure, then, is the absolute number of inhabitants in a country's largest city as recently used by Ades and Glaeser (1995). Strictly speaking, this is not a direct measure of concentration since the size of a country's largest agglomeration should be strongly affected by the country's total population, with more populous countries also having larger cities. Not surprisingly, then, Ades and Glaeser (1995, table 3) report that

DOI https://doi.org/10.3790/978-3-428-50499-2

in their sample the country's main city size is most highly correlated with the country's total population (with a correlation coefficient of 0.537). However, after correcting for this relationship by including a country's total population into the regression as an explanatory variable, the population in a country's main urban agglomeration is a feasible measure of population concentration.

In a slight modification of this regression specification, the share of the largest city in total urban population is used as dependent variable.[13] This ratio has been widely used in the literature (see, for example, Moomaw and Shatter [1996] for a recent application). The main difficulty with this index (and also with the first measure) is, however, that it largely ignores the size distribution of cities below the largest agglomeration. To compensate at least partly for this deficiency, variants of the primacy index have been formulated covering a larger number of cities. Junius (1999), for example, has also calculated the share of the two, three, and four largest cities in total population, respectively. His results, however, were basically identical for those modified indices as dependent variables. In my sample, the correlation between the ratio of the largest city to a country's total population and the ratio of the two largest cities to total population is 0.979.

A third primacy measure relates the largest city to the population in the second largest metropolitan area. Focusing exclusively on the upper bound of the size distribution of cities, this ratio illustrates more directly the dominance of a country's largest agglomeration. Again, however, the measure provides no information about the size distribution of cities below the top two largest. Variants of this index, therefore, include a larger number of cities in the denominator. Rosen and Resnick (1980), for example, calculate ratios of the largest city to the sum of the top five and the top 50 cities,

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[13] Note that both formulations yield mathematically different regression equations. In the first specification, we have

$$\ln(CITY) = \alpha + \beta_l \ln(POP) + \ldots$$

which is mathematically equivalent to

$$\ln(CITY) - \beta_l \ln(POP) = \alpha + \ldots$$

or

$$\ln\left(\frac{CITY}{POP^{\beta_l}}\right) = \alpha + \ldots,$$

while the second measure yields the specification

$$\ln\left(\frac{CITY}{POP}\right) = \alpha + \ldots .$$

DOI https://doi.org/10.3790/978-3-428-50499-2

respectively. However, their modifications provide largely no new insights as there is a strong correlation of 0.922 between the two measures. Also due to lack of sufficient data, then, the empirical analysis in this chapter applies the most simple formulation of this index, focusing on the ratio between the two largest cities of a country.

Finally[14], following Wheaton and Shishido (1981) and others, an H index of concentration is computed which is defined as

$$H = \sum_{i=1}^{n} \left(\frac{CITY_i}{POP} \right)^2 , \tag{4.1}$$

where $CITY_i$ is the population of city i, POP is the country's total urban population, and n is the number of cities included in the calculation of the index. In contrast to the previous measures, the basic advantage of this index is that it considers the size distribution of *all* cities within a system. As is familiar from the application of comparable distribution measures in other contexts (such as market segmentation), the H index can vary between 0 and 1, with large values of H indicating a higher population concentration. In the extreme case of $H = 1$, the total population of a country is concentrated in a single city. A number of studies also emphasize the interesting economic insight of the reciprocal of H, measuring the number of equal-sized cities which would generate the urban population.

The main difficulty in calculating the H index is the definition of the cut-off point, i.e., how many cities should be included in the computation of H. Here it has become common procedure to include the largest agglomerations of a country that account for a given fraction of the total urban population. An alternative definition of including all cities above a fixed population size, say larger than 100,000, would distort the results for small countries. In this study, the cut-off is (limited by data availability) set at 60% of the total urban population in a country[15] and, thus,

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[14] The list of primacy measures discussed above and used in the following analysis is far from being exclusive. For example, an also frequently used measure, which offers the advantage of considering the entire spectrum of city sizes, is the Pareto exponent. Starting from the assumption that the city-size distribution can be expressed in mathematical form as

$$R = AS^{-\nu}$$

where R is the number of cities with population S or more, A is a constant and S is the population of the city, the exponent ν shows deviations from the "rank-size rule", with values of $\nu > 1$ indicating a more evenly distributed population.

[15] In a number of cases, even this bound is not reached. As Morris Adelman (1969) illustrates, however, the impact of this loss of information on the value of

Table 4.1

Simple Correlations

	MAINCITY	MAINSHARE	FIRSEC	HCONC
Absolute Population of Main City (MAINCITY)	1.00			
Share of Main City in Total Urban Population (MAINSHARE)	–0.20	1.00		
Ratio of Main City to Second Largest City (FIRSEC)	0.22	0.41	1.00	
H Concentration Measure (HCONC)	–0.15	0.94	0.34	1.00

somewhat lower than, for example, Wheaton and Shishido's (1981) cut-off of 70%.

Having explained the details of constructing different measures of population concentration, table 4.1 gives simple correlation coefficients. Interestingly, the interaction is weak in most cases, supporting the assumption that different measures also reflect different aspects of urban concentration. The only correlation which is significantly high is between the share of the largest city in total urban population and the *H* concentration measure. Given the large similarity in constructing both measures, this finding is not too surprising, but also suggests that the additional insight gained by incorporating the city structure below the largest city is rather limited in this sample.

Figure 4.2 visualizes these observations showing weighted averages of the variables of interest across time. Given the familiar pattern in trade

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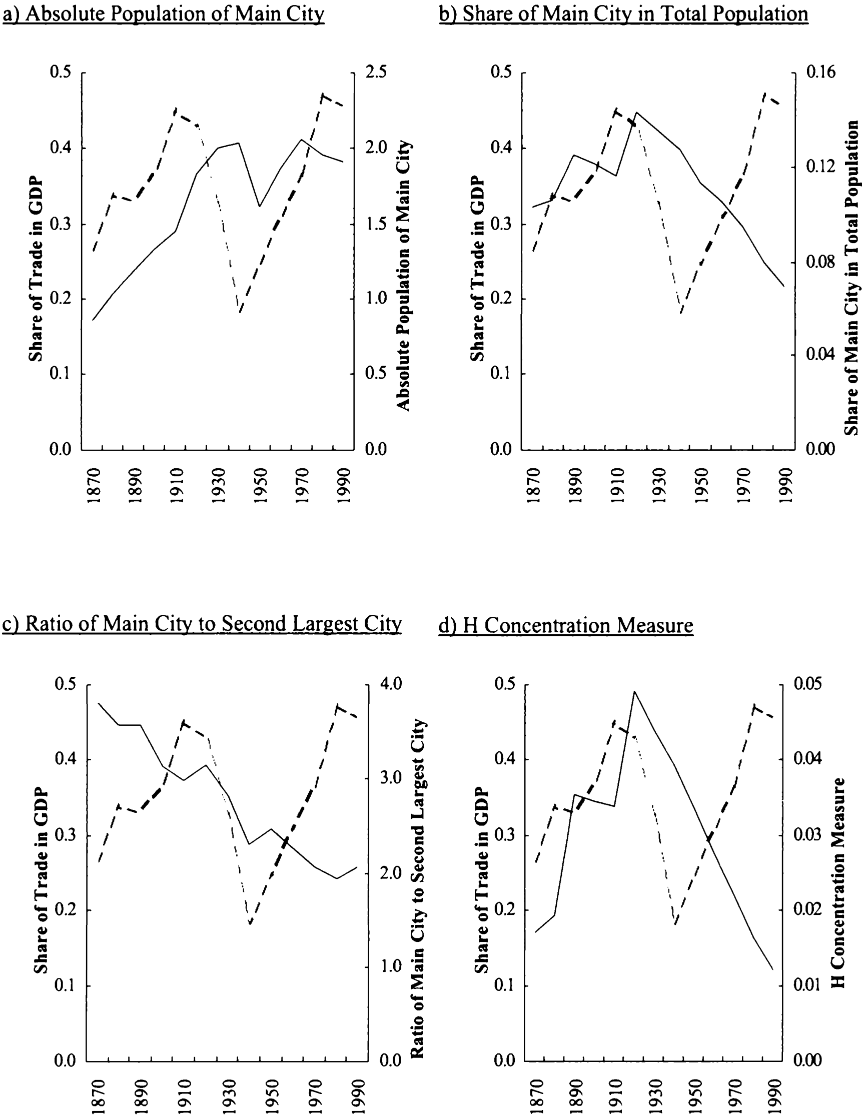
the *H* measure can often be neglected. Consider, for example, the city structure in France in 1980. My data set comprises all 104 cities with a population of more than 50,000, summing up to a total population of 12.05 million. Given a total urban population of about 42 million, then, the available city data covers only 28.7% of urban population in France, considerably less than the conceived cut-off of 60%. The calculated *H* of the 104 largest cities is 0.0040. At a maximum, however, there could be 263 ($= [0.6 * 42 - 12.05]/0.05$) more cities, each with a weight of 0.12% in urban population ($= 0.05/42$). Their contribution to *H* would be 0.0004 ($= 263 * [0.05/42]^2$) so that the maximum *H* for France in 1980 would be 0.0044. But even this upper bound result, although more precise, would be only marginally different from the initially calculated *H* value of 0.0040.

DOI https://doi.org/10.3790/978-3-428-50499-2



Notes: The solid line represents the concentration measure (right scale) and the dotted line is the openness ratio.

Figure 4.2: The Evolution of the Openness Ratio and Measures of Urban Concentration

openness, no clear correlation with the various measures of urban concentration is evident. For example, the absolute size of the largest city has increased in almost every decade since 1870, apparently reflecting overall population growth, while there has been a strong downwards trend in the ratio between a country's largest and second largest city. A third pattern is provided by central city size corrected for a country's population. Both related measures, the share of the largest city in total population and the *H* concentration measure, have risen in the first half of the time period under investigation and have fallen since then to values even below the 1870 level.

4.4 Results

4.4.1 Replicating Ades and Glaeser (1995)

The first set of estimates attempts to replicate the results in Ades and Glaeser (1995) who find in a cross sample of 85 countries a statistically significant negative relationship between the share of trade in GDP and the size of the largest city. Specifically, the aim is to analyze whether their finding is also valid for a European (sub)sample of 13 countries, which were – with the exception of Sweden – also all included in Ades and Glaeser's original analysis. Moreover, focusing on almost the same time period, averaging data for 1970, 1980 and 1990[16], has the advantage of using exclusively high-quality data. Thus, possible problems in constructing the historical data set can be ignored. The basic shortcoming of this cross-country exercise, however, is the small sample size of only 13 observations which clearly limits the degrees of freedom. Robustness checks, therefore, apply alternative estimation methods which allow to explore jointly the information from separate years (instead of averaging data for the whole time period).

Table 4.2 presents the results. Column (1) repeats Ades and Glaeser's (1995) benchmark regression which relates the log of average population in the main city to a standard set of controls: a capital city dummy, the log of non-urbanized population, the log of urbanized population outside the main city, the log of land area, the log of real per capita GDP, and the share of the labor force outside of agriculture. None of these explanatory variables has a coefficient that is statistically significant at conventional levels of confidence. Given that also in Ades and Glaeser's study with 85 observations not all estimated coefficients are significant[17], this is not too disap-

[16] Ades and Glaeser (1995) use averages of 1970, 1975, 1980 and 1985 observations.

Table 4.2

Replicating Ades and Glaeser (1995)

Dependent Variable: Log of Population in Main City	(1) Avg.1970–90	(2) Avg.1970–90	(3) Avg.1970–90	(4) Avg.1970–90	(5) 1970	(6) 1980	(7) 1990
Constant	–0.026	22.527**	22.300**	14.430*	3.685	12.398*	22.484*
	(8.345)	(4.940)	(4.102)	(4.833)	(5.408)	(5.020)	(9.230)
Capital City Dummy	0.703	0.448**	0.450**	0.445**	0.742*	0.474*	0.297
	(0.368)	(0.111)	(0.106)	(0.123)	(0.224)	(0.146)	(0.279)
Log of Non–Urbanized Population	0.460	0.510**	0.489**	0.509**	0.536**	0.488**	0.530**
	(0.302)	(0.122)	(0.047)	(0.076)	(0.134)	(0.086)	(0.085)
Log of Urbanized Population Outside the Main City	0.187	–0.022					
	(0.220)	(0.119)					
Log of Land Area	0.111	–0.259*	–0.257*				
	(0.098)	(0.091)	(0.082)				
Log of Real GDP per Capita	0.276	–2.940**	–2.872**	–1.855*	–0.188	–1.763*	–3.334*
	(0.781)	(0.726)	(0.498)	(0.579)	(0.478)	(0.591)	(1.257)
Share of the Labor Force Outside of Agriculture	0.154	16.384*	15.833**	11.229**	4.810*	12.907**	17.559**
	(4.224)	(4.363)	(2.164)	(2.187)	(1.657)	(2.865)	(4.802)
Share of Trade in GDP	–3.743**	–3.677**	–2.256**	–2.272*	–2.451**	–1.594**	
	(0.765)	(0.540)	(0.355)	(0.768)	(0.407)	(0.452)	
# of Observations	13	13	13	13	13	13	13
Adjusted R^2	0.717	0.957	0.964	0.909	0.873	0.932	0.831

Notes: White heteroskedastic–consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

pointing. Interestingly, however, even the scaling variables such as population or land area which control for the size of a country have no predictive power for the population size of the country's largest city. Taking into account that the adjusted R^2 is nevertheless quite respectable at 0.72, this result suggests that the regression specification apparently misses an important explanatory variable. It is also possible that the result suffers from the small sample size and the limited degrees of freedom.

As I am particularly interested in the relationship between openness and urban concentration, regression (2) includes the average share of trade in GDP as an additional independent variable. Three points are noteworthy. First, the estimated coefficient on openness is negative and statistically highly significant, indicating that a rise in the share of trade in GDP by 1 percent is associated with a reduction in the size of the largest city by about 3.7 percent. Holding population constant, this result provides some first support for Krugman and Livas Elizondo's (1996) hypothesis of a negative relationship between trade openness and urban concentration. Moreover, the estimated effect is considerably larger than Ades and Glaeser's (1995) finding of –0.6 for a sample of 85 countries.

Second, with openness included, the overall estimation results improve considerably. The fit of the regression is excellent, with an adjusted R^2 of 0.96. This suggests that openness captures a large share of the difference in the size of the largest cities. To analyze the relationship between an economy's exposure to international trade and the size of its largest city in more detail, figure 4.3 provides a simple scatter plot of the ratio of trade to GDP and the log of the population in the main city. The figure illustrates a strong negative correlation (of –0.751) between the two variables. Highly open economies such as Belgium and Switzerland have metropolises which are small in absolute size, while relatively closed economies (e.g., Spain, France, and Italy) have the largest cities in the sample. As there is a well-known association between country size and openness, with larger countries being relatively less open to trade[18], this finding itself is not surprising. The surprising fact is rather that this expected pattern is apparently mostly captured by the openness measure in the sample, while the coefficients on both land area and urbanized population are statistically less significant.

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[17] Only the log of non-urbanized population, the log of land area and the share of labor force outside of agriculture have statistically highly significant coefficients.

[18] Jeffrey Frankel (1997, p. 57), for example, notes that "[a] Singapore or a Luxembourg is highly dependent on trade, in part because it lacks many natural endowments and because it lacks room to exploit economies of scale in the domestic market. ... There is an additional reason for this pattern. Interstate trade in the United States is considered domestic; interstate trade within the European Union is considered international trade."

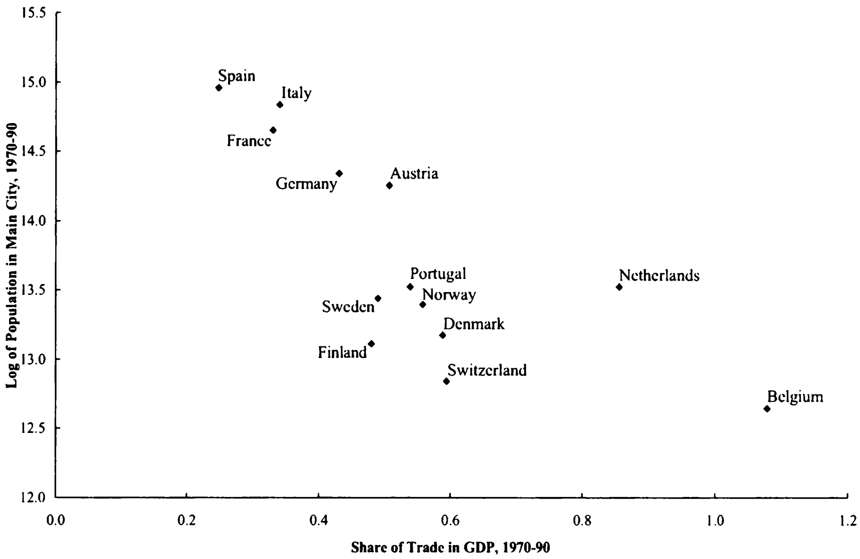

Figure 4.3: Share of Trade in GDP and Population in Main City, 1970–90

Finally, also the coefficients on most of the other explanatory variables become statistically significant in regression (2). The estimated coefficient on the capital city dummy is now statistically highly significant (at the 1% level), indicating that main cities are on average 45 percent larger if they are also capital cities[19] – a magnitude which is comparable to Ades and Glaeser's result of an elasticity of 0.42. Thus, even in a sample which includes almost exclusively countries with an established democratic political system (with the exceptions of Portugal and Spain in 1970), I find support for Ades and Glaeser's claim that political power attracts population. Interestingly, I also replicate their somewhat surprising empirical result that of the two population controls only the log of non-urbanized population is statistically significant. The positive coefficient of about 0.5 indicates that more populous countries have larger central cities, with urban areas growing about half as much as their countries' (non-urban) populations. In contrast to this finding, the coefficient on the log of land area (which is statistically significant at the 5% level) takes the wrong sign, suggesting that

[19] In my sample, three out of 13 cities are not the capital of their respective country (Antwerp/Belgium, Hamburg/Germany, Zurich/Switzerland). This is a considerably larger share than in Ades and Glaeser's study who report that 77 of the 85 large cities in their sample are also capitals.

geographically larger countries tend to have *smaller* cities. As the partial correlation between country size and main city population, however, is positive (with a coefficient of 0.614), the estimated coefficient on land area possibly reflects a spurious correlation after controlling for the effects that more populous and more closed economies have larger metropolises. As expected, the share of the labor force outside of agriculture is positively associated with the size of the main city. A 1 percent increase in the fraction of the population that is not working in the agricultural sector tends to raise the size of the largest city by about 16 percent. At first sight, this elasticity appears to be implausibly large. However, given that the sample mainly consists of highly industrialized countries with only small deviations from an average non-agricultural labor share of 0.95, this result is not unrealistic. Contrary to expectations, the estimated coefficient on per capita income is actually negative, indicating that a 1 percent rise in GDP per capita decreases the size of the main city by about 2.9 percent. It is worth remembering, however, that the share of labor outside of agriculture already controls for a country's state of industrial development, suggesting that this variable captures even more-than-proportionally the expected positive effect of economic development on city size. This interpretation is consistent with Ades and Glaeser's (1995) findings who report that in their sample the coefficient on income (although positive) loses size and significance whenever they also control for the non-agricultural labor share. Moreover, the results might be distorted by an outlier. The country with the highest per capita income in the sample, Switzerland, has on average the second smallest main city.

