Quantitative Studies on European Economic Integration Between 1880 and 1939

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Working papers

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Heinemeyer, Hans C. (2006), "The Impact of new Borders on Trade: World War I and the Economic Disintegration of Central Europe", *Discussion Papers, No. 2006/14*, Freie Universität Berlin.

Ich danke ich meiner Familie und meinen Freunden für ihre Unterstützung während der gesamten Arbeit und für ihre Nachsicht gegenüber meiner mangelnden Aufmerksamkeit und geringen Zeit für privaten Kontakt.

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Preface

I gratefully acknowledge financial support from the Fritz Thyssen Foundation (project grant "The trade network of Central Europe, 1850–1939"). I would like to thank my supervisors Niko Wolf and Irwin Collier for discussions and helpful feedback as well as Nermeen Azzam, Helge Berger, Piotr Franaszek, Volker Nitsch, Albrecht Ritschl, Moritz Schularick, Ariel D. Stern, Andreas Thams, Martin Uebele, seminar participants at the 2nd BETA-Workshop in Historical Economics (Strasbourg), MAREM (Berlin), at the 2007 workshops of the Marie Curie research training network (Rome), Conference in Historical Economics (Lund), and at Humboldt Universität and Freie Universität (Berlin).

CHAPTER 1

Introduction

The period from 1880 to 1939 encompasses an enormous variety of political and economic developments within only 60 years. Europe, which was at the top of its power during the first 30 years of this period, provides unique circumstances to analyze political shocks and economic dynamics: for cross-sectional comparison it is ideal that economic, political, and social changes took place simultaneously in different places; for comparison in the time domain it is ideal that progress took place at a different pace within nations or regions. There are probably few world regions which have seen the advent of so many countries in such a short period of time as Europe did after 1918. The same holds for fundamental changes in government: Germany, for instance, was a monarchy in 1880, became a democracy in 1919, and came under authoritarian rule in 1933. Society changed along with extraordinary social dynamics, which can be illustrated by the example of the German capital Berlin: its number of inhabitants grew from about 400.000 in 1850 to 1.000.000 in 1880 and reached 4 million by 1920.[1] Berlin in 1910 or 1920 was more similar to Berlin in 2000 than Berlin in 1850: hundreds of trains arrived and left the city per day, thousands of automobiles filled the streets, while artists and intellectuals filled the cafés, and modern electronics and chemical industries were developing.

For these reasons, the time and the area provide the variation, which is necessary to study cause and effect of political and economic integration at different stages: the rise of nationalism and nation building, unification of Italy and of Germany versus the breaking up of Austria-Hungary, Germany, and tsarist Russia, the spreading of modern capitalism and of socialism as well as of democracy and of autocracy, economic Golden Ages (1890–1913 and the late 1920s) and also Great Depressions (in the 1880s and 1930s), hyperinflation (in the 1920s) and hyper-*de*flation (in the 1930s), free trade (in the 1880s) versus autarky (in the 1930s), the introduction of international currency standards and their dissolution, or the First World War[2], to name only some important examples.

In economic history there is agreement that two antagonistic trends predominated integration during that period: growing economic integration at the beginning of the period, followed by extraordinary disintegration after the outbreak of the First World War. Analyzing world trade from 1870 to 1939, Estevadeordal et al. (2003, p.359) conclude that "[measured] by the ratio of trade to output, the period 1870–1913 marked the birth of the first era of trade globalization and the period 1914–1939 its death." In the introduction to *The Disintegration of the World Economy Between the World Wars* (1996), Mark Thomas, the editor, summarizes the state of research and draws a gloomy picture of integration during the interwar period:

> "Far from the 19th-century imagery of growth, integration, and international cooperation, the defining characteristics of the interwar

[1] However, part of this growth was due to the incorporation of small, surrounding communities in 1920 (cf. Statistisches Reichsamt 1927).

[2] Throughout this thesis, I refer to the period before World War I (WWI) as the *prewar* period, whereas *postwar* period denotes the time after World War II (WWII), and *interwar* period denotes the time between the world wars.

> years were crisis and confrontation. [...] The issue is the impact of the First World War on the global economy. The war destroyed an international economy that had evolved painstakingly over centuries [...] The world economy as it stood in the summer of 1914 was sophisticated, complex and accidental. Fifty months later, the international economy stood in ruins." (pp. *xi-xii*)

'Economic integration' is not a clearly defined term, however. Its use in economic research is often linked with political projects, such as German and European unification processes (e.g. Collier and Siebert 1991, Alesina, Spolaore, and Wacziarg 2000, Artis, Krolzig, and Toro 2004 to name just a few examples). In the political sphere, integration has in a way become a blurry buzzword of politicians in the European Union (EU). Its appeal might stem from the fact that integration is commonly regarded as a task for economic policy, an idea reflected in the EU's 1992 Single European Market Programme. My thesis refers to economic integration solely in the sense of relatively stronger economic links, be it disproportionately high exchange of goods —reflecting a sophisticated division of labor— or stronger dependency of economies on one another at the macro-level —on which I infer from the analysis of macro-economic variables such as industrial production and nominal wages.

The evolution and rise of modern statistics in Europe and elsewhere between 1880 and 1950 looms large in the following studies. To both the historian and the economist statistical data are as crucial as the political and economic developments themselves: They allow you to glance a long way back into history and to observe, compare, analyze, and eventually understand the mechanisms at work. The historical perspective shows that the evolution of statistics itself is subject to political and economic interests, whereas nowadays statistics are often regarded as objective information provided by organizations without self-interest, i.e. in a way as exogenous. Examples of the endogeneity of statistics include the focus of modern statistics on the nation state and the corresponding efforts of national movements in Central Europe at the turn of the 20th century to establish 'national' statistics at the sub-state level, e.g. regional 'foreign' trade statistics for Polish Galicia (then being part of Austria-Hungary) or the Kingdom of Poland (then being part of Russia). These were intended to demonstrate that the new nation states, which activists were struggling for, would be economically sensible and viable.

My thesis analyzes various types of economic integration over about half a century. It consists of four parts. Chapter 2 strives to separate the 'treatment effect' of national border demarcation. Earlier research into prices and trade has presented robust evidence that political borders divert trade beyond explicit border costs. Price data show that political borders correlate with deviations from the law of one price. Trade data indicate lower than expected exchange of goods across borders. Although research has made progress since Obstfeld and Rogoff (2000) have declared border effects one of the six major puzzles in international economics, they are not well understood and their actual extent remains unclear. To this end, I determine the actual 'treatment effect' of borders by considering the new German-Polish border in 1919/1921 as natural experiment. In the spirit of Alesina et al.

(2000) such treatment effects of borders can be interpreted as the economic costs of new demarcation incurred by new nation states. Put differently, the higher treatment effects are, the more economically costly is political disintegration.

Chapter 3 applies the approach elaborated in chapter 2, to trade integration in all of Central Europe after the peace treaty of Versailles in 1919 in order to evaluate the economic impact of border drawing. Appendix A introduces a new data set of regional trade in Europe between 1885-1933, which is analyzed in chapter 2 and chapter 3. War, the sowing the seeds of hatred, the massive destruction of infrastructure, and the foundation of many new nation states in Central Europe did certainly not foster trade integration. Moreover, trade conflicts disturbed the European and later the whole Atlantic economy, while the USSR pursued a strategy to become economically autark from the rest of the world after the New Economic Policy implemented in the early 1920s. Because chapter 2 indicates that the treatment effect of new political borders depends on the extent of prewar integration, chapter 3 examines whether economic costs of the peace were perhaps lower than is suggested in the initial statements of Estevadeordal et al. (2003) and Thomas (1996).

Chapter 4 evaluates synchronicity of business cycles during the interwar period and hence another indicator of economic integration. It catches one's eye that for a long time US researchers have analyzed the Great Depression considering the US economy only. It has taken some time for US centric research to admit that the Great Depression can be understood much better as a global phenomenon than by regarding only the US. A sign of this conceptual change is that the term "Great Depression" has become a synonym for the global economic crisis during the interwar years. Although turning their attention to international dependencies, many US researchers still continue to suspect the origin of the recession in the US. Among European economists, by contrast, the discussion has grown to suspect the origin of the recession in the periphery as well. My thesis argues that of the manifold identification issues (e.g. Tobin 1965), the questions where and when the crisis started have been neglected. Despite a certain consensus that comparative analysis is key in getting a grip on the "Holy Grail of macroeconomics" (Bernanke 1995), neither a comparative dating of national business cycles nor a dating of the global business cycle exist to date.[3] Burns and Mitchell's (1946, p.8) general criticism of business cycle research applies to the (peculiar) business cycle of the Great Depression in particular, namely that "[numerous] writers have invented plausible explanations [...] without knowing definitely the actual behavior for which their explanations should account [...]". The absence of such a business cycle dating is partly due to the history of economics and statistics. Among others, the concept of gross domestic product (GDP) as an overall measure of economic activity was not applied before 1940. Hence, GDP figures for the interwar period have to be constructed under often problematic assumptions. Second, the interwar period is short, and GDP is measured quarterly, or even annually.[4] To this end, chapter 4

[3]Existing dating exercises are hardly comparable and the dating of turning points is often guided by the interest in the economic history of one particular country.

[4]Similarly, among the 47 studies in *The Disintegration of the World Economy Between the World Wars*, collected by Thomas (1996) for their importance for the subject, there is only one

proposes a comparative dating of national business cycles and of the global business cycle during the interwar period. It investigates the course of business cycles in 12 countries by applying three standard techniques of business cycle research. As such, my findings can be used as stylized facts e.g. in theory building to determine whether an economy was in phase with the global cycle or if it followed a special path that requires separate analysis and explanation.

Chapter 5 addresses the hypothesis that nominal wage rigidity represents the link between deflation and depression during the 1930s. Rigidity in nominal wages is commonly, but often implicitly, seen as the main transmission channel for money to affect output during the depression. The combination of demand side shocks and supply side rigidities at national level resulted in a global depression, since economies were tied together through the interwar Gold Standard. The role of sticky wages is of immediate policy interest even today. The impact on policy of understanding the mechanisms at work cannot be underestimated as the Federal Reserve's chairman Ben Bernanke has justified its crisis management multiple times with insights from the historical experience of the Great Depression (e.g. Bernanke 2010). However, the degree of country-specific wage rigidity during the interwar period is unknown, despite it being a prerequisite to understanding the impact of money on output during both recession and recovery. Other things being equal, economies with a low degree of wage rigidity should have been less depressed during the crisis than countries with a high degree of rigidity. What is less obvious is that they, for the same reasons, should have also recovered more slowly. Such an analysis has been impossible for existing studies, most notably Bernanke and Carey (1996), because of their reliance on low frequency data. The study uses a newly compiled set of international wage data to (a) measure how large wage stickiness was during the 1920s and 1930s and (b) evaluate if the common assumption is justified that it was similar across countries.[5]

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single study, which uses data with a higher than annual frequency: Fisher's (1933) examination of the crisis in the US. Further examples of studies which are based on annual data include standard publications such as Svennilson (1954), Temin (1986), and Eichengreen (1992).

[5] As a side-effect of the interwar gold standard, monetary policy can be excluded as the major source of variation through enforcing similar monetary policies.

Computations and graphs in this thesis were obtained using *GNU R*, v2.4.0 to v2.14.1. Exceptions are indicated.

CHAPTER 2

Do Political Borders Actually Affect Trade? Some Evidence on the Treatment Effect of Borders

A large body of research has documented huge 'border effects', following McCallum's (1995) finding that the US-Canadian border has a negative effect on trade, which cannot be explained by tariff and non-tariff barriers. My study investigates whether the effect is due to self selection: Is it actually political demarcation that diverts trade or do borders follow lines of economic fragmentation, across which trade integration would be lower even without a border in place?

In order to identify the 'treatment effect' of national borders on trade, I estimate the impact of changes in national border demarcation after World War I on economic integration. I treat the emergence of the new Polish nation state and the separation of several provinces from the German Empire as a natural experiment. The set-up allows to control selection bias when estimating borders effects. The main results are, first, that systematic deviations of the observations under 'border treatment' are found. Pairs of regions that became separated by a new national border after WWI, already tended to have below average levels of economic integration before the war. A comparison of actual and biased results for goods-specific trade yields a difference of one tenth to two thirds in the advalorem tariff equivalent, and might well be higher for certain goods. Second, there remains a 'border treatment' effect, which cannot be explained by tariff or non-tariff barriers. Thirdly, trade after WWI was less diverted by new borders than by old borders. Put differently, the treatment effect of borders is smaller than the common border effect, which contains a 'fixed effect'. This 'border before the border' explains a substantial part of the conventional 'border effect'.

1. Introduction: Do political borders affect trade at all?

The question, whether political borders actually affect trade, sounds trivial: McCallum's (1995) study of the impact on trade of the national border between Canada and the US has not only coined the term 'border effect', but even triggered an entire strand of research, which is often referred to as 'border literature'. This literature has detected strong trade diverting effects of many historical and current political borders worldwide. A comprehensive literature review is provided in Anderson and van Wincoop (2004).

The border effect is usually defined as the average deviation of trade across borders from trade within borders. The effect is identified from cross-sectional variation taking into account further trade frictions, which coincide with political barriers. It is therefore common to estimate border effects controlling for many additional variables, e.g. language. This way, one can not account for a related issue, however: If national borders were determined endogenously in the economic system, trade diversion would be attributed to political demarcation even if it was not caused by it. There is reason to consider the possibility that borders are not entirely exogenous to the economy. Ritschl and N. Wolf (2011) demonstrate that a similar problem exists with trade effects of currency areas (as in Rose 2000), which can be subject to selection bias. In a transfer to border effects: Estimation will be biased, if integration between regions separated by national borders differs systematically from within-region trade, even if no border exists between them. The finding that there is a 'home bias' in trade among US states (H. Wolf 2000) is evidence of such systematic trade frictions in the absence of national borders. If these frictions resulted in a bias, it would affect even advanced studies that analyze panel data and estimate border effects in differences.

This issue has not received attention in the literature, most probably because its extent is unknown. The bias is difficult to measure as one must observe trade across borders that vary over time, i.e. the border indicator must not be itself a fixed effect. To my knowledge, no systematic attempt in this direction has been undertaken to date[1] —national borders could be, at least partly, wrongly blamed. Research has instead focused on groups of countries, for which borders have remained stable over time. In industrial economies, only few changes in demarcation occurred after WWII, anyway. If a study explicitly considers border changes, then its focus will be on political *integration* due to immediate policy interest, e.g. in case of European unification. However, it is political disintegration that involves the creation of new borders needed to identify frictions that affect trade before a border is put in place.[2] The problem is that if a sample includes new demarcation, the evaluation of border effects becomes very difficult or is not feasible at all, since political disintegration induces disruptions in statistical records. Put differently, trade data before and after break-ups are incomparable, because exporting and importing regions are not the same anymore.

In this study, national breakups due to WWI serve as a natural experiment, comprising political dissolution, unification, and stable national borders at the same time. I make use of a new data set, which contains railway shipment data of seven commodities at regional level. The most important feature of this sample is the fact that observations on trade flows before and after border changes are comparable. An Anderson and van Wincoop (2004)-type gravity model of trade is applied to evaluate the impact of new national borders and control for deviations of the treatment group from the general population.

The remainder of this chapter is structured as follows. In the following section, I will summarize the development of economic integration and trade policies in Central Europe before and after the First World War. In section 3, I will provide a derivation of the theory-based gravity equation. Section 4 will present the data and discuss the requirements of the estimation approach. I will present the main findings as well as some robustness checks in section 5. Section 6 will conclude this chapter.

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[1] Other studies concerned with national disintegration (e.g. Fidrmuc and Fidrmuc 2005, or de Ménil and Maurel 1994) have neither identified the effect of new borders nor controlled for possible self-selection.

[2] Disintegration is a frequent event as the number of countries in the world increased from 74 in 1946 to 192 in 1995 (Alesina et al. 2000, p.1276). Hence, understanding its economic implication is at least as important.

2. The economic and political integration of Germany and Poland, 1880–1939

At the end of the First World War, national breakups in Central Europe had a huge impact on the political map. Even at the outbreak of the war, national movements in Central and Eastern Europe recognized the opportunity to found independent nation states. Between 1917 and 1921, Central Europe's political geography was entirely reshaped. A number of small and middle-sized states that were mostly democratic and nationally homogeneous took the place of three vast, multinational monarchies, namely of Austria-Hungary, the German Empire, and the Russian Empire. The largest of the new countries was the Second Republic of Poland.

Poland had not existed as a sovereign state since 1795, when its neighboring countries had occupied and divided the Aristocratic Republic of Poland. The Eastern partition area of the *Rzeczpospolita Polska* became the Russian governorate 'Kingdom of Poland', the Western partition area became the Prussian province 'West Prussia', and the Southern partition area became the Austrian crown land 'Galicia and Bukovina'. However, the Poles maintained the idea of an independent Polish state despite all attempts at political integration of these areas by the members of the Holy Alliance during the period of partition, enforced by "cultural and ideological ties that united [them]" (Tomaszewski 2002, p.127). After WWI, additional major territorial changes occurred at Germany's Western border: Alsace and Lorraine became part of France, while the *Saargebiet* was governed by the League of Nations between 1920 and 1935.

The impact of Germany's and Poland's new borders on economic integration is less clear than the political effect. Whereas economic integration across borders is generally regarded to have been low before the war, the degree of economic integration within the countries is disputed for this period. Nonetheless, it is the natural reference point. Economic historians, most notably Polish ones, have suggested deep economic integration of the partition areas with their respective partitioning power before WWI (Jezierski 1967, Landau and Tomaszewski 1977). Landau (1992, p.144) asserts that the authorities of each of the partitioning powers systematically conducted the economic integration of the respective Polish region. This view usually stresses that the partitioning powers installed their legal, economic, and political institutions in the respective Polish region. This process is commonly assumed to have furthered their economic integration.

Additionally, the way in which railway infrastructure was extended is likely to have weakened trade links across borders among the partition areas. During the late nineteenth and early twentieth century, the extension of railway infrastructure became a crucial factor for economic integration. Since railway construction in Central Europe was subject to military-strategic considerations, rail tracks often ran parallel to national borders instead of crossing them (Müller 2002, pp.56–59). The Kingdom of Poland's foreign trade by rail in 1913 took place via only seven connections crossing the border to Germany and only two connections crossing the border to Austria-Hungary. Among these lines, there were no direct connections between

Table 1: Religious adherence in German-Polish regions (in 100)

Region	**Year**	**Roman Catholic**	**Greek-Catholic**	**Jewish**	**Protestant**
	1910	60.6%	/	1.1%	37.7%
West Prussia	1921	81.3%	0.1%	0.5%	18.2%
	1931	89.6%	0.1%	0.3%	9.5%
	1910	90.6%	/	0.8%	8.5%
Upper Silesia	1921	89.2%	0.0%	1.5%	9.2%
	1931	92.3%	0.1%	1.5%	5.9%
	1910	46.5%	42.1%	10.9%	0.5%
Galicia	1921	45.4%	44.3%	9.7%	0.5%
	1931	47.2%	42.9%	9.1%	0.4%

Source: GUS (2003), pp. 191-192, 385-387.
Notes: Prewar figures for Silesia are corrected for the territorial change by weighting them with results from the 1910 census. Post-WWI figures for West Prussia are given by the unweighted average of the voivodships of Pomerania and Poznan. Post-WWI figures for Galicia are given by the unweighted average of the four Southern voivodships.

Polish industrial centers that were situated on different sides of the partitioning borders, e.g. between Warsaw and Poznan (Posen). Instead, railway connections were directed to the respective capital city, e.g. Berlin. As a further obstacle, Russian railways used broad gauge tracks instead of the European standard track and accordingly did so in the Russian partition area.

Müller (2002, p. 55) opposes the hypotheses of high integration of the Eastern provinces into the German economy. He argues that even on the brink of WWI the agricultural sector of these provinces was underdeveloped and unable to exploit its exporting potential due to imperfect grain markets in western Germany. Müller's statement implies that trade frictions along the line that was to become the border after WWI, might have already been present before the war. His research points to a potential bias in an estimation of border effects: Without controlling for prewar trade frictions, the impact of new borders on interwar trade might be overestimated. Is there reason to suggest that national border demarcation after WWI was correlated with prewar economic integration of regions? Schultz (2002) analyzes anecdotal evidence from the peace negotiations at Versailles in 1919. She presents mixed evidence: During the negotiations, politicians from the new Central European states argued that economic self-sufficiency would be a necessary condition for the establishment of the young nation states. Schultz opines that economic arguments were important, but she concludes that economic arguments only came third after nationality and history when negotiators at last decided about the final border demarcation.

If network effects are important for trade, then substantial changes in the population could have led to disintegration rather than the change in borders itself. There are no reports of large-scale forced migration after WWI. In order to assess whether the ethnic composition of some regions changed nonetheless, I evaluate religious demographics as a proxy of the ethnic population shares in the German and Austrian partition area.[3] Table 1 shows that there are no major changes in the composition of the regional population, except for the proportion of Protestants and Catholics in West Prussia. Hence, the estimation should provide a means of checking robustness against a potential network effect.

The picture of a well integrated European economy before WWI has been sharply contrasted with the interwar period. Economic historians have characterized the interwar period by extraordinary economic disintegration triggered by the war (e.g. Kindleberger 1989, or Pollard 1981). A similar view has been taken already by contemporary economic researchers (e.g. Schilling 1931, pp.25–29). Yet, the only quantitative contemporary study on the subject suggested that the negative impact of the war was limited (Gaedicke and von Eynern 1933). Thus, I find it difficult to follow the general statement that WWI was devastating for economic integration, except for its immediate impact. There are further reasons for doubt. Firstly, the prewar level of integration is unclear, although it is the natural point of reference when evaluating the impact of new borders on interwar trade. Secondly, disintegration processes often coincide with political unification - as in the case of Poland at the end of WWI. Unification processes may smooth the impact of political disintegration. For instance, Wolf (2005) demonstrates empirically a successful process of economic integration of the former partitioned regions between 1926 and 1934, evaluating price movements and inter-regional trade. Thirdly, international integration after the war may have been positively influenced by the westward shift of the German border: Railway infrastructure in Central Europe was no longer structured according to national demarcation, e.g. Poland obtained direct connections to all German industrial centers by receiving part of Germany's provinces and its existing dense railway infrastructure.

Fourthly, fundamental economic relations survived the war. An example is the French need for coke, which arose from the gain of tremendous iron ore deposits in Lorraine. Yet in 1927, the French production of coke amounted to merely 18% of the annual consumption of coke in Lorraine, the rest of which was imported from the Saargebiet and Germany (Schilling 1931, p.42). Obviously, certain characteristics continued to dominate the economy of regions after they changed national alignment, compared to which political intentions had only limited influence. The Saargebiet and Upper Silesia had huge coal deposits and were thus important producers of coke as well as iron and steel (semi-)manufactures. Lorraine had tremendous iron ore deposits, making Alsace-Lorraine an important producer of iron and steel manufactures (cf. Schilling 1931). Even the Polish partition areas were very different from each other. Prior to the war, West Prussia had developed into a very advanced agricultural region referred to as Germany's bread basket. Galicia displayed typical characteristics of a developing region before WWI: While slowly industrializing, its agricultural sector lagged behind. Although it was endued with many natural resources, e.g. hard coal, most finished products and even foodstuff had to be imported. The Kingdom of Poland was divided: Its western part

[3] I consider them to be more reliable than statements about nationality, which had territorial implications during the interwar period.

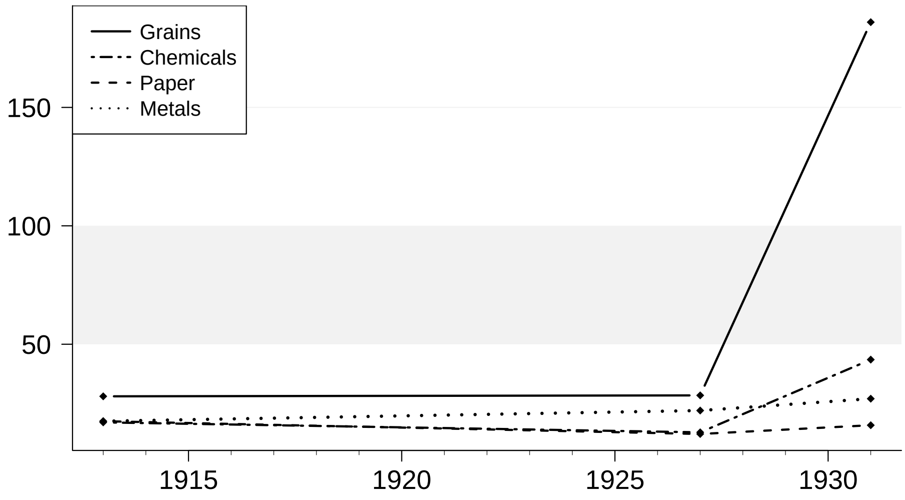
around Warsaw and Lodz was industrialized, producing mainly textiles, but also iron and steel semi-manufactures, paper, and chemicals. In the South, there were large deposits of coal, but unsuitable for the production of coke. Its eastern part was characterized by a productive agriculture, but possessed neither any significant industry nor mineral resources (cf. Landau and Tomaszewski 1977).

These four points argue for smaller than expected trade diversion through WWI. They do not contradict the idea of a natural experiment, because political disintegration will always involve some of the above aspects.[4]

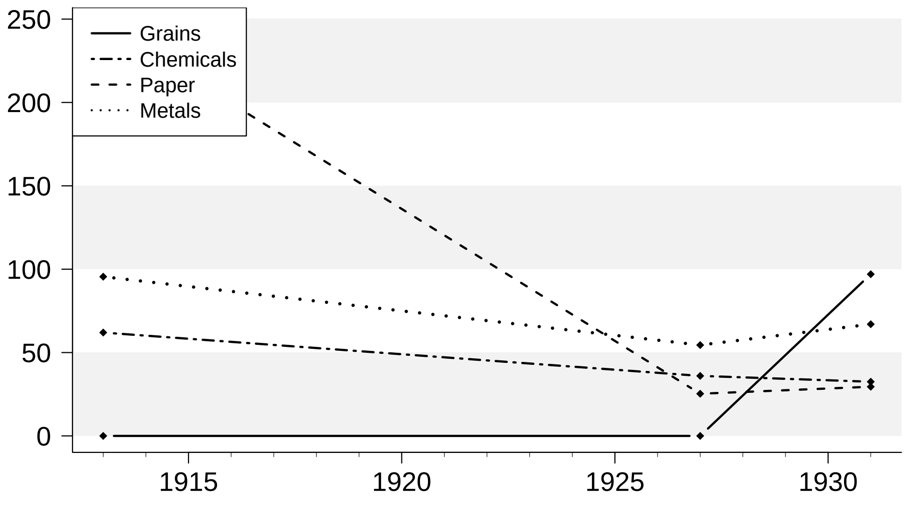
Figure 1 shows that both German and Polish tariff levels increased only modestly after WWI. Tariff levels in Austria, Hungary, and Czechoslovakia increased more strongly. The reason is that opportunities for capital formation in the successor states of the Russian, German, and Austrian Empires were rare (Teichova 1989, p. 893). Therefore, these countries began to levy relatively high duties in order to produce trade surpluses with one another. Germany found itself in a particularly adverse situation. Up until January 1925, the country was forced by the treaty of Versailles to unilaterally and unconditionally grant most favored nation status to each of the victorious powers as well as Poland. This obligation narrowed Germany's leeway in conducting a trade policy favorable in its terms. In spite of being dependent on good trade relations, the Polish and German governments failed to regulate the common trade relations for the time after the expiration of the Versailles treaty. Since the early 1920s, Poland had begun to settle agreements with potential trading partners and had reduced its economic dependency on Germany.[5] During the years 1922–1924 alone, Poland signed 13 agreements (Puchert 1963, p.48). Germany had signed only a treaty with Russia until 1925. According to the most favored nation principle Poland would have had to open up much more than Germany, including sectors where the German industry was more advanced (Puchert 1963, pp.48–56). Hence, for Poland an agreement with Germany based on this principle could only be unfavorable. The German side hesitated in the negotiations in order to exploit its advantageous position. Finally, Germany and Poland ended up in escalating the conflict by repeated mutual tariff increases from mid 1925. The *Zollkrieg* (tariff war) —or *Wirtschaftskrieg* (economic war)— lasted until 1934. Table 2 shows the massive reorientation of Polish trade away from Russia and with exports into the UK almost as large as into Germany in 1926. Comparing prewar and interwar German and Polish 'foreign' trade suggests the greatest change in their respective integration with Russia (see table 2). Before WWI, Polish export was almost completely oriented on the Russian market. The overweight of Russia in Polish exports can be explained by high Russian external tariffs, particularly on finished goods. This urged the German industry to export semi-finished goods to the Kingdom of Poland in order to let them be manufactured or grafted there. This explanation is supported by the fact that the Kingdom of Poland accounted for about two thirds of German exports to the Russian Empire at the time (Müller 2002, p.64). Thereby, the new demarcation might have destroyed

[4]An earlier study of this hypothesis is provided by de Ménil and Maurel's (1994), who find comparably strong trade links among successor states of Austria-Hungary after WWI.

[5]In spite of that, trade with Germany still accounted for about half of Poland's imports and about one quarter of its exports in 1925.



(a) German tariff rates



Notes: Figures for 1913 give Russian tariffs.

(b) Polish tariff rates

Figure 1: Change in German and Polish import tariffs by product group
Source: Figures based on table 1 in appendix 3.

such division of labor that economically integrated these regions across borders. The table also points to different developments of the trade balance of Germany and Poland.

Table 2: Foreign trade of Germany and Poland with selected countries 1910, 1926 (millions of Reichsmark)

	Imports / Exports 1910			Imports / Exports 1926		
	Rye	Coals	Total	Rye	Coals	Total
Germany's trade with						
Poland	*na*	*na*	*na*	0.8/0.1	1/0.1	283/192
Austria-Hungary	0/4.8	10.3/113.7	759/822	—	—	—
Russia	39/19.6	0.1/13.2	1387/547	9.2/0	0/3.7	344/265
Great Britain	0.0/1.0	135.1/0.0	767/1102	0/0.3	27.7/44.9	863/1169
Total	43/112	158/323	7475/8934	46/50	60/620	9951/9783
Poland's trade with						
Russia	5.0/0.7	0.8/1.1	596/895	0.4/0	0/8.4	7/23
Great Britain	*na*	*na*	*na*	0.4/7.9	0.1/50.1	88/211
Total	16.0/1.0	16/4	1016/986	23/107	3/44	847/1232

Sources: Figures for Germany were taken from Statistisches Bundesamt (1927/1978b) and Kaiserliches Statistisches Amt (1911b). Figures for Poland were taken from Tennenbaum (1916) and from the Polish statistical yearbook 1927.
Notes: The table displays the foreign trade of Germany and Poland with selected trading partners denoted in current Reichsmark (RM). 'Poland' in 1910 refers only to the Kingdom of Poland. Figures for Poland were converted into RM using the following ratios: RM/tons, rubles/tons, and złoty/tons (for ratios cf. the above sources).

Nonetheless, Kowal (2002) stresses that antagonism between Germany and Poland at the state-level is only one side of the story. He highlights the efforts of companies, interest associations, and single ministries on both sides to normalize economic relations during the tariff conflict. These efforts actually led to reductions of and exceptions from barriers to trade. Furthermore, informal contacts were a feature of German-Polish trade relations that could substitute to some extent the absence of a bilateral treaty (Kowal 2002, pp. 150–153). As a further aspect fostering Polish interwar trade, Müller (2002, pp. 60–62) mentions the abolition of additional costs incurred at the prewar border of the Russian partition area by nonofficial but state-guided protectionist measures. Also Liepmann (1938) concludes that 'border Europe', i.e. the new European countries, became economically integrated into the European trade —at least considering industrial goods.

The examination of the contradicting hypotheses discussed in the previous two sections is facilitated by the fact that potentially important sources of bilateral transaction costs were similar before and after the war. The railway infrastructure destroyed during WWI was soon rebuilt. The Polish government decided to make large investments in tracks and equipment only shortly after the war (Landau 1992, p. 147). Until 1927, tariff-barriers were partly even lower than before the war, while NTB remained unimportant at least until 1932.

3. Gravity model and empirical strategy

3.1. The gravity model of trade. The interdependence of all trade relations is one main insight of Anderson and van Wincoop's (AW) (2003, 2004) gravity model of trade. Their theory-based model implies that changes in the economic integration between two regions affect their integration with other regions as well. The model applied subsequently is based on AW (2004).

AW assume separable preferences and technology. This allows for the two-stage budgeting, where bilateral trade is determined in a conditional general equilibrium, i.e. the allocation decisions within region i are separable from the bilateral allocation of trade across regions i and j.[6] Secondly, AW (2004) assume that all goods are differentiated by place of origin. The aggregator of varieties is assumed to be identical across regions. Thirdly, consumers in all regions are supposed to have identical, homothetic preferences, approximated by a constant elasticity of substitution (CES) utility function. After formulating the consumer maximization function, AW (2004) can derive their model, which is described by the following system of equations (for the complete derivation, see section 1 in appendix B)

$$X_{ij}^k = \frac{Y_i^k E_j^k}{Y^k} \left(\frac{t_{ij}^k}{P_j^k \Pi_i^k} \right)^{1-\sigma_k} \tag{3.1}$$

$$\left(\Pi_i^k\right)^{1-\sigma_k} = \sum_j \left(\frac{t_{ij}^k}{P_j^k} \right)^{1-\sigma_k} \frac{E_j^k}{Y^k} \ \forall i \tag{3.2}$$

$$\left(P_j^k\right)^{1-\sigma_k} = \sum_i \left(\frac{t_{ij}^k}{\Pi_i^k} \right)^{1-\sigma_k} \frac{Y_i^k}{Y^k} \ \forall j \tag{3.3}$$

where

X_{ij}^k denotes region i's shipments into region j in product class k.
Y_j is the nominal income of region j's inhabitants.
E_j^k is the nominal expenditure of region j's inhabitants on product k.
σ_k is the elasticity of substitution between varieties in product class k, with $\sigma_k \neq 1$.
p_{ij}^k denotes the price charged by region i for exports of product k to region j.

This system of equations models trade resistance with three components: (i) the bilateral trade resistance t_{ij}^k, (ii) the exporter i's resistance to trade with all regions Π_i ('outward multilateral resistance'), and (iii) the importer j's resistance to trade with all regions P_j ('inward multilateral resistance'). Even though the Π_i's and P_j's are not observable, equation (3.2) and (3.3) can be simultaneously solved for all Π_i's and P_j's in terms of product specific income shares $\{Y_i^k/Y^k\}$, bilateral trade resistance t_{ij}^k, and elasticities σ_k.

[6]The term two-stage budgeting refers to the consecutiveness of allocation decisions. In a first step, consumers allocate their budget to either traded or non-traded goods. In the second step, only the decision is considered on which variety of the traded goods they spend their budget.

$$Y^k = \sum_i Y_i^k = \sum_j E_j^k \tag{3.4}$$

is the overall output of k, which has to be equal to overall expenditure on k.

The inward and outward multilateral resistance terms represent relative barriers as they summarize the average trade resistance between one region and its trading partners. Given $\sigma_k > 1$, equations (3.1)–(3.3) demonstrate the importance of *relative* barriers to trade. They imply that bilateral trade positively depends on the inward multilateral resistance P_j^k, i.e. the flow of goods from region i to j increases when trade costs of other suppliers to j increase. But also "high resistance to shipments from i to its other markets, captured in outward multilateral resistance Π_i^k, tips more trade back into i's market in j." (AW 2004, p.21)

It is important to note that theory-based gravity implies that relative economic size of trading partners is crucial for trade patterns as well. A small region, in economic terms, depends more on foreign trade than a large region, which can make use of the size of its domestic market and shift foreign trade flows to its internal market. Therefore, a small region's multilateral resistance to trade is more affected by changes in overall trade barriers than a large region's multilateral resistance.

The next step is to move from the theoretical model to an operational equation and then to discuss requirements for the trade cost function. Taking the natural logarithm of (3.1) and adding the normally distributed error term ϵ_{ij}^k yields the objective equation

$$\begin{aligned} ln\left(X_{ij}^k\right) = \quad & a^k + ln\left(Y_i^k\right) + ln\left(E_j^k\right) + \\ & (1-\sigma_k)\, ln\left(t_{ij}^k\right) - (1-\sigma_k)\, ln\left(\Pi_i^k\right) + (1-\sigma_k)\, ln\left(P_j^k\right) + \epsilon_{ij}^k \end{aligned} \tag{3.5}$$

where a denotes the constant. Note that the sample consists of trade by weight, whereas (3.5) requires trade by *value* as dependent variable. Following Wolf (2005), it is not necessary to impose additional restrictions. It is possible to make use of an implicit assumption in the derivation of the gravity model (cf. equation 1.11 on page 213 in appendix B). Substituting this equation for the unobserved values X_{ij}^k, one obtains the observable quantities c_{ij}^k as dependent variable, explicitly

$$\begin{aligned} ln\left(c_{ij}^k\right) = \quad & a^k + ln\left(Y_i^k\right) + ln\left(E_j^k\right) + (-\sigma_k)\, ln\left(t_{ij}^k\right) - \\ & ln\left(p_i^k\right) - (1-\sigma_k)\, ln\left(\Pi_i^k\right) + (1-\sigma_k)\, ln\left(P_j^k\right) + \epsilon_{ij}^k \end{aligned} \tag{3.6}$$

The unobservable resistance terms and production variables in (3.6) are accounted for by a set of inward and outward region specific dummy variables, yielding (with the introduction of a time index t)

$$ln\left(c_{ij,t}^k\right) = a_t^k + A_{i,t}^k + A_{j,t}^k + (-\sigma_k)\, ln\left(t_{ij}^k\right) + \epsilon_{ij}^k \text{ , where} \tag{3.7}$$

$$A_{i,t}^k = ln\left(Y_{i,t}^k\right) - (1-\sigma_k)\, ln\left(\Pi_{i,t}^k\right) - ln\left(p_{i,t}^k\right) \text{ and} \tag{3.8}$$

$$A_{j,t}^{k} = ln\left(E_{j,t}^{k}\right) - (1-\sigma_k)\, ln\left(P_{j,t}^{k}\right) \tag{3.9}$$

The import and export dummies of each region take the value of 'one' whenever that region enters as an importer or exporter, respectively. Otherwise they take the value of 'zero'. Import and export dummies control for all the region-specific features, i.e. multilateral resistance terms, expenditure, income, and prices. The final building block is to link the unobservable trade cost factor t_{ij}^{k} to a function of m observables, including the variables of interest

$$t_{ij,t}^{k} = \prod_{m=1}^{M} \left(z_{ij,t}^{m}\right)^{\gamma_t^m} \tag{3.10}$$

Let z_{ij}^{m} be normalized such that $z_{ij}^{m} = 1$ represents zero trade barriers associated with the m-th variable. Thus, $\left(z_{ij}^{m}\right)^{\gamma^m}$ is equal to one plus the tariff equivalent of trade barriers associated with the m-th variable. In the analysis below, equation (3.10) is modeled as a function consisting of (i) transport costs proxied by distance ($dist_{ij}$) and (ii) costs that are incurred when crossing an international border (b). Its most general form is given by

$$t_{ij,t}^{k} = (dist_{ij})^{\rho^k} \left(b^k\right)^{\delta_{ij}} \tag{3.11}$$

where δ represents an indicator variable for border 'treatment'. In (3.11), δ is equal to unity if regions i and j do not belong to the same country, otherwise it is equal to 'zero', which is a commonly used specification to estimate border effects in a cross-section. A negative and significant coefficient of this variable reflects a trade diverting border effect, meaning that *ceteris paribus* regions traded less if they were located on different sides of a national border.

3.2. Identification of the treatment effect. As defined above, δ denotes a binary treatment indicator: $\delta = 1$ indicates treatment, i.e. the trade flow crosses a political border, and $\delta = 0$ indicates no treatment. The average effect a national border exerts on bilateral trade X_{ij} is given by

$$E\left[X_{ij}^{(\delta=1)}\middle|\delta_{ij}=1\right] - E\left[X_{ij}^{(\delta=0)}\middle|\delta_{ij}=1\right] = E\left[X_{ij}^{(1)} - X_{ij}^{(0)}\middle|\delta_{ij}=1\right] \tag{3.12}$$

where the right hand side represents the the average treatment effect of the (trade flows actually) treated (ATET).[7] Hence, trade is conditioned on treatment in (3.12). To estimate trade diversion caused by borders, one would ideally like to compare trade between regions under border treatment with the counterfactual trade between the same region pair, but without a border in place. Naturally, the individual cannot be in both states $X_{ij}^{(1)}$ and $X_{ij}^{(0)}$, i.e. it is not possible to observe

[7] Cf. Angrist (2001) for a discussion about different strategies to identify the ATET properly.

trade of identical pairs of regions simultaneously with and without a border: "[In] effect, the problem we face is one of missing data." (Wooldridge 2002, p. 604)

Empirical studies, using a gravity model framework, approximate the control group for bilateral trade under treatment by bilateral trade with the remaining regions k —including all combinations of *ii*, *ik*, *kj*, and *ji*. Typically, cross-border is compared to within-country trade, i.e. the control group consists only of X_{ii}. Following Angrist (2001), this simple comparison of treated and untreated trade can be formulated as

(3.13)

$$E\left[X_{ij}^{(1)}\middle|\delta_{ij}=1\right]-E\left[X_{ii}^{(0)}\middle|\delta_{ii}=0\right]= E\left[X_{ij}^{(1)}-X_{ij}^{(0)}\middle|\delta_{ij}=1\right]+ \left\{E\left[X_{ij}^{(0)}\middle|\delta_{ij}=1\right]-E\left[X_{ii}^{(0)}\middle|\delta_{ii}=0\right]\right\}$$

where the second term on the right hand side is a bias term, which is equal to zero only under particular conditions. It is equivalent to state that selection into border treatment happens randomly or that a mechanism ensures independence of variation in border treatment from the level of bilateral trade, i.e. $corr\left(X_{ij},\delta_{ij}\right)=0\ \forall\ i,j$. It is the standard assumption in the border literature, because only if it equals zero, the ATET is identical to the average treatment effect (ATE). If selection into border treatment happens non-randomly, estimates are likely to be subject to selection bias (cf. Heckman 1990). In fact, the previous section has provided anecdotal evidence that economic integration affected decisions on border drawing after WWI.

A glance at figure 2 adds to the impression that those pairs of regions which came under treatment first during the interwar period were trading much less than region pairs that did not become separated (cf. also figure 1 in appendix 2). Typical controls, regional characteristics —such as GDP, population, and price levels— and basic elements of pair-wise characteristics —such as distance, adjacency, or common language—, do ensure to account appropriately for this deviation.

If such pair-wise heterogeneity is not captured in the model specification, the estimation attributes to the border coefficient not only the actual impact of borders on trade, but also the impact of factors that happen to vary systematically along these borders. This study addresses this issue by estimating the treatment effect of borders on trade with a difference-in-differences estimator. Let τ be a point in time during the sample period, at which trade between i and j becomes exposed to treatment.[8] Then, δ^{tiw} is defined as

$$\delta_{ij,t}^{tiw}=\begin{cases}1 & \text{if } ij \text{ became separated by a border in } \tau \text{ and } t\geq\tau\\ 0 & else\end{cases} \tag{3.14}$$

[8]For the sake of simplicity, I assume that τ is the same for all region pairs, which were separated by political borders after WWI.

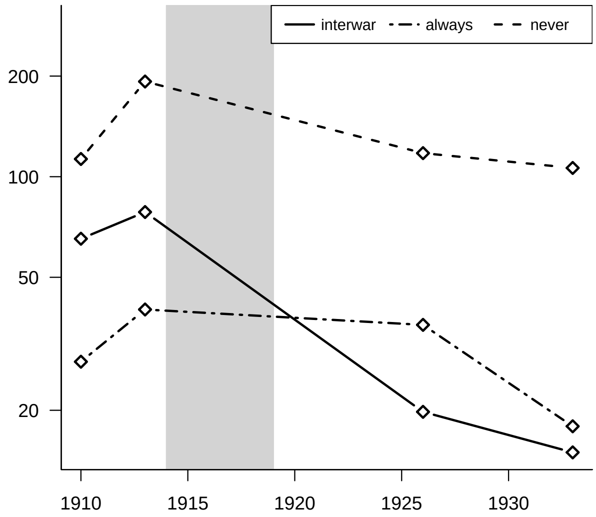


Figure 2: Bilateral trade in hardcoal depending on the period of border treatment.
Notes: Unweighted average in 1000 tons, log scale. '◇' represents actual observations. 'interwar' denotes trade that became subject to treatment only after WWI. For details see 1 in app. 2.