As the most obvious problem in this type of regression (using period averages) is the small sample size, I provide a number of robustness checks. In a first attempt, I reduce the number of explanatory variables to increase the degrees of freedom. In particular, I drop the log of urbanized population (regression 3)[20] which has been found to have no predictive power for the population size in the main city and the log of land area (regression 4) from the list of explanatory variables. As expected, departing from Ades and Glaeser's standard set-up does not affect the basic results. In fact, the only slight reduction in the adjusted R^2 from 0.96 to 0.91 suggests that not much information is lost. The most notable change, then, is the fall in magnitude in the estimated coefficient on openness by more than one-third. This apparent sensitivity of the estimated elasticity to the exclusion of land area supports the intuition that the implausible negative coeffi-

[20] I also tried running regressions with the country's total population outside the main city as an explanatory variable instead of entering a country's non-urbanized and urbanized population separately. Merging both initial measures of a country's population size into a single variable, however, did not improve the results.

DOI https://doi.org/10.3790/978-3-428-50499-2

cient on land area[21] is indeed the result of a misspecification so that I use regression (4) as my preferred specification.

Columns (5) to (7) present separate regressions for the years 1970, 1980 and 1990 to make sure that the previous findings for period averages are not driven by a specific year. As shown, the basic regression results hold for all individual years. Most notably, the estimated coefficient on the share of trade in GDP is always statistically significant, but varies between –1.6 and –2.5 across different years.

The results in table 4.3 deal even more explicitly with the issue of small sample size. In particular, I apply different pooling techniques to take fuller account of the available information for the time period from 1970 to 1990. In a first step, regressions (1) and (2) combine observations from the different years, using the method of a seemingly unrelated regression (SUR). Specifically, I estimate a system of three year-specific equations with separate intercepts for each year, but impose a constancy restriction on the remaining parameters. Having increased the number of observations to 39, the basic results are qualitatively unchanged from the comparable OLS regressions with averaged data. The estimated coefficient on the variable of interest, openness, turns out to be negative and statistically highly significant at the 1% level. The magnitude of the estimated elasticity, however, is lower at about –1.4.

Columns (3) and (4) report regressions with pooled data for 1970, 1980 and 1990 in which dummy variables for 1970 and 1980 (not shown) capture time effects. Although the overall results appear to be somewhat weaker, this set-up basically confirms previous findings. As before, the share of trade in GDP enters negatively and statistically highly significant. It is also interesting to note that in both specifications which use a larger number of observations the coefficient on the log of land area is far from significant, supporting my initial decision to ignore this variable in the preferred specification of regressions examining period averages and yearly data. Moreover, the regressions strongly suggest that the previously implausible result of a negative coefficient on GDP per capita is not robust. Contrary to the estimates for averaged data, per capita income is neither in SURs nor in OLS regressions with pooled data statistically different from zero.

In a further exercise in sensitivity analysis, I drop observations for Austria and Germany to make sure that the results are not affected by distorted data due to border changes.[22] Columns (5) to (7) of table 4.3 display the

[21] Junius (1999) also finds a negative relationship between country size and urban concentration and explains this result by arguing that a large land area apparently increases the probability of forming several metropolises.

Table 4.3

Alternative Estimation Techniques and Sample Sizes

Dependent Variable: Log of Population in Main City	Full Sample				Without Austria and Germany		
	(1) SUR 1970–90	(2) SUR 1970–90	(3) Pooled 1970–90	(4) Pooled 1970–90	(5) Avg. 1970–90	(6) SUR 1970–90	(7) Pooled 1970–90
Capital City Dummy	0.644** (0.157)	0.604** (0.165)	0.581** (0.145)	0.573** (0.145)	0.345 (0.234)	0.766** (0.175)	0.691** (0.204)
Log of Non–Urbanized Population	0.401** (0.094)	0.555** (0.074)	0.414** (0.089)	0.565** (0.051)	0.503** (0.079)	0.520** (0.063)	0.555** (0.046)
Log of Urbanized Population Outside the Main City	0.171 (0.072)				0.149# (0.082)		
Log of Land Area	–0.029 (0.076)	–0.060 (0.082)					
Log of Real GDP per Capita	–0.068 (0.268)	0.039 (0.319)	–0.471 (0.580)	–0.460 (0.525)	–2.024# (0.841)	0.054 (0.264)	–0.290 (0.639)
Share of the Labor Force Outside of Agriculture	1.857* (0.859)	2.663* (1.093)	3.557 (2.996)	5.217* (2.100)	11.656* (3.074)	2.434* (0.919)	4.135 (2.567)
Share of Trade in GDP	–1.297** (0.341)	–1.399** (0.306)	–1.566* (0.737)	–1.540** (0.406)	–2.353* (0.633)	–1.263** (0.273)	–1.280* (0.515)
# of Observations	13 × 3	13 × 3	39	39	11	11 × 3	33
Adjusted R^2	0.769,0.828,0.500	0.822,0.858,0.565	0.847	0.838	0.941	0.854,0.891,0.516	0.860

Notes: White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively. The seemingly unrelated regression (SUR) includes year-specific intercepts that are not reported here. All other regressions also include a constant which is not reported. The pooled OLS regression additionally include dummy variables for 1970 and 1980.

results for different estimation techniques. As shown, the estimated coefficients are virtually unchanged from the regression specifications using the larger sample of countries so that I can rule out that the results are driven by distorted or corrected data.

To check the robustness of the finding of a negative association between openness and the size of the largest city, I experiment with various extensions of my benchmark specification. In particular, I follow Ades and Glaeser (1995) in entering a number of additional control variables. Table 4.4 shows the results. Note that these regressions include the complete set of independent variables considered in regression (4) of table 4.2, but the table only reports the estimated coefficients on the two variables of particular interest to avoid clutter. Regressions (1) and (2) then examine the trade-city size connection in more detail by including additional measures on the openness of a country. In a first step (column 1), I add the average ratio of import duties to total imports. This variable is intended to capture the impact of protectionist trade policies on the size of the largest city. Specifically, given a negative relationship between openness and main city size, I would also expect that a larger share of tariff revenues is associated with a more populous central city. The estimated coefficient on the import duties/imports ratio is indeed positive and significant so that there is some support for the intuition. Two points, however, are noteworthy. First, with a coefficient which is significant only at a 10% level of confidence, the variable has only weak predictive power. Second, the estimated coefficient on openness is largely unaffected and remains highly significant. A possible explanation for this result is the generally low level of tariffs in industrialized countries. Moreover, as most of the countries in my sample are members of a common customs union, the European Union, a large share of their international trade is actually free of tariffs.[23] Hence, differences in the ratio of import duties to imports do not necessarily indicate different trade policies, but may mainly reflect differences in the regional and commodity structures of the countries' international trade position.

A better way, then, to discriminate between different trade policies may be to focus on actual tariff rates as a measure of a government's intention to protect the domestic market. The result (column 2), however, is even weaker than the finding for effective protection. Although the average tariff

[22] Even a very crude analysis suggests that persistence effects might play an important role. While Austria has about the same population size as Belgium, Portugal and Switzerland, its largest city, Vienna, has on average four times as much inhabitants as the main cities in other countries of comparable size.

[23] In fact, compiling the data from the IMF's *Government Finance Statistics Yearbook*, no entry is found for the Netherlands' revenues from import duties in 1990 as the actual figure is less than half of the reported digits.

DOI https://doi.org/10.3790/978-3-428-50499-2

Table 4.4

Additional Explanatory Variables

Dependent Variable: Log of Population in Main City	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share of Trade in GDP	−2.086**	−2.235**	−2.844**	−2.788**	−2.129**	−2.172**	−2.329**
	(0.313)	(0.382)	(0.388)	(0.259)	(0.296)	(0.317)	(0.376)
Other Openness Measures							
Ratio of Import Duties to Imports	4.005#						
	(1.926)						
Average Tariff Rate on Imports		6.262					
		(6.877)					
Internal Infrastructure							
Log of Railway Density, 1970–90			0.313**				
			(0.100)				
Log of Passenger Cars, 1970–90				−1.822*			
				(0.519)			
Political Variables							
Dictatorship Dummy, 1970					0.381		
					(0.314)		
Number of Cabinet Changes, 1970						−0.020	
						(0.025)	
Number of Changes in Effective Executive, 1970							0.012
							(0.041)
# of Observations	13	13	13	13	13	13	13
Adjusted R^2	0.914	0.912	0.957	0.937	0.906	0.900	0.896

Notes: Other regressors not shown in the table are a constant, a capital city dummy, the log of non-urbanized population, the log of real GDP per capita, and the share of the labor force outside of agriculture. White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

rate also enters positively, the coefficient is insignificant while the coefficient on the share of trade in GDP remains unchanged.

In regressions (3) and (4), I control for the internal infrastructure of a country. It has often been argued that internal transport costs are an important determinant of the national urban structure, although the sign of the relationship is a priori unclear. My results do support this ambiguity. On the one hand, I find a highly significant, positive coefficient on the log of railway density, indicating that countries with a better developed railway infrastructure have larger cities. On the other hand, the log of passenger cars per capita enters negatively and significant, indicating that countries which are better equipped with passenger cars measured in relation to the country's total population tend to have on average smaller main cities. Most notably for my point of interest, however, the estimated coefficient on the share of trade in GDP remains quantitatively unchanged and is statistically highly significant for both transportation variables.

Finally, also controlling for the political structure of a country does not change the finding of a robust negative relationship between openness and city size. Column (5) includes a dictatorship dummy which takes a value of one if the country in question had a dictatorial regime in 1970. In effect, the dummy assignes a value of one to the two Southern European and least developed countries in my sample, Portugal and Spain. However, the estimated coefficient is statistically not significant, even though it has the correct sign. Consistent with their theoretical discussion, Ades and Glaeser's (1995) find a positive coefficient on a dictatorship dummy, indicating that countries with a political system which ignores the political rights of their citizens have on average larger cities.

As my sample mostly comprises countries with a democratic political structure, columns (6) and (7) add variables which reflect the *stability* of the political system. Both controls, however, enter with insignificant coefficients which, in addition, take different signs, suggesting that in my sample political instability has no impact on city size.

In sum, the results largely confirm Ades and Glaeser's (1995) findings for a subsample of 13 European countries. In fact, given the immense statistical problems raised by the small size of my sample, the results are surprisingly robust. Most notably, I find a tight and robust negative relationship between the openness of a country and the size of its largest city. Moreover, there is also strong support for the hypothesis that main cities are on average larger if they are also capital cities.

4.4.2 More Years of Data

While in a first set of estimates I basically tried to replicate Ades and Glaeser's (1995) empirical results, I now begin to explore the advantages of my data set. In particular, I use the availability of historical data to check whether the negative empirical association between openness and main city size is robust over time. In the previous section, results applying data for separate years have shown that the estimated coefficient on openness consistently turns out to be significant at conventional levels of confidence. Moreover, the coefficient is in all cases clearly negative and economically large in magnitude. Nonetheless, there is some variation in the economic and statistical significance of the coefficient on the share of trade in GDP across individual years.

As I have data going back to 1870, I repeat the regressions for the individual years 1970, 1980, and 1990 for each of the earlier decades using, for comparability, the same set of control variables. Table 4.5 displays the results. At a first look, there is – as before – considerable variance in the results for separate years. Starting with the estimates for 1870[24], all coefficients have the expected, theoretically correct sign and are statistically highly significant. Most notably, openness has a measurable negative impact on the size of the largest city with an estimated elasticity of –7.6 which is more than three times higher than that found for later years. With an adjusted R^2 of 0.99, the fit of the regression is excellent.

In the following decades, the overall results are generally weaker with, for instance, the coefficients on per capita income and the non-agricultural share of the labor force losing statistical significance. Also the estimated coefficient on the trade-to-GDP ratio is smaller in size and becomes statistically insignificant. Although the coefficient consistently turns out to be negative, with the exception of 1920, the estimated elasticity is typically well below 0.5 and, thus, considerably smaller than in the estimates for the time period from 1970 to 1990. Beginning in 1880, then, only two controls enter consistently with statistically significant coefficients. The capital city dummy is positive and with values around 1.0 somewhat larger in magnitude than in the regressions for the period from 1970 to 1990. Not surprisingly (given previous results), also the coefficient on the non-urban population of a country turns out to be highly significant, taking the expected positive values.

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24 As I have no data on the structure of the labor force in Portugal in 1870 and 1880 and the non-urbanized population in the Netherlands (1870–1890), Portugal (1870–1880) and Spain (1870), columns (1) to (3) include less than 13 observations.

DOI https://doi.org/10.3790/978-3-428-50499-2

Table 4.5

More Years of Data

Dependent Variable: Log of Population in Main City	(1) 1870	(2) 1880	(3) 1890	(4) 1900	(5) 1910	(6) 1920	(7) 1930	(8) 1940	(9) 1950	(10) 1960
Constant	–16.772**	–8.326	–9.972	–6.066	–4.165	–2.688	–8.243#	–2.360	1.679	1.290
	(2.391)	(4.661)	(5.661)	(4.838)	(2.915)	(6.798)	(3.891)	(5.620)	(5.434)	(4.706)
Capital City Dummy	0.657**	0.851#	0.872*	0.882*	0.902**	1.048**	0.968**	1.114**	1.271*	1.019**
	(0.065)	(0.421)	(0.329)	(0.295)	(0.241)	(0.249)	(0.207)	(0.318)	(0.456)	(0.291)
Log of Non–Urbanized Population	0.411**	0.622*	0.553*	0.701**	0.698**	0.850**	0.869**	0.580*	0.569*	0.546*
	(0.052)	(0.203)	(0.194)	(0.172)	(0.139)	(0.146)	(0.151)	(0.171)	(0.219)	(0.188)
Log of Real GDP per Capita	3.143**	1.332	1.774	0.869	0.592	0.011	0.867#	0.635	0.031	0.154
	(0.453)	(0.773)	(1.193)	(0.799)	(0.606)	(1.044)	(0.400)	(0.577)	(0.501)	(0.420)
Share of the Labor Force Outside of Agriculture	3.132*	2.725	2.279	1.559	2.137#	3.335	0.365	1.583	3.757	3.413
	(0.773)	(1.922)	(1.388)	(1.407)	(1.117)	(1.991)	(1.734)	(1.912)	(2.501)	(1.938)
Share of Trade in GDP	–7.600**	–1.793	–2.920	–0.156	–0.101	0.119	–0.236	–0.816	–1.869	–1.709
	(0.740)	(1.154)	(1.950)	(0.263)	(0.180)	(0.718)	(0.781)	(1.268)	(1.489)	(1.099)
# of Observations	10	11	12	13	13	13	13	13	13	13
Adjusted R^2	0.990	0.840	0.876	0.868	0.842	0.717	0.685	0.642	0.597	0.750

Notes: White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

The results again change after 1940. The most interesting difference is that the coefficient on the variable of interest, openness, although still not statistically significantly different from zero, more than doubles in magnitude. The value of about –1.8 is not very different from the significant estimates for later years.

In summary, the year-specific estimates provide some interesting insights. First, even though differences in results for separate years should be interpreted with care due to the small sample size, there is convincing evidence that the initial regression results for the period from 1970 to 1990 are not robust over time. In particular, I find considerable variation in the predictive power of some controls. As both the magnitudes and the significance levels of those variables, however, are often robust for a number of consecutive years, this variation is apparently not the result of measurement problems or data inconsistencies, but rather seem to reflect structural changes. It is also interesting to note that in its actual specification the fit of the regression consistently decreases in the first half of this century. While the adjusted R^2 is above 0.86 in 1900, the controls explain less than 60% of the variation in the log of main city population in 1950.

Second, the finding of a negative association between trade exposure and the size of a country's largest city is confirmed for 1870, but does not hold for later years. This means that the share of trade in GDP enters only significant in four separate years (of which three – 1970, 1980 and 1990 – make up the time period which is usually examined in other studies!) and, thus, in just one third of my total time series of 120 years of data. This result, however, forcefully questions the empirical robustness of a relationship between openness and main city size.

A potential explanation for the observed differences in the results for individual years are boundary changes. Remember that after World War II, for example, I use data based on the former West German territory while before I have used information covering the former Deutsches Reich territory. Moreover, from this time on, I ignore Berlin as the largest German city and consider instead Hamburg as the main city. Despite those ad hoc conventions, however, I find, for instance, a positive and statistically significant coefficient on the capital city dummy which is robust for the complete time period from 1890 to 1990. In combination with earlier results showing the robustness of estimated coefficients after the exclusion of Austria and Germany, this suggests that the distorting effect of allowing for boundary changes is rather limited.

DOI https://doi.org/10.3790/978-3-428-50499-2

4.4.3 More Cities

An interesting alternative to deal with the problem of small sample size is to increase the number of cities *per country* used as dependent variable. The idea is that, instead of focusing exclusively on a country's largest city, the regression framework can also be applied to explain the size of a wider range of cities. Specifically, as I have data on the 20 largest cities in a country, this extension raises the number of observations in each year to 260.[25]

The modification allows to address three sets of issues. First, and most importantly, it is possible to check whether previous results also hold for a much richer data base. A simple verification of previous findings, however, requires the application of a basically similar regression specification which might itself be affected by small sample size. Therefore, the increase in the degrees of freedom associated with a larger number of observations should also be used to reconsider the chosen set of control variables. Finally, covering a broader range of cities in each country allows to explore possible differences in the relationship between openness and city size *within* countries. Specifically, an additional variable can be included which is designed to test the extent to which central cities in closed economies are disproportionately large relative to other large cities in the country.

I first consider the robustness of the benchmark specification. In particular, I experiment with various versions of the regressions reported in table 4.2, including different combinations of the available set of independent variables. The results (not reported here) strongly confirm the chosen benchmark specification (column 4 of table 4.2). Most notably, the coefficient on the log of land area turns out to be statistically insignificant, confirming that any observed association between openness and city size is not a spurious one possibly arising from the omission of this variable. The only difference, then, affects the included scaling variable. While the log of non-urban population has a good predictive power for the size of a country's largest city, this variable is replaced by the log of total population which provides a better empirical fit in the large sample.