Hence, the approach makes use of the fact that δ^{tiw} changes over time, where 'tiw' denotes treatment first in the interwar period. Eventually, the model can be formulated as

$$X_{ij,t} = A_{i,t} + A_{j,t} + a_{ij} + d_t + \delta_{ij}^{aws} b^{aws} + \delta_{ij,t}^{tiw} b^{tiw} + \epsilon_{ij,t} \tag{3.15}$$

where $A_{i,t}$ and $A_{j,t}$ are individual, time-varying effects —e.g. of regional GDP—, d_t is an economy-wide component, and a_{ij} is a bilateral, time-invariant component, such as distance. b^{aws} represents the conventional, i.e. time-invariant, effect of border that were always in place during the sample period, whereas b^{tiw} reflects the effect of political borders put in place after WWI. As discussed above, selection into treatment is possibly correlated with the level of trade $corr\,(X_{ij}, \delta_{ij}) \neq 0\;\forall\, i, j$. Hence, (3.15) is not appropriate as it should account for differences between the treatment and control group in the permanent component of bilateral trade.

One option to obtain the ATET would be to apply some type of Arellano-Bond estimator by taking first differences of (3.15) (cf. Heckman et al. 1999). However, the data set is inappropriate for this approach, because differencing requires a panel with regular time intervals and a lot more observations in the time domain. Additionally, the approach is not well suited to estimate gravity models in general and to learn about selection bias in border treatment in particular: Differencing eliminates all time-invariant coefficients, such as the indicator variables on most borders as well as on distance. Hence, it is impossible to compare the actual treatment effect of borders ATET to the ATE, because the latter is based on time-invariant borders (δ_{ij}^{aws}). The problem can be illustrated by the following very simple DiD estimator (neglecting the autoregressive component)

$$X_{ij,t} = a_{i,t} + a_{j,t} + a_{ij} + \delta_{ij}^{aws} b^{aws} + \delta_{ij,t}^{tiw} b^{tiw} + \epsilon_{ij,t} \tag{3.16}$$

In period $(\tau - 1)$ this relationship becomes

$$X_{ij,(\tau-1)} = a_{i,(\tau-1)} + a_{j,(\tau-1)} + a_{ij} + \delta_{ij}^{aws} b^{aws} + \epsilon_{ij,(\tau-1)} \tag{3.17}$$

because $\delta^{tiw} = 0$ for $t < \tau$. Subtracting (3.17) from (3.16) gives the DiD

$$X_{ij,t} - X_{ij,(\tau-1)} = (a_{i,t} + a_{j,t} - a_{i,(\tau-1)} - a_{j,(\tau-1)}) + \delta_{ij,t}^{tiw} b^{tiw} + (\epsilon_{ij,t} - \epsilon_{ij,(\tau-1)}) \tag{3.18}$$

where the first term on the RHS is constant across region pairs and all time-invariant, pair-specific effects have been removed.

In order to deal with the irregular time structure of my data set and to avoid to kill the gravity equation, I fall back on a difference-in-differences estimator in levels (DDL) proposed by Ashenfelter (1978) and Ashenfelter and Card (1985) as alternative approach. The DDL's trick is to introduce a fixed effect of the treated (FET).[9] The DDL has the shortcoming that it does not address anticipation effects, which introduce time variation in the difference between treatment and control group. Yet, substantial anticipation effects are unlikely with respect to WWI (cf. section 2) and the evidence in the data is mixed, at the utmost (see figure 2 above and figure 1 in appendix B). Hence, the DDL allows to estimate the effect of selection bias (or endogeneity) separately from the actual treatment effects. Let

$$\delta_{ij}^{FET} = \begin{cases} 1 & \text{if } ij \text{ became separated by a border first in } \tau \\ 0 & else \end{cases} \tag{3.19}$$

The time-invariant FET accounts for differences between the treatment and control group in the permanent component of bilateral trade. If it is significantly different from zero, it indicates selection bias. Alternatively, the FET can be interpreted as path dependency or persistence in trade relations. Hence, one can distinguish between the proper treatment-effect of changing a political or administrative border from the impact of unobservable pair-wise heterogeneity attributed to the new border

[9]For a recent application of the DDL within the gravity model framework, see Ritschl and Wolf (2011).

$b_{FET} = 0,\ b_{tiw} = 0 \Rightarrow$ effect neither endogenous nor treatment (ATE = ATET = 0)
$b_{FET} = 0,\ b_{tiw} \neq 0 \Rightarrow$ effect entirely due to treatment (ATE = ATET $\neq$ 0)
$b_{FET} \neq 0,\ b_{tiw} = 0 \Rightarrow$ entirely endogenous, no treatment effect (ATE $\neq$ ATET)
$b_{FET} \neq 0,\ b_{tiw} \neq 0 \Rightarrow$ effect partly endogenous, partly due to treatment (ATE $\neq$ ATET)

3.3. Empirical strategy. The empirical strategy is to estimate different specifications of the trade cost function independently for the k goods or groups of goods, respectively. The base-line specification of (3.11) is

$$t^k_{ij,t} = (dist_{ij})^{\rho^k} \left(b^{preWWI,k}\right)^{\delta^{preWWI}_{ij,t}} \left(b^{postWWI,k}\right)^{\delta^{postWWI}_{ij,t}} \tag{3.20}$$

The trade cost specification consists of transport costs, proxied by bilateral distance ($dist$) as well as of estimates of border effects during the prewar and interwar period, irrespective of the moment of demarcation: $\delta^{preWWI}_{ij,t}$ takes the value of unity if $t < \tau$ and regions i and j do not belong to the same country prior to WWI, otherwise it takes the value of zero. The interwar equivalent is $\delta^{postWWI}$, which equals unity if $t \geq \tau$ and regions i and j do not belong to the same country after WWI. This specification is estimated for illustrative purpose: First, neglecting potential selection bias, these estimates yield ATEs and are thus comparable to border estimates in other studies. Second, they are used to benchmark the ATET estimates in order to evaluate the extent of the bias.

In order to identify the actual border treatment effect, the DDL estimator is implemented and prewar as well as interwar border effects are decomposed. The trade cost function becomes

$$t^k_{ij,t} = (dist_{ij})^{\rho^k} \left(b^{taws,k}\right)^{\delta^{taws}_{ij}} \left(b^{diss,k}\right)^{\delta^{diss}_{ij,t}} \left(b^{tiw,k}\right)^{\delta^{tiw}_{ij,t}} \left(b^{FET,k}\right)^{\delta^{FET}_{ij}} \tag{3.21}$$

where $b^{taws,k}$ reflects trade diversion of those borders that were always in place during the sample period, $b^{diss,k}$ relates to borders that were dissolved in τ, i.e. that existed only prior to WWI. Coefficient $b^{tiw,k}$ represents the effect of treatment first in the interwar period, i.e. the ATET as defined in (3.14). Correspondingly, $b^{FET,k}$ captures the permanent deviation of bilateral trade in the treatment group from the control groups.

The specifications (3.20) and (3.21), respectively, are then used to substitute the trade cost term in the gravity model. In order to estimate border effects most studies log-linearize an empirical gravity equation, such as (3.4), and apply OLS. This strategy represents the standard approach, but it incurs several problems. First, table 3 shows that many observations of the dependent variable in the data set are zero. This fact does not reflect a data problem. Also nowadays, the observation that bilateral trade flows are small or zero is frequent if one considers disaggregated data, i.e. at regional or sectoral level. However, if one wants to log-linearize the gravity equation, the value of the dependent variable has to be an element of number space $\Re^+$. If one simply excludes those entries and applies the least squares (LS) procedure to the sample, the LS estimator will be biased and inconsistent as zero trade is usually correlated with covariates such as distance or GDP (cf. Judge et al. 1988, pp.796–799). An alternative strategy is to add an incremental unit to all observations of the dependent variable and to scale the resulting estimates in order to penalize the absence of trade (Eichengreen and Irwin 1995, p.9). This factor increases in proportion to zero-trade observations, e.g. it is equal to two if half of the observations are equal to zero. Another strategy is to apply the Tobit estimator, which has the drawback that its coefficient estimates that cannot be interpreted intuitively.

Santos Silva and Tenreyro (2006) demonstrate that log transformations of the data can lead to coefficient estimates which are strongly biased. The reason is that the expected value of a log-transformed random variable does not only depend on the mean of the random variable but also on its higher moments. This is known as Jensen's inequality stating that $E[ln(y)] \neq ln(E[y])$, with y being a random variable. Given this, heteroscedasticity of the error term in the stochastic formulation of the model would result in an inefficient, biased and inconsistent estimator.[10] Hence, Santos Silva and Tenreyro (2006) recommend to estimate the gravity model in its multiplicative form, preferably using the pseudo maximum-likelihood (PML) approach. The PML is less sensitive than the ML estimator if the distribution assumptions are violated, specifically because it is "consistent and reasonably efficient under a wide range of heteroscedasticity patterns ..." (Santos Silva and Tenreyro 2006, p.645). As trade data can be considered as count data, Santos Silva and Tenreyro (2006) suggest to use the Poisson distribution as link function. It attributes the same informative weight to all observations. Hence, the estimator is preferable to others without further information on the heteroscedasticity.[11] Santos Silva and Tenreyro (2006) stress that all that is needed for this Poisson PML (PPML) estimator

> "to be consistent is the correct specification of the conditional mean, that is, $E[c_{ij}|x] = exp(x_{ij}\beta)$. Therefore, the data do not have to be Poisson at all .... This is the well-known PML result first noted by Gourieroux, Monfort, and Trognon (1984)." (p.645)

PPML methods are therefore robust against over-dispersion, i.e. that the conditional mean of the dependent variable is much greater than its conditional variance.[12] Not being required to linearize the model provides a straightforward means to deal with the large number of zero observations in the data.

In this study, (3.20) and (3.21) are implemented into (3.6). The main part of the estimation is conducted using PPML estimator. Not the least, because a simple test for overdispersion shows that trade observations in my data set are

[10] In fact, in the application of gravity models the resulting estimation errors very often display heteroscedasticity (e.g. Santos Silva and Tenreyro 2006).

[11] They present the results of a horse race between various estimation strategies including Tobit, nonlinear least squares and Poisson regression models. Investigating simulated and real trade data, they conclude that only the latter approach and NLS deliver consistent estimates, but that NLS is less efficient because the structure of heteroscedasticity is unknown.

[12] This violation of the Poisson distribution is natural in trade data and also the case with my data set.

strongly overdispersed, i.e. the variance of the data is greater than expected from the poisson distribution.[13] In order to gain efficiency, the estimates are additionally corrected for heteroscedasticity using a robust covariance matrix estimator within the PPML framework. In order to evaluate the extent of selection bias under different estimation strategies, I complement the PPML estimation by scaled OLS and LSDV.

[13]The test compares the ratio of the observed variance to the theoretical variance times the number of observations - 1 to a Chi-square distribution with (no. observations - 1) degrees of freedom.

4. Data

In order to estimate the gravity model (3.6) with trade cost function (3.21), the data need to contain not only bilateral shipments across borders, but also shipments within borders. The panel analyzed hereafter comprises information on exports from 29 Central European regions into 44 regions in all of Europe in 1910, 1913, 1926, and 1933.[14] An overview of all regions included in the sample is provided in table 1 in appendix 2. For each of the 29 exporters, the data set comprises, first, shipments within each region, subsequently denoted as *internal trade*. Secondly, it comprises shipments from each exporter into all the other 28 exporting regions. Thirdly, there is additional data on shipments from each of these 29 regions into the remaining 15 regions, which are not themselves included as exporters in the sample. An example would be exports from Bavaria or Poland to Great Britain.

The sample comprises a maximum of 35,728 observations, as there are 5,104 observations for each of seven goods or groups of goods. The majority of these data is taken from German railway transport statistics, which document for each selected commodity (i) shipments *within* every German railway transport district (TD) at time t, (ii) shipments *from* each of these German TD into each of the remaining 43 regions, and (iii) shipments *into* each German TD from each of the remaining 43 regions. Next to the notable fact that such detailed statistics exist at all and are almost unchanged over the course of 60 years, three facts are even more notable: Before WWI, the statistics accounted for certain regions within foreign countries separately, e.g. for Poland within Russia and Galicia within Austria-Hungary. After WWI, the German statistics continued to account separately for the former German regions within Poland and France. Additionally, the Poles set up railway statistics modeled on the German example. They even defined Polish TD that can easily be aggregated such as to fit their counterparts in the German statistics.

Prior to WWI, 27 out of 29 exporting regions are German TDs; after WWI, 23 out of 29 exporting regions belong the Germany, four regions belong to the new Republic of Poland, one regions belongs to France (Alsace-Lorraine), and another region is under international control (Saargebiet). In all of the exporting regions, railways accounted for the major share in inter-regional trade. Thus, it appears sufficient to consider data on railway shipments only.[15] The data is mainly taken from three annual series, namely the *Statistik der Güterbewegung auf Deutschen Eisenbahnen* before WWI and *Die Güterbewegung auf Deutschen Eisenbahnen* after WWI as well as *Rocznik Statystyczny Przewozu Towarów na Polskich Kolejach*

[14] It would certainly be desirable to choose year more proximate to WWI in order to compare the state of economic integration. However, the interpretation of 1926 data is more immediate than that at earlier dates. Such data would be biased by all the direct consequences of war, e.g. guerilla fights.

[15] The share of railway shipments in Polish domestic trade was about 99% in the 1930s (N. Wolf 2005). In Germany, the 1926 share of railways was smaller, namely about four fifth (Statistisches Bundesamt 1926/78). In Russia, the respective share in 1910 was at least two thirds and much higher for most sectors (Žarago 1914), in Germany the share in 1910 was about 84% for rye and coal (Kaiserliches Statistisches Amt 1913).

Państwowych for Poland.[16] These sources report annual railway shipments of different commodities and product groups, respectively. All data is given in metric tons. Nonetheless, some data on the residual foreign trade of the foreign regions as well as on their internal trade that is not recorded in these sources, e.g. exports from the Kingdom of Poland into Britain. Therefore, I approximated these figures as far as possible using evidence on production as well as on trade from about 30 further statistical sources (details are provided in tables 2 and 3 in appendix A).

Seven commodities and groups of goods are included in the sample. They represent important parts of the economy. Rye, an agricultural commodity, is used in private consumption and by the food industry. Brown coal and hard coal are mineral resources, which differ in their properties and usage: brown coal is mainly used for heating, but also for power generation. Hard coal is most important for power generation and serves as raw material for making coke. Coke is used as an input for processing iron and steel. There are two groups of preliminary products, namely chemical raw materials, subsequently *chemicals*, as well as iron and steel semi-manufactures, henceforth *iron and steel*. Paper and cardboard, subsequently *paper*, is a basket of finished products, which are consumed in all parts of the economy.

Each cross-section consists of 1276 observations (per commodity). In 1910 and 1913, 540 observations per cross-section document cross-border trade. Each interwar year comprises 728 observations on cross-border trade, of which 199 trade flows cross a border that was first established after WWI; 11 observations document bilateral trade crossing a border before WWI but none after WWI, e.g. between West Poland and East Poland.

The considerable change in Germany and Poland's respective territories is, altogether, quite unproblematic for the compilation of the data set. Territorial changes between Germany and Poland took place almost entirely along the boundaries of the areas defined as TDs (cf. map 1).[17] Most importantly, the German interwar statistics kept up the definition of TDs if possible and defined new foreign TDs. A few disruptions in the statistical records due to the division of TDs complicate the analysis nonetheless. One of these new TDs results from the division of the former Upper Silesia (Oberschlesien), into a German and a Polish part, called Upper Silesia and East Upper Silesia (Ost-Oberschlesien), respectively. Another new TD results from the division of the districts of Posen and of West-Prussia. A small part of each district remained part of Germany. Both of these parts were merged to become the Borderland of Posen-West Prussia (TD 12: Grenzmark Posen-Westpreußen) after the war. The larger part of each former TD merged to form a new district, West Poland (TD 47), that was part of interwar Poland. Since the number of regions in each cross-section of the panel must be the same, one basically has two options.

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[16]Titles in English (in the order of appearance): *Statistics of the Movement of Goods on German Railways*, *Movement of Goods on German Railways*, and *Statistical Yearbook of the Movement of Goods on Polish State Railways.*

[17]That is the case with the Saargebiet, West Prussia as well as Alsace and Lorraine. The reshaping of TDs in the cases of Northern Schleswig, Eupen and Memel is unproblematic because of their negligible economic influence.

Table 3: Observations of trade flows per cross section

	Browncoal	Chemicals	Iron, Steel	Rye	Paper	Hardcoal	Coke
#observations	1276	1276	1276	1276	1276	1276	1276
thereof							
#observations subject to border treatment							
Before WWI	540	540	540	540	540	540	540
After WWI	728	728	728	728	728	728	728
thereof							
#observations subject to newly created and to remaining borders							
$\delta_{ij,t}^{tiw} = 1$	199	199	199	199	199	199	199
$\delta_{ij}^{taws} = 1$	529	529	529	529	529	529	529
#observations equal to zero							
1910	983	371	301	833	358	803	881
1913	958	320	308	780	327	793	868
1926	923	360	368	866	413	794	848
1933	936	337	444	936	380	834	875
Scale factor	3.95	1.39	1.39	3.13	1.42	2.74	3.16

Notes: The scale factor is given by the inverse proportion of trade observations which are non-zero. It can be used to rescale coefficient estimates as in Eichengreen and Irwin (1995).

One may opt either for non-ambiguous demarcation, i.e. to compile regions alongside the actual interwar barriers, or for comparable geographical shape of regions before and after the war, i.e. to construct bi-national regions. As this study focuses on border effects, I have decided to follow the former approach.

A second problem is that the interwar TDs of "East Poland" and "Galicia" are not identical to prewar "Kingdom of Poland" and "Galicia". The interwar TD "East Poland" corresponded to both the former Kingdom of Poland and to the region east of it, i.e. the Eastern Borderlands (*kresy*). Thus, the interwar area of East Poland is larger than the former Kingdom. In contrast, the TD "Galicia" was smaller after WWI, because it had included the Bukovina beforehand.[18] I presume that both issues only pose a minor problem for the empirical analysis, since the changes occurred in terms of geography rather than in terms of economic activity. Both the *kresy* and the Bukovina were sparsely populated and purely agricultural areas. The use of region-specific, time-varying fixed effects should be sufficient to control for these changes, since the additional heterogeneity can be expected to affect trade with all trading partners and not just bilateral trade, i.e. it is captured as a drop in the region's overall demand and supply. Distances between region-pairs required by the model are calculated as center-to-center Great Circle distances.[19] Internal distances are computed as[20]

[18]Data were taken for Galicia *and* the Bukovina from the Austrian statistics as to make them comparable to the German definition of the TD Galicia.

[19]Center cities are determined according to their economic importance and position in space, see App.2.

[20]Data on the area were taken from statistical yearbooks, namely Kaiserliches Statistisches Amt (1910), Statistisches Reichsamt (1927), GUS (2003), and K.K. Statistische Zentralkommission (1913).

$$dist_{ii} = 0.67\sqrt{area_i/\pi} \tag{4.1}$$

According to Head and Mayer (2000) this measure has worked well for similar purposes despite the strict underlying assumptions.

5. Results

Estimates for different formulations of the trade cost function (3.10) are presented, comparing the impact of borders on trade in all seven goods in consideration. Several estimation methods are applied to the data. The central approach is PPML. Additionally, OLS is conducted for illustrative purpose to link my findings to the earlier literature. After all, OLS has been the predominant approach starting with Tinbergen (1962) and in its scaled version since Eichengreen and Irwin (1995). PPML is becoming standard for estimation of gravity models, beginning with Santos Silva and Tenreiro's (2006) criticism of OLS estimation in the presence of excess numbers of zero in the dependent variable (cf. section 3.3). Furthermore, a hurdle estimator is applied, which explicitly models excess numbers of zero in the data. Its main purpose here is to better understand results obtained from PPML and to test how sensitive the results are to distributional assumptions.

The first set of regressions, presented in table 4, analyzes a sub-sample of the data, specifically trade of the area defined by Germany's prewar borders.[21] The rationale to use this sub-sample is to have a clear-cut definition of treatment and control group: Interwar borders in this sub-sample are all put in place first after WWI, which avoids problems related to missing data.[22] The overall fit of the models estimated via OLS is good with an adjusted R^2 ranging from 0.68 to 0.84. As expected, parameter estimates for distance are always negative and statistically significant. In accordance with Anderson and van Wincoop (2004), the regressions yield that high value-to-weight goods are less penalized by transport costs. Time dummies that are supposed to capture economy-wide changes in integration, are significantly different from zero for a few goods in 1933 only.

The coefficient $b^{interwar}$ represents the ATE of the selected new borders on trade. Because it neglects the potential selection bias, it is subsequently denoted as 'naïve' estimator. The border coefficients are statistically and economically significant for trade in all seven goods.[23] This finding suggests that the new demarcation had a negative impact on trade. The coefficient values are being interpreted further below. These ATE estimates serve as a benchmark for my ATET estimates, because they are obtained similarly to border estimates in most of the literature. If a bias is found in the 'naïve' border estimate, it will still be difficult to integrate this finding in future studies, because the necessary data does usually not exist. Hence, the benchmarking helps to assess, firstly, in what kind of previous and future studies one should suggest selection bias and, secondly, to what extent it affects border estimates.

[21] It comprises only TDs as exporters and importers that belonged to the German Empire before WWI, i.e. interwar TDs 1-37, 44, 47, and 47a.

[22] The sample has been balanced, i.e. if $c_{ij,t}$ is not observed at all t, c_{ij} is omitted.

[23] The estimated coefficients must be read in logs, because the actual coefficient hinges on the unobserved elasticity of substitution σ_k equation, as (3.11) was plugged into (3.7).