The main results are in table 4.6. As before, the columns show year-specific regressions from 1870 to 1990, but now in 20-year-intervals to avoid clutter. A first observation is that the results look in general very similar to those found for the small sample. The capital city dummy and the estimated coefficient on the log of total population enter both positively and are statistically highly significant for each date, implying that more popu-

[25] I have no data for Portugal and Spain in 1870, reducing the number of observations for this year to 220.

Table 4.6

More Cities

Dependent Variable: Log of City Population	(1) 1870	(2) 1890	(3) 1910	(4) 1930	(5) 1950	(6) 1970	(7) 1990
Constant	–3.897#	–7.150**	–5.165**	–11.156**	–7.915**	–1.756	13.372**
	(2.284)	(1.546)	(1.602)	(1.623)	(1.987)	(2.444)	(3.842)
Dummy (Largest City = Capital City)	1.710**	2.189**	2.339**	1.934**	1.849**	1.794**	1.440**
	(0.374)	(0.299)	(0.234)	(0.393)	(0.332)	(0.279)	(0.229)
Log of Total Population	0.630**	0.869**	0.858**	1.020**	0.969**	0.752**	0.671**
	(0.051)	(0.042)	(0.040)	(0.045)	(0.064)	(0.049)	(0.041)
Log of Real GDP per Capita	0.344	0.381	0.081	0.668**	0.353#	–0.188	–2.624**
	(0.380)	(0.239)	(0.247)	(0.207)	(0.194)	(0.314)	(0.576)
Share of the Labor Force Outside of Agriculture	5.042**	1.611**	2.712**	0.266	0.738	3.795**	14.195**
	(0.766)	(0.499)	(0.568)	(0.600)	(0.549)	(1.314)	(2.553)
Share of Trade in GDP	–1.988**	0.153	0.020	0.322	0.065	–0.935**	–0.742**
	(0.681)	(0.191)	(0.101)	(0.348)	(0.501)	(0.352)	(0.181)
Dummy (Largest City) × Share of Trade in GDP	1.849#	0.994	0.561	2.440**	2.573**	1.570**	1.556**
	(0.947)	(0.709)	(0.432)	(0.831)	(0.927)	(0.593)	(0.401)
# of Observations	220	260	260	260	260	260	260
Adjusted R^2	0.757	0.738	0.728	0.743	0.688	0.694	0.709

Notes: White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

DOI https://doi.org/10.3790/978-3-428-50499-2

lous countries have larger cities and that the central city is disproportionately large if it is also the capital city of the country. The coefficients on the log of real per capita income and the share of labor force outside agriculture are each positive (with the exception of GDP per capita in 1970 and 1990), so that city size tends to increase with economic development, holding other variables constant. The levels of significance, however, vary over time, thereby confirming previous findings. While the positive relationship between city population and development is first captured by the economic structure variable, in later decades the coefficient on per capita income becomes significant. Interestingly, I am also able to reproduce the surprising change in the sign on per capita income which turns negative from 1970 to 1990. However, as the estimated coefficient on the closely related share of labor in agriculture (which is positive) increases dramatically in size, this result has apparently no real economic content.

More important from my perspective, I also replicate earlier results on the variable of interest, the share of trade in GDP. The estimated coefficient on openness is negative and statistically significant (at the 1% level) for only three years, 1870, 1970, and 1990, exactly matching previous findings derived from the small sample. For all other years, the coefficient turns out to be completely insignificant and actually enters positively. Hence, we have clear confirmation that the regression analysis in sections 4.4.1 and 4.4.2, although based on a very limited number of observations, is robust. The empirical association between openness and city size appears to be rather fragile.

Probably the most interesting finding of this exercise, then, is the estimated coefficient on the additionally included variable which interacts openness with a dummy for the country's largest city. The basic idea is to see whether, controlling for the effect of openness on city size, countries with lower exposure to international trade possibly have disproportionately large central cities. It turns out, however, that the estimated coefficient is *positive* and, for the subperiod after 1930, highly significant. This indicates that, if anything, it is rather that *open* economies tend to have disproportionately large metropolises.

Since openness is already entered as a regressor (and is mostly negative), it is possible that the positive coefficient on the interaction term largely compensates for this effect so that the net impact of openness on the population size of a country's largest city would be virtually nil. However, a Wald test rejects the assumption that both coefficients have the same absolute value. Hence, while countries open to international trade may generally have a less concentrated population (and therefore relatively small cities at the top of the city size distribution), they tend to have central cities which are – relative to this experience – disproportionately large.

DOI https://doi.org/10.3790/978-3-428-50499-2

In sum, we can conclude that previous findings based on a small sample of 13 European central cities turn out to be strongly robust. Extending the sample to cover the 20 largest cities in each country yields qualitatively identical results. As before, a negative correlation between an economy's exposure to international trade and city size holds for only three out of seven dates: 1870, 1970, and 1990. Moreover, for these dates, openness appears to work not only through its consequence on the size of the largest city but lowers city size also in other large cities. In fact, once this effect is controlled for, central cities even *increase* with openness.

4.4.4 Other Concentration Measures

In a third extension, I examine whether the negative relationship between openness and main city size is robust for different measures of urban concentration. In particular, I regress three alternative primacy measures on my standard set of explanatory variables.[26] Such a procedure is justified for at least three reasons. First, as I have argued that all controls are potential economic determinants of urban concentration, there is no need to change the regression specification. Accordingly, I would also expect exactly the same relationships. Second, other recent studies focusing, for example, on the share of a country's largest city in urban population such as Moomaw and Shatter (1996) basically include the same independent variables or slight variants of it. Finally, using an unchanged set of controls has the advantage that the results are directly comparable with my previous findings.

Table 4.7 shows the results. The estimates vary considerably across the different specifications. In fact, not only the signs and significance levels of single coefficients differ, but also the empirical fit of the regression varies markedly. While the set of controls explains about 70% of the variation in the share of the main city in total population and the H concentration measure, the adjusted R^2 for the ratio of the population size between a country's two largest cities is only 0.1. This finding supports the hypothesis that different measures of urban primacy indeed cover different aspects of the urban structure.

In the light of this observation, then, it is somewhat surprising that the findings for the share of trade in GDP are quite unequivocal. In all three specifications, the trade/GDP ratio enters negatively but with an estimated coefficient that is not statistically different from zero. This finding provides further evidence that a negative association between openness and urban concentration is empirically not robust.

[26] I have again replaced the log of non-urbanized population with the log of a country's total population which slightly increases the fit of the regressions.

DOI https://doi.org/10.3790/978-3-428-50499-2

Table 4.7

Other Concentration Measures

Dependent Variable:	(1) Log of Share of Main City in Total Urban Population	(2) Log of Ratio of Main City to Second Largest City	(3) H Concentration Measure
Constant	22.978*	4.036	37.996*
	(8.438)	(10.272)	(12.948)
Capital City Dummy	0.232	0.145	0.091
	(0.157)	(0.280)	(0.352)
Log of Total Population Outside the Main City	–0.720**	–0.270	–1.058**
	(0.168)	(0.169)	(0.240)
Log of Real GDP per Capita	–1.266	0.302	–2.645
	(1.194)	(1.293)	(1.700)
Share of the Labor Force Outside of Agriculture	–0.645	–1.542*	1.895
	(6.154)	(5.937)	(7.936)
Share of Trade in GDP	–1.587	–0.818	–2.155
	(0.870)	(0.974)	(1.262)
# of Observations	13	13	13
Adjusted R^2	0.717	0.102	0.687

Notes: White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively. All data are averages for the period from 1970 to 1990.

4.4.5 Full Time Period, 1870–1990

In a next step, I turn to the question whether there is a statistically significant association between openness and urban concentration, exploring the full time period for which I have data. This exercise might be of particular interest for at least two reasons. First, reviewing the results of the cross-country regressions, the small size of my sample might indeed be a serious problem. In fact, it is quite striking that I find almost consistently a negative coefficient on the share of trade in GDP, but with varying degrees of statistical significance. Yet this fragility of the empirical results does not necessarily reflect fundamental differences in the relationship between trade openness and urban concentration across individual years. With only 13 observations, it may also be simply due to some mismeasured data in individual years. Extending the time period then and, thereby, increasing the number of observations would possibly minimize the impact of any distorted data.

Second, even if the results for various specifications are not distorted and the different significance levels illustrate "true" variations in the importance of trade policies for the degree of urban centralization, it would be interesting to know which effect dominates and, thus, whether the finding of a statistically significant negative relationship also holds for the entire time period of 120 years.

The statistical results are in table 4.8. Each column reports the result of a pooled regression for the period from 1870 to 1990. As before, the parameter estimates vary considerably across different specifications. Most notably, the development controls take on changing signs for different concentration measures. Also the explanatory power of the controls varies markedly across the regressions, ranging from 0.13 to 0.85.

Given this general fragility, then, the estimated coefficients on openness deliver a somewhat more convincing picture as they are consistently negative. But only for two of the four different dependent variables, the trade/GDP ratio enters with a statistically significant (at the 5% level of confidence) coefficient. Thus, the much larger number of observations hardly affects previous findings. If anything, they confirm that the association between trade openness and urban concentration is not robust, depending on the chosen measure of urban concentration.

Almost similar results are obtained in a panel estimation with fixed country effects. This panel formulation allows to control for unobservable or unmeasurable individual country characteristics and, thus, to correct for a possible omitted variable bias. As can be seen from the results presented in table 4.9, the structural parameters often take the same sign and are of similar magnitude as the corresponding estimates obtained from pooled regression. The most notable change in the results, then, affects the variable of interest, the estimated coefficient on a country's share of trade in GDP. For specifications in which the absolute population in the main city or the share of the largest city in total population serve as concentration measures, the previously significant coefficients on openness lose their statistical significance and even become positive. Only when the H concentration measure is used as dependent variable the estimated coefficient on the trade-to-GDP ratio remains negative (and is weakly significant at the 10% level). In sum, the adoption of the panel approach further questions the robustness of a relationship between openness and concentration.

DOI https://doi.org/10.3790/978-3-428-50499-2

Table 4.8: **Full Time Period, 1870–1990**

Dependent Variable:	(1) Log of Population in Main City		(2) Log of Share of Main City in Total Urban Population		(3) Log of Ratio of Main City to Second Largest City		(4) Log of H Concentration Measure	
Constant	–4.869**	(1.072)	7.698**	(1.266)	–19.799*	(8.748)	1.606**	(0.278)
Capital City Dummy	0.937**	(0.072)	0.693**	(0.074)	2.151**	(0.297)	0.027**	(0.009)
Log of Non–Urbanized Population	0.751**	(0.034)						
Log of Total Population Outs. Main City	–0.472**	(0.030)			–0.069	(0.179)	–0.036**	(0.005)
Log of Real GDP per Capita	0.518**	(0.142)	–0.329#	(0.172)	3.404**	(1.128)	–0.138**	(0.035)
Share of Labor Force Outs. Agriculture	2.183**	(0.396)	–0.320	(0.464)	–5.593	(3.678)	0.083	(0.078)
Share of Trade in GDP	–0.317*	(0.154)	–0.353*	(0.164)	–0.779	(0.568)	–0.033	(0.023)
1880	0.228	(0.192)	0.118	(0.178)	–0.195	(1.029)	0.011	(0.023)
1890	0.388*	(0.182)	0.280	(0.188)	–0.330	(1.080)	0.053	(0.035)
1900	0.485*	(0.187)	0.406*	(0.186)	–0.779	(1.049)	0.074*	(0.035)
1910	0.544**	(0.176)	0.520**	(0.190)	–0.853	(1.084)	0.097**	(0.037)
1920	0.685**	(0.189)	0.592**	(0.187)	–0.693	(1.160)	0.116**	(0.038)
1930	0.634**	(0.206)	0.709**	(0.192)	–1.416	(1.146)	0.143**	(0.036)
1940	0.453*	(0.208)	0.569*	(0.222)	–2.172*	(1.064)	0.131**	(0.035)
1950	0.458*	(0.212)	0.654**	(0.222)	–2.149#	(1.099)	0.146**	(0.035)
1960	0.244	(0.219)	0.801**	(0.241)	–3.100**	(1.133)	0.198**	(0.042)
1970	–0.061	(0.247)	0.856**	(0.284)	–4.476**	(1.298)	0.235**	(0.048)
1980	–0.248	(0.264)	0.824**	(0.302)	–5.208**	(1.394)	0.256**	(0.055)
1990	–0.267	(0.291)	0.825**	(0.315)	–5.659**	(1.527)	0.273**	(0.060)
# of Observations	163		163		167		163	
Adjusted R^2	0.849		0.727		0.136		0.383	

Notes: White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

Table 4.9

Panel Estimation with Fixed Effects

Dependent Variable:	(1) Log of Population in Main City	(2) Log of Share of Main City in Total Urban Population	(3) Log of Ratio of Main City to Second Largest City	(4) Log of H Concentration Measure
Capital City Dummy	0.527** (0.094)	0.469** (0.077)	0.353** (0.100)	0.018 (0.017)
Log of Non–Urbanized Population	0.365** (0.068)			
Log of Total Population Outside the Main City		–0.592** (0.054)	0.107 (0.069)	–0.104** (0.012)
Log of Real GDP per Capita	0.509** (0.177)	0.245# (0.143)	0.254 (0.182)	–0.007 (0.032)
Share of the Labor Force Outside of Agriculture	1.962** (0.445)	0.145 (0.347)	0.389 (0.452)	–0.065 (0.077)
Share of Trade in GDP	0.072 (0.125)	0.066 (0.103)	0.128 (0.130)	–0.045# (0.023)
# of Observations	163	163	167	163
Adjusted R^2	0.884	0.925	0.802	0.740

Notes: The table reports the results of a least squares panel estimation with country and year dummies. Only the results on structural variables are reported to save space. Standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

4.4.6 Changes in Urban Concentration

Finally, I explore the time series dimension of my sample in more detail. As noted before, there are not only considerable differences across the European economies, but there is also a broad variation across time. Figure 4.4 graphs the changes in the variables of interest against the previous period. The figures illustrate wide fluctuations in the growth performance and, again, there is no uniform pattern observable for the different measures of concentration. After 1940, however, urban centralization (except when measured as the absolute population of the main city) and openness apparently move in parallel, providing some mild support for the thesis of a negative association between the two variables.[27] In short, it might be interesting to check in more detail whether the hypothesis of a negative correlation between an economy's exposure to international trade and the country's degree of population centralization holds for changes across time.

Besides of simply testing the results of the cross-country regressions, however, this specification may also yield a number of additional insights. For one thing, the analysis of changes provides a somewhat different perspective on the relationship between openness and urban concentration. In particular, it allows to examine a related version of the argument of a negative association, namely whether, holding population growth constant, central cities grow less quickly when economies become more open and vice versa.

But there are econometric implications as well. First, previous results may suffer from omitted variables. In the first-differenced version, however, problems related to the measurement or omission of variables will not bias the results if these unobserved country characteristics are constant over time.[28] Second, a country's exposure to trade may also be a function of urban concentration: in a highly centralized economy, domestic firms might benefit from a cost advantage over foreign suppliers which would negatively affect the amount of foreign trade. This raises the potential problem of endogeneity. Timing, i.e., the analysis of the correlation between initial variables and later changes, might then provide an (admittedly crude) check of causality.

Table 4.10 presents the results of simple pooled regressions in the first-difference specification with the estimations running from 1880 to 1990 (the first period being used in data construction). Across all concentration measures, the precision of the coefficients, as measured by the standard errors, is lower than in the traditional level specification. For none of the

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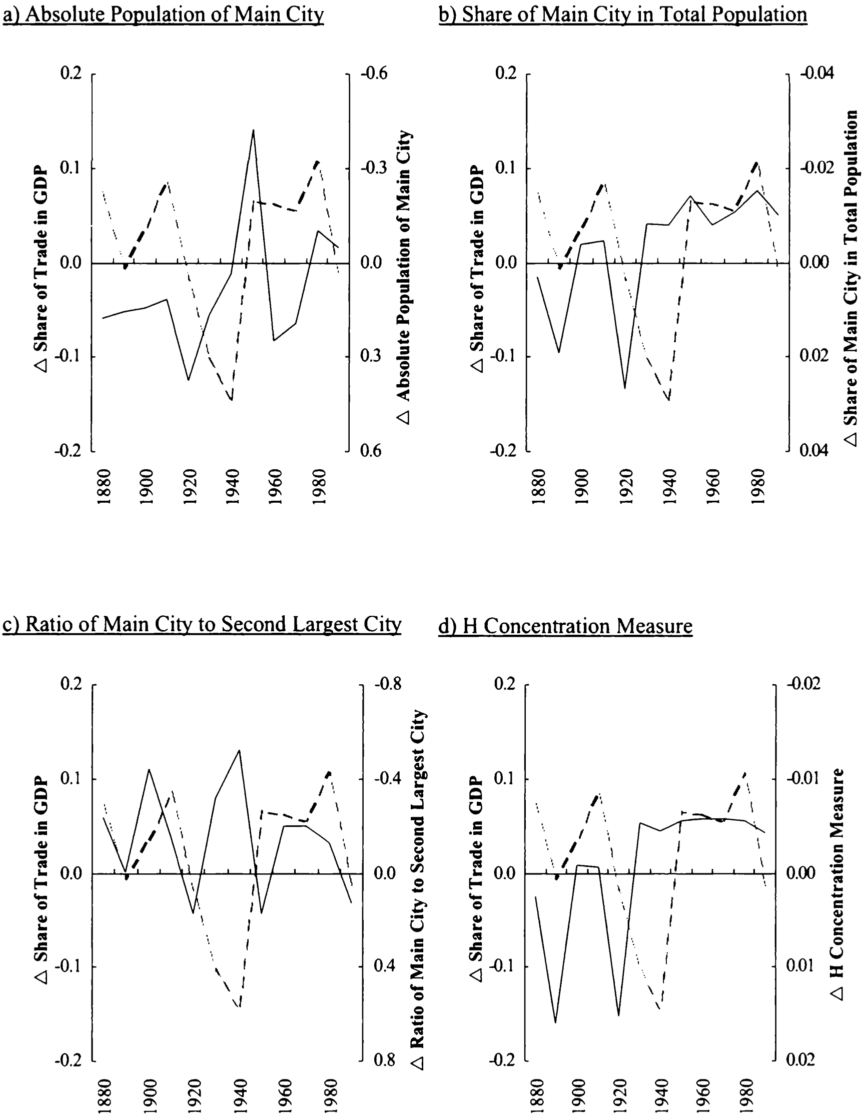
[27] Note that the concentration measures are plotted on an inverted scale.

[28] The panel estimation already controls for fixed effects.



Notes: The solid line represents the change in the concentration measure (right scale, inverted) and the dotted line is the change in the openness ratio.

Figure 4.4: Changes in the Openness Ratio and Measures of Urban Concentration

Table 4.10: **Changes in Urban Concentration, 1870–1990**

Dependent Variable:	(1) Δ Log of Population in Main City		(2) Δ Log of Share of Main City in Total Urban Population		(3) Δ Log of Ratio of Main City to Second Largest City		(4) Δ Log of H Concentration Measure	
Constant	0.219**	(0.048)	–0.002	(0.068)	–0.075	(0.049)	0.062	(0.110)
Δ Capital City Dummy	0.244	(0.173)	0.273*	(0.158)	0.112#	(0.066)	0.385#	(0.220)
Δ Log of Non–Urbanized Population	0.171**	(0.059)						
Δ Log of Total Population Outs. Main City			–0.615**	(0.080)	0.142*	(0.061)	–1.303**	(0.124)
Δ Log of Real GDP per Capita	0.241	(0.191)	0.200	(0.162)	–0.057	(0.147)	0.328	(0.228)
Δ Share of Labor Force Outs. Agriculture	0.531	(0.447)	0.092	(0.345)	0.188	(0.417)	–0.287	(0.546)
Δ Share of Trade in GDP	0.066	(0.048)	0.058	(0.050)	0.024	(0.070)	0.066	(0.072)
1880–90	0.005	(0.069)	0.041	(0.083)	0.076	(0.088)	0.081	(0.137)
1890–00	–0.056	(0.058)	0.004	(0.072)	0.010	(0.071)	–0.012	(0.118)
1900–10	–0.103#	(0.057)	0.009	(0.069)	0.055	(0.053)	0.014	(0.110)
1910–20	–0.056	(0.069)	0.096	(0.084)	0.149*	(0.067)	0.112	(0.129)
1920–30	–0.151*	(0.071)	–0.009	(0.081)	0.062	(0.061)	–0.051	(0.127)
1930–40	–0.171*	(0.067)	–0.042	(0.077)	0.010	(0.078)	–0.108	(0.116)
1940–50	–0.206*	(0.091)	–0.079	(0.089)	0.122*	(0.058)	–0.172	(0.129)
1950–60	–0.247**	(0.082)	–0.100	(0.089)	0.036	(0.059)	–0.116	(0.181)
1960–70	–0.340**	(0.086)	–0.221*	(0.088)	–0.026	(0.082)	–0.389**	(0.134)
1970–80	–0.364**	(0.062)	–0.237**	(0.075)	–0.011	(0.061)	–0.364**	(0.132)
1980–90	–0.224**	(0.082)	–0.098	(0.094)	0.151*	(0.068)	–0.312*	(0.127)
# of Observations	150		150		154		150	
Adjusted R^2	0.297		0.426		0.020		0.510	

Notes: White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

basic explanatory variables, I find a result which is robust for the four different dependent variables. Either the degrees of statistical significance vary considerably or the coefficients even take on changing signs. Given these generally weak results then, it is not surprising that also the estimates on openness give no convincing picture. The coefficient is in no case statistically different from zero and actually has the wrong sign. The consistently positive estimate suggests that, holding other effects constant, central cities *grew* on average when economies became more open to trade. This finding, however, is perhaps driven by a decrease in urban concentration in the inter-war period when also the volume of trade contracted. Dividing the entire time period into three sub-periods 1880–1910, 1920–1940, 1950–1990 and running separate regressions supports this hypothesis (results not shown). The change in openness enters negatively (and weakly significant) in the first period, then turns positive and statistically highly significant in the inter-war years, and remains positive (but insignificant) in the period from 1950 to 1990.

To provide a rough test for causality, I slightly change the simple first-difference specification and replace the change in openness by the share of trade in GDP at the beginning of the decade as explanatory variable. I now examine whether openness is a statistically significant predictor of the change in urban concentration in the subsequent decade. The results, displayed in table 4.11, are not encouraging. For each of the concentration measures, the coefficient on external trade takes the correct (negative) sign but is not statistically significant at conventional levels of confidence. In regressions not reported here, I have also experimented with longer (20-year) intervals. The basic results were virtually unchanged. The negative coefficient on the trade-to-GDP ratio slightly increases in magnitude, but remains statistically insignificant.

In sum, there is no evidence for the hypothesis that central cities grow faster in countries with more protectionist trade policies. Once again, this casts doubt on the theoretical presumption of a robust negative association between openness and urban concentration.

4.5 Conclusion

This chapter examines the empirical relationship between an economy's exposure to international trade and the degree to which a country's population is concentrated in the largest city. In contrast to previous work (Rosen and Resnick [1980], Ades and Glaeser [1995], Moomaw and Shatter [1996]) which is largely based on wide cross-country samples, the analysis in this chapter concentrates on only one region of the world, Europe. This focus has the advantage that, on the one hand, not much information is lost

Table 4.11: **Causality Test, 1870–1990**

Dependent Variable:	(1) Δ Log of Population in Main City		(2) Δ Log of Share of Main City in Total Urban Population		(3) Δ Log of Ratio of Main City to Second Largest City		(4) Δ Log of H Concentration Measure	
Constant	0.234**	(0.050)	0.009	(0.070)	–0.064	(0.051)	0.072	(0.109)
Δ Capital City Dummy	0.240	(0.174)	0.269#	(0.158)	0.110#	(0.066)	0.381#	(0.221)
Δ Log of Non–Urbanized Population	0.169**	(0.059)						
Δ Log of Total Population Outs. Main City			–0.620**	(0.080)	0.144*	(0.060)	–1.310**	(0.125)
Δ Log of Real GDP per Capita	0.239	(0.195)	0.201	(0.165)	–0.063	(0.147)	0.330	(0.232)
Δ Share of Labor Force Outs. Agriculture	0.508	(0.433)	0.070	(0.335)	0.176	(0.407)	–0.312	(0.535)
Share of Trade in GDP	–0.021	(0.049)	–0.011	(0.053)	–0.022	(0.048)	–0.007	(0.064)
1880–90	–0.001	(0.070)	0.035	(0.083)	0.076	(0.090)	0.074	(0.137)
1890–00	–0.060	(0.059)	0.000	(0.073)	0.012	(0.071)	–0.017	(0.119)
1900–10	–0.098#	(0.058)	0.012	(0.070)	0.059	(0.052)	0.017	(0.110)
1910–20	–0.064	(0.068)	0.086	(0.085)	0.149*	(0.067)	0.099	(0.130)
1920–30	–0.161*	(0.072)	–0.020	(0.082)	0.062	(0.058)	–0.064	(0.129)
1930–40	–0.185**	(0.067)	–0.055	(0.076)	0.005	(0.076)	–0.123	(0.115)
1940–50	–0.209*	(0.091)	–0.081	(0.089)	0.120*	(0.058)	–0.174	(0.129)
1950–60	–0.248**	(0.083)	–0.100	(0.090)	0.037	(0.060)	–0.117	(0.181)
1960–70	–0.340**	(0.089)	–0.222*	(0.089)	–0.024	(0.083)	–0.391**	(0.136)
1970–80	–0.360**	(0.065)	–0.234**	(0.076)	–0.007	(0.061)	–0.362**	(0.132)
1980–90	–0.228**	(0.077)	–0.103	(0.089)	0.153*	(0.063)	–0.319*	(0.127)
# of Observations	150		150		154		150	
Adjusted R^2	0.295		0.424		0.020		0.509	

Notes: White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

as there are still considerable cross-country differences, while, on the other hand, a wide variety of additional information, most notably historical data, is available. In short, I explore data from 13 European countries, covering the time period from 1870 to 1990.

The results can be summarized as follows. First, I am able to replicate Ades and Glaeser's finding of a statistically significant negative relationship between openness and the size of the largest city for a European subsample for the time period from 1970 to 1990. This finding also proves to be robust to the inclusion of a wide range of controls.

Second, the results are much weaker for other time periods. Examining the entire period from 1870 to 1990 in year-specific regressions, I find a statistically significant negative coefficient on the share of trade in GDP in only four cases and, thus, in less than one-third of the years for which I have data. As this fragility, however, may at least in some part be due to the small sample size, I also implement pooled estimation techniques. However, these more advanced specifications provide no further insights. In a pooled regression for the whole 120-year period, the estimated coefficient on openness is negative and statistically significant at the 5% level. However, the coefficient loses its significance in a panel estimation with fixed country effects.

Third, the linkage between trade policy and urban centralization is also not robust for alternative measures of urban concentration. Replacing absolute central city size by other concentration measures which are frequently used in the literature yields negative, but insignificant coefficients on the trade-to-GDP ratio.

Fourth, in a more dynamic interpretation of the hypothesis that there is a negative relationship between trade orientation and urban concentration, there is also no evidence that economies become less centralized when a country turns to more open trade policies and thereby increases its exposure to international trade.

In conclusion, the apparent fragility of the empirical association between openness and urban concentration obviously calls into question the validity of Krugman and Livas Elizondo's (1996) urban structure model.

"If there is one single area of economics in which path dependence is unmistakable, it is in economic geography – the location of production in space. The long shadow cast by history is apparent at all scales, from the smallest to the largest – from the cluster of costume jewelry firms in Providence to the concentration of 60 million people in the Northeast Corridor."

Paul Krugman (1991c, p. 80)

Chapter 5

Does History Matter for City Growth? The Case of Vienna

5.1 Introduction

A central feature of recent models of city formation is that, given the model displays multiple equilibria, small chance events early in history play a large role in determining which equilibrium actually occurs. Once random economic events have selected a particular path (e.g., a specific location), the choice typically becomes locked-in regardless of the advantages of the alternatives. An illustrative example for this type of models is Paul Krugman's (1993, 1996b) highly stylized "'59 Cadillac" model. Motivated by the desire to explain the emergence of complex urban landscapes through spontaneous self-organization in space, Krugman starts from a random distribution of workers across 12 clock-wise arranged locations. Assuming then backward and forward linkages in production and allowing workers to move toward locations with above-average wages, all workers eventually end up in two locations. In Krugman's simulations one of the concentrations is typically the location which starts (by chance) with the largest share of workers so that there is a pattern of reinforcement of initial advantage. The second concentration then often emerges at one of the initially large locations on the opposite side of the circle. In sum, models of this type attempt to demonstrate that the details of the emerging geography depend sensitively on initial conditions.

Surprisingly, explicit empirical tests of the importance of history in city growth have been very rare. One notable exception is a paper by James Rauch (1993) who analyzes the pricing behavior of developers of industrial parks in the United States. He finds that developers typically attract an industry to a new location by subsidizing the first firm(s) and subsequently increase the prices for land. Rauch interprets this time path of prices as

DOI https://doi.org/10.3790/978-3-428-50499-2

evidence for history creating a first-mover disadvantage that can otherwise prevent the relocation of firms from an established, high-cost site to a new, low-cost site.

This chapter provides another attempt to explore the role of history in city growth empirically. In particular, it is argued that the dissolution of the Austro-Hungarian Empire in 1918 provides a natural experiment to examine the existence of path dependence. Specifically, the break-up of the Habsburg Empire gives, for itself, no reason for people to move to a new location. Thus, 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. In contrast, if history plays largely no role in city growth, the dissolution of Austria-Hungary should lead to a gradual reduction of the urban dominance of (then overdimensioned) Vienna in relation to other European capitals.[1]

To preview the main results, I find indeed evidence that Vienna's primacy falls over time. This effect, however, appears to fade (and even to reverse) in later decades so that in the long run of more than a half century there is some support for the existence of path dependence. In fact, using differenced equations, only in the immediate after-break-up period Vienna's importance declines significantly relative to the other European cities in the sample.

The plan of the remainder of this chapter is as follows. Section 2 details the main features of the dissolution of the Austro-Hungarian Empire. Section 3 discusses some first evidence. After describing the data set and the basic empirical framework, section 4 presents regression results. Section 5 concludes.

5.2 The Dissolution of the Austro-Hungarian Empire

The six and a half centuries long history of the Habsburg Empire is marked by a series of territorial acquisitions, military conflicts and, most of all, dynastic marriages through which the Habsburgs emerged as the dominant dynastic power in Europe. In their heyday of geographical expansion, under Maximilian I, the Habsburgs eventually ruled – in addition to their original Austrian possessions – over the Netherlands, the Burgundian provinces, and the Kingdom of Spain and its overseas possessions Naples, Sicily, and Sardinia. Even on the eve of its collapse, the Habsburg Empire ranked as a major European power. With a population of roughly

[1] As the dissolution of Austria-Hungary was mainly forced by ethnic tensions which, history tells us, often give rise to massive population movements, it is also highly plausible to expect that Vienna's population size decreased by outmigration of non-Austrians. However, this should have largely been a one-time effect, occurring in the immediate after-break-up period.

50 million, it stood third in size behind Russia and Germany (Good [1984, pp. 1–3]).

Despite its immense geographical size, however, the political power of the Habsburgs was to a large extent limited by the disunity inside the Empire. On the one hand, this was the result of ethnic heterogeneity and wide variations in the economic conditions and social structures of different parts of the Monarchy. On the other hand, territories were mostly acquired through marriages. According to C. A. Macartney (1969, p. 13), "[a] corollary of this method of Empire accumulation [through simple transference from one hand to another of this or that Kingdom or Duchy] was that the links between the components, at least when first formed, were purely dynastic. ... This individuality, constitutional and perhaps even more, sentimental, of the different Lands, was and remained a continual feature of the entire monarchy, throughout its history." Moreover, territories often changed hands. In fact, the Habsburgs have reduced their influence by their practice of dividing the family heritage between them, thus splitting it at times into two or even three blocs. The most visible example of this disunity is probably the Dual Compromise of 1867 which effectively led to a large autonomy of Hungary. Figure 5.1 displays a map of the Austro-Hungarian Empire.

Even though the political integration of the Habsburg Empire was precarious and there is disagreement among historians about the actual extent of economic unity of the Habsburg lands, the city of Vienna was clearly the nucleus of Austria-Hungary. Besides of being the political and cultural hub[2] of the Monarchy, Vienna was also the railroad and financial center of the Empire. David Good (1984, pp. 99–104), for example, notes that by 1873 the basic outlines of the Empire's railroad system had been established with a central north-south axis centered on Vienna (1841), lines from this axis northwest to Prague and southwest to Budapest (1854), links from Vienna westward to Salzburg (1860) and the Tyrolean Alps and eastward to as far as Czernowitz. Post–1873 construction then basically amounted to extending the primary network and supplementing it with feeder lines and double tracking. Not surprisingly, the expansion of the financial network shows remarkable parallels to that of the railroad system. Following the establishment of a branch network of the Austrian National Bank, Viennese banks also erected sizable branch bank networks which allowed the channeling of funds from the financial core to the economic growth centers of the Empire: the Alpine provinces in Austria, the industrial districts of the

[2] The cultural dominance stretches from education to arts. In some fields (e.g., medicine, philosophy), the University of Vienna was one of the leading institutions of its kind in the world. Also the Viennese Opera and the Burgtheater had a very high reputation, and there was a busy and productive musical, literary and artistic life (Macartney [1969, p. 636]).

DOI https://doi.org/10.3790/978-3-428-50499-2

Bohemian crownlands, and several regions in the eastern hinterland. Despite this shift in credit activity, however, Vienna's share of bills discounted and secured loans granted by the national bank was still approximately 35 percent on the eve of World War I, illustrating Vienna's uncontested position as the Empire's primary financial center (Good [1984]).

In the final decades of the Austro-Hungarian Monarchy, Vienna registered a strong increase in population. Benefiting from both industrialization and prosperity, the population size of the Empire's capital more than tripled from about 610,000 in 1870 to more than 2 million in 1910. At this time, Vienna was probably one of the ten largest cities in the world.[3]

Only a few years later, however, the Habsburg Empire was no longer existent. In the fall of 1918, the end of World War I was also the end of the Monarchy. With the defeat of Austria and growing economic misery, the internal stability of the ethnically diverse Empire simply collapsed. In rapid succession, there were revolutions in various regions of the Empire, nationalities declared their independence, and provisional governments took over. In Austria, a brief attempt to sustain the Monarchy failed and the Austrian Republic was proclaimed. As shown in table 5.1, the territorial redistribution was immense. Austria and Hungary were radically cut back. In fact, the newly established Republic of Austria comprised less than one-third of the territory of the former Austrian part of the Habsburg Empire. The corresponding population size fell from 29 million to 7 million. Measured in terms of the whole Empire, territory and population were even reduced by about 85 percent.

This experience, then, provides the background for the empirical analysis. In particular, it is argued that the reduction in country size provides for itself no reason for Vienna's inhabitants to move to a new location, especially as the newly emerging states (Czechoslovakia, Poland) and the countries gaining some parts of the Empire (Romania, Yugoslavia, Italy) were of different nationalities. Hence, the disintegration of Austria-Hungary should have had no direct effect on the evolution of Vienna. If, in contrast, the impact of history on city growth is rather limited, it can be expected that Vienna gradually decreases in size over time since the former capital of the Habsburg Empire is probably too large in size as the central city of the Austrian Republic.[4]

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[3] Paul Bairoch (1988, p. 225) notes "[O]n the eve of the First World War, Europe had five cities with populations of more than 2 million (Berlin, Leningrad, London, Paris, and Vienna), to which should be added three others outside Europe (New York, Chicago, and Tokyo)."

[4] Strictly speaking I do not test an explicit model, because I do not have a well-specified alternative hypothesis. The main objective in this chapter is simply to check whether or not predictions of lock-in effects are consistent with the data.

DOI https://doi.org/10.3790/978-3-428-50499-2

Source: Macartney (1969).