Table 4: Conventional estimates of the border effect in the interwar period (sub-sample)

	Brown coal		**Chemicals**		**Iron and steel**		**Rye**		**Paper**		**Hard coal**		**Coke**	
Method	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML
Coefficient	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
$-\sigma ln(distance)$	-2.78	-2.36	-1.99	-1.27	-2.14	-1.24	-3.20	-2.95	-2.07	-1.22	-3.64	-2.19	-3.10	-1.72
	0.08	0.13	0.06	0.04	0.06	0.04	0.07	0.08	0.06	0.04	0.10	0.11	0.08	0.13
$-\sigma ln(b^{interwar})$	-1.09	-7.33	-2.93	-2.86	-2.68	-1.81	-1.60	-1.65	-3.59	-3.29	-2.17	-2.67	-2.60	-3.42
	0.51	0.65	0.43	0.62	0.48	0.48	0.58	0.68	0.35	0.59	0.64	0.40	0.52	0.47
$D_{time1933}$	0.61	-0.70	0.68	-0.99	-0.46	-0.40	-0.75	-0.73	-0.41	-0.25	-1.16	-2.03	0.25	1.06
	1.02	0.23	0.55	0.24	0.46	0.35	0.69	0.29	0.38	0.40	0.93	0.71	0.93	0.84
N	2912	2912	2884	2884	2912	2912	2908	2908	2904	2904	2908	2908	2912	2912
R^2	0.68	–	0.79	–	0.84	–	0.69	–	0.83	–	0.73	–	0.71	–

Notes: The table presents coefficient estimates of components of the trade cost function and their respective standard errors (in the row underneath). Coefficient estimates of the intercept, 1913 and 1926 time dummies and of time-varying, region-specific dummy variables were omitted. Following Eichengreen and Irwin (1995), the OLS estimates need to be multiplied by the scale factor in table 3. The covariance matrix of coefficient estimates is heteroskedasticity consistent.

Table 5: ATE decomposed into ATET and FET using the DDL approach (sub-sample)

	Brown coal		**Chemicals**		**Iron and steel**		**Rye**		**Paper**		**Hard coal**		**Coke**	
Method	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML
Coefficient	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
$-\sigma ln(distance)$	-2.78	-2.35	-1.99	-1.27	-2.15	-1.23	-3.17	-2.81	-2.05	-1.22	-3.63	-2.15	-3.08	-1.68
	0.09	0.13	0.06	0.04	0.06	0.04	0.07	0.07	0.06	0.04	0.10	0.11	0.08	0.13
$-\sigma ln(b^{tiw})$	-1.13	-6.25	-2.79	-2.89	-3.04	-1.68	-0.67	-0.57	-2.85	-2.97	-1.75	-2.11	-1.78	-2.75
	0.67	0.73	0.51	0.67	0.60	0.51	0.65	0.69	0.45	0.63	0.74	0.47	0.58	0.52
$-\sigma ln(b^{fet})$	0.03	-1.08	-0.14	0.03	0.36	-0.14	-0.96	-1.18	-0.76	-0.32	-0.43	-0.56	-0.84	-0.70
	0.43	0.35	0.27	0.26	0.35	0.16	0.30	0.11	0.29	0.22	0.39	0.25	0.28	0.24
$D^{time1933}$	0.62	-0.70	0.68	-0.99	-0.45	-0.40	-0.77	-0.75	-0.43	-0.26	-1.17	-2.04	0.22	1.06
	1.02	0.23	0.55	0.24	0.46	0.35	0.70	0.35	0.38	0.40	0.93	0.72	0.94	0.86
N	2912	2912	2884	2884	2912	2912	2908	2908	2904	2904	2908	2908	2912	2912
R^2	0.68	–	0.79	–	0.84	–	0.69	–	0.83	–	0.73	–	0.71	–

Notes: See table 4.

In order to test for potential selection bias, trade cost specification (3.21), which implements the DDL, is being evaluated. The results are given in table 5. Whereas the distance coefficients remain unchanged, border coefficient $b^{interwar}$ is now decomposed into the actual treatment effect b^{tiw} and the fixed effect of the treated (FET) b^{fet}. The estimation yields that the FET is negative and significantly different from zero for trade in brown coal, rye, hard coal, and coke (cf. table 5). This finding implies the the estimated border effect is not due solely to the presence of a (new) border: Trade pairs, which became subject to national borders first after WWI, are a non-random selection from the matrix of bilateral trade flows. The negative coefficients of the FET imply that selection bias in the naïve ATE is caused by the fact that, already before WWI, trade of the treatment group was significantly lower than of the control group. In economic terms, political borders that were put in place after WWI separated regions from Germany that were economically integrated below average already before the war. This finding is valid at least for trade in the selected groups of goods.

One question arising from my findings is why no FET is found in trade of chemicals, iron and steel semi-manufactures, and paper? One hypothesis could be that path dependency of informal barriers played a role only in the basic or traditional industries such as agriculture and mining. After all, trade relations in these sectors had developed over decades or centuries at the time the Versailles borders were put in place. As my focus here is on detection rather than explanation, this complex issue must be left for further research. An important result in this regard is that I am able to demonstrate that FET were economically and statistically significant in more than one relevant product class.[24]

Further below, I will analyze the evidence of a "border before a border" not only in this particular set-up, but also in the case of other borders and trade. If the latter was the case, it would suggest to view border effects in general with caution. After all, the estimated treatment effect of interwar borders decreases if FET are significant: For trade in the above mentioned goods, the estimation yields border effects b^{tiw} that are substantially lower than the ATE $b^{interwar}$.[25] The largest difference between b^{tiw} and $b^{interwar}$ can be observed in rye trade. The border coefficient is by two thirds smaller than the naïve estimate and becomes statistically insignificant. The border coefficient for hard coal and coke is smaller by one fifth and it is still smaller by one seventh for brown coal.

In order to attribute an economic meaning to these estimates, I compute ad-valorem tariff equivalents of the border coefficients. The theoretical model links tariff equivalents to the coefficient estimates by the goods-specific elasticities of

[24]Rye and coals alone accounted for about 5% of German exports in 1910 and for about 7% in 1926 (cf. the respective statistical yearbook).

[25]On top of the impact of demarcation, the trade conflict between Germany and Poland —intended to stop German-Polish trade altogether— should result in further detrimental effects, which would be attributed to the border coefficient b^{tiw}. Hence, the conflict can be regarded as a test against the hypothesis of selection bias: If such political intervention was successful, it would result in a larger than otherwise border treatment effect. Accordingly, it would result in a smaller than otherwise fixed effect, the indicator of selection bias.

substitution σ^k as given in equation (3.7) and explained above. Note that σ^k is inversely related to k's degree of aggregation: The higher the cost of substituting one variety for the other, the lower is the elasticity of substitution between varieties of the product.[26] Hence, one needs to assume a value for σ^k in order to make inferences on the coefficient b. AW (2004) review several studies on this issue and conclude that reasonable values for σ of total trade is likely to be between 5 and 10. Based on $\sigma^k = 5$ for all k, the border estimates in AW (2003, p.182) result in a tariff equivalent of 49% for the US-Canada border and 78% for the border between Canada and the rest of the world in 1995. Furthermore, AW (2004, p.717) obtain tariff equivalents between 77% and 116% for 19 OECD countries in 1990 based on border estimates by Eaton and Kortum (2002). Table 4 presents cost equivalents of my estimates calculated at different σ^k. A comparison with AW's (2004) figures suggests that border costs during the first half of the 20th century have been remarkably similar to the 1990s. Assuming $\sigma^k = 5$ my estimates imply tariff equivalents of the ATE between 39% and 98% or 333%, respectively. As an example of the computation consider rye trade: The coefficient estimate $-\sigma ln(b^{interwar}) = -2.67$ (cf. column (10) of table 4) implies a tariff equivalent of 70%, assuming $\sigma^{hardcoal} \equiv 5$. This figure is computed as $exp\left(\left\{-\sigma^{hardcoal} ln(b^{interwar})\right\} / - \sigma^{hardcoal}\right) - 1 = exp(-2.67/-5) - 1 = 0.70$. For the remaining goods, tariff equivalents of the ATE of new borders after WWI range from 39% to 333%. Because the assumption is implausible that $\sigma^k \equiv 5$ for all k, I evaluate elasticities suggested by Evans (2003a) and Broda and Weinstein (BW) (2006), which are based on empirical estimates of σ^k along the standard international trade classification (SITC).[27]

Table 6 summarizes tariff equivalents inferred from coefficient estimates based on the sub-sample and compares ad-valorem tariff equivalents of the border barriers computed from both the ATE ($b^{interwar,k}$) and the ATET ($b^{tiw,k}$). The comparison demonstrates that controlling for FET leads to different tariff equivalents of borders, if the FET is statistically significant. These differences in the trade cost are economically significant, independent from choosing sigma values according to Evans (2003a) or to BW (2006): Implied tariff equivalents are overstated by 4% (coke) to 42% (brown coal) and lower the original estimate by one tenth to two thirds. Considering again coal trade, the difference between the naïve and the actual estimates is $86\% - 119\% = -33\%$ assuming elasticities according to BW.[28] The difference of tariff equivalents obtained from ATE and ATET, is similarly large in trade of brown coal and rye, specifically -42% and -39%. Subsequently, tariff

[26] Whereas rye and coke are considered very homogeneous goods, varieties of coals, especially of hard coal, differ a lot in their product properties. The manufactured products are given in baskets implying heterogeneity.

[27] Evans (2003a) provides estimates at 2-digit SITC level, specifically $\sigma^{non-metallicminerals} = 2.7$, $\sigma^{chemicals} = 4.04$, $\sigma^{iron-steel} = 3.09$, $\sigma^{foodstuff} = 4.63$, $\sigma^{paper} = 3.89$. Based on unweighted averages of BWs (2006) estimates of σ^k at 3-5-digit SITC level, the following figures are inferred (SITC in parenthesis): $\sigma^{browncoal} = 7.2$ (3222), $\sigma^{chemicals} = 3.6$ (5239, 5410), $\sigma^{iron-steel} = 6.4$ (673, 674, 675, 679, 691), $\sigma^{rye} = 3.5$ (45), $\sigma^{paper} = 6.7$ (6411, 6412, 6413, 6415, 6416, 6417, 6418, 6419, 6421, 6422, 6424), $\sigma^{hardcoal} = 3.4$ (3221), $\sigma^{coke} = 18$ (3232).

[28] Assuming elasticities suggested by Evans would result in a difference of -50% tariff equivalent.

Table 6: Implicit border cost from ATE and ATET estimates for different σ^k

Elasticity / Good	ATE (ie. naïve)				ATET (ie. actual)				Difference	
	5	10	Evans	BW	5	10	Evans	BW	Evans	BW
Brown coal	333	108	1411	177	249	87	912	138	-499	-39
Chemicals	77	33	105	122	78	34	106	123	1	2
Iron, steel	44	20	79	33	40	18	72	30	-8	-3
Rye	39	18	43	60	12	6	13	18	-30	-43
Paper	93	39	133	63	81	35	114	56	-18	-8
Hard coal	70	31	169	119	53	24	119	86	-50	-33
Coke	98	41	255	21	73	32	177	17	-78	-4

Source: Benchmark elasticities are taken from Evans (2003a) and Broda and Weinstein (2006).
Notes: The table gives ad-valorem tariff equivalents computed from coefficient estimates in tables 4 and 5. Elasticities from Evans (2003a) are based in general on sectoral data at a higher degree of aggregation. Evans' figures applied to coals, coke, and rye are actually related to non-metallic mineral products and foodstuff, respectively. The difference between naïve and actual results is based on the benchmark.

equivalents are always based on BW (2006), because elasticities in Evans (2003a) are inappropriate considering the level of aggregation of the data. The elasticities are originally related to groups of goods, such as non-metallic mineral products and foodstuff, and hence are too low.

The first part of the analysis has shown that new borders caused similar trade diversion after WWI as OECD borders in 1990. Furthermore, I was able to demonstrate substantial differences between the treatment and control group for several traded goods. In the next part, I will extend the analysis to a larger set of observations including trade of all regions belonging either to prewar Germany or to interwar Poland.[29] Most importantly, the sample contains also data on exports of all 'German' and 'Polish' regions into European regions that are themselves not included as exporters, e.g. France, Italy, or Russia. The main difference is that this enlargement of the sample allows to compare the ATE of borders before and after WWI. This full sample includes region pairs that establish a further control group, because they remain continuously under treatment.

Estimation results are shown in table 7. One notable result is that prewar ATE were economically significant and even of similar extent as interwar ATE.[30] A hypothesis test, based on the PPML results, cannot prove that the ATE of borders increased strongly after WWI: The null hypothesis that b^{prewar} and $b^{interwar}$ are equal, is rejected at the 5% level only for trade of iron and steel.[31] This finding puts into perspective the introductory statement of Thomas (1996) that WWI and subsequent border drawing devastated a well integrated European economy. My result, on the contrary, suggest that cross-border trade integration did not deteriorate or even collapse altogether. Trade diversion due to WWI was not the consequence of stronger treatment by borders, but of the 35% increase in the number of trade flows under treatment.

[29] Effectively, this sample comprises two further exporting regions, East Poland and Galicia, which did not belong to Germany before WWI.

[30] Note that ATE estimate $b^{interwar,k}$ is not directly comparable to the similar coefficient in table 4.

[31] Evaluated by OLS results, the null hypothesis would be rejected more often though.

Implementing the DDL and applying the model to the full sample confirms the above evidence that FET are large for some goods. If controlling FET, the ATE can immediately be compared to the ATET, i.e. the impact of permanent borders b^{taws} to the impact of new borders b^{tiw}.[32] I find that the coefficient estimates of the ATET are always smaller than estimates of the ATE. This suggests that trade was less diverted from borders established by the peace treaties than from borders in place already before the war. The finding adds to the evidence that the standard approach can mistake the effective impact of borders. It also provokes the question whether FET pose a fundamental issue for the border literature. After all, I cannot exclude the possibility that $ATE \neq ATET$ for b^{taws} as well. One might suspect that the German-Polish tariff war or the apparent emigration of ethnic Germans out of Poland after WWI bias the estimation of b^{tiw}. After all, it was explicitly intented by both sides to cut economic ties between the two countries (Puchert 1963). In order to account for the particular conditions at the German-Polish border, the regressions were repeated allowing for the treatment effect of the German-Polish border to deviate from the average treatment effect b^{tiw}. The outcome of this extended model exhibits large goods-specific differences: For some goods the specific ATET of the German-Polish border is more negative than the ATET of the remaining borders. For other goods it is not different or even smaller. Political measures applied as part of the tariff war seem to have been effective in preventing trade of certain goods, but did not put an end to German-Polish trade altogether.

The intuition of the gravity model provides a consistent interpretation for the finding of relatively moderate interwar economic disintegration in Central Europe despite the war and national breakups. This explanation is closely related to the increased regionalization in Central Europe after the war (cf. Irwin and Eichengreen 1995). The model shows that bilateral trade depends on relative barriers and stresses the influence of relative economic size. Poland was a small economy, which could not rely on a large domestic market like all the other new Central European countries. Since border barriers, especially NTBs, vis-á-vis Western Europe increased strongly, the new countries were essentially forced into trade with Germany and with one another. The result mirrors Wolf's (2005) finding of path dependent trade relations within interwar Poland arising from the common history of regions: During the interwar period, Poland's internal trade was influenced by trade links with the former partitioning powers. This dependency attenuated disintegration, particularly between the new nation states and their trading partners.

[32] Note that I cannot control potential fixed effects in case of b^{taws} though.

Table 7: Average treatment effects of prewar and postwar borders compared (entire sample)

	Brown coal		**Chemicals**		**Iron and steel**		**Rye**		**Paper**		**Hard coal**		**Coke**	
Method	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML
Coefficient	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
$-\sigma ln(distance)$	-2.40	-2.38	-2.03	-1.28	-2.26	-1.27	-2.91	-2.96	-2.09	-1.21	-3.59	-2.37	-2.97	-1.76
	0.07	0.13	0.05	0.04	0.06	0.04	0.06	0.07	0.05	0.04	0.08	0.10	0.07	0.13
$-\sigma ln(b^{prewar})$	-0.62	-3.18	-2.52	-3.82	-2.92	-6.89	-0.29	-2.98	-2.84	-5.15	-1.25	-3.71	0.38	-6.03
	0.29	0.34	0.26	0.42	0.32	0.61	0.70	0.34	0.28	0.51	0.41	0.29	0.33	0.83
$-\sigma ln(b^{interwar})$	-1.18	-3.22	-3.11	-3.68	-3.05	-2.29	-1.59	-2.09	-3.76	-3.90	-2.46	-2.78	-1.86	-3.33
	0.17	0.41	0.19	0.29	0.18	0.24	0.20	0.61	0.14	0.35	0.25	0.27	0.20	0.45
$H_0 : b^{prewar} = b^{interwar}$	NO	NO	YES	NO	NO	YES	YES	NO	YES	NO	YES	NO	YES	NO
N	5068	5068	4732	4732	5024	5024	4896	4896	4968	4968	5040	5040	5044	5044
R^2	0.60	–	0.78	–	0.81	–	0.64	–	0.81	–	0.69	–	0.67	–

Notes: See table 4.

Table 8: The particular impact of the new German-Polish border (entire sample, using DDL)

	Brown coal		**Chemicals**		**Iron and steel**		**Rye**		**Paper**		**Hard coal**		**Coke**	
Method	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML	OLS	PPML
Coefficient	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
$-\sigma ln(b^{tiw})$	-0.94	-2.80	-1.76	-2.58	-0.47	-1.76	-1.35	-2.75	-2.67	-2.86	-0.92	-1.99	-1.30	-1.04
	0.33	0.62	0.37	0.51	0.41	0.39	0.34	0.92	0.34	0.83	0.51	0.41	0.38	0.53
$-\sigma ln(b^{tiw}) * d^{gpl}$	0.67	-4.16	-0.33	12.69	-3.21	-1.62	0.34	2.23	-0.70	-0.32	-1.80	-0.72	-0.26	-5.06
	0.37	1.00	0.36	0.74	0.47	0.66	0.42	1.11	0.34	0.94	0.56	0.60	0.44	0.68
$-\sigma ln(b^{fet})$	-1.12	-1.33	-0.65	0.09	0.34	-0.18	-0.87	-1.16	-0.71	-0.24	-0.15	-0.43	-0.97	-0.97
	0.22	0.36	0.24	0.30	0.22	0.13	0.20	0.13	0.20	0.21	0.32	0.19	0.20	0.35
$-\sigma ln(b^{taws})$	-0.84	-3.20	-3.14	-4.04	-3.49	-3.57	-0.83	-4.65	-3.40	-4.83	-2.00	-3.72	-0.55	-6.45
	0.18	0.27	0.19	0.29	0.18	0.53	0.36	0.44	0.18	0.40	0.26	0.32	0.22	0.48
$-\sigma ln(b^{diss})$	-2.76	-6.23			-2.97	-3.68	0.73	-2.89	-3.55	-5.51	-0.35	-2.71	-1.07	-3.48
	0.51	0.73	–	–	0.61	0.30	0.80	0.41	0.46	0.43	0.86	0.29	0.73	0.68
N	5068	5068	4732	4732	5024	5024	4896	4896	4968	4968	5040	5040	5044	5044
R^2	0.60	–	0.77	–	0.81	–	0.65	–	0.81	–	0.69	–	0.67	–

Notes: See table 4.

The dominance of Germany in Poland's trade after the war was furthered by the absence of Russian-Polish trade. Russia's introduction of almost prohibitive barriers to trade after WWI directly translated into a relative decrease in barriers to trade within Central Europe. This explains why Polish trade was so strongly oriented toward the German market despite Poland's attempt to avoid this particular economic dependency. Moreover, Germany itself faced large barriers to trade introduced by its Western trading partners. It appears, though, that also trade links between Germany, the Saargebiet, and Alsace-Lorraine improved substantially between 1926 and 1933, perhaps due to the special trade regulations between France and Germany concerning the Saargebiet.

Allowing border coefficients to change over time casts doubt on the suggestion that emigration of ethnic Germans from Poland after WWI is the dominant force behind economic disintegration. Whereas trade integration of both countries stagnated or even improved between 1926 and 1933, the share of Protestants in Poland continued to decrease between 1921 and 1931 according to table 1.[33]

The robustness of the main result —economically and statistically significant FET may bias the estimation of ATE of borders— is checked with regard to two criticisms. One issue is the way trade districts were aggregated in case of Silesia and West Prussia as discussed in section 4. Another issue is whether PPML is an appropriate estimator to deal with the data structure. In order to examine the first issue, I re-estimated (3.21) using data, which were compiled differently: In this data set, interwar aggregate TDs strictly follow the boundaries of prewar TDs instead of following the new national borders.

I find that the resulting coefficients for FET and for border treatment effects lead to the same inference as discussed above. As a pragmatic approach, I consider the coefficient estimate from the first set-up as upper bound and those obtained from the alternative approach as lower bound estimate. To understand why, it is useful to reflect on differences in the coefficients resulting from the respective approach. In the non-modified set of data, the assignment of border indicator variables relates to the actual border demarcation, but refers to TD that strongly differ in size before and after WWI. Yet, the issue is not so much the change of the area, but rather if the change in the TD's economy affected all of Upper Silesia's trade relations evenly.[34] East Upper Silesia (EUS) was highly industrialized (cf. Landau and Tomaszewski 1977) and hence resembled quite well the characteristics of prewar 'Upper Silesia'. Despite that, one can argue that East Upper Silesia might have been less well economically integrated into the German economy before WWI for ethnic reasons. This would result in overestimating the border effect.[35] Merging interwar TD 12 with 47 and TD 13 with 47a creates bi-national TD. In such a modified data set, the assignment of border indicators refers to an identical area

[33]It is highly unlikely that the proportion of ethnic Germans increased sharply again from 1931 to 1933.

[34]Time-dependent, area-specific intercepts account for the change in Upper Silesia's economic size as major negative economic shock. They do not account for a differential effect on trade relations, however.

[35]This overestimation is due to an illusion of deeper than actual integration between EUS and Germany before WWI, because the prewar statistics refer to the entire Silesian trade.