Figure 5.1: Map of Austria-Hungary after 1867


RUSSIA
R.Vistula
Cracow
Teschen
Przemysl
Lemberg
Tarnopol
GALICIA
Kassa
(Pressburg)
Miskolcz
Czernowitz
BUKOVINA
Budapest
R.Tisza
Debreczen
Nagyvárad
KINGDOM OF HUNGARY
TRANSYLVANIA
Kolozsvár
Szeged
Arad
Temesvár
R.Maros
Brassó
Orsova
ROUMANIA
Belgrade
SERBIA
R.Danube
Sarajevo
SANJAK OF NOVIBAZAR
BULGARIA
MONTENEGRO
Established 1913
ALBANIA
Austrian occupation 1878-1912
0 50 100
Miles
ARTHUR BANKS

Table 5.1

The Disintegration of the Austro-Hungarian Empire

	Austrian Empire				Hungarian Kingdom				Austro–Hungarian Empire			
	Territory		Population		Territory		Population		Territory		Population	
	sq. km	% of total	millions	% of total	sq. km	% of total	millions	% of total	sq. km	% of total	millions	% of total
Prewar status	300.0	100.0	28.6	100.0	324.4	100.0	20.9	100.0	624.4	100.0	49.5	100.0
Ceded to:												
Austria	79.6	26.5	6.6	23.1	5.1	1.6	0.4	1.9	84.7	13.6	7.0	14.1
Hungary	–	–	–	–	92.7	28.6	7.9	37.8	92.7	14.8	7.9	16.0
Czechoslovakia	77.8	25.9	9.8	34.3	62.9	19.4	3.6	17.2	140.7	22.5	13.4	27.1
Romania	10.4	3.5	0.8	2.8	102.8	31.7	5.3	25.4	113.2	18.1	6.1	12.3
Yugoslavia	29.3	9.8	1.7	5.9	66.5	20.5	4.1	19.6	95.8	15.3	5.8	11.7
Poland	80.3	26.8	8.4	29.4	–	–	–	–	80.3	12.9	8.4	17.0
Italy	22.6	7.5	1.5	5.2	–	–	–	–	22.6	3.6	1.5	3.0

Notes: The percentages may not sum to 100 due to rounding.
Source: adapted from Dornbusch (1992).

5.3 First Evidence

Figure 5.2 shows the absolute population of Vienna. The figure reveals an unmistakable decline in population size after the break-up of Austria-Hungary. Peaking at more than 2 million in 1910, the number of inhabitants shrinks over time by about one fourth to 1.5 million in 1990. Moreover, table 5.2 shows that the growth experience of Vienna differs markedly from that of other Austrian cities. The first half of the table displays the average growth rate in population size for all cities with an initial population of more than 20,000 over the time period from 1870 to 1910. Before the disintegration of Austria-Hungary, then, Vienna experienced not only the largest absolute increase in population size in the Empire, but also had – with the exception of the Bohemian city of Pilsen – the by far largest rate of expansion, tripling the population within five decades. After the break-up, the picture is exactly the opposite. In the five decades following the end of the Habsburg Empire, Vienna has the lowest rate of population growth among the largest cities in Austria and, in fact, is with the exception of Wiener Neustadt the only city with an actual decline in population size.

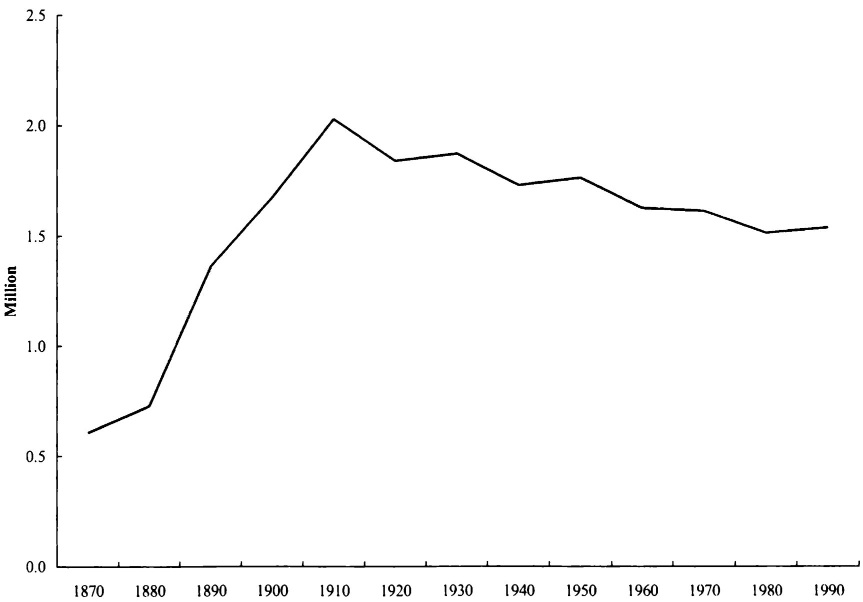

Figure 5.2: The Evolution of the Population Size in Vienna

DOI https://doi.org/10.3790/978-3-428-50499-2

Table 5.2: **City Growth in Austria**

a) Before Disintegration (Austrian Part of the Habsburg Empire)

City	Region	1870	1910	%
Austrian Territory				
Wien (Vienna)		607,514	2,031,498	234.4
Graz		81,119	151,781	87.1
Linz		33,394	67,817	103.1
Salzburg		20,336	36,188	78.0
Other Regions				
Prag	Bohemia	157,713	223,741	41.9
Lemberg	Galicia	87,109	206,113	136.6
Brünn	Moravia	73,771	125,737	70.4
Triest	Trieste	68,580	161,653	135.7
Krakau	Galicia	49,835	151,886	204.8
Czernowitz	Bukovina	33,884	87,128	157.1
Pilsen	Bohemia	23,681	80,343	239.3
Laibach	Carniola	22,593	41,727	84.7
Reichenberg	Bohemia	22,394	36,350	62.3
Tarnow	Galicia	21,779	36,731	68.7
Tarnopol	Galicia	20,087	33,871	68.6
Iglau	Moravia	20,049	25,914	29.3

b) After Disintegration (Republic of Austria)

City	1920	1970	%
Wien (Vienna)	1,841,326	1,614,841	–12.3
Graz	157,032	248,500	58.2
Linz	93,473	202,874	117.0
Innsbruck	55,659	115,197	107.0
Salzburg	36,450	128,845	253.5
Wiener Neustadt	35,023	34,774	–0.7
Klagenfurt	26,111	74,326	184.7
St. Pölten	23,061	50,144	117.4
Villach	21,896	34,595	58.0
Baden	21,095	22,631	7.3
Steyr	20,234	40,578	100.5

Notes: The tables are compiled from Austrian statistical yearbooks and include all cities with a population of more than 20,000 at the beginning of the respective time period. I use for all cities German names as for most places no common English name exists. The exact census dates are 31 December 1869, 31 December 1910, 31 December 1920, and 12 May 1971, respectively.

DOI https://doi.org/10.3790/978-3-428-50499-2

Figure 5.3 illustrates a similar pattern for two alternative measures of urban concentration. Graph (a) shows the share of Vienna in total Austrian population. Confirming the earlier picture of absolute population figures, the urban dominance of Vienna has gradually increased from 1870 to 1918, and – after a jump in levels resulting from the reduction in total country population due to the disintegration of Austria-Hungary – it has continually fallen since then. Similarly, the ratio of Vienna to Austria's second largest city, Graz, shown in graph (b), has almost doubled in the half century before 1918 and fallen back to its 1870 level after the break-up of the Habsburg Empire.

Taken together, this evidence suggests that there is little support for the existence of path dependence. It rather appears that there is a process of "normalization" at work which has led to a gradual reduction of the possible overdimension of Vienna after the loss of a large part of its hinterland.

5.4 Probing Deeper

5.4.1 Data and Methodology

In evaluating the extent to which the actual fall in Vienna's primacy is indeed the result of the disintegration of Austria-Hungary, one has to correct for other factors which have the potential to affect a country's degree of urban concentration. The decrease in Vienna's dominance could, for example, simply reflect the rise in average per capita income in Austria over time. In fact, studies of urban primacy have long established a negative relationship between economic development and urban concentration (see, for example, Rosen and Resnick [1980] and Moomaw and Shatter [1996]).

Furthermore, it may be helpful to relate Vienna's experience to the development of other metropolises in Europe. If, for example, there is a general tendency towards smaller central cities unexplained by the set of independent variables, this approach at least allows us to capture effects of possibly missing variables which affect the sizes of Vienna and large cities in other countries alike.

In sum, the basic analytical framework is very similar to recent studies which seek to explain differences in urban concentration across countries. Specifically, I consider an equation of the form

$$URBCONC_{it} = \alpha_t + \beta_t VIENNA + \gamma_t \mathbf{Z}_{it} + \varepsilon_{it} \qquad (5.1)$$

where i is the country; t is the time period; *URBCONC* is the indicator for urban concentration; *VIENNA* is a dummy variable which takes the value

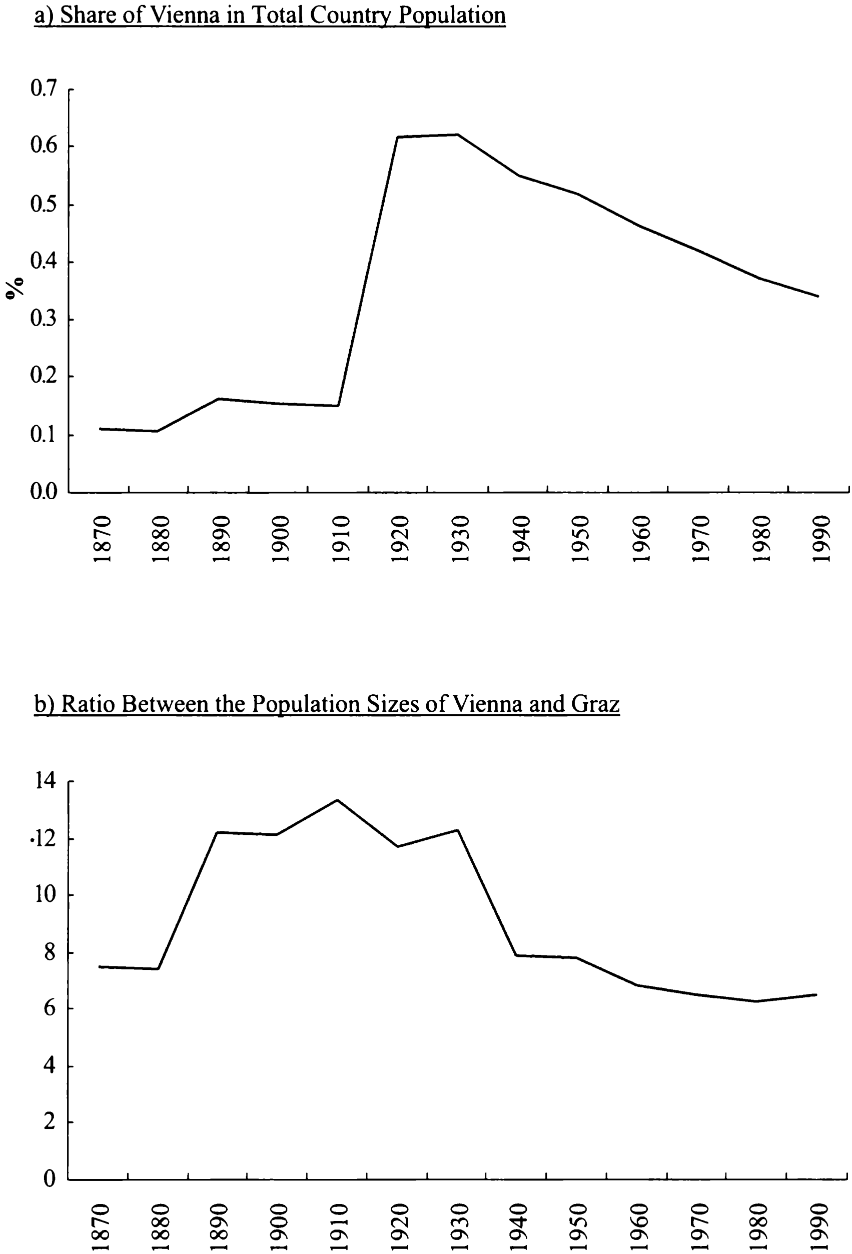

Figure 5.3: The Evolution of Urban Concentration in Austria(-Hungary)

of one for Austrian data; **Z** is a vector of variables that influence the extent of urban concentration; and ε is an error term. The basic idea is that β captures the extent to which the largest city in Austria, Vienna, is larger than the target that is determined by the **Z** variables. In the following, I discuss each term in more detail.

The dependent variable is a measure of urban primacy, whereby it is one of the contributions of this chapter to allow for three different indices of urban concentration.[5] Specifically, I will show results for the absolute number of inhabitants in a country's largest city, the share of the largest city in total urban population and the ratio of the largest city to the population in the second largest metropolitan area.

Previous studies on the determinants of urban concentration in large cross sections of countries have focused on a number of explanatory variables. As noted above, for example, the existence of giant metropolitan areas is usually negatively related to measures of economic development, so that urban concentration can be expected to fall with per capita income. Other variables typically considered are political and structural indicators (see, for example, Ades and Glaeser [1995] and Moomaw and Shatter [1996]). In light of these studies, my benchmark regression includes the following explanatory variables: per capita GDP, the share of labor force outside of agriculture, (non-urbanized) population, the share of trade in GDP, and a dummy which takes the value of one if the country's largest city is also its capital.

A dummy variable is then added to represent Austrian data. In fact, as the primary purpose of this chapter is to see how much of the decline in Vienna's primacy can be explained by simple economic factors common to central cities throughout Europe, and how much is left over to be attributed to the break-up of Austria-Hungary, this dummy will be the main variable of interest. Specifically, the dummy variable allows to check whether the level and time trend of Vienna's dominance is exceeded by that in the rest of the sample. Given then a possible overdimension of Vienna after the end of the Habsburg Empire, I would expect a significantly positive coefficient on the Vienna dummy in 1920. If history matters, this coefficient should stay at this level also in later periods, indicating that Vienna remains dis-

[5] As there is no generally agreed upon measure of urban primacy, most of the literature discusses different ways to define urban concentration but then the analysis often focuses on a single measure which is apparently preferred by the respective author(s). Given that the correlation between alternative measures of urban concentration is often weak, with different indices representing different aspects of the national urban structure, it is not surprising that the vast empirical literature on the determinants of urban primacy reports ambiguous results on a variety of single explanatory variables.

proportionately large in comparison with other European metropolises. If, in contrast, path dependencies play only a minor role, the coefficient will continuously get smaller in magnitude and may even lose significance.

My sample consists of 12 European countries.[6] While the actual selection is largely dictated by the availability of reliable historical data[7], the focus on European countries has an additional advantage. The sample covers an almost contiguous territory with a fairly homogeneous economic development which is, moreover, largely comparable to that of Austria. Therefore, the data should be free of any biases possibly arising from major external shocks (such as World War II). The main disadvantage of the data set is the small number of observations. With only 12 data points in a pure cross-country analysis, there is only a very limited number of degrees of freedom. To deal with this problem, I pool data for two consecutive periods and include a dummy variable to capture year-specific effects.[8]

The analysis is based on city population data which is compiled from national statistical yearbooks. This data probably has several deficiencies. For one thing, the definitions of metropolitan areas may differ across countries, sometimes comprising the inner city and sometimes referring to the whole agglomeration. In addition, as cities grow, there are probably also changes in the definition of city borders across time, implying discrete shifts in population size. As there is no indication, however, that the data in particular countries or time periods are consistently biased, the impact on the empirical results should be not too damaging.

The other data is taken from two sources. Maddison (1995) provides data on population and real per capita income. The main shortcoming of this data set for my purposes is that Maddison already corrects for border changes so that his historical Austrian data refer to the territory of the Republic of Austria. Therefore, population for Austria-Hungary is taken from Mitchell (1992). To use also the advantages of Maddison's data set, however, allowing comparability over time, I have decided to take Maddison's per capita GDP data for Austria as an approximation for per capita income in the Habsburg Empire. This assumption is justified by Good (1984, table

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[6] The countries are Austria, Belgium, Denmark, Finland, France, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, and Switzerland.

[7] A second selection criterion is that the countries have been basically unaffected by boundary changes. The most serious problem then might arise in the case of France which lost Alsace and Lorrain in 1870 and regained these territories in 1918. According to Maddison's estimates (1995, p. 129), however, this territorial change temporarily lowered France's GDP and population in this period by less than 5 percent.

[8] I have also experimented with other pooling techniques. However, the results were basically identical.

DOI https://doi.org/10.3790/978-3-428-50499-2

24) who shows that income per capita in the Alpine lands, i.e., the central core of the Austrian economy, is not very different from the average of the Austrian part of the Empire.[9] The share of labor force in agriculture and the openness ratio are calculated from Mitchell (1992).

5.4.2 Is Vienna Too Large?

In a first step, then, I use the analytical framework to explain the absolute size of the country's largest city. Table 5.3 displays the results. Since I have data reaching back to 1870, I also present regressions for the five decades before the disintegration of Austria-Hungary. Moreover, as noted above, all data are pooled for two consecutive periods so that each column shows the results for a ten-year-interval. Thus, column (1) reports the results for city size in 1870 and 1880.

The fit of the regressions is generally excellent, with an adjusted R^2 of 0.77–0.92. Further, the results are basically consistent with Ades and Glaeser (1995) even though the explanatory variables are not always statistically significant. The positive coefficient on the capital city dummy indicates that main cities typically double in size if they are also capital cities. Thus, even in a sample which includes almost exclusively countries with an established democratic political system, I find support for Ades and Glaeser's claim that political power attracts population. Non-urbanized population also enters with a positive sign and is statistically highly significant. With values below one, the parameter suggests that urban areas grow with their countries but less than proportionately.[10] The results for the other three controls are somewhat weaker. The estimated coefficient on per capita income changes signs across time. The positive coefficient on the labor share outside agriculture indicates that cities rise with the fraction of the population that is not tied to natural resources. Openness consistently enters with a negative sign, confirming Krugman and Livas Elizondo (1996), but is statistically significant only for the period from 1960 to 1990.

The key parameter of interest is the measure of Vienna's dominance. As expected, there is a remarkable break in the time trend of the estimated

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[9] According to Good, per capita income in the richest Austrian province, Lower Austria, is 50 percent above the Austrian average. Due to high per capita income in the large economies of the Bohemian lands, however, the second richest Austrian province, Salzburg, is only at 113 percent of the Austrian average and there are even provinces in the Alpine lands (Styria, Carinthia) which have a per capita income that is below the Empire's average.

[10] I have also experimented with alternative population measures such as a country's urbanized population and total population. Similar to Ades and Glaeser's (1995) findings, however, these variables did not add much explanatory power.

DOI https://doi.org/10.3790/978-3-428-50499-2

Table 5.3

The Impact of Austro-Hungarian Disintegration on the Absolute Size of Vienna

Dependent Variable: Log of Population in Main City	Before Disintegration				After Disintegration						
	(1) 1870–80	(2) 1880–90	(3) 1890–1900	(4) 1900–10	(5) 1920–30	(6) 1930–40	(7) 1940–50	(8) 1950–60	(9) 1960–70	(10) 1970–80	(11) 1980–90
Constant	–13.084**	–11.308**	–8.720*	–7.385#	–6.224*	–2.759	–1.107	–0.885	0.500	3.221	16.469**
	(3.319)	(2.881)	(3.699)	(3.664)	(2.314)	(3.191)	(3.407)	(2.566)	(2.633)	(3.852)	(5.451)
Vienna Dummy	–0.702#	–0.527	–0.296	–0.255	1.220**	1.079**	0.935**	0.700**	0.485**	0.404**	0.534**
	(0.319)	(0.337)	(0.203)	(0.250)	(0.113)	(0.129)	(0.112)	(0.159)	(0.084)	(0.059)	(0.075)
Capital City Dummy	0.860**	1.110**	1.125**	0.992**	0.823**	0.742**	1.084**	1.173**	1.037**	0.750**	0.399#
	(0.247)	(0.206)	(0.194)	(0.170)	(0.129)	(0.176)	(0.190)	(0.174)	(0.152)	(0.184)	(0.228)
Log of Non–Urbanized Population	0.786**	0.761**	0.834**	0.761**	0.825**	0.684**	0.605**	0.609**	0.559**	0.536**	0.517**
	(0.162)	(0.132)	(0.102)	(0.098)	(0.093)	(0.135)	(0.133)	(0.099)	(0.067)	(0.050)	(0.045)
Log of Real GDP per Capita	1.663*	1.406*	0.863	0.887	0.695#	0.556#	0.414	0.370	0.277	–0.071	–2.363*
	(0.650)	(0.551)	(0.532)	(0.539)	(0.343)	(0.281)	(0.269)	(0.284)	(0.312)	(0.490)	(0.855)
Share of the Labor Force Outside of Agriculture	1.927	2.864**	2.746**	1.974*	1.014	1.278	1.699#	1.873	2.460#	3.723#	13.800**
	(1.119)	(0.899)	(0.834)	(0.901)	(0.784)	(0.797)	(0.928)	(1.341)	(1.285)	(1.908)	(3.548)
Share of Trade in GDP	–1.995	–1.559	–0.341	–0.167	–0.016	–0.732	–0.654	–0.786	–1.320*	–1.688**	–1.796**
	(1.271)	(0.950)	(0.255)	(0.132)	(0.422)	(0.527)	(0.640)	(0.569)	(0.512)	(0.472)	(0.321)
Year Dummy	0.252	0.025	0.011	0.008	–0.041	–0.165	0.051	–0.109	–0.186	0.014	0.262#
	(0.205)	(0.192)	(0.180)	(0.145)	(0.195)	(0.169)	(0.151)	(0.130)	(0.143)	(0.134)	(0.126)
# of Observations	19	21	23	24	24	24	24	24	24	24	24
Adjusted R^2	0.894	0.896	0.829	0.815	0.833	0.767	0.799	0.856	0.903	0.923	0.903

Notes: White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

coefficients. Before the disintegration of Austria-Hungary, the coefficient is statistically insignificant and even takes a negative sign, indicating that Vienna is, if anything, too small given the size of the Habsburg Empire. This result probably reflects the relatively small extent of centralization of Austria-Hungary under Dualism. Immediately after the break-up of the Austro-Hungarian Monarchy, however, the coefficient becomes positive and is highly significant at the 1 percent level (column 5). The estimated coefficient of 1.2 for 1920–30 implies that Vienna is about 230 (= exp[1.2]–1) percent larger than is explained by the economic size of the Republic of Austria. Thus, instead of its actual size of 1.85 million inhabitants in 1920, the population size of the Austrian capital should not have exceeded 560,000. In the following decades, the coefficient remains statistically significant but falls considerably in magnitude. In fact, by 1970–80, the parameter has decreased by more than two-thirds to 0.4. Thus, the overdimension of Vienna is gradually reduced.

Somewhat surprisingly, then, the estimated coefficient on the Vienna dummy increases again in 1980–90, the last period for which I have data. A plausible explanation for this disproportionate rise in primacy would be that Vienna increasingly benefits from its role as a seat of international organizations such as the United Nations or OPEC. In addition, it is also possible that Vienna has begun to establish itself as a trade center close to Eastern Europe. With only a single observation, however, the empirical evidence that something like this is at work here remains rather fragile.

Table 5.4 shows regressions in which the dependent variable is now the share of the largest city in total urban population. As before, the empirical fit is quite respectable. There are, however, three notable differences to the previous results.

First, one notes that the population control now enters negatively, capturing the effect that the population in larger countries tends to be relatively less concentrated in a single city. Second, turning to the variable of interest, the estimated coefficient on the Vienna dummy is now highly significant for the period from 1890–1910. In contrast to the previous regressions, this result suggests that Vienna was already disproportionately large in the final decades of the Habsburg Empire. In fact, the break-up of Austria-Hungary does not affect the estimated extent of Vienna's overdimension as the reduction of Vienna's hinterland is matched by the accompanying rise in Vienna's share in total population. Third, while the estimated coefficient on the Vienna dummy initially declines in magnitude after Austro-Hungarian disintegration, supporting the earlier finding that there is, at best, only weak evidence for path dependence, the parameter rises again after 1960–70. Thus, although a disproportionately large share of the population is already concentrated in Austria's largest city, Vienna's dominance of the national urban structure in-

Table 5.4

The Impact of Austro-Hungarian Disintegration on the Relative Size of Vienna

Log of Share of Main City in Total Urban Population	Before Disintegration				After Disintegration						
	(1) 1870–80	(2) 1880–90	(3) 1890–1900	(4) 1900–10	(5) 1920–30	(6) 1930–40	(7) 1940–50	(8) 1950–60	(9) 1960–70	(10) 1970–80	(11) 1980–90
Constant	6.171*	4.334	6.961#	9.523*	10.688**	12.749**	12.339*	9.862**	10.450**	13.084*	14.443**
	(2.493)	(2.803)	(3.387)	(3.760)	(2.694)	(3.428)	(4.402)	(3.303)	(3.352)	(5.122)	(4.534)
Vienna Dummy	0.318	0.460	0.609**	0.717**	0.783**	0.792**	0.624**	0.606**	0.645**	0.803**	0.936**
	(0.226)	(0.338)	(0.178)	(0.237)	(0.100)	(0.077)	(0.117)	(0.130)	(0.145)	(0.143)	(0.114)
Capital City Dummy	0.823**	0.957**	0.977**	0.848**	0.600**	0.435*	0.488#	0.511#	0.508*	0.342	0.157
	(0.225)	(0.198)	(0.165)	(0.182)	(0.165)	(0.189)	(0.250)	(0.255)	(0.206)	(0.209)	(0.167)
Log of Total Population Outside the Main City	–0.445**	–0.466**	–0.340**	–0.371**	–0.496**	–0.640**	–0.714**	–0.689**	–0.649**	–0.568**	–0.488**
	(0.085)	(0.104)	(0.071)	(0.079)	(0.091)	(0.109)	(0.118)	(0.100)	(0.102)	(0.094)	(0.083)
Log of Real GDP per Capita	–0.325	0.019	–0.631	–0.875#	–0.608#	–0.501	–0.275	0.093	0.006	–0.310	–0.428
	(0.404)	(0.352)	(0.426)	(0.464)	(0.288)	(0.322)	(0.444)	(0.387)	(0.424)	(0.692)	(0.713)
Share of the Labor Force Outside of Agriculture	3.187**	2.239*	1.834*	1.613	0.625	0.181	–0.951	-2.238	-2.387	-3.307	–4.887
	(0.849)	(0.852)	(0.822)	(0.927)	(0.539)	(0.565)	(1.170)	(1.752)	(2.085)	(3.063)	(3.551)
Share of Trade in GDP	-1.738*	-1.688#	–0.332	–0.182	-1.037*	-1.504**	–0.926	–0.835	-1.190#	-1.133*	–0.715*
	(0.766)	(0.782)	(0.204)	(0.113)	(0.438)	(0.444)	(0.613)	(0.598)	(0.629)	(0.495)	(0.296)
Year Dummy	0.127	0.042	0.041	0.097	0.074	–0.275	0.185	0.197	0.134	0.212	0.097
	(0.153)	(0.126)	(0.180)	(0.150)	(0.158)	(0.181)	(0.187)	(0.152)	(0.163)	(0.133)	(0.119)
# of Observations	19	21	23	24	24	24	24	24	24	24	24
Adjusted R^2	0.801	0.797	0.699	0.726	0.834	0.818	0.756	0.790	0.824	0.849	0.850

Notes: White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

creases even further relative to other European metropolises, beginning in 1960. In 1980–90, then, the dominance is larger than at the declaration of the Austrian Republic. The estimated coefficient of 0.94 indicates that the share of Vienna in Austria's total population is, corrected for country characteristics, about 150 percent larger than in the rest of the sample, compared with 120 percent in 1920–30. For whatever reason the trend change finally has occured, this increase in primacy in an already overdimensioned city suggests that history may indeed matter for city growth.

In a third exercise, I use the ratio between the two largest cities in a country as a measure of urban concentration. In calculating this ratio, I focus exclusively on the second largest city on Austrian territory, Graz. Since I ignore Budapest, Prague, Trieste and Lemberg, then, the calculated value for Austria-Hungary is effectively too large so that I expect a significantly positive coefficient on the Vienna dummy before disintegration. With the declaration of the Republic of Austria, for which the ratio is properly calculated, this coefficient can be expected to fall in size, since smaller countries often have disproportionately large central cities.

Table 5.5 shows the results. As expected, the Vienna dummy enters positively and is statistically highly significant for Austria-Hungary, and the estimated coefficient is smaller in magnitude after the dissolution of the Habsburg Empire. I am mainly interested, however, in the course of the estimated coefficients for the period of the existence of the Austrian Republic. Here, I observe the by now familiar process of "normalization" in the early years. For the period from 1920 to 1970, the parameter decreases, indicating a gradual reduction in Austria's disproportionately large population concentration in the central city. Similar to earlier findings, however, this downward trend ceases and, after about half a century, reverses. After 1970, the estimated coefficients increase in magnitude, even though they do not reach again the level of the early years. Nonetheless, this rise in primacy supports earlier results, suggesting that lock-in effects are at work which defend the overdimensioned size of Vienna in a European context.

5.4.3 Does Vienna's Primacy Fall Over Time?

The last section estimates the degree of Vienna's overdimension for individual years (pooled to cover a decade) from 1870 to 1990. Comparing then the estimated coefficients over time gives some rough indication about the change in Vienna's primacy relative to other European agglomerations. In this section I shall explore the dynamic issue in more detail. In particular, I will estimate the baseline regressions in first differences. This specification basically offers two advantages. First, as now the *change* in the log of the urban concentration measure is the dependent variable, adding a

Table 5.5

The Impact of Austro–Hungarian Disintegration on the Urban Dominance of Vienna

Log of Ratio of Main City to Second Largest City	Before Disintegration				After Disintegration						
	(1) 1870–80	(2) 1880–90	(3) 1890–1900	(4) 1900–10	(5) 1920–30	(6) 1930–40	(7) 1940–50	(8) 1950–60	(9) 1960–70	(10) 1970–80	(11) 1980–90
Constant	–5.101	–3.247	–2.332	–2.176	–0.582	1.838	2.064	0.900	1.771	4.051	–0.609
	(5.688)	(3.693)	(2.495)	(2.991)	(4.246)	(4.145)	(4.857)	(4.435)	(4.060)	(4.219)	(5.040)
Vienna Dummy	1.074*	1.347*	1.905**	1.863**	1.540**	1.172**	0.969**	0.918**	0.870**	0.937**	0.978**
	(0.366)	(0.468)	(0.320)	(0.366)	(0.165)	(0.213)	(0.119)	(0.179)	(0.127)	(0.117)	(0.095)
Capital City Dummy	0.923**	1.210**	1.138**	0.866**	0.565#	0.467#	0.399	0.344	0.263	0.104	0.058
	(0.258)	(0.226)	(0.185)	(0.230)	(0.281)	(0.248)	(0.271)	(0.272)	(0.229)	(0.140)	(0.194)
Log of Total Population Outside the Main City	–0.134	–0.066	–0.011	–0.056	–0.159	–0.288#	–0.345#	–0.331*	–0.290*	–0.239*	–0.146
	(0.207)	(0.121)	(0.076)	(0.128)	(0.155)	(0.145)	(0.163)	(0.149)	(0.115)	(0.089)	(0.089)
Log of Real GDP per Capita	1.005	0.440	0.118	0.294	0.523	0.491	0.610	0.801#	0.620	0.239	0.908
	(1.072)	(0.573)	(0.419)	(0.396)	(0.376)	(0.327)	(0.376)	(0.398)	(0.410)	(0.486)	(0.677)
Share of the Labor Force Outside of Agriculture	1.825	3.252*	4.173**	2.331	–1.300#	–1.308	–1.681	–2.244	–2.131	–1.517	–5.241
	(1.865)	(1.502)	(1.318)	(1.609)	(0.705)	(0.757)	(1.096)	(1.571)	(1.601)	(2.323)	(3.359)
Share of Trade in GDP	–2.686#	–1.376**	–1.008**	–0.626**	0.124	–0.024	–0.105	–0.618	–0.605	–0.882#	–0.418
	(1.343)	(0.441)	(0.210)	(0.171)	(0.584)	(0.560)	(1.177)	(0.982)	(0.733)	(0.481)	(0.442)
Year Dummy	0.174	–0.086	–0.137	–0.016	–0.054	–0.040	0.049	–0.051	–0.136	0.050	–0.014
	(0.266)	(0.205)	(0.147)	(0.179)	(0.246)	(0.201)	(0.184)	(0.199)	(0.208)	(0.138)	(0.143)
# of Observations	22	23	24	24	24	24	24	24	24	24	24
Adjusted R^2	0.412	0.610	0.791	0.678	0.561	0.567	0.479	0.509	0.555	0.680	0.641

Notes: White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

Vienna dummy allows to examine whether the Austrian capital grows significantly less in primacy than the other European metropolises. Second, in the first-differenced version also regressors are changes in the respective variables so that fixed effects drop out of the regression. In case that the benchmark specification misses some important time-invariant regressors, this will no longer affect the results.

Table 5.6 reports the results for the change in the log of the absolute population of the largest city. Again, I pool data for two consecutive periods so that column (1) displays estimates which cover a 20-year-interval, imposing the same coefficients for changes from 1880 to 1890 (using the 1870–80 period for the construction of the explanatory variable) and from 1890 to 1900. The results are rather inconclusive. In fact, the adjusted R^2 varies between 0.04 and 0.75. Only the change in the log of city population in the preceding period turns out to have some predictive power for the change in the absolute city size. The estimated coefficient is positive, implying that there is persistence in city growth. Cities which grow above average in one period also tend to grow significantly faster in the subsequent period.

More important from my perspective, the estimated coefficient on the Vienna dummy is not statistically different from zero. Holding other effects constant, the change in Vienna's population is not statistically different from the experience of other large central cities in Europe at conventional levels of confidence. The most straightforward interpretation of this result appears to be that history matters for city growth. Despite its disproportionately large size after the end of the Habsburg Empire, Vienna does not grow significantly less than other European metropolises.

Table 5.7 repeats the first-differenced regressions for the change in the log of the largest city's share in total population. Again, there is wide variation in the explanatory power of the specification and, again, I find a strong pattern of persistence. Most notably, however, the estimated coefficient on the Vienna dummy in 1920–40 now turns out to be negative and statistically highly significant. Consistent with earlier findings for the change in levels, this result indicates that Vienna's dominance declines relative to other cities immediately after the break-up of the Austro-Hungarian Monarchy. In the subsequent decades, the coefficient first remains negative but is not statistically significant anymore and then even changes the sign. In fact, I am also able to replicate the above result of a relative *increase* in Vienna's primacy. Column (8) shows the estimates for the changes from 1960–70 and from 1970–80. The coefficient on the Vienna variable is positive and statistically different from zero at the 10 percent level, implying that, holding other effects constant, the share of Vienna in total Austrian population rises relative to that of other European metropolises in their respective countries.

DOI https://doi.org/10.3790/978-3-428-50499-2

Table 5.6

The Impact of Austro–Hungarian Disintegration on the Change in the Absolute Size of Vienna

Dependent Variable: Δ Log of Population in Main City	Before Disintegration			After Disintegration					
	(1) 1880–1900	(2) 1890–1910	(3) 1900–20	(4) 1920–40	(5) 1930–50	(6) 1940–60	(7) 1950–70	(8) 1960–80	(9) 1970–90
Constant	0.182*	0.117#	0.078*	0.072	0.054	0.058	–0.027	–0.036	–0.050
	(0.079)	(0.064)	(0.035)	(0.049)	(0.064)	(0.034)	(0.030)	(0.030)	(0.044)
Vienna Dummy	0.135	–0.102	0.036	–0.078	–0.094	–0.031	0.026	0.045	–0.015
	(0.216)	(0.105)	(0.033)	(0.085)	(0.076)	(0.049)	(0.082)	(0.043)	(0.049)
Δ Log of Population in Main City in Previous Period	0.291	0.448*	0.420	0.489	0.498#	0.766**	0.921**	0.630**	0.449
	(0.246)	(0.192)	(0.196)	(0.305)	(0.270)	(0.119)	(0.110)	(0.171)	(0.298)
Δ Log of Non–Urbanized Population	0.155	0.480#	0.122**	0.139	–0.362	–0.198	0.182	0.146	0.209
	(0.292)	(0.257)	(0.017)	(0.306)	(0.246)	(0.316)	(0.147)	(0.126)	(0.221)
Second Period Dummy	–0.045	–0.047	–0.021	–0.012	0.038	–0.072	–0.034	–0.029	0.140
	(0.067)	(0.056)	(0.038)	(0.048)	(0.048)	(0.047)	(0.035)	(0.035)	(0.094)
# of Observations	21	23	24	24	24	24	24	24	24
Adjusted R^2	0.036	0.198	0.365	0.210	0.213	0.459	0.751	0.485	0.039

Notes: All regressions are pooled OLS for changes against the previous decade over two consecutive periods. Column (1), for example, pools data for the changes from 1880 to 1890 and from 1890 to 1900. White heteroskedastic–consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

Table 5.7

The Impact of Austro-Hungarian Disintegration on the Change in the Relative Size of Vienna

Dependent Variable: Δ Log of Share of Main City in Total Urban Population	Before Disintegration			After Disintegration					
	(1) 1880–1900	(2) 1890–1910	(3) 1900–20	(4) 1920–40	(5) 1930–50	(6) 1940–60	(7) 1950–70	(8) 1960–80	(9) 1970–90
Constant	–0.057 (0.058)	–0.059* (0.027)	0.058# (0.028)	0.037 (0.055)	0.030 (0.040)	0.074 (0.064)	0.000 (0.046)	–0.093 (0.059)	0.133 (0.175)
Vienna Dummy	0.266 (0.160)	–0.055 (0.035)	–0.019 (0.034)	–0.144** (0.036)	–0.077 (0.050)	–0.044 (0.049)	0.024 (0.065)	0.068# (0.036)	–0.097 (0.093)
Δ Log of Share of Main City in Total Urban Population in Previous Period	–0.245# (0.133)	0.007 (0.134)	0.294 (0.253)	0.117* (0.041)	0.297 (0.179)	0.594** (0.199)	0.625* (0.253)	0.417* (0.199)	0.798# (0.407)
Δ Log of Total Population Outside the Main City	0.628 (0.649)	0.708 (0.659)	–0.601** (0.016)	–0.970# (0.510)	–0.825 (0.480)	–1.183# (0.603)	–0.572# (0.322)	–0.720# (0.411)	–3.068# (1.655)
Second Period Dummy	–0.025 (0.050)	0.015 (0.046)	–0.016 (0.033)	0.001 (0.044)	0.010 (0.044)	–0.020 (0.045)	–0.087 (0.051)	–0.004 (0.030)	0.064 (0.053)
# of Observations	19	21	23	24	24	24	24	24	24
Adjusted R^2	0.179	–0.138	0.946	0.054	0.026	0.234	0.374	0.241	0.412

Notes: All regressions are pooled OLS for changes against the previous decade over two consecutive periods. Column (1), for example, pools data for the changes from 1880 to 1890 and from 1890 to 1900. White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

Finally, table 5.8 shows the results for changes in the log of the ratio between a country's two largest cities. Not surprisingly, the fit of the regressions is generally weak with a maximum R^2 of 0.31 (column 6). Nonetheless, there is an interesting difference to previous results. As before, the estimated coefficient on the Vienna dummy is negative in the early years after the disintegration. But now the parameter turns out to be also statistically significant for the decades from 1930 to 1960 and, thus, for a longer time period than has been indicated by the other measures of urban concentration. A plausible explanation for this result is that Austria's second largest city, Graz, increasingly establishes itself as a regional center in that period and thereby lowers the ratio between the two cities. This effect, however, appears to fade after 1960. Similar to the results for other concentration measures, the estimated coefficient on the Vienna dummy in 1960–80 is actually positive (but far from significant).