Table 9: Robustness check: Method of estimation and compilation of data

	Chemicals		Iron, Steel		Paper		Hardcoal	
			Count component					
$-\sigma ln(dist)$	-1.95	-1.93	-2.22	-2.20	-2.23	-2.18	-4.22	-4.20
	0.05	0.05	0.05	0.05	0.05	0.05	0.30	0.30
$-\sigma ln(b^{prewar})$	-1.99		-2.22		-1.87		-3.67	
	0.22		0.29		0.24		1.32	
$-\sigma ln(b^{postwar})$	-3.08		-3.10		-3.05		-2.16	
	0.24		0.17		0.21		1.02	
$-\sigma ln(b^{tiw})$		-4.15		-3.14		-3.16		-3.12
		0.54		0.24		0.31		1.24
$-\sigma ln(b^{tiw}) * d^{gpl}$								
$-\sigma ln(b^{fet})$		0.21		0.24		-0.42		-0.04
		0.31		0.13		0.17		0.65
$-\sigma ln(b^{taws})$		-2.25		-2.81		-2.09		-3.12
		0.18		0.18		0.18		0.96
$-\sigma ln(b^{diss})$				-2.43		-3.56		-0.66
				0.47		0.44		1.56
			Zero component					
$-\sigma ln(distance)$	-4.47	-4.06	-4.74	-4.39	-4.64	-4.26	-4.73	-4.64
	0.26	0.23	0.29	0.29	0.27	0.26	0.21	0.21
$-\sigma ln(b^{prewar})$	-1.76		-3.14		-2.47		-1.42	
	0.71		0.64		0.58		0.47	
$-\sigma ln(b^{postwar})$	-5.41		-5.00		-6.34		-4.11	
	0.59		0.44		0.55		0.40	
$-\sigma ln(b^{tiw})$		-0.09		-3.06		-4.51		-1.62
		1.07		0.75		0.71		0.59
$-\sigma ln(b^{tiw}) * d^{gpl}$								
$-\sigma ln(b^{fet})$		-0.26		0.05		-1.48		-1.07
		0.63		0.62		0.47		0.42
$-\sigma ln(b^{taws})$		-5.56		-5.95		-4.72		-3.98
		0.55		0.48		0.47		0.45
$-\sigma ln(b^{diss})$				-5.07		-3.64		-4.18
				1.05		0.95		0.96
N	4732	4732	5024	5024	4968	4968	5040	5040
Loglik (in '000)	-24.6	-24.6	-28.5	-28.5	-25.9	-24.9	-20.2	-20.2
iterations	116	118	146	136	150	149	161	170

Notes: The table presents coefficient estimates of components of the trade cost function and their respective standard errors (in the row underneath). All estimates obtained using a hurdle model (cf. Cameron and Trivedi 2005). A negative binomial distribution is assumed for positive counts. A binomial model is employed for the hurdle component. *N* gives the number of observations, *Loglik* the log likelihood of the model, and *iterations* indicates the number of iterations needed for the model to converge.

of the TD before and after WWI. As a serious disadvantage of this approach the indicator is not related to the actual border demarcation. Its assignment becomes imprecise, because some borders run through TD. Therefore, this approach will probably result in underestimating the actual border effect.

The second potential criticism addressed here is that the PPML estimator might be inappropriate for the data structure, specifically in dealing with a larger number of zero observations than the Poisson model allows for. Therefore, the robustness of my findings that fixed effects potentially bias border estimates is tested with respect to the estimator. To this end, I re-estimated (3.21) using a negative binomial hurdle estimator.[36] The hurdle estimator explicitly models two separate components in the

[36]Hurdle models were introduced into the econometrics literature in the 1980s. An overview of applications is provided by Cameron and Trivedi (2005). Additionally, I employed zero-inflated negative binomial regression (ZINB), i.e. negative binomial count model augmented with additional probability weight for zero counts. The results from ZINB do not change the conclusions

data: A binary component indicates the impact on the probability that two regions trade with one another, i.e. that trade is larger than zero. A count component is employed for positive counts in data only, hence it is truncated.

The results are presented in table 9. A notable problem of the hurdle estimator is that the algorithm does not converge in all cases due to the large number of regressors. The coefficients for the zero or hurdle component show that distance highly negatively affects the probability to trade. Whereas in the count component, interwar borders are not clearly more trade diverting than prewar borders, their negative effect on the zero component is much larger. Concerning border effects before a border, the count component is negative and significant for paper trade, but not for hard coal trade. The hurdle component is negative and significant for both paper trade and hard coal trade.[37] The resulting coefficients of FET and of border treatment effects lead basically to the same inference as discussed above. It seems though that fixed effects are slightly more important for the probability of trading than for the level of trade. This finding is reasonable if one suggests that path dependency of trade —reflected in the fixed effect— often originates in a common 'trade history' of two regions.

from the hurdle estimator. For the application of count data models cf. Zeileis, Kleiber, and Jackman (2008).

[37]Some coefficients become instable when the interaction term capturing the impact of the German-Polish border ($ln(b^{tiw}) * d^{GPL}$) is introduced. Even in these cases though, the estimates of fixed effect become larger.

6. Conclusions

The object of this study was to evaluate the impact of new border demarcation on trade. It took into account the possibility that estimation of border effects is subject to selection bias. This aspect has been widely ignored in the literature on border effects, but has recently attracted attention in research on currency effects on trade. The study examined the effects of political disintegration in Central Europe after WWI for its consequence on economic integration. It did not directly link economic (dis)integration to WWI, but instead made inferences based on the creation of barriers that the war precipitated.

Taken together, there are four main results of my evaluation of regional trade. First, the analysis confirms the suspicion of unaccounted-for fixed effects in bilateral trade in the subset of observations under border treatment. The statistical significance of the FET indicates a systematic self-selection. Pairs of regions that became separated by a new national border after WWI tended to have below average levels of economic integration before the war. A comparison of actual and biased result for goods-specific trade demonstrates the economic significance of this result: In the benchmark case, the proportion of the border effect in terms of ad-valorem tariff equivalents which can be explained by the FET amounts to one tenth to two thirds, and might well be higher for certain goods. In order to calculate the treatment effect of new borders correctly, it is necessary to control for the potential "border before border" effect, e.g. by estimating difference-in-differences. If this is impossible, the estimate represents an upper bound at most.

Secondly, border effects after WWI were negative and large. However, their negative impact on trade was not extraordinarily larger than the impact of Central European borders before the war. An exception to this finding is given by trade in coals and coke, which was much more diverted across borders after the war than beforehand. The impact of hostile trade policies around the year 1930 is even less apparent. Evidence is presented that in several sectors regional trade integration across borders was substantially higher in 1933 than in 1926. This finding sharply contrasts with both the overall development of tariffs and of NTBs as well as with expected effects of the German-Polish tariff conflict.

Thirdly, in the interwar period international trade was less diverted by borders first established after the war than by borders already existing before WWI. This result indicates path dependency of bilateral trade relations across national borders. Apparently, the long-established inter-regional division of labor persisted after the war. Fourthly, pairs of regions that became politically unified after WWI, namely the Polish regions, were economically integrated comparatively well across prewar borders.

There are two interrelated explanations for a high degree of economic interdependency across borders within Central Europe. One reason could be that Germany had to service huge reparations to the allies, despite the fact that these countries had largely closed their markets for imports. Therefore, its Eastern European neighbors gained importance as trading partners. This argument points to a more general explanation, namely that the struggle for economic independence was a sign of small home markets. Independent of political intentions, Central European countries depended strongly on trade with their neighbor countries. They were to a certain extent forced into trade with one another despite the demarcation of new borders, the common announcement of economic self-sufficiency after the war, and hostile trade policy around the year 1931. These explanations fit well with Eichengreen and Irwin's (1995) findings of regionalization of world trade during the interwar period.

CHAPTER 3

On the Economic Consequences of the Peace: Trade and Borders after Versailles

The First World War radically altered the political landscape of Central Europe. The new borders after 1918 are typically viewed as detrimental to the region's economic integration and development. We argue that this view lacks historical perspective. It fails to take into account that the new borders followed a pattern of economic fragmentation that had emerged during the late nineteenth century. We estimate the effects of the new borders on trade and find that the "treatment effects" of these borders were quite limited. There is strong evidence that border changes occurred systematically along barriers which existed already before 1914.

1. Introduction: Were borders created after WWI economically sound?

The First World War and its immediate aftermath fundamentally changed the map of Central and Eastern Europe. The Habsburg Monarchy was dismembered and its territories and population were either incorporated into new nation states or ceded to neighboring countries. Austria and Hungary were reduced to small landlocked countries, while Czechoslovakia, Yugoslavia and Poland emerged as independent nation states. The lands once tied together in a political, customs and monetary union of more than 51 million inhabitants were now separated by no less than eleven national borders. Germany lost Alsace and Lorraine to France, northern Schleswig to Denmark and extensive Prussian territories in the east to the reborn Polish nation state whose control also extended over lands previously under Austrian and Russian rule.

While in some cases the new borders had become a *fait accompli* already in late 1918, almost all changes in state borders were discussed and codified in a series of post-war treaties concluded at the Paris Peace Conferences of 1919–20. The Treaty of Versailles determined defeated Germany's new borders with France, Belgium and Denmark and its eastern frontiers with Poland and Czechoslovakia.[1] Dismembered Austria's new borders with Italy, Czechoslovakia and Yugoslavia were fixed in the Treaty of St. Germain. Signed by a severely truncated Hungary, the Treaty of Trianon imposed the new state's fiercely disputed new frontiers with Czechoslovakia, Romania and Yugoslavia. Defeated Bulgaria signed the Treaty of Neuilly which determined its new borders with Yugoslavia, Greece and a territorially now much enlarged Romania. Finally, the Treaty of Sèvres sought to redefine the borders between Turkey as successor to the Ottoman Empire in Greece's favor, but its stipulations were eventually superceded by those of the 1923 Treaty of Lausanne. The latter significantly, also "ratified Europe's only large-scale post-1918 'population exchange', whereby about 1.1 million Greek refugees [. . .] were resettled in Greece and about 380,000 Muslims were expelled from Greece to Turkey" (Bideleux and Jeffries, 1998, pp. 408). This was the one major deviation from the proclaimed aim of the architects of Europe's post-war order to re-draw the new state boundaries along the lines of the existing ethnic distribution of population rather than 'relocating' populations to fit already existing state borders.

The peace negotiations and their outcomes have received a bad press from Keynes's devastating critique onwards. Apart from the settlement of war debts

[1] The German-Polish frontier was finalized in 1921.

and reparations, the many dramatic border changes that were imposed by the peace treaties are frequently listed among the major causes of Central Europe's economic difficulties during the inter-war years. Implicitly or explicitly, the situation after 1918 is contrasted with a seemingly well-integrated region in 1914: "The interference of frontiers and of tariffs was reduced to a minimum [...] within the three empires of Russia, Germany, and Austria-Hungary" (Keynes, 1920, p. 13). The changes of the European map in 1919 are regarded as responsible for the disruption of a pan-European division of labour, which "represented a major shock to the international economy. It was a cause of widespread resource misallocation, resulting in lower output and higher prices, particularly in Central and Eastern Europe." (Feinstein et al., 1997, p. 32).

There can be little doubt that the 7,000 miles or so of new customs borders across Europe after 1918 did not help economic integration in Central Europe. However, in order to gauge the economic effect of these border changes, one needs to acknowledge a post-war historical situation which left little, if any, room for the extreme alternative of no revisions to state boundaries. The policymakers at the Paris peace conferences, literally, faced and assumed the task of redrawing the map of Europe. The collapse of the ruling dynasties in Austria-Hungary, Germany, and Russia, which had left a power vacuum in its wake, made taking up the question of new borders practically inevitable (cf. MacMillan 2004, Steiner 2005). One way or other, they had to address the demands and aspirations of the national independence movements that originated in the 19th century and that strived for the creation of new, sovereign nation states in Central and Eastern Europe. This holds irrespective of whether or not particular national movements enjoyed the support of the victorious Allies and regardless of whether the new borders imposed were politically 'right', economically 'optimal', historically 'sound' or socially 'fair'. Put differently, we argue that the economic effect of the new borders should not be assessed against a theoretical counterfactual of perfect integration along the political borders of 1914 as Keynes implied. Rather, an assessment of the costs and benefits of border changes needs to take into account the many lines of fragmentation that characterized the region.

Given that we cannot observe directly the preferences of the people of Central Europe except in the very few circumstances where citizens could actually vote on changes in political borders that affected them[2], we cannot calculate the welfare effects of observed or counterfactual border changes. It is equally difficult to assess how individuals valued direct political participation or a government formed by members of their own national or ethnic group. Hence, it is difficult to assess the economic benefits of the new borders. However, it is fairly straightforward to estimate some of the economic costs imposed by the new borders, namely the costs they entailed on trade. We hypothesize that these economic costs were considerably lower than the conventional view maintains. There are good reasons to think that the new borders were not drawn randomly but rather tended to follow an

[2]Note, though, that even when plebiscites were held, the results were not always accepted, as in the case of the transfer of Eupen-Malmedy to Belgium or the eastern part of Upper Silesia to Poland.

already existing, pre-war pattern of fragmentation across the region. For example, the evidence from grain prices suggests that the disintegration of the Habsburg Empire started some 25 years prior to the Great War and did so roughly along the future borders. In a sample over the period 1878-1910 it can be shown that grain prices started to be diverted along the post-war borders from about 1883/84 onwards. This effect peaked in the 1890s before it stabilized (Schulze and Wolf, 2009). Similarly, the data on trade in several major commodities indicate that the eastern (Polish-dominated) parts of the German Reich started to integrate with the Polish parts of the Russian Empire already prior to 1914 (Müller, 2008). Like Alsace-Lorraine in the west of Germany, they remained poorly integrated with other German regions before the First World War. If so, the new borders may just have codified already existing lines of fragmentation without any direct effect on trade costs and hence trade. However, we note that this codification may have had implications in the longer run, especially during the Great Depression.

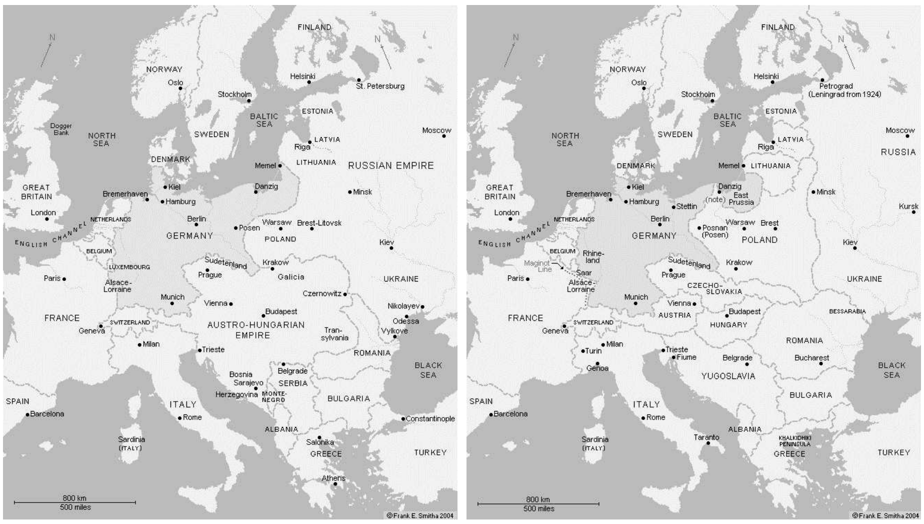
We explore our hypothesis within the framework of a gravity-model (McCallum, 1995; Anderson and van Wincoop, 2003), employing a difference-in-difference estimator (Ashenfelter, 1978; Ashenfelter and Card, 1985; Ritschl and Wolf, 2011). In conjunction with an extensive new data set on trade flows at sub-national (regional) level, that spans over time and across regions, this strategy allows estimating the actual 'treatment' effect of changes in political borders between countries as distinct from the impact of unobservable pair-wise heterogeneity across regions. In essence, the approach and the data permit overcoming a serious identification problem that has plagued so many studies on the impact of political borders on trade. The problem stems from the practical inability to observe directly the trade between identical pairs of regions at any given time both with and simultaneously without a border between them. Yet it is, precisely, this problem that so far has made it so difficult to assess the impact of re-drawing the map of Europe on trade and integration after the Great War.

Here, we estimate the change in trade costs as a result of a change in borders between pairs of regions, controlling for all forms of pair-wise heterogeneity that historically characterize these regions. We do this using a difference-in difference approach. One set of differences is over time, namely the difference in trade between regions that were separated by a new border (such as Brandenburg and West-Prussia) after 1918 with trade between the same regions prior to the war. The other set of differences is in the cross-section: the difference in trade between regions separated by a new border (Brandenburg and West-Prussia) with trade between regions that were not (for example Brandenburg and Lower Silesia). The difference-in-difference approach combines these two and estimates how the change in trade before and after the border differed between 'treated' and 'non-treated' regions. Hence, we can estimate something that the data typically does not allow us to do: the 'treatment effect' of a new border on trade, controlling for frictions that existed prior to the change in borders.

The rest of this paper is organized as follows. Sections 2 and 3 provide some historical background to the border changes in 1919–20 and to tariff and non tariff barriers between 1880 and 1940, respectively. Section 4 describes the new dataset of regional trade flows across Central Europe 1885-1933, before we set out our empirical strategy in section 5. The basic empirical results are presented and discussed in section 6, followed by several robustness checks in section 7, including a comparison among adjacent regions only, a test for a 'counterfactual border', and a distinction between effects before and during the Great Depression. In section 8 we explore to what extent the pattern of ethno-linguistic heterogeneity between regions can explain our previous findings and speculate about other explanations. Section 9 summarizes the main findings and concludes.

2. 'Re-formatting' Central Europe: The Paris Peace Treaties and After

Maps 1a and 1b document a geo-political shake-up, namely the dramatic shifts in state boundaries brought about by the Great War and the post-war settlements, especially so with regards to Central and East-Central Europe. Most significantly, the Habsburg Empire was broken up, its territories divided between several independent successor states and neighboring countries. German territorial and population losses, though considerable, were modest in comparison to those inflicted on Austria, Hungary, Bulgaria and Russia. Poland was resurrected as a sovereign nation state and re-fashioned from lands that since the Partitions of Poland (1772–1795) had been under Habsburg, Hohenzollern and Romanov rule. Germany lost most of the Prussian provinces of Posen and of West Prussia, and the eastern part of Upper Silesia, whilst Austria ceded Galicia, with an ethnically very diverse population, to the new Polish state that also incorporated the formerly Russian Kingdom of Poland and other eastern territories annexed from Russia and Lithuania. Czechoslovakia was created from regions previously under Austrian (Bohemia, Moravia, Silesia) and Hungarian (Slovakia, Carpathian Ruthenia) rule and was to include large German and Hungarian minorities which, together, made up more than a quarter of the new state's population. Yugoslavia was formed from the union of former Habsburg lands (Croatia-Slavonia, Slovenia, Bosnia and Hercegovina) with Serbia and, later, Montenegro. Romania's territorial acquisitions included Transylvania from Hungary, Bessarabia from Russia, the Dobrudja from Bulgaria and the Bukovina from Austria.



(a) Europe, 1914 (b) Europe, 1921

Figure 1: National demarcation in Europe before and after WWI

Although the processes of territorial change and new state formation had started already before the Paris Peace Conferences of 1919–20, and continued thereafter as in the case of the Polish-Russian war that ended with the 1921 Treaty of Riga, the main markers of how the post-war map of Central and East-Central Europe was to look like were put down in Versailles, St. Germain, and Trianon. Yet it was not only lands and territories that were re-allocated, but those living there were brought under new political authorities. Initially, the victorious powers attempted to organize this re-shaping of the region after 1918 roughly along the lines of President Wilson's Fourteen Points (Boemeke et al., 1998, pp. 371ff.), in part to enhance state legitimacy and the process of democratization in Central Europe, in part to provide for a *cordon sanitaire* against Soviet Russia and Germany. A proclaimed key principle was that of 'national self-determination', apparently intended to put Central Europe on a path toward Western-type democracies. The territorial section of the peace conference noted that 'frontiers should be drawn as far as possible on ethnic lines' (Schultz, 2002, p. 112). That is, the prospective 'nation states' were defined first and foremost in terms of ethnicity. However, and perhaps unsurprisingly, the very notion of national 'self-determination' proved deeply problematic as a guiding principle for territorial change when applied in context of the ethnically heterogeneous settlement patterns that were prevalent in much of Central Europe. Different nationalities often shared the same territories. In many cases the very classification of people into 'ethnic' or 'national' groups was difficult or impossible (Mick, 2005). Moreover, the national movements in Central Europe attempted to maximize the political and economic power of their new states, which resulted in numerous territorial disputes, for example, between Poles and Czechs over railways or between Poles and Germans over access to the sea (Schultz 2002, pp. 116f). The fundamental problem of the doctrine of national self-determination, at least from a political perspective, was the underlying belief that borders in Central and East-Central Europe should (and could) be drawn 'in accordance with historically established lines of allegiance and nationality'. This, Bideleux and Jeffries (1998, p. 408) argue, effectively ignored the realities in a part of Europe where 'nations and nationalities were relatively recent mental constructs, which were not even based on uniformly applicable criteria. They were variously defined in terms of 'national' language or 'national' religion or common territory or shared history or dynastic allegiance'. Whichever way the borders would be drawn, one nation's gain and 'justice' were often another's loss and 'injustice'.

How did the new, post-1918 borders come about? This is not the place to review in detail the extensive historiography on the peace settlement. However, the literature points to several factors that governed post-war border changes. With the exception of Poland, whose resurrection as a sovereign nation-state had already been agreed amongst the Allies during the war, well before the start of the peace negotiations (albeit crucially without agreement on its eventual boundaries)[3], at first President Wilson did not intend to establish new borders in Central Europe when he argued for the principle of national self-determination in his famous Fourteen Points of January 1918. For the case of Austria-Hungary, he rather envisaged

[3]Poland's borders with Germany were finally settled only after a plebiscite in 1921 and those with Russia in the Treaty of Riga (1921) ending the Polish-Russian war.

a multi-national federation within the old boundaries, supported by the British government until mid-1918 (see Ádám, 2004; Boemeke et al., 1998; Schultz, 2002). This position, however, became progressively unsustainable in light of events 'on the ground' and growing pressure from the independence movements of the nations that had lived under Habsburg rule. By the time the Paris Peace Conferences started, the new state of Czechoslovakia, for example, had already been recognized by the Allies and had most of the territories it wanted in its possession (MacMillan, 2004, pp. 240-253). The redrawing of Central Europe's map - before, during and even after the conferences - was a messy process where historical claims on territory, geographical and strategic concerns as well as claims for ethnic homogeneity of states' populations clashed. The profound dilemmas faced by the Peacemakers of 1919-20 are only too evident in the case of Poland which Wilson had initially envisaged to include territories that are 'indisputably Polish'. This could mean, on the one hand, a Polish state in its historically widest extent, thus including a large number of non-Poles. Or, on the other, only the Polish 'heartlands', which would have implied a large number of Poles remaining outside of the new state (MacMillan, 2004, pp. 207-239). Both definitions seemed out of line with the principle of national self-determination. Eventually, Schultz (2002, p. 111) argues that

> "an ethnic principle was established for the Polish state [...] as well as for the Italian frontiers [...]. A historical principle was used to determine the borders between several Balkan states [...]. All these were combined with geopolitical and economic considerations [...]." (Schultz 2002, p. 111).

On the evidence of an 'American expert' cited in MacMillan (2004, p.247), the case for Czechoslovakia's post-war borders was argued on the basis of both 'historic frontiers' with reference to Bohemia which included a large German minority and, at the same time, 'the rights of nationality' with reference to Slovakia which for a long time had been politically part of the Hungarian state. Hence, the evidence on the principles shaping the process of border changes is far from conclusive. As Alan Sharp put it "the signature of the armistice ensured that the map [...] would be recast by the peace conference but the extent and method of this reshaping remained obscure" (Sharp, 1991, p. 102). On balance, though, Schultz (2002, p.115) maintains "economic arguments tended to come third, after history and nationality" in the process of redrawing state boundaries in Central and Eastern Europe.

What do we know about the effect of these new borders on Europe's economies? A host of contemporary (mainly German or Austrian) publications in the 1920s and early 1930s argued that the new borders dismembered previously well integrated economic areas, with devastating consequences for trade and production (e.g. Haver 1933, Stähler 1929, Luschin von Ebengreuth 1921, Michaelis 1919) and this is still conventional wisdom in the modern literature (e.g. Aldcroft and Morewood, 1995, pp.1-11; Feinstein et al., 1997, pp.25-32). However, to our knowledge the only empirical study that makes a serious attempt to trace the effects of new borders on trade with the statistical tools available in the interwar years (Gaedicke and von Eynern, 1933), comes to a surprising result:

> “[In] the rebuilding of European integration after the war only gradual dislocations occurred, which could alter in no way the fundamental equilibrium within European trade relationships.” (Gaedicke and von Eynern 1933, p.35)

This motivates the key questions of our paper. Did the Paris Peacemakers succeed after all in redrawing the European map in such a way that limited damage to trade? Did the new borders reflect a pattern of trade fragmentation that was already present prior to the war, following ethnic, geographical, historical or other lines of fragmentation? And if so, to what extent did the new borders impose additional barriers to trade?

3. Between free trade and protectionism. Trade policies and tariffs 1880–1940

In the late 19th and early 20th century, tariffs constituted the most important official measure affecting trade. Non-tariff barriers (NTB) were seldom applied until 1932 (cf. Liepmann, pp.39–41). In the context of this study it is important to know how specific tariffs developed, as they increasingly became an instrument used selectively to penalize certain cross-border trade. After the end of a phase of free trade around 1880, European governments began to institute tariff barriers. In the 1890s, for instance, German Chancellor von Caprivi intended to replace imports of raw materials mainly from the U.S. with imports from Central and Eastern Europe (Bairoch 1989, pp.61–62). Thus, Germany concluded agreements with Austria-Hungary in 1891 and Russia in 1894 granting exemptions from barriers to trade in agricultural products.

The spread of protectionism in the decade before WWI resulted in the splitting of Europe into a region with high tariffs in Eastern Europe and comparably low tariffs in the rest of Europe (see table 3 on page 224). Before WWI, the Kingdom of Poland's exports were almost completely oriented towards the Russian market. Tennenbaum (1916) holds the very high Russian external tariffs responsible for the overbearance of Russia in the Kingdom's exports. Some tariffs even prohibited foreign trade completely, e.g. tariffs on natural resources. Thereby, Poland was almost completely cut off from certain Western trade despite its proximity to Western Europe and had to import many goods from remote Russian deposits, such as coals from the Donetzk region.

When the war ended, the creation of new states in Central Europe turned intra-national into inter-national trade. Unstable political systems in Central and Eastern European countries evoked economic crises, e.g. hyperinflation in Poland and Germany during the early 1920s. Despite the tense economic situation, concerted action for implementing a new European economic order was not taken after the war. The dearth of attention given to such a goal in Woodrow Wilson's Fourteen Points provides further indication that this was apparently not foreseen.[4] The struggle for economic independence caused a rise in tariff levels throughout Europe. By 1927, Eastern Europe had experienced a strong increase in tariff levels, although tariffs in this region were already well above the European average in 1913. Polish tariffs constituted an exception, because they were high but still lower than they had been under Russian rule. Soviet Russia aimed at autarky and independence from the capitalist economies. It restricted imports to the minimum, which brought Russian-Polish trade virtually to an end (Tomaszewski 1970). Contrary to Eastern Europe, tariff levels in Western Europe, Germany, and the successor states of Austria-Hungary remained modest.

[4]Only one point, the third, of the Fourteen Points issued by U.S. President Wilson on January 8, 1918 was concerned with the economy and was not even explicitly aimed at Europe: "[The program of the world's peace . . . is this:] III. The removal, so far as possible, of all economic barriers and the establishment of an equality of trade conditions among all the nations consenting to the peace and associating themselves for its maintenance."

A remaining issue is the extent to which railway tariffs were differentially applied to trade within and between the regions of interest. Reviewing the literature, anecdotal evidence on tariffs as a systematic protectionist measure is rather weak. Biegeleisen (1921) and Filippi (1910) analyze Galician trade with other partition areas before WWI. Biegeleisen (1921) condemns the trade policy of Russia and Austria-Hungary as being detrimental particularly for Polish-Galician trade. Filippi (1910) holds the Austrian rates in general responsible for the fact that in Lemberg in 1910 Silesian coal cost about 15% less than Galician coal. Neither of them mentions systematic discrimination of Galician trade by railway tariffs. Lewy (1915) analyzes the specific conditions for the Kingdom of Poland. He stresses that Polish railways were a priori excluded from Russian differential tariffs on rye, pig iron and coal, since reductions only applied to shipments above 500 km —a distance which could not be reached within the Kingdom. Coals from certain Russian deposits were charged with especially low rates. Usually, these rates did not cover the costs of transport and, therefore, were financed by higher rates in the periphery (Lewy 1915, p.144 and pp.190–199). Nonetheless, Poland imported much of its coke from Upper Silesia, because of the adverse circumstances for shipments within Russia. As for the interwar period, Landau and Tomaszewski (1977) only mention efforts of the German government to affect Polish trade by transport rates in favor of German harbors. It seems that, if anything, differential tariffs favored the transport of foreign goods, dampening the impact of border barriers.

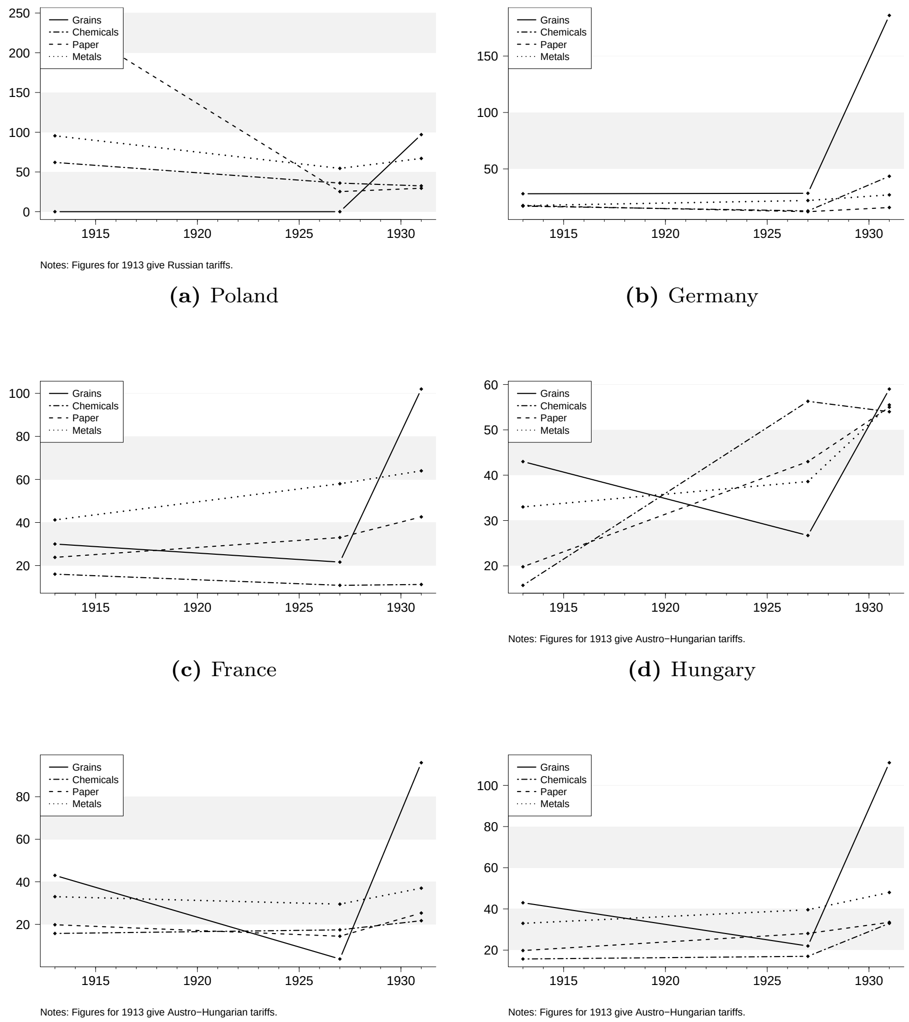
Liepmann (1938) has compiled probably the most elaborated account of tariff levels in Europe in to compare prewar and postwar rates. He calculated *potential tariff levels* in 15 important importing countries for 144 commodities and three years, 1913, 1927, and 1931. For those countries, which applied fixed-rate tariffs, he computed implicit ad-valorem tariffs based on c.i.f. prices and imported quantities. In case a country did not import a certain commodity at all, Liepmann inferred on its 'potential tariff' by applying its tariff rates to the other countries' average import prices. From these figures, he derived product- and country-specific averages of ad-valorem tariffs.[5]

Figures 2a–2f present the resulting tariffs, which are relevant for this study, i.e. for six European countries and four groups of products in our sample (The underlying tariffs can be found on page 224). Liepmann's (1938) account did not comprise any information on tariffs in coal and coke trade. The likely reason is that only few countries imposed tariffs on such trade at the time.

The literature usually conveys the impression that the interwar period was characterized by frequent increases in tariffs and retaliatory non-tariff measures. Liepmann's account shows that this description applied mainly to agrarian tariff policy. Despite numerous commercial treaties in the years 1926-27, to counteract the steep rise in tariff levels at the beginning of the 1920s, the German general tariff level increased to over 80% and the Polish level to over 100% in 1931. The picture of hostile trade policies fits less well the development in industrial sectors. Tariffs

[5]Liepmann calculated unweighted good-specific averages arguing that these have proved to yield results similar to weighted averages of ad-valorem tariffs (1938, pp. 25-26).

in many other sectors rose strongly between 1922 and 1925, but the overall tariff level remained at only 5% above the level of 1913 (Liepmann 1938, p.104).



(a) Poland **(b)** Germany

(c) France **(d)** Hungary

(e) Austria **(f)** Czechoslovakia

Figure 2: Import tariffs in Central Europe 1913, 1927, and 1931 (in percent)

So, how high were tariffs in coal and coke trade before WWI and thereafter? Between 1850 and 1914 many countries had imposed tariffs on imports of coal, but usually only for a short period of time.[6] The primary goal of these tariffs was the generation of state income, but there was also a growing tendency toward

[6]The list of countries includes Bulgaria, Denmark, Germany, the UK, France, Romania, Russia, Switzerland, Serbia, Spain, Turkey, and Hungary (Lenz 1933).

Table 1: Ad-valorem and fixed-rate tariffs on coal imports, interwar period

Country	**Year**	**Type**	**Ad-valorem**	**Fixed-rate** Tariff per metric ton			
				Currency	Coals	Coke	Briquettes
Germany	1931	general	2%		-	-	-
France	continous	minimum	-	Franc	1.20	1.20	1.20
Ireland	1932	discriminatory (only UK)		Pound	5	-	-
Italy	continous	n.a.	-	Lira	-	5	-
	1931	general	10%		-	-	-
	1932	additional	-	Lira	3	15	3
Yugoslavia	continous	n.a.	-	Dinar, gold	-	-	-
- at border			-		7	n.a.	n.a.
- seaway			-		0.5	n.a.	n.a.
Portugal	n.a.	general	-	Escudo, gold	1.1	1.1	1.2 - 4.5
		minimum	-		0.35	0.35	0.4 - 1.4
Switzerland	n.a.	n.a.	-	Franken	1	1	1
Spain	n.a.	general	-	Peseta, gold	22.5	27	27
		conventional	-		7.5	9	9
Turkey	n.a.	general	-	T.Pound	2	3	n.a.
Hungary	n.a.	Customs fee	4%		-	-	-
		Discriminatory VAT for coal	2%		-	-	-

Source: Lenz (1933), pp.50–58.

protectionism, especially in Russia. High Russian tariffs at its Western border had the primary aim to divert exports of coal from Upper Silesia to Russia and its Polish part (Lenz 1933, pp.9–10).

In addition, some countries applied coal export tariffs, but these were only of minor importance, except for the UK between 1901 and 1906. A publication of the League of Nations (1927) listed only four countries that applied tariffs in 1913, namely France, Romania, Serbia, and Spain. Although this list might be incomplete (it certainly omitted Russia), it still proves that import tariffs on coal and coke were all but common at the time.

With only few exceptions, coal imports in most European countries remained free from duties during the 1920s. We were able to derive ad-valorem duties in case of four countries. The figures give a 2% general import tax in Germany since 1931, a 6% mark-up in Hungary composed of a discriminatory VAT and a customs fee, and 1% in France resulting from a fixed-rate tariff of 1.20 Franc per ton of coal.[7] Italy introduced a 10% ad-valorem tariff on coals and coke in 1931 and further fixed-rate tariffs in 1932.[8]

During WWI, European nations were trying to assure the continuous supply with coal and coke. Thus, the attention shifted away from taxing imports toward controlling the outflow of coal. After the war, rules aiming at the prevention of coal exports were quickly abolished (Lenz 1933, pp.13–14). Another aspect of the special nature of coal and coke were the rules set in place by the treaty of Versailles. Between 1920 and 1928, it required Germany to ship to France 9%–15% of its production as reparations 8% in 1929 and 4% in 1930. On the other hand,

[7] We calculated this ad-valorem rate from ton prices of *Toutvenant gras 30/35* coal from France, which cost FRF 121 in 1927 and FRF 113 in 1932 (prices taken from Lenz 1933)

[8] Figures taken from Lenz (1933, pp.50–58 and pp.87–150).

Table 2: The development of NTB in coal and coke trade after WWI (selected countries)

Country	Type	Year	Countries mainly concerned
Germany	licensing	1922	UK, ČSR, Saargebiet, Netherlands, France
Germany	quotation	1924	UK, ČSR, Saargebiet, Netherlands, France
Germany	Prohibition	1925	Poland
Germany	licensing	1932	Poland (exports into German Upper Silesia)
Germany	Discriminatory railway tariffs	1929	UK, Netherlands (indirect measures already since 1925)
France	licensing, quotation	1931	Germany, UK, Belgium
Poland	Discriminatory railway tariffs	1932	UK
Portugal	Discriminatory railway tariffs	1932	UK, Germany (indirect measures)
Hungary	licensing	1931	Poland, ČSR, Yugoslavia
Belgium	licensing, quotation	1931	Germany, Netherlands, UK, France
Austria	licensing, quotation	1932	Poland, ČSR, Germany
ČSR	quotation	1932	Poland, Hungary
Romania	quotation	1932	Poland
Yugoslavia	Discriminatory railway tariffs	1931	Germany, Poland, ČSR

Source: Lenz (1933), pp.27–33, pp.64–65.
Notes: 'Indirect measures' denote subsidized railway tariffs for domestic coal.

Germany was obliged to buy 500.000 tons of coal per month from Poland between 1922 and 1925, because of the German-Polish agreement on Upper Silesia in 1922. During the first years after WWI, Czechoslovakia too was obliged by Versailles to export hard coal and especially brown coal to the other successor states Austria, Poland, and Yugoslavia (Lenz 1933, pp.19-20).

The selection in table 1 suggests that even if tariffs or comparable duties were introduced, the rates remained at low levels compared to other commodities. Instead, some countries, notably Germany, began to make excessive use of non-tariff barriers (NTB). These measures included quotations, licenses, rules as well as discriminatory railway tariffs. On top, quotas were not fixed, but usually lowered over time. There were as well first attempts of prohibition of imports, i.e. import stops. In general, NTB became an important measure only after 1930.

Coal and coke trade provides a good example for alternative measures that were applied instead of tariffs. Moreover, they demonstrate how protectionism during the world economic crisis culminated in a wave of new measures.

4. Description of data and sample

This study draws on an extensive new data-set that comprises exports of 31 into 43 Central European regions before and after the First World War, including a total of about 50,000 observations. The data-set covers six years, namely three years (1885, 1910 and 1913) before and three years (1925, 1926, and 1933) after the First World War. All border changes in our country sample occurred within 1919–21, hence well after 1913 and well before 1925. Due to the chaotic political (war, revolution) and economic (hyperinflation) circumstances, data for the period 1914 - 1924 are either unavailable or unusable. The focus is on railway shipments (accounting for about 85 per cent of total trade of the region) of seven commodity groups which represent a wide array of sectors of the economy: rye — an important agricultural product; brown coal and hard coal — natural resources used for power generation in industry and transport and for domestic heating, as well as coke, which is a key input in iron and steel making. Furthermore, the data-set covers three groups of processed industrial products: iron and steel (semi-) manufactures, cardboard and paper-products, and finally chemical products.

The main data sources are two series of railway statistics published annually by the German authorities from 1885 onwards. Up until 1909, the Prussian Ministry of Public Works and, thereafter, the Imperial Statistical Office published the *Statistik der Güterbewegung auf Deutschen Eisenbahnen* (Statistics of the Movement of Goods on German Railways). After the war, their successor, the German Statistical Office continued the series nearly unchanged throughout the 1930s. These series document railway shipments between all parts of Germany in 1914 borders —split into 27 transport districts (TD)— and shipments between them and 16 European neighboring countries or regions. We extended this dataset significantly by adding commodity-specific railway shipments of three districts covering the Habsburg Empire (or its successor states, respectively) and of the Kingdom of Poland. For the pre-war period, this involved reconstructing trade flows for the Russian Kingdom of Poland and the different regions of the Habsburg Empire. For Russian Poland we used the railway and customs data compiled by Henryk Tennenbaum (1916) in his *Bilans Handlowy Królestwa Polskiego* (The Trade Balance of the Kingdom of Poland). Although some efforts towards national statistics were undertaken in various parts of the multi-national Habsburg state, only Hungary (Transleithania) produced usable trade statistics. Here we relied on *A Magyar Szent Korona Országainak 1882–1913. Évi Külkereskedelmi Forgamla* (Foreign Trade of the Lands of the Holy Hungarian Crown, 1882–1913) - a foreign trade statistics based on railway shipments. For regions in the Austrian half of the empire (Cisleithania), trade flows have been reconstructed in a two-stage process. First, data on trade in mining products (brown coal, hard coal, coke) were taken directly from the official Austrian *Bergbaustatistik* (Mining statistics) which documents not only regional production levels, but also sales of regions to other regions within Cisleithania and abroad (including Hungary). Second, for all other goods covered here Austrian trade with non-German districts was isolated using the official foreign trade statistics of the Austro-Hungarian customs union (*Waaren-Ausfuhr; Statistik des* *auswärtigen Handels*) and substracting the import and export quantities pertaining to Hungary (from the Hungarian foreign trade statistics), Bohemia and Galicia (from a range of sources). Internal (i.e. intra-distric) trade was approximated on the basis of regional output net of regional exports. The complete list of sources as well as an overview of necessary adjustments is given in table 2 and 3 in appendix A.

For the period after 1918, the analysis draws on data collected by the statistical administrations which were quickly set-up in all of the newly formed independent states in Central Europe. Detailed railway statistics are available for Poland and the Saargebiet with geographical definitions of TDs that are almost identical to the ones used in the German statistics. Moreover, we used the official commodity-specific trade statistics of Poland, Czechoslovakia, Hungary, Austria, and the statistics of the French Département Haut Rhin. For the few cases where information on internal trade proved unavailable, we proxied internal trade by subtracting exports from production following Wei's (1996) procedure. In some very rare cases, where this procedure was not feasible, we used circumstantial evidence on the absence of certain trade relations, whenever the sources could be regarded as reliable. Where neither of these approaches was feasible or sensible, observations were entered as missing. To assure that these last cases do not affect our interpretation of the data, all reported estimates refer to balanced samples where full information at all points in time is available. Table 1 in section 2 in appendix A lists all 43 transport districts in our sample. With some necessary aggregations we have a total of 1333 shipments per year and traded good. For 31 transport districts (27 German in the pre-war borders, three Habsburg and one Russian) we have internal (that is intra-district) shipments as well as data on their exports to and imports from all other TDs. All data are given in metric tons. Shipments of less than 0.5 tons were documented as zero. Figure 3 shows their location.

There are several notable features of the railway statistics used here. To start with, our data is shipment data on railways between regions, excluding transits. This shipment data is generally well suited to explore the level of economic integration between regions within states as it represents by far the largest share of total trade flows in Central Europe. The share of railways in total trade was quite constant across our sample at about 85%, with some variation between regions but little over time.[9] Whenever a shipment from region A to C was sent via region B, this is documented as shipment from A to C as long as the shipment is on railways only. In the case of a change in transport mode (for example if goods are first shipped by railway from A to B and then by waterway from B to C) the shipment data would imply a bias, especially towards trade hubs, such as ports. Data for 1913 suggests that this was an issue, because 20% of all railway shipments in hard coal

[9]For Germany, the share of railways in total domestic shipments varied from 83.4% (1913) to 88.8% (1933). For the Habsburg Empire, the share of railways was above 95% prior to 1914 and between 93% (Austria, 1933) and 96% (Czechoslovakia 1933) afterwards. For Poland, was have data on the Kingdom of Poland prior to 1914 suggesting that the share of railways varied between goods from about 75% (rye) to 97% (hard coal). After the war we suspect that these shares tended to increase du to improved railway infrastructure without significant changes in waterway infrastructure.