To summarize, the results of the first-differenced regressions basically confirm the earlier findings. After the break-up of Austria-Hungary, it seems that there is a transition period in which Vienna's urban dominance declines. This period, however, ceases relatively quickly. In fact, from 1960 to 1980, Vienna may even have gained in primacy relative to other central cities in Europe.

5.5 Conclusion

This chapter explores the role of history for city growth. In particular, it is argued that the break-up of Austria-Hungary in 1918 provides a natural experiment to examine the existence of path dependence in city development. If history matters, the 85% reduction in country size and population should have had no sizeable effect on subsequent population growth in the largest city of the former Habsburg Empire, Vienna. If, in contrast, path dependence plays largely no role, Vienna is probably too large and this overdimension should be gradually reduced.

Using several measures of urban concentration, I find convincing evidence that Vienna's dominance of the national urban structure in Austria decreases in the first three to four decades following the dissolution of Austria-Hungary. A half century after the declaration of the Republic of Austria, however, this process reverses and Vienna's primacy increases again, despite its overdimension, relative to a sample of 11 other European countries. I interpret these results as evidence in favor of lock-in effects. In the long run, history matters.

DOI https://doi.org/10.3790/978-3-428-50499-2

Table 5.8

The Impact of Austro–Hungarian Disintegration on the Change in the Urban Dominance of Vienna

Dependent Variable: Δ Log of Ratio of Main City to Second Largest City	Before Disintegration			After Disintegration					
	(1) 1880–1900	(2) 1890–1910	(3) 1900–20	(4) 1920–40	(5) 1930–50	(6) 1940–60	(7) 1950–70	(8) 1960–80	(9) 1970–90
Constant	–0.076 (0.104)	0.015 (0.053)	–0.003 (0.025)	–0.020 (0.063)	0.048 (0.074)	0.218* (0.076)	–0.010 (0.049)	–0.049 (0.051)	0.063 (0.139)
Vienna Dummy	0.203 (0.250)	–0.039 (0.107)	0.094** (0.025)	–0.186 (0.224)	–0.329* (0.132)	–0.212** (0.064)	–0.010 (0.076)	0.059 (0.050)	–0.050 (0.073)
Δ Log of Ratio of Main City to Second Largest City in Previous Period	0.274 (0.250)	0.336# (0.167)	0.415** (0.122)	0.236 (0.229)	–0.147 (0.265)	–0.166 (0.169)	0.373 (0.355)	0.684** (0.110)	0.268 (0.216)
Δ Log of Total Population Outside the Main City	0.863 (0.951)	–0.276 (0.761)	0.127** (0.017)	0.094 (0.454)	–0.559 (0.857)	–1.746* (0.703)	–0.498 (0.566)	–0.353 (0.566)	–1.826 (2.002)
Second Period Dummy	–0.003 (0.084)	0.026 (0.053)	0.031 (0.038)	0.027 (0.063)	0.092# (0.052)	–0.073# (0.041)	–0.028 (0.069)	0.045 (0.059)	0.097 (0.060)
# of Observations	24	24	24	24	24	24	24	24	24
Adjusted R^2	0.067	0.127	0.301	0.021	0.271	0.314	–0.050	0.256	0.153

Notes: All regressions are pooled OLS for changes against the previous decade over two consecutive periods. Column (1), for example, pools data for the changes from 1880 to 1890 and from 1890 to 1900. White heteroskedastic-consistent standard errors are in parentheses. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

Appendix A

Comparing Zipf Exponents for Different Sample Sizes

Table A.1

Comparing Estimated Zipf Exponents for Different Sample Sizes

	Based on 20 observations taken from tables 2.1a–d	Based on 50 observations taken from Rosen and Resnick (1980)	Difference in %
Belgium	1.787	–	–
Denmark	1.157	1.374	–15.8
Finland	1.066	1.084	–1.7
Italy	1.167	1.046	11.6
Netherlands	1.100	1.266	–13.1
Norway	0.937	1.265	–25.9
Portugal	0.825	–	–
Spain	1.129	1.133	–0.4
Sweden	1.234	1.410	–12.5
Switzerland	0.956	1.095	–12.7

Notes: The table lists estimated Zipf exponents for 1970 for different sample sizes. Some of the observed differences could also be due to different sources for underlying city data. The estimate for Italy is based on 25 observations instead of 20 observations.

Appendix B

Examining the Robustness of Eaton and Eckstein's (1997) Results

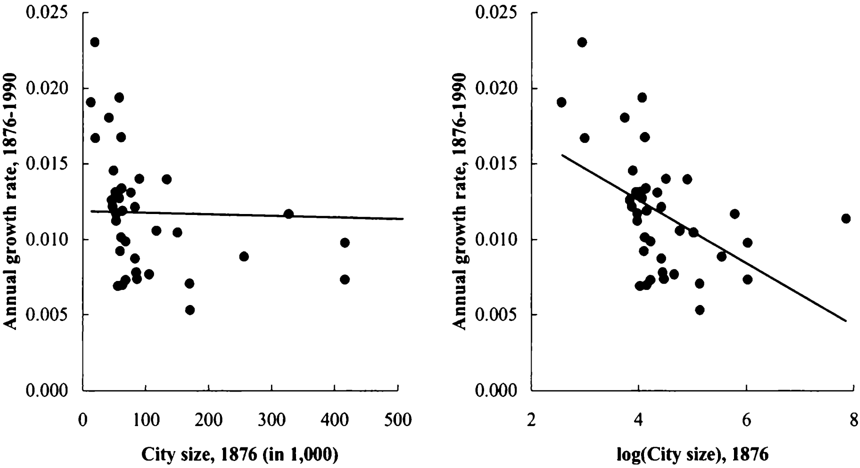
In an interesting and by now widely cited paper, Jonathan Eaton and Zvi Eckstein (1997) (hereafter, EE) argue that cities at all scales follow on average the same growth process. Analyzing the growth pattern of cities in France and Japan over a period of 114 and 60 years respectively, they find that there is no correlation between a city's initial level of population and its subsequent growth rate. As they are also able to show that the size distribution of cities remains remarkably stable over time, EE suggest that this is strong evidence for a parallel growth of cities.

This appendix examines the robustness of their results. Specifically, using EE's original city data, taken from the January 1994 working paper version of their paper (NBER Working paper #4612), I apply several parametric and nonparametric techniques to explore the relationship between initial city size and subsequent growth rates in more detail. In particular, the aim is to analyze whether EE's finding that there is no correlation for the full period possibly results from changes in the growth process over time.

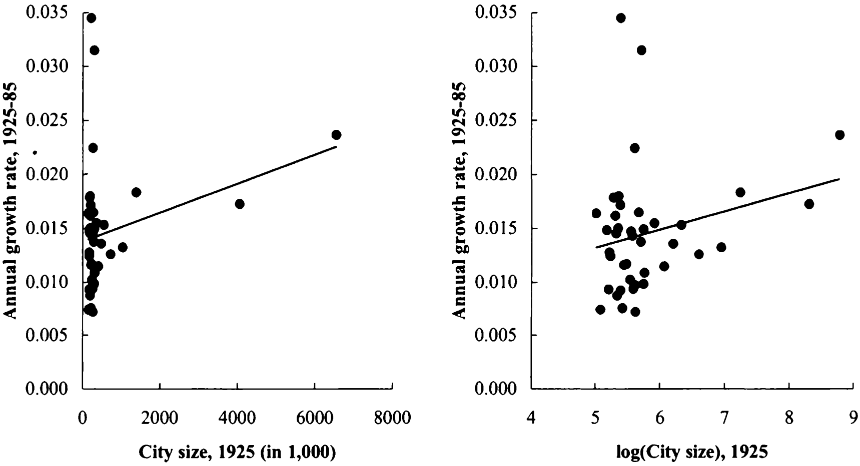
I begin, then, with a replication of EE's results. The graphs on the left hand side of figure B.1 present simple scatter plots of absolute city size in the base year and subsequent annual growth rates and, thereby, repeat EE's figures 3 and 4. Both figures nicely illustrate EE's key point. In France, there is obviously no correlation between size and growth, while the positive slope in Japan is mainly due to an outlier (Tokyo). Appendix table B.1 reports accompanying regression estimates which support this visual impression.

A first irritating observation is, however, that EE use exclusively the absolute size of a city as their explanatory variable. In their footnote 13, EE correctly note that their analysis is similar to procedures applied in the economic growth literature. It turns out, however, that – in contrast to the literature on per capita income growth across countries – EE's results are strongly driven by the countries' largest cities. In France, Paris is more than 6 times larger than the second largest city. In Japan, 35 cities (88% of the sample) cluster in a size group from 150 to 600, while the five largest cities are spread in a range which is larger by almost factor 10. Therefore,

a) France, 1876-1990



b) Japan, 1925-1955



Notes: Following Eaton and Eckstein (1997), the data point for the largest French city (Paris) is suppressed in the upper left graph.

Figure B.1: Alternative Ways to Explore the Relationship Between Growth Rates and Initial City Size

Table B.1

Evidence from Growth Regressions

		Full Sample			
Expl. Var. (1)	Period (2)	Coefficient (3)	Stand. Dev. (4)	R^2 (5)	# (6)
France					
Abs. Size	1876–1990	–1.04E–06	1.59E–06	0.01	39
Abs. Size	1876–1911	–7.46E–07	3.86E–06	0.00	39
Abs. Size	1911–1954	–3.79E–07	9.89E–07	0.00	39
Abs. Size	1954–1990	–6.42E–07	1.01E–06	0.01	39
Log Size	1876–1990	–0.0021**	0.0006	0.24	39
Log Size	1876–1911	–0.0042**	0.0015	0.17	39
Log Size	1911–1954	–0.0013	0.0008	0.07	39
Log Size	1954–1990	–0.0013	0.0011	0.03	39
Rank	1876–1990	2.08E–04**	4.55E–05	0.36	39
Rank	1876–1911	3.78E–04**	1.22E–04	0.20	39
Rank	1911–1954	1.44E–04*	5.62E–05	0.15	39
Rank	1954–1990	7.79E–05	8.11E–05	0.02	39
Japan					
Abs. Size	1925–1985	1.34E–06#	7.72E–07	0.07	40
Abs. Size	1925–1955	8.58E–07	6.41E–07	0.04	40
Abs. Size	1955–1985	1.04E–06#	5.75E–07	0.08	40
Log Size	1925–1985	0.0017	0.0011	0.06	40
Log Size	1925–1955	0.0009	0.0009	0.02	40
Log Size	1955–1985	0.0033*	0.0015	0.12	40
Rank	1925–1985	–7.30E–05	7.86E–05	0.02	40
Rank	1925–1955	–2.13E–05	6.49E–05	0.00	40
Rank	1955–1985	–2.60E–04*	1.02E–04	0.15	40

Table B.1 (Continued)

Evidence from Growth Regressions

		Excluding Largest City			
Expl. Var. (1)	Period (2)	Coefficient (7)	Stand. Dev. (8)	R^2 (9)	# (10)
France					
Abs. Size	1876–1990	−1.63E–05*	6.32E–06	0.16	38
Abs. Size	1876–1911	−2.52E–05	1.60E–05	0.06	38
Abs. Size	1911–1954	−7.89E–06	4.76E–06	0.07	38
Abs. Size	1954–1990	−6.29E–06	5.57E–06	0.03	38
Log Size	1876–1990	−0.0032**	0.0007	0.37	38
Log Size	1876–1911	−0.0069**	0.0018	0.29	38
Log Size	1911–1954	−0.0023*	0.0011	0.12	38
Log Size	1954–1990	−0.0018	0.0016	0.04	38
Rank	1876–1990	2.24E–04**	4.70E–05	0.39	38
Rank	1876–1911	4.15E–04**	1.27E–04	0.23	38
Rank	1911–1954	1.55E–04*	5.88E–05	0.16	38
Rank	1954–1990	7.37E–05	8.55E–05	0.02	38
Japan					
Abs. Size	1925–1985	9.24E–07	1.42E–06	0.01	39
Abs. Size	1925–1955	−1.98E–08	1.17E–06	0.00	39
Abs. Size	1955–1985	1.50E–06	1.27E–06	0.04	39
Log Size	1925–1985	0.0009	0.0014	0.01	39
Log Size	1925–1955	−0.0001	0.0012	0.00	39
Log Size	1955–1985	0.0032#	0.0019	0.07	39
Rank	1925–1985	−4.20E–05	8.06E–05	0.01	39
Rank	1925–1955	6.87E–06	6.61E–05	0.00	39
Rank	1955–1985	−2.37E–04*	1.06E–04	0.12	39

Upper Half				Lower Half			
Coefficient (11)	Stand. Dev. (12)	R^2 (13)	# (14)	Coefficient (15)	Stand. Dev. (16)	R^2 (17)	# (18)
6.96E–07	1.07E–06	0.02	20	–1.68E–04**	4.82E–05	0.42	19
2.07E–06	1.58E–06	0.09	20	–6.21E–04**	1.28E–04	0.58	19
1.74E–07	9.33E–07	0.00	20	–1.59E–04*	6.70E–05	0.25	19
–4.39E–07	1.12E–06	0.01	20	5.93E–05	8.30E–05	0.03	19
0.0002	0.0007	0.01	20	–0.0056**	0.0016	0.42	19
0.0010	0.0010	0.05	20	–0.0213**	0.0041	0.61	19
0.0001	0.0010	0.00	20	–0.0116*	0.0049	0.25	19
–0.0012	0.0016	0.03	20	0.0062	0.0085	0.03	19
–4.92E–06	1.01E–04	0.00	20	3.38E–04*	1.49E–04	0.23	19
–7.01E–05	1.52E–04	0.01	20	1.26E–03*	4.39E–04	0.33	19
–3.33E–05	1.51E–04	0.00	20	4.07E–04*	1.59E–04	0.28	19
2.38E–04	2.32E–04	0.05	20	–1.62E–04	2.29E–04	0.03	19
1.33E–06	7.90E–07	0.14	20	7.90E–06	4.89E–05	0.00	20
9.19E–07	6.36E–07	0.10	20	–2.12E–05	4.12E–05	0.01	20
6.79E–07	6.69E–07	0.05	20	–2.04E–05	3.65E–05	0.02	20
0.0019	0.0014	0.10	20	0.0021	0.0098	0.00	20
0.0013	0.0011	0.07	20	–0.0001	0.0083	0.01	20
0.0019	0.0020	0.04	20	–0.0001	0.0114	0.02	20
–1.93E–04	2.21E–04	0.04	20	–7.52E–05	2.34E–04	0.01	20
–1.42E–04	1.75E–04	0.04	20	5.72E–05	1.99E–04	0.00	20
–3.95E–04	3.28E–04	0.07	20	8.79E–05	2.45E–04	0.01	20

Notes: The regressions use OLS to estimate equations of the form $Avg.\ growth_{kij} = \alpha + \beta\ Initial\ condition_{ki} + \varepsilon_k$, where only the results for β are reported. Column (1) reports the the variable which describes the initial condition in the respective specification. **, * and # denote significant at the 1%, 5% and 10% level, respectively.

a focus on absolute city size may be inappropriate to explore the relationship between size and growth across cities.

There are basically two ways then to deal with this problem. First, I drop the largest city from the sample. The results, reported in columns (7) to (10) of table B.1, confirm the intuition about the distorting impact of the largest cities. For example, if Paris is excluded from the sample of French cities, the negative coefficient on initial city size becomes statistically significant at the 5% level, suggesting that there is a process of convergence. On the other hand, the statistical significance (at the 10% level) of the positive slope coefficient in Japan is solely due to the country's largest city and disappears as soon as Tokyo is excluded from the sample.

Second, I experiment with alternative explanatory variables. The graphs on the right hand side of figure B.1, for example, illustrate the size-growth relationship for the log of initial city size. While this modification has almost no impact on the Japanese sample, there is now a strong negative correlation between initial city size and later growth in France. The regression results in table B.1 show that this finding of convergence is robust for the exclusion of the largest city. Also using the rank of the city as explanatory variable (and, thus, holding the absolute difference between two neighboring city sizes constant) strongly suggests that in France smaller cities (i.e., cities with a higher rank number) tend to grow faster than initially larger cities. This result clearly questions EE's claim that French cities follow a parallel growth pattern.

In a next step, I analyze the relationship between initial population and city growth for shorter time periods. Specifically, I split the full period into sub-periods of about 30 years each. Two results are particularly noteworthy. First, the convergence pattern in France appears to be particularly strong for the period from 1876 to 1911. However, this finding is hardly surprising since EE note that their sample comprises the cities with a 1911 population of at least 50,000. Thus, the results may suffer from selection bias. The statistically significant results for the subsequent period from 1911 to 1954 suggest, however, that the selection procedure is not the sole driving force behind the convergence pattern. Second, the positive coefficient on the initial size of Japanese cities becomes statistically significant (at the 5% level) in the second sub-period. This implies that from 1955 to 1985 there has been some additional concentration in large cities, further questioning EE's hypothesis of parallel growth.