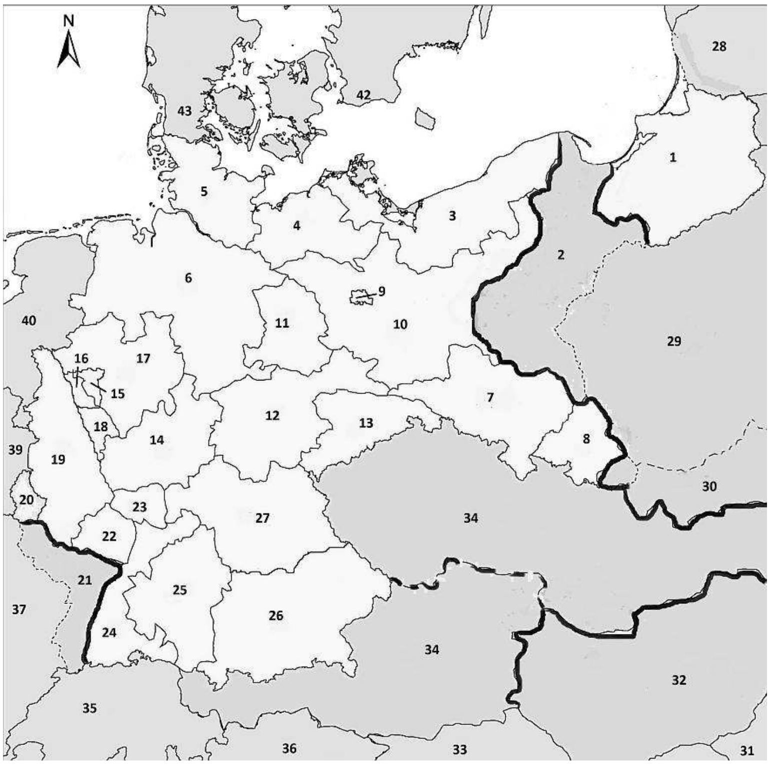


Figure 3: Central Europe, consolidated transport districts in the data set

and 11% of shipments in grains either originated at an inland or sea-port or had a port as their final destination.[10] However, note that the new borders were drawn in regions that lacked major waterways, with the exception of Alsace-Lorraine, where the new border largely followed the Rhine. We will explain in the next section, how our estimation method can take this issue into account.

Next, the sources document trade at the sub-state level for Germany and some of her neighbours. For example, the Habsburg Empire is covered in three TDs that, together, include four major regions: Galicia (with Bukovina), Hungary (including Croatia-Slavonia, Slovakia, Bosnia), Bohemia, and German Austria (with Moravia). Importantly, shipments from and into the Kingdom of Poland are also reported separately from those of the Russian Empire (of which it was a part). Moreover, the geographical definition of German and foreign TDs prior to the war matches very closely the demarcation of new countries after the First World War. While we lack specific information on this issue, the definition of TDs apparently followed quite closely pre-existing administrative units. Finally, after 1919 the German authorities largely maintained the geographical definition of previously German TDs. This is a most remarkable feature of the data-set. For example, for the post-war Republic of Poland the sources distinguish between 'East Poland' (the former

[10]See data in Statistisches Bundesamt (1914/1977), p. XXI.

Russian part), 'West Poland' (the former German part except Upper Silesia), 'East Upper Silesia', and 'Galicia' (the former Austrian part). Similarly, shipments to and from Alsace-Lorraine were reported separately from those relating to France even after 1919. These unique features allow us to trace regional trade flows across Central Europe over the whole period 1885-1933, despite the profound changes in political-administrative borders.

5. Analytical Approach: Identifying the Treatment Effect of Borders

Ideally, one would like to compare trade flows at time t between two regions i and j separated by a border with trade flows at time t between two identical regions i and j but not separated by a border (the control group). The difference in trade flows would equal the 'treatment effect' of a border on trade. However, we never observe identical pairs of regions at time t *with* and simultaneously *without* a border. Empirical studies have typically approximated the control group in a gravity model framework, where trade flows at time t between region pairs separated by a border were compared to trade flows at time t between regions pairs not separated by a border after controlling for regional characteristics and some basic pair-wise characteristics. However, we can never rule out that there is some unobserved pair-wise heterogeneity, not captured by the gravity model that essentially drives the estimated border effects. Thus it is not clear whether it is actually the treatment effect of borders on trade that has been estimated or the effects of some other factors that just happen to vary along that border, such as, for example, the effects of ethno-linguistic networks or natural geography.

The solution to such a problem is estimating a difference-in-difference (DD) estimator: compare the difference in the difference of trade flows over time between two regions i and j without a treatment to that of regions k and l with a treatment. The first set of differences (in the cross-section) accounts for otherwise unobservable pair-wise heterogeneity, the second accounts for the treatment (changes over time). The right kind of data would allow distinguishing between the proper treatment effects of changing a political or administrative border from the impact of unobservable pair-wise heterogeneity. The difficulty is that we usually cannot observe trade flows at time t between two regions i and j without a border comparable to trade flows at time $t+1$ between the same regions i and j with a border. This is so, because national statistical systems including for example the definition of regions, commodity groups and the like tend to be changed together with changes borders. However, the new data-set exploited here removes this major constraint.

Difference-in-difference (DD) estimators are widely used to assess the treatment effect of a policy (see Meyer 1995, Bertrand, Duflo, and Mullainathan 2004). Here we estimate the treatment effect of borders on trade with a DD estimator in levels, as suggested by Ashenfelter (1978) and Ashenfelter and Card (1985) and Ritschl and Wolf (2011). We implement this estimator within the framework of the now standard micro-founded formulation of a gravity model on trade flows from Anderson and van Wincoop (2003, and 2004), modified for our historical data. Following their approach, at any time t exports X from region i to j in a certain period can be explained by the relative economic size of the exporter and the importer, expressed as the proportion of the product of the exporter's income Y and the importer's expenditure E in overall income. Additionally, X depends on the bilateral resistance to trade (denoted by "t", which is 'one' plus the tariff equivalent of trade barriers) relative to the overall barriers to trade of the respective trading partners, i.e. the inward "multilateral resistance" P and the outward "multilateral resistance" Π. The elasticity of substitution between varieties of k from different exporters i is denoted by σ. The gravity model is then formulated as (for good k)

$$X_{ij}^k = \frac{Y_i^k E_j^k}{Y^k} \left(\frac{t_{ij}^k}{P_j^k \Pi_i^k} \right)^{1-\sigma_k} \tag{5.1}$$

The variables in (5.1) are not directly observable to us. However, as all these variables except the trade costs are region-specific, but not pair-specific, it is still possible to consistently estimate the average effect of trade costs on trade in (5.1) by introducing two sets of time-varying dummies for each region and product class k, namely $A_{i,t}^k$ and $A_{j,t}^k$ (see Anderson and van Wincoop 2004, p.27). These dummy variables are equal to one whenever a region enters the equation as an importer or exporter, respectively. Furthermore, the model requires trade flows in values whereas our sample comprises (commodity-specific) information on physical quantities. Following Anderson and van Wincoop (2003, 2004) we assume trade costs to be proportional in trade values so that we are dealing with $X_{ij}^k = p_i^k t_{ij}^k Z_{ij}^k$, where Z_{ij}^k is the volume of exports in metric tons. We may substitute X, since Z_{ij}^k denotes the observed quantities shipped from i to j and the term p_i^k is exporter-specific and thus reflected by the respective (time-varying) exporter dummy. Therefore, we replace the unknown terms in (5.1) as described above by time-varying exporter $A_i^{k'}$ effects —now including price effects p_i^k— and importer A_j^k effects (again dropping the time index)

$$Z_{ij}^k = C A_i^{k'} A_j^k \left(t_{ij}^k \right)^{\sigma k} \tag{5.2}$$

where C is a constant and the importer and exporter specific dummies capture all undirected region-specific heterogeneity, including price effects, multilateral resistance, region-specific infrastructure and the like. Importantly, these dummies also take into account that inland- and sea-ports will generate much higher railway traffic than other regions. This will limit the possible bias implied by the fact that our data is restricted to railway shipments.[11] The variable t_{ij}^k denotes again one plus the tariff equivalent of bilateral trade barriers, which are the main focus of our study.

To analyze these barriers, we have to make some assumptions about the functional form of t_{ij}^k. We assume that costs are incurred (i) by transporting goods over distance, which we proxy by a linear function of geographical distances dist, (ii) when crossing existing political borders as well as (iii) when crossing prospective political borders. We are agnostic about the origins of the latter, but they would capture all factors that systematically affect the trade intensity between the relevant region pairs, including effects of existing ethno-linguistic networks between regions or geographical factors such as a mountain range between regions. Our estimation must therefore account for possible border effects present both before and after the war as well as border effects that were present only before or only after the war. As an illustration, consider first the following functional form of t_{ij}^k:

[11] In addition, we tested whether our results (see tables below) are different for Alsace-Lorraine by adding a specific-control dummy. Our results are robust to this change.

$$t_{ij}^{k} = (dist_{ij})^{\delta} (D_{prw})^{\gamma_{prw}} (D_{pow})^{\gamma_{pow}} \tag{5.3}$$

where D_{prw} is 'one' plus the tariff equivalent of crossing a border before the war. The variable γ_{prw} is a dummy equal to 'one' if regions i and j did not belong to the same state prior to the war, otherwise it is equal to 'zero'. The post-war equivalents are D_{pow}, and γ_{pow} where the latter equals 'one' if regions i and j did not belong to the same state after the war. A negative and significant coefficient on these dummies reflects a trade diverting border effect, meaning that ceteris paribus regions traded less when they were located on different sides of state borders. Note that this is the standard procedure to estimate border effects in a cross-section. We estimate this for illustrative purposes only, but it does not yet implement the DD estimator as spelled out above.

In a second step, we implement the DD estimator in levels by decomposing the post-war border effect into three components: the continuing effect of those borders on post-war trade that existed already prior to the war (D_{old}), the effect of new borders on post-war trade (D_{new}), and the fixed effect of all factors that affected the trade intensity between the relevant region pairs along the lines of these new borders, but independently so from the time of their formal codification (FET_{new}).

$$t_{ij}^{k} = (dist_{ij})^{\delta} (D_{prw})^{\gamma_{prw}} (D_{old})^{\gamma_{old}} (D_{new})^{\gamma_{ntreat}} (FET_{new})^{\alpha_{nfe}} \tag{5.4}$$

This specification of trade costs allows us to assess the treatment effect of the new political borders as established by the 1919–20 peace treaties on regional trade, controlling for time-invariant pair-wise heterogeneity along these lines. We compare the difference in the change of trade flows before and after the change in borders between two regions i and j without a treatment (no border before or after WWI) to that of regions k and l with a treatment (no border before but a border after WWI) —controlling for possible changes in regional characteristics over time and controlling for the differences in pair-wise distance. Put differently, one set of differences is over time, namely the difference in trade between regions that were separated by a new border (such as Brandenburg and West-Prussia) after 1918 with trade between the same regions prior to the war. The other set of differences is in the cross-section: the difference in trade between regions separated by a new border (Brandenburg and West-Prussia) with trade between regions that were not (for example Brandenburg and Lower Silesia). The difference-in-difference approach combines these two and estimates how the change in trade before and after the border was different across "treated" and "non-treated" regions. Hence, we can estimate something that the data typically does not allow us to do: the "treatment effect" of a new border on trade, controlling for frictions that existed prior to the change in borders.

The standard approach is to substitute the trade cost function (5.3) or (5.4) into the gravity model (5.1) or (5.2), to log-linearise the resulting equation, and to estimate the model with OLS or some system estimator. However, Santos Silva and Tenreyro (2006) caution that this approach leads to biased estimates unless very specific assumptions are met. The basic difficulty is that the expected value of a log-transformed random variable does not only depend on the mean of the random variable but also on its higher moments.[12] Given this, heteroskedasticity of the error term in the stochastic formulation of the model would result in an inefficient, biased and inconsistent estimator.[13] Santos Silva and Tenreyro (2006) demonstrate the magnitude of this inconsistency and strongly recommend estimating the gravity model in its multiplicative form to avoid this problem. An appealing side effect of this strategy is that one circumvents as well the serious problem of zero observations of the dependent variable, which arises by linearizing equation (2), since the log of zero is not defined.[14] Santos Silva and Tenreyro (2006) propose a Poisson maximum-likelihood (PML) estimator, since it is "consistent and reasonably efficient under a wide range of heteroskedasticity patterns [...]" (p.645).[15] For the PML, it is sufficient to assume that the conditional mean of a dependent variable is proportional to its conditional variance. This estimator is preferable to others without further information on the heteroskedasticity according to Santos Silva and Tenreyro (2006, p.645). It attributes the same informative weight to all observations. Moreover, the estimator is numerically equal to the Poisson pseudo-maximum-likelihood (PPML) estimator, which is used for count data models. In order to gain efficiency, it is possible to correct for heteroskedasticity using a robust covariance matrix estimator within the PPML framework. This is the approach that we adopted in our estimation.[16]

[12] This can be framed in terms of Jensen's inequality stating that $E(ln(y)) \neq ln(E(y))$, with y being a random variable.

[13] In fact, in the application of gravity models the resulting estimation errors display very often heteroskedasticity (e.g. Santos Silva and Tenreyro 2006, but also Heinemeyer 2007 who analyzed a subset of our data).

[14] The appearance of zero observations may be due to mistakes or thresholds in reporting trade, but bilateral trade can actually be zero. This event is particularly frequent if one investigates trade flows at a regional and/or sectoral level. The occurrence of zero trade is usually correlated with the covariates.

[15] They present the results of a horse race between various estimation strategies including Tobit, non-linear least squares and Poisson regression models. Investigating simulated and real trade data, they conclude that only the latter approach and NLS deliver consistent estimates, but that NLS is less efficient because the structure of heteroskedasticity is unknown.

[16] As a robustness check we repeated all estimations with conventional Tobit and scaled OLS estimation and a (scaled) SUR-estimator. This left all of our findings qualitatively unchanged.

6. Basic Results 1: Cross-sectional Border Effects are Large

We start the analysis by simply exploring the *average* effects of borders on trade prior to the First World War and after the First World War. We estimate the basic gravity model in levels with time-varying importer and exporter effects as in (5.2) using PPML, with trade costs as specified in (5.3) as a function of distance, political borders before the war (pre-war border) and political borders after the war (post-war border). We do this for each product class separately and limit attention to the balanced sample only. We always group our data into pre-treatment and post-treatment observations. Table 3 gives the results.

Table 3: Average border effects before and after WWI

Coefficient	Hard coal	Coke	Brown coal	Iron and steel s.m.	Chemical products	Paper and cardbord	Rye
Distance	−2.33∗	−1.78∗	−2.49∗	−1.27∗	−1.25∗	−1.21∗	−2.97∗
	(-29.27)	(-16.42)	(-22.38)	(-37.39)	(-36.21)	(-33.92)	(-37.58)
Pre-war border	−1.74∗	−1.52∗	−0.96∗	−4.21∗	−3.48∗	−3.73∗	−4.33∗
	(-6.43)	(-4.30)	(-4.28)	(-18.45)	(-7.09)	(-23.88)	(-9.45)
Interwar border	−2.46∗	−3.26∗	−2.32∗	−2.95∗	−2.40∗	−3.66∗	−4.27∗
	(-12.05)	(-10.45)	(-9.50)	(-15.22)	(-15.38)	(-17.62)	(-5.48)
Imp, Exp effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# of Obs.	7724	7882	7762	7675	7423	7482	7528
Adj R^2	0.92	0.87	0.86	0.85	0.74	0.87	0.89

Notes: ∗ denotes the 5% level of statistical significance. PPML Estimation based on balanced sample using robust standard errors. z-statistics given in parentheses.

In general, the fit of the model is very good. Both, distance and the border dummies come with the expected negative coefficients and are highly significant in all cases, after controlling for time-varying importer and exporter effects as suggested by Anderson and van Wincoop (2003). The average effect of national borders on trade between regions was apparently large both before and after the war, but we need to make some assumptions about product-class specific elasticities of substitution to assess these effects. For simplicity we will follow most of the empirical literature and assume that the elasticity of substitution can vary across product-classes but is stable over time. This also implies that different assumptions about the elasticity of substitution will have only very minor implications for the relative magnitude of border effects over time or between different types of borders.[17] With this we see that the average border effects for various kind of coal are (significantly) larger for the years after the war than before. Instead, the average border effects

[17] Broda and Weinstein (2006) estimated elasticities of substitution for the US over the period 1972 and 2001 and find some considerable changes over time. However, most of these changes are due to changes in the composition of trade. For the goods that closely correspond to the ones in our sample, their estimated elasticities remain nearly constant over the 30 years under consideration, e.g. for hard coal (SITC 5-digit category 32110) they estimate for 1972–1988 an elasticity of about 3.4, for 1990–2001 and elasticity of 3.6). Moreover, for our sample of goods these estimates are very similar to the ones used by Evans (2003) who derived an elasticity of substitution for agricultural products of 4.63.

for iron and steel products, chemical products, and paper are all somewhat smaller after 1919 than before, while those for rye are virtually constant over time.

What explains the changes over time? To start with, the interwar period saw a large rise in tariffs and quotas across all products and on nearly all state borders already during the 1920s, but especially after 1929 in reaction to the Great Depression. The best source of comparable tariff data across European states is Liepmann (1938), who collected data for 1913, 1927 and 1933 for Germany, the Habsburg Empire and its successor states, the Russian Empire and Poland (after 1918). Figure 3 reproduces the relevant data for our sample products: except for trade across the borders of the newly established Polish state tariffs generally increased after the war during the 1920s and then again sharply until 1933.

Moreover, our estimated border effects should reflect not only tariffs along borders but also the impact of quotas and exchange control systems that were imposed on cross-border trade during the Great Depression. All this would suggest an increase in the estimated average border effects after the war. To interpret our results in the light of this intuition note that the estimated border effects γ can be easily converted into tariff-equivalents as $exp\left(\gamma / - \sigma^k\right) - 1$ (see equation (5.3)). If we take the product-group specific elasticities of substitutions from Evans (2003), the implied tariff-equivalent of the average 'pre-war border' on trade in hard coal after the war would be 90 per cent, that of the 'post-war borders' 150 per cent, for iron and steel (semi-) manufactures 291 and 160 per cent, for chemicals 137 and 81 per cent, for paper and related products 161 and 156 per cent, and for rye 155 and 152 per cent, respectively. Compared to figures 2a–2f, these estimates strongly suggest that tariffs are only some part of the story.

The decline in the average border effect after the war that we observe for some goods seems at odds with the increase in tariffs and quotas. But note that these average effects do not reflect the level of trade fragmentation in the sample. First, the number of borders has increased over time. While prior to the war about 7 per cent of all trade flows in our sample crossed a national border, it was roughly 10 per cent after the war (see table 4). Therefore, a slight decline in the average border effect, as for example for paper, should not be misinterpreted as evidence for better overall integration.

Moreover, we can decompose the post-war borders into 'old borders' that existed already before 1914 and 'new borders' that were drawn after the war. The lower average border effect that we find in some cases after 1918 might stem from a difference between the effects of 'old borders' and those of 'new borders'. Were the new borders really excessively trade-diverting, i.e. more trade diverting than the old borders, as argued by the losers of the war (parts of whose territories were ceded to neighbouring and/or successor states)? Or were instead the peace-makers in Versailles, St.Germain, and Trianon reasonably successful in redrawing the European map such as to minimize additional frictions? In table 5, we repeat the analysis of border effects but distinguish between the effects of new and old borders after the war (but still without the FET).

The trade diverting effect of the new, post-war borders on trade is visible, and it is significant. But the effect of these new borders is, on the whole, below that

Table 4: Number of bilateral trade flows crossing borders in our balanced sample

Coefficient	**Hard coal**	**Coke**	**Brown coal**	**Iron and steel s.m.**	**Chemical products**	**Paper and cardbord**	**Rye**
Total	7724	7882	7762	7675	7423	7482	7528
Prewar	571	576	580	569	487	542	542
Interwar	764	767	770	756	678	739	731
thereof							
Old	561	566	570	559	487	542	534
New	203	201	200	197	191	197	197
New BFE	203	201	200	197	191	197	197

Notes: These numbers are irrespective of the fact if trade actually crossed these borders. Numbers vary across goods because the amount of data differs —balancing the sample amplifies the differences. 'New BFE' gives the number of bilateral trade flows that crossed would-be borders prior to 1914.

Table 5: Average border effects before and after WWI, old and new borders

Coefficient	**Hard coal**	**Coke**	**Brown coal**	**Iron and steel s.m.**	**Chemical products**	**Paper and cardbord**	**Rye**
Distance	−2.33∗	−1.76∗	−2.49∗	−1.26∗	−1.25∗	−1.20∗	−2.96∗
	(-29.16)	(-15.34)	(-22.43)	(-37.61)	(-36.21)	(-33.79)	(-37.37)
Prewar Border	−1.74∗	−1.54∗	−0.95∗	−4.22∗	−3.48∗	−3.73∗	−4.34∗
	(-6.43)	(-4.02)	(-4.24)	(-18.48)	(-7.10)	(-23.91)	(-9.46)
Old Border	−3.20∗	−4.35∗	−1.51∗	−3.86∗	−2.36∗	−4.12∗	−5.16∗
	(-15.16)	(-8.73)	(-6.66)	(-15.74)	(-11.82)	(-16.29)	(-4.19)
New Border	−2.33∗	−2.82∗	−3.24∗	−2.56∗	−2.46∗	−2.72∗	−3.87∗
	(-10.78)	(-9.25)	(-9.51)	(-10.41)	(-14.70)	(-12.06)	(-4.19)
Imp, Exp Effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
# of Obs.	7724	7882	7762	7675	7423	7482	7528
Adj R^2	0.92	0.88	0.86	0.85	0.74	0.88	0.89

Notes: ∗ denotes the 5% level of statistical significance. PPML Estimation based on balanced sample using robust standard errors. z-statistics given in parentheses.

of the old borders, which helps to explain why the average border effects declined after the war in some cases. What is more, this can be interpreted as a first piece of evidence that borders did not change randomly, but tended to follow some existing structures.