I also group cities by initial size, divide the sample into two halves and analyze the growth pattern for these two sub-samples separately. The results are particularly interesting for France, where the convergence process is exclusively driven by the lower half of the sample. Moreover, it should

DOI https://doi.org/10.3790/978-3-428-50499-2

Table B.2

Comparing Mean Growth Rates

		Mean growth rate		t-test
		Upper half	Lower half	
France	1876–1990	0.0096	0.0140	4.183**
France	1876–1911	0.0111	0.0174	2.156*
France	1911–1954	0.0053	0.0080	2.031*
France	1954–1990	0.0147	0.0164	0.917
Japan	1925–1985	0.0150	0.0140	0.577
Japan	1925–1955	0.0153	0.0150	0.191
Japan	1955–1985	0.0169	0.0109	2.509*

Notes: **, * and # denote significant at the 1%, 5% and 10% level, respectively.

be noted that in the upper half of the sample comprising the 20 largest cities, there is a change in the sign of the estimated coefficients from positive in the first period to negative in the final period. Even though the coefficients are in no case statistically different from zero, this result confirms similar findings for other countries in chapter 2.

If there is parallel growth of cities, I would also expect that the cities in the two sub-samples have on average the same annual growth rate. Appendix table B.2 compares the mean growth rates for both halves of the sample. The results support the regression estimates. In France, small cities tend to grow significantly faster than large cities, while in Japan there is additional concentration in large cities, with the upper half of the sample displaying a significantly larger growth rate than the lower half from 1955 to 1985.

Finally, random growth of cities across different sizes requires that the growth rates of cities are not positively correlated over time. If a city grows faster than the sample average in one period, it should not display above average growth in the subsequent period. Appendix table B.3 reports simple correlation coefficients. In contrast to EE's hypothesis of parallel growth, however, it turns out that the growth rates of cities are positively correlated over time. The other reported correlations basically confirm earlier findings of a negative (positive) correlation between starting level and subsequent growth in France (Japan) and strong persistence within the city size distribution.

The results can be summarized as follows. There is no evidence that cities grow independently of initial size. It rather appears that EE's result

Table B.3

Correlation Coefficients

		Pearson Correlation	Shearman Rank Correlation
France	Grwth1876–1911,Grwth1911–54	0.242	0.039
France	Grwth1911–1954,Grwth1954–90	0.380*	0.420**
France	Grwth1876–1911,Grwth1954–90	–0.287	–0.182
Japan	Grwth1925–55,Grwth1955–85	0.618**	0.403**
France	Size1876,Grwth1876–1990	–0.107	–0.566**
France	Size1876,Grwth1876–1911	–0.032	–0.283
France	Size1911,Grwth1911–1954	–0.063	–0.366*
France	Size1954,Grwth1954–1990	–0.103	–0.129
Japan	Size1925,Grwth1925–1985	0.272	0.169
Japan	Size1925,Grwth1925–1955	0.212	0.041
Japan	Size1955,Grwth1955–1985	0.281	0.330*
France	Size1876,Size1990	0.995**	0.667**
France	Size1876,Size1911	0.998**	0.863**
France	Size1911,Size1954	0.999**	0.888**
France	Size1954,Size1990	0.999**	0.914**
Japan	Size1925,Size1985	0.984**	0.786**
Japan	Size1925,Size1955	0.994**	0.920**
Japan	Size1955,Size1985	0.997**	0.920**

Notes: **, * and # denote significant at the 1%, 5% and 10% level, respectively.

of parallel city growth across different sizes crucially hinges on an inappropriate procedure to analyze the data. An application of several alternative techniques unequivocally suggests that there is convergence across French cities and, in slightly weaker form, divergence in Japan. Unfortunately, there is also no convincing support for the results in chapter 2 which suggest that there has been a change in the growth pattern over time from divergence to convergence. Possible explanations for this deviation range from EE's decision to leave their sample unchanged over time to the availability of more comprehensive agglomeration data for France and Japan.

Appendix C

Replicating Krugman and Livas Elizondo (1996)

To allow total comparability, the numbered equations refer to Krugman and Livas Elizondo's original paper.

Assumptions:

$L = 1$
$\sigma = 4$
$\tau = 1.4$
$\gamma = 0.2$
$Z_0 = 10$

Hypothesis:

For $\rho = 1.83$ and $L_1 = 1$ $(L_2 = 0)$ the real wage differential $(\omega_1 - \omega_2)$ is a positive number (see their Figure 1).

Calculation:

First, I calculate the net labor input of a location using equation (3):

$Z_1 = 0.9 \qquad Z_2 = 0$

which is, according to equation (11), also the number of products produced at this location.

Using (12) and (16), I get some input for the calculation of the price indices:

$\lambda_0 = 0.91743 \qquad \lambda_1 = 0.08257 \qquad \lambda_2 = 0 \qquad K = 0.45102$

As the model cannot be solved analytically, I have to use numerical simulations to solve the equations for the wage rates ([27] and [28]) simultaneously. Assume, then, that we have

$w_1 = 1.07687$ and $w_2 = 1.02011$.

Using (4), I can calculate the income at a location:

$Y_0 = 10 \qquad Y_1 = 0.96918 \qquad Y_2 = 0$

Now I am able to solve for the price indices from equations (13)–(15):

$T_0 = 0.45352 \qquad T_1 = 0.75190 \qquad T_2 = 0.80819$

Finally, I can calculate the real wage rates using (17):

$\omega_1 = 1.14575 \qquad \omega_2 = 1.26222$

Contrary to Krugman and Livas Elizondo's claim, then, the real wage differential is negative:

$\omega_1 - \omega_2 = -0.11647$

As a cross-check I use my results to solve equations (27) and (28). As expected I get my assumed wage rates:

$w_1 = 1.07687 \qquad w_2 = 1.02011.$

Appendix D

Description of the Data

City Population

Data on city population are compiled from national statistical yearbooks. In some cases, the data refer to the nearest census date available. Appendix table D.1 lists the country's largest cities.

GDP

Data on GDP (measured in million 1990 Geary-Khamis Dollars) are taken from Angus Maddison *Monitoring the World Economy 1820–1992* (Paris: OECD) Table C-16. In case of missing data, figures are derived by interpolation (Portugal 1880, 1910, 1920; Spain 1880; Switzerland 1880, 1890). Data for Portugal in 1930 and 1940 are from 1929 and 1938, respectively. Data for Austria before 1920 and for Germany before 1950 are calculated by multiplying GDP per capita taken from Angus Maddison *Monitoring the World Economy 1820–1992* (Paris: OECD) Table D-1 with population taken from Brian R. Mitchell *International Historical Statistics Europe 1750–1988* (New York: Stockton Press) Table A1.

Population

Data on population are taken from Angus Maddison *Monitoring the World Economy 1820–1992* (Paris: OECD) Table A-3. Missing data for Spain (1880) and Portugal (1880) are derived by interpolation. Data for Austria before 1920 and for Germany before 1950 are taken from Brian R. Mitchell *International Historical Statistics Europe 1750–1988* (New York: Stockton Press) Table A1.

Area

Area data is taken from Arthur S. Banks *Cross-Polity Time-Series Data* (Cambridge: MIT Press) Segment 1 and is converted to square kilometers (by multiplying the original data in square miles with 2.59). The data generally refer to the current territory, except for Austria and Germany.

Table D.1

Main Cities

	Austria	Belgium	Denmark	Finland	France	Germany	Italy	Nether-lands	Norway	Portugal	Spain	Sweden	Switzer-land
1870	*Vienna*	*Brussels*	*Copenhagen*	*Helsinki*	*Paris*	*Berlin*	Naples	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Geneva
1880	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	*Berlin*	Naples	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich
1890	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	*Berlin*	Naples	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich
1900	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	*Berlin*	Naples	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich
1910	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	*Berlin*	Naples	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich
1920	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	*Berlin*	Naples	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich
1930	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	*Berlin*	Milan	*Amsterdam*	*Oslo*	*Lisbon*	Barcelona	*Stockholm*	Zurich
1940	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	*Berlin*	*Rome*	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich
1950	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	Hamburg	*Rome*	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich
1960	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	Hamburg	*Rome*	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich
1970	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	Hamburg	*Rome*	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich
1980	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	Hamburg	*Rome*	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich
1990	*Vienna*	Antwerp	*Copenhagen*	*Helsinki*	*Paris*	Hamburg	*Rome*	*Amsterdam*	*Oslo*	*Lisbon*	*Madrid*	*Stockholm*	Zurich

Notes: The table lists the countries' largest city which population data were used as dependent variable in the regressions. Italic cities are also capitals.

Trade

Data on the share of trade in GDP are calculated from Brian R. Mitchell *International Historical Statistics Europe 1750–1988* (New York: Stockton Press) Tables E1 and J1. In some cases, the data refer to the nearest census date available. Missing values are calculated from Angus Maddison *Monitoring the World Economy 1820–1992* (Paris: OECD) and Arthur S. Banks *Cross-Polity Time-Series Data* (Cambridge: MIT Press) Segment 5. Historical data for Portugal (1870–1940) is derived from Ana Bela Nunes, Eugenia Mata and Nuno Valerio "Portuguese Economic Growth 1833–1985" *Journal of European Economic History* 18 (Fall 1989): 291–330. Appendix E gives a detailed description of the construction of the trade-to-GDP data set.

Labor Force Outside of Agriculture

Data on the share of the labor force outside of agriculture, forestry and fishing are calculated from Brian R. Mitchell *International Historical Statistics Europe 1750–1988* (New York: Stockton Press) Table B1. In some cases, the data refer to the nearest census date available. Missing data are derived by interpolation (Belgium 1940, Finland 1890, Germany 1900, Italy 1890, Netherlands 1880, 1940, Norway 1880, Portugal 1920, Spain 1930, Switzerland 1880). Data for Germany 1870 is taken from Angus Maddison *Monitoring the World Economy 1820–1992* (Paris:OECD) Table 2-5 and data for Finland 1870, Norway 1870 and Switzerland 1870 are estimated. Data for 1990 are taken directly from Brian R. Mitchell's original source for data from 1968 to 1981, the International Labour Organization's *Yearbook of Labour Statistics* (Geneva: ILO).

Urbanized Population

Data on the share of urban population are available for the time period from 1950–1990 from the United Nations' *Demographic Indicators of Countries* (New York: UN). Historical data compatible with UN estimates are derived from Peter Flora (with Franz Kraus and Winfried Pfenning) *States, Economy, and Society in Western Europe 1815–1975* (Frankfurt: Campus) volume 2, chapter 3 (except for Portugal and Spain) and from Pedro Pereira and Maria Mata (eds.) *Urban Dominance and Labour Market Differentiation of a European Capital City: Lisbon 1890–1990* (London: Kluwer) for Portugal. Specifically, comparable data is calculated by using the reported share of population in localities with less than 5,000 inhabitants (for Denmark and Norway less than 2,000; for Italy less than 10,000). In the time period with overlapping data (usually 1950–70), the R^2 between both measures is higher than 0.99 for Austria, Finland, France, Germany, Italy, Norway, Sweden and Portugal, about 0.9 for Belgium and Switzerland, and only for the Netherlands at 0.35. For Spain, I use the share of population in province capitals in total population as a proxy for urbanization. From 1950–70, there is a very strong correlation between this measure and the urbanization rate reported by the UN (with an R^2 of 0.99).

Given the share of urban population, I can then easily calculate the figures for urbanized population outside the main city and for nonurbanized population.

Ratio of Import Duties to Imports

The ratio is calculated by dividing import duties compiled from various issues of the International Monetary Fund's *Government Finance Statistics Yearbook* (Washington: IMF) Table A line 6.1 by total imports taken from the IMF's *International Finance Statistics Yearbook* (Washington: IMF) line 71.

Tariff Rate

Data on the tariff rate is taken from Jong-Wha Lee "International Trade, Distortions, and Long-Run Economic Growth," IMF Working Paper 92/90, who provides the actual average tariff rate on imported inputs, intermediate, and capital goods in or around 1980.

Passenger Cars

Data on the usage of passenger cars are calculated by dividing the number of motor vehicles (private cars) in use taken from Brian R. Mitchell *International Historical Statistics Europe 1750–1988* (New York: Stockton Press) Table F6 by total population. Due to missing car data, the number of cars refers in some cases to the nearest census date available.

Railway Density

Data on railway density are calculated by dividing the length of railway lines open (in kilometers) taken from Brian R. Mitchell *International Historical Statistics Europe 1750–1988* (New York: Stockton Press) Table F1 by land area. Missing data for Austria 1940 is derived by interpolation.

Dictatorship

Data on the presence of dictatorships are based on Frances Nicholson (ed.) *Political and Economic Encyclopaedia of Western Europe* (Harlow: Longman).

Cabinet Changes

Cabinet changes refers to the number of times in the preceding decade that a new premier is named and/or 50% of the cabinet posts are occupied by new ministers. The series is compiled from yearly data taken from Arthur S. Banks *Cross-Polity Time-Series Data* (Cambridge: MIT Press) Segment 1. Missing values are replaced by the decade average calculated from the remaining years for which data is available.

Changes in Effective Executive

Changes in effective executive refers to the number of times in the preceding decade that effective control of the executive power changes hands. The series is compiled from yearly data taken from Arthur S. Banks *Cross-Polity Time-Series Data* (Cambridge: MIT Press) Segment 1. Missing values are replaced by the decade average calculated from the remaining years for which data is available.

Appendix E

Construction of Openness Measure

As a complete historical data series on the openness of countries is not available, data on the share of trade in GDP is constructed in a three-step procedure, combining information from a number of different data sources.

First, the basic data is taken from Brian R. Mitchell *International Historical Statistics Europe 1750–1988* (New York: Stockton Press) which provides data on exports and imports (Table E1) and GDP (Table J1), all in current prices and national currency. The openness ratio can then be easily calculated by dividing the sum of exports and imports by GDP.

While the trade data is almost complete (with the exception of Switzerland in 1870 and 1880), however, no GDP values are available for most countries before 1900. Specifically, data on GDP are missing for the Neth-

Table E.1

Comparing Openness Ratios from Maddison and Mitchell

Country	# of overlapping observations	1913 ignored	R^2	1988 adj. R^2
Austria	4		0.82	0.90
Belgium	5	Yes	0.85	0.87
Denmark	6	Yes	0.63	0.80
Finland	6	Yes	0.67	0.83
France	6		0.68	0.79
Germany	6		0.89	0.91
Italy	6		0.84	0.92
Netherlands	5	Yes	1.00	0.96
Norway	6		0.70	0.87
Portugal	3		1.00	1.00
Spain	5		0.67	0.81
Sweden	6		0.91	0.88
Switzerland	5	Yes	0.95	0.86

erlands and Spain before 1890, for Austria, Belgium and Switzerland before 1900, and for Portugal before 1940. In sum, a full time series on openness can only be calculated from Mitchell for Denmark, Finland, France, Germany, Italy, Norway, and Sweden.

Therefore, in a second step, I use Angus Maddison *Monitoring the World Economy 1820–1992* (Paris: OECD) to get reasonable GDP estimates (in national currency and current prices) for 1870. In particular, Maddison (Tables 2-4 and I-2) reports exports-to-GDP ratios for the six specific years 1870, 1913, 1929, 1950, 1973, and 1992. Comparing, then, Maddison's and Mitchell's data sets country by country yields consistently high correlations (cross checking revealed unrealistically high and apparently distorted values in Mitchell for some small countries in 1913 which therefore have been ignored). In simple regressions of the exports-to-GDP ratios taken from Maddison on exports-to-GDP calculated from Mitchell, the adjusted R^2 varies between 0.63 and 1.00. Moreover, correcting for the fact that the last observation in Mitchell refers to 1988 while Maddison's data refer to 1992, further improves the fit. Multiplying Mitchell's 1988 exports-to-GDP ratio by factor 1.15 raises the lowest R^2 in the country-specific regressions to 0.79 (for France, for which we have complete data from Mitchell anyway).

Using the results of the basic regressions, Maddison's 1870 exports-to-GDP ratio is re-scaled to fit Mitchell's data. Given this exports-to-GDP ratio and absolute exports values in Mitchell, comparable estimates for GDP (in national currency and current prices) are calculated. Finally, repeating step #1, 1870 openness measures are constructed from Mitchell's total trade volumes.

Third, starting from the calculated GDP values for 1870 in national currencies, missing GDP values between 1870 and 1900 or 1910 are approximated using information on the change in real GDP taken from Maddison (and re-scaled so that the data fit the first year for which data is available from Mitchell).

The complete data set on openness is given in Table E.2.

DOI https://doi.org/10.3790/978-3-428-50499-2

Table E.2

Share of Trade in GDP

	Austria	Belgium	Denmark	Finland	France	Germany	Italy	Nether-lands	Norway	Portugal	Spain	Sweden	Switzer-land
1870	0.32	0.60	0.36	0.32	0.24	0.33	0.18	0.69	0.34	0.11	0.13	0.32	0.53
1880	0.43	0.86	0.46	0.53	0.33	0.34	0.20	0.98	0.36	0.11	0.19	0.39	0.59
1890	0.39	0.73	0.48	0.39	0.28	0.32	0.19	1.50	0.44	0.10	0.20	0.47	0.66
1900	0.42	0.79	0.53	0.48	0.27	0.32	0.23	2.04	0.43	0.11	0.23	0.41	0.64
1910	0.61	1.35	0.55	0.51	0.33	0.36	0.30	2.45	0.48	0.11	0.20	0.35	0.83
1920	0.59	1.01	0.61	0.48	0.44	0.32	0.41	0.81	0.57	0.35	0.22	0.45	0.56
1930	0.39	0.86	0.56	0.44	0.29	0.31	0.24	0.67	0.40	0.21	0.23	0.33	0.43
1940	0.27	0.64	0.34	0.28	0.19	0.11	0.13	0.32	0.35	0.20	0.08	0.24	0.33
1950	0.30	0.51	0.49	0.31	0.21	0.20	0.20	0.68	0.46	0.33	0.05	0.37	0.42
1960	0.40	0.68	0.56	0.41	0.22	0.30	0.24	0.77	0.51	0.35	0.13	0.39	0.48
1970	0.44	0.89	0.52	0.46	0.26	0.35	0.31	0.79	0.55	0.41	0.19	0.42	0.55
1980	0.54	1.14	0.66	0.58	0.37	0.47	0.39	0.89	0.62	0.56	0.26	0.52	0.65
1990	0.53	1.21	0.59	0.41	0.36	0.48	0.32	0.89	0.51	0.64	0.29	0.53	0.58

Notes: Appendix 4.2 gives a detailed description of the data sources used in constructing the share of trade in GDP.

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DOI https://doi.org/10.3790/978-3-428-50499-2