6.1. Basic Results 2: Treatment Effects of Borders are Small. The evidence from table 5 brings us to our main question: to what extent do our estimates capture the treatment effects of borders in the sense of codified political institutions? And to what extent do they actually capture some underlying unobserved heterogeneity, trade frictions that simply run along the same lines? Obviously, we can explore this question only with regard to those borders that were newly established in 1919-20. To this end we estimate the gravity model in levels from (5.2) with trade costs as specified in eq:REVgravityTC2. This implements the DD estimator in levels, which allows us to distinguish the genuine treatment effect of the news borders (active from 1919 onwards) from a pair-wise fixed effect on the treated (FET), active over all periods. Table 6 shows the result.

Table 6: Treatment effects of new borders on trade

Coefficient	**Hard coal**	**Coke**	**Brown coal**	**Iron and steel s.m.**	**Chemical products**	**Paper and cardbord**	**Rye**
Distance	−2.27∗	−1.72∗	−2.44∗	−1.25∗	−1.24∗	−1.20∗	−2.70∗
	(29.65)	(−15.95)	(−23.79)	(−36.35)	(−35.48)	(−33.79)	(−48.24)
Pre-war border	−2.02∗	−2.81∗	−1.29∗	−4.39∗	−3.60∗	−3.83∗	−5.74∗
	(7.65)	(−6.91)	(−6.35)	(−18.79)	(−7.24)	(−23.17)	(−10.43)
Old border	−3.22∗	−4.40∗	−1.56∗	−3.87∗	−2.37∗	−4.12∗	−5.31∗
	(−15.47)	(−10.87)	(−6.94)	(−15.77)	(−11.85)	(−16.29)	(−7.35)
New border FE	−0.74∗	−1.59∗	−3.66∗	−0.29∗	−0.65∗	−0.29∗	−2.57∗
	(−4.76)	(−4.57)	(−7.59)	(−2.41)	(−3.90)	(−4.67)	(−9.19)
New border treatment	−1.60∗	−1.25∗	−0.39	−2.28∗	−1.82∗	−2.42∗	−1.35
	(−6.07)	(−2.63)	(0.67)	(−8.38)	(−7.93)	(−10.34)	(−1.45)
Imp, Exp Effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
# of Obs.	7724	7882	7762	7675	7423	7482	7528
Adj R^2	0.93	0.88	0.96	0.85	0.74	0.88	0.95

Notes: ∗ denotes the 5% level of statistical significance. PPML Estimation based on balanced sample using robust standard errors. z-statistics given in parentheses.

The data clearly support the idea that the border changes followed an already existing pattern of fragmentation, visible before 1914: the new border fixed effects (FET) are always negative and highly significant. After controlling for these effects, we find that the treatment effect of new borders is much smaller than the naïve cross-sectional estimates in tables 3–5 suggested.[18] If, again, we use the elasticity of substitution from Evans (2003), the implied tariff-equivalent of the treatment effect of new borders on trade in hard coal is 81 per cent (instead of 137 per cent as suggested by the cross-sectional estimate from table 5), for iron and steel (semi-) manufactures it is 109 instead of 129 per cent, and zero for rye (or not statistically significantly different from zero, instead of 130 per cent). The difference between the two estimates gives the tariff equivalents of time-invariant barriers to trade that run along the new borders. Our results suggest that this is in most cases quite considerable.

To get a more general number on this, we also estimate a pooled version of the PPML-estimator of table 6, where we pool all data over commodities and allow for the (time-varying) region-effects as well as for the distance-effects to be commodity-specific. These commodity-specific effects capture for example differences in prices between commodities, regions and over time. With this procedure, we find for our total sample of more than 50,000 observations an average treatment effect of the new borders of -0.99 and a new border fixed effect of -1.38 (both highly significant). Taking -3.45 as the average of the commodity-specific elasticities from Evans (2003) this translates into a tariff-equivalent of the treatment effect of 34 per

[18] We also tested, whether our result are driven by positive serial correlation in our data, which might lead to false rejections of the null-hypothesis of no border effects as argued in Bertrand, Duflo, and Mullainathan (2004). To this end, we repeated our estimation for the years 1913 and 1925 alone, hence restricting the dataset to one point in time before and one after the treatment only. The results were qualitatively unchanged. Similarly, if we take the average of all pre-war (1885, 1910, 1913) and post-war (1925, 1926, 1933) observations, all our results remain valid.

cent (compared to 103 per cent from a naïve pooled estimation similar to table 5). For a very wide range of assumptions on the elasticity of substitution between -2 and -10 we would get slightly different tariff-equivalents. But the relative magnitude of effects would remain nearly unchanged; in any case the treatment effect would be about 1/3 of the naïvely estimated effect that does not control for FET.[19]

[19]We replicated our estimation using other methods, such as Tobit, scaled OLS and SUR as suggested by a referee, with little effect on our results. While the magnitude of the estimated coefficients on the border varied, the key results —that there were significant fixed effects prior to the change in borders, which reduce the treatment effect— all hold.

7. Robustness checks: Adjacent regions, counterfactual borders, and the Great Depression

The results of our analysis so far point to political borders having changed broadly along the lines of already existing trade frictions. For this interpretation to hold, trade frictions before 1914 between those pairs of regions that after 1918 were not separated by a new border, should be negligible or at least much smaller than frictions between regions that afterwards were separated by a border. Our model in table 6 estimated the trade frictions between all pairs of regions that were later separated by a new border using all other pairs of regions as control group. But we can put the idea of a "border before the border" to a harder test. A standard result from the gravity literature is that neighbouring or "adjacent" regions trade significantly more with each other than average trading partners. Let us explore, whether this adjacency effect was present before 1914 for all neighbouring regions. If instead we find prior to 1914 a difference between those adjacent regions that after 1918 were separated by a new border and the others that were not, the pre-existing trade frictions must have been rather strong. Table 6 limits attention to trade flows prior to the war (1885, 1910 and 1913 only) and shows that there were indeed striking differences in adjacency effects prior to the war.

Table 7: "Border before a border"? Differences in trade between adjacent regions, 1885–1913

Coefficient	**Hard coal**	**Coke**	**Brown coal**	**Iron and steel s.m.**	**Chemical products**	**Paper and cardbord**	**Rye**
Distance	−2.49∗	−1.82∗	—	−1.21∗	−1.30∗	−1.27∗	−3.09∗
	(−19.21)	(−10.78)	—	(−22.95)	(−20.65)	(−19.36)	(−32.52)
Pre-war border	−2.46∗	−1.93∗	—	−4.22∗	−3.41∗	−3.69∗	−5.00∗
	(−7.82)	(−5.21)	—	(−18.16)	(−6.79)	(−22.58)	(−9.66)
Adjacency	1.06∗	1.8∗	—	0.14	0.34∗	0.28∗	0.62∗
	(7.98)	(5.28)		(1.44)	(2.01)	(3.29)	(5.07)
Adjacency NFE	−1.59∗	−1.37∗	—	−0.11	−0.13	−0.25	−3.04∗
	(−6.04)	(−2.97)	—	(−0.59)	(−0.53)	(−1.90)	(−9.98)
Imp, Exp Effects	*Yes*	*Yes*	—	*Yes*	*Yes*	*Yes*	*Yes*
# of Obs.	3792	3941	—	3767	3713	3670	3698
Adj R^2	0.94	0.89	—	0.86	0.62	0.80	0.97

Notes: ∗ denotes the 5% level of statistical significance. PPML Estimation based on balanced sample using robust standard errors. z-statistics given in parentheses.

As expected we find that trade between adjacent regions is significantly above the sample average. However, those adjacent regions that after 1918 were separated by a new border were different: here trade flows prior to 1914 were significantly smaller in rye, hard coal, coke, and paper.[20] Note that these are exactly the products were we find the largest new border fixed effects (table 6). In the other two cases (iron and steel products and chemical products), there were generally no significant adjacency effects, nor differences in adjacency effects along future borders.

Another way to test our hypothesis is to test for 'counterfactual borders', in the spirit of Hugh Trevor-Roper's famous phrase that 'history is not merely what

[20]The estimation for brown coal did not converge for the pre-war dataset.

happened: it is what happened in the context of what might have happened'[21]. To this end, we first introduce a dummy for a 'counterfactual border' that divides the 43 regions of our sample roughly into a northern and a southern half.[22] Do we find an effect for such a 'border' as well? The results (not reported here) are clear: there are no systematic trade frictions evident along such an imaginary line, neither before nor after the war. This result holds regardless of whether or not we control for the actual borders or not.[23]

We also considered changes that were discussed at the Paris peace conference but were not actually implemented. In most cases though, our data is too highly aggregated to consider these as they affect only small territories. It is for example not possible to test for the existence of trade frictions within the Teschen-region in Silesia that after 1918 was divided between Poland and Czechoslovakia. The only counterfactual reflected in discussions at Paris that our data allow us to consider is the plan for a 'Danubian Federation'. President Wilson and to some extent also the British were, for quite a long time and well into 1918, favourably disposed to the notion of such a Federation that, replacing the crumbling Habsburg Empire, would allow for political autonomy and national 'self-determination' of its peoples but, crucially, without major changes in its internal and external borders.[24] We should note that after the war the former members of the Habsburg Empire made several attempts to limit the additional trade frictions implied by the new borders and to foster integration of the regions, all of which failed at early stages. Given that a 'Danubian Federation' would have involved fewer borders rather than additional ones, it is not entirely straightforward how to test for this counterfactual. We re-estimated the model from table 6, hence controlling for all factual changes in borders after 1918 but adding dummies for those pairs of regions that would have become part of the 'Danubian Federation', namely Austria with Czechoslovakia and Hungary, Slavonia, Croatia and Bosnia (32 and 34 of our TDs). Were these regions prior to 1914 well integrated so that the new borders disrupted their trade network? If so, we would should find positive "new border fixed effects" for the years 1885-1913 and rather strong negative "treatment effects". In general, we don't. For brown coal, hard coal, coke and rye we find (significant) strongly negative new border fixed effects and negative but weak treatment effects in line with all our previous findings. For the remaining commodities estimates did either not converge (chemicals) or they were altogether insignificant. We interpret this as evidence that the 'Danubian Federation' did not materialise among other things, because its economic underpinnings were poor: economic integration in this region prior to 1914 was actually weak (see Schulze and Wolf 2009).

Finally, let us explore to what extent the effect of the new borders on trade changed over time, especially with the Great Depression. While the new borders may have had little effect of trade in the short-run insofar as they largely followed

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[21]See Trevor-Roper (1980), p.15.

[22]We define 21 regions in our sample as 'northern', namely 1-6, 9-10, 17-20, 28-29, and 37-43 from table table 1 in appendix A.

[23]Details on the relevant regression results are available from the authors.

[24]Cf. Ádám (2004) and Jeszensky (1989).

existing lines of fragmentation, they nevertheless provided a framework for the introduction of new trade costs in form of tariffs, quotas and or exchange controls. Given the sizeable literature on the increase in tariffs and other trade costs in the wake of the Great Depression one would expect the new borders to become much more trade diverting after 1929. To test for this we change the specification underlying table 6 —the gravity model in levels from (5.2) with trade costs as specified in (5.4)— in two ways: first we split the constant in a constant for 1885-1913 and year specific constants for 1925, 1926 and 1933. This allows visualizing directly the changes in the average level of trade costs that so far have been controlled for by the time-varying importer and exporter effects. Second, we interact the distance variable and the border-dummies with year effects for the post-war period. Table 8 gives the results.

Table 8: Treatment Effects Before and During the Great Depression

Coefficient	**Hard coal**	**Coke**	**Brown coal**	**Iron and steel s.m.**	**Chemical products**	**Paper and cardbord**	**Rye**
Constant	23.66∗	17.05∗	—	16.12∗	15.33∗	17.18∗	22.43∗
	(47.22)	(19.25)	—	(39.48)	(37.76)	(41.29)	(38.74)
1925	−0.74	1.38	—	1.29∗	−0.09	0.12	−0.69
	(−1.07)	(1.27)	—	(2.75)	(−0.17)	(0.27)	(−1.53)
1926	−0.80	1.46	—	0.96∗	−0.09	0.04	−0.73
	(−1.14)	(1.34)	—	(2.04)	(−0.18)	(0.08)	(−1.64)
1933	−0.52	1.58	—	0.69	0.39	−0.27	−0.03
	(−0.56)	(0.93)	—	(1.26)	(0.66)	(−0.54)	(−0.04)
Distance	−2.32∗	−1.67∗	—	−1.19∗	−1.30∗	−1.26∗	−2.76∗
	(−19.50)	(−9.46)	—	(−22.14)	(−21.49)	(−20.20)	(−33.70)
Distance x (d1925	0.13	−0.10	—	−0.08	0.14	0.09	0.16
+ d1926)	(0.81)	(−0.46)	—	(−1.16)	(1.96)	(1.14)	(1.38)
Distance x d1933	0.01	−0.11	—	−0.16	−0.02	0.11	−0.05
	(0.03)	(−0.30)	—	(−1.70)	(−0.25)	(1.21)	(−0.26)
Pre-war border	−1.98∗	−2.81∗	—	−4.48∗	−3.49∗	−3.75∗	−5.66∗
	(−7.31)	(−5.98)	—	(−18.14)	(−6.99)	(−21.76)	(−10.18)
Old border x	−3.05∗	−4.31∗	—	−3.96∗	−1.83∗	−4.19∗	−3.46∗
(d1925 + d1926)	(−12.96)	(−9.01)	—	(−16.55)	(−5.24)	(−13.99)	(−12.32)
Old border x	−3.85∗	−4.67∗	—	−3.60∗	−2.97∗	−4.15∗	−11.36∗
d1933	(−9.14)	(−6.21)	—	(−7.82)	(−12.49)	(−8.63)	(−11.57)
New border FE	−0.72∗	−1.62∗	—	−0.33∗	−0.59∗	−0.29∗	−2.56∗
	(−4.61)	(−4.59)	—	(−2.67)	(−3.53)	(−4.48)	(−9.12)
New border	−1.43∗	−1.38∗	—	−2.06∗	−1.81∗	−2.49∗	−0.80∗
treatment x	(−5.12)	(−2.85)	—	(−6.64)	(−6.13)	(−8.96)	(−2.10)
(d1925 + d1926)							
New border	−2.42∗	−0.95	—	−2.59∗	−1.78∗	−2.38∗	−1.58
treatment x d1933	(−4.96)	(−1.47)	—	(−5.67)	(−6.51)	(−5.66)	(1.84)
Imp, exp effects	*Yes*	*Yes*	—	*Yes*	*Yes*	*Yes*	*Yes*
# of obs.	7724	7882	—	7675	7423	7482	7528
Adj R^2	0.93	0.88	—	0.86	0.74	0.89	0.95

Notes: ∗ denotes the 5% level of statistical significance. PPML Estimation based on balanced sample using robust standard errors. z-statistics given in parentheses.

Generally we find that trade frictions increased between 1926 and 1933 as reflected in changes in the constant and the effect of distance on trade flows. In addition to this, we also find an increased treatment effect of borders in 1933. However, the increase in trade costs was often larger along the old borders than along the new ones (consider for example the trade costs for rye, but also for chemical products). The new borders set the political-administrative boundaries for the imposition of additional trade costs during the depression. However, if anything, this increase in trade costs was smaller along the new borders than along the old ones. The new borders did clearly not improve economic integration across Central Europe, but given that border changes were hardly avoidable in 1918, the way these borders were drawn made economic sense —intentionally or not: they imposed only modest additional trade costs.

8. What explains the "border before a border"?

The key result of this paper is that the border changes after 1918 had a much less detrimental effect on trade than earlier research suggested, because they changed along some pre-existing trade frictions. According to the historical literature on the Paris Peace conferences (see section 2), the changes in borders were motivated in speeches or memoranda by a variety of arguments that only occasionally followed economic reasoning. More often did the peacemakers argue in terms of ethnic settlement spaces, sometimes based on historical rights, in still other cases referring to strategic arguments (for example the need to gain access to the sea or some railway station) or just nationalist ambition. One way to interpret our findings is that much of this rhetoric was a smoke-screen behind which the peacemakers actually followed more sober considerations. At least the demand for a new border had a higher chance to succeed if it followed some existing economic fault-line.

However, this raises new questions and here we aim to answer at least some of them. Most importantly, we need to understand which factors account for the "border before a border" effect within the empires of Germany, Austria-Hungary and Russia, 1885–1913. The most obvious candidates are the effects of ethno-linguistic networks and effects from natural geography. First, consider the prime suspect that also features in the historical literature on the Paris peace settlement: formal or informal institutions that developed along ethno-linguistic lines may have affected regional trade flows across Central Europe prior to 1914. Recent qualitative work by historians on the prevalence of intra-state economic nationalism in Central and Eastern Europe suggests that ethnically-based institutions increasingly affected trading costs between different ethnic groups by systematically directing trade towards the own group and putting a cost on trade with others (Jaworski 2004, Lorenz 2006). For example Jaworski's (2004) research on boycott movements between different ethnic groups within the multi-national setting of East Central Europe points to ethnic mobilization as a key element of intra-state economic nationalism at work prior to 1914. 'Self-integrating national communities' (Bruckmüller and Sandgruber 2003) ventured to keep 'others' out, via boycotts and the threat to boycott. We also see the emergence of ethnically orientated trade institutions within the German and the Habsburg Empire prior to 1914, especially cooperatives. "Through national segregation on the regional, and, increasingly, on the local level, cooperatives evolved from socially organized and a-national, into inter-societal, nationally organized institutions" (Lorenz 2006, p.22) during a phase of 'ethnic segregation' in the 1860s and '70s. This was followed by a phase of 'ethnic mobilization', much in line with intensifying national conflicts within the old empires during the late 19th century up to the First World War. In earlier work on the Habsburg Empire, some of us found that the effect of ethno-linguistic networks on grain markets starts to be visible around 1883/84, increases up to the mid-1890s, before it stabilises at a level similar to that in the mid-1880s until the First World War (Schulze and Wolf 2009).

To what extent did ethno-linguistic institutions indeed create barriers to regional trade flows, visible before the actual creation of borders along their lines? Can they account for the observed borders-before-the-border (as visible in the

FET)? And did this effect change between 1885 and 1910? To explore these questions we collected language statistics, which are available for all our regions in 1910. Denote by α_i^k the share of people that declare in the statistic language k as their mother tongue in region i. Similar to a Herfindahl-index we can then construct an index of pair-wise ethno-linguistic heterogeneity.[25]

$$Language_{ij} = \frac{1}{n}\sum_{k=1}^{n}\left(\alpha_i^k \alpha_j^k\right)^2 \quad (8.1)$$

The index takes on values between 0 and 1. An index value of 0 would reflect a pair of regions that has identical shares in each language group; an index value close to 1 would reflect a pair of regions with no overlap in languages spoken. If indeed ethno-linguistic institutions created barriers to regional trade already prior to 1914, such an index should help capture them. In addition to this we can make an attempt to capture the effects of natural geography that might have separated regions prior to the change in state borders. A simple approach is to distinguish between regions that were connected by navigable rivers (or canals) and those that were not. The first will typically have been better connected already prior to the age of railways, and we can exclude that they were separated by very rugged territory or mountain ranges, while some of the latter may have been. We implement this idea with an indicator variable "No-River" equal to one whenever a pair of regions is not connected by a river or canal. This is arguably a rough approximation to the complex effects of natural geography on trade, but it should give us an indication whether geography matters for the "border before a border" or not.[26] In both cases, ethno-linguistic heterogeneity and natural geography, we distinguish between their effect before and after the war.

There is indeed evidence that ethno-linguistic institutions had some trade diverting effect both before and after 1919, with a lot of variation across product-classes. Except for chemical products, ethno-linguistic heterogeneity affected trade flows (either only prior to the war as for hard coal and coke or throughout) and in most cases that effect is quite large. In addition, natural geography as captured by the indicator-variable "No-River" mattered. In all cases except hard coal, regions that were not connected by navigable rivers did trade less on railways. Together with the variable on ethno-linguistic heterogeneity this helps explain a considerable part of the FET that we estimated above. In all cases except for chemical products is the FET significantly reduced, hence we can explain a large part of the effect by ethno-linguistic heterogeneity and natural geography. In some cases (iron

[25]In the context of Central and East Central Europe, language (measured as either 'mother tongue' or 'main language spoken') proves a strong proxy for ethnic identity (cf. Schulze and Wolf 2009).

[26]If railways were complementary to waterways, we would expect that the absence of waterways had a positive effect on railway trade. However, earlier research suggests that at least in Germany railways were typically built along existing waterways, partly due to the costs of building a railway through mountainous territory, partly (and related) due to an effort to ensure the profitability of new lines by building along existing routes of traffic (see Fremdling 1975).

Table 9: Endogenous border changes? Ethno-linguistic heterogeneity and borders

Coefficient	**Hard coal**	**Coke**	**Brown coal**	**Iron and steel s.m.**	**Chemical products**	**Paper and cardbord**	**Rye**
Distance	−2.25∗	−1.63∗	−2.23∗	−1.23∗	−1.01∗	−1.09∗	−2.43∗
	(−27.05)	(−14.27)	(−19.42)	(−29.65)	(−23.39)	(−28.37)	(−36.54)
Pre-war border	−1.89∗	−2.39∗	0.09	−4.24∗	−3.89∗	−3.35∗	−4.50∗
	(−7.00)	(−6.09)	(0.26)	(−17.81)	(−7.11)	(−19.75)	(−9.62)
Old border	−3.30∗	−4.19∗	−1.22∗	−3.63∗	−2.47∗	−3.91∗	−3.78∗
	(−16.15)	(−11.57)	(−5.45)	(−12.98)	(−10.73)	(−16.42)	(−5.63)
New border FE	−0.57∗	−1.35∗	−2.28∗	−0.21	−0.68∗	0.13	−1.75∗
	(−3.01)	(−3.42)	(−6.99)	(−1.51)	(−3.85)	(1.39)	(−6.15)
New border treatment	−1.57∗	−1.39∗	−0.74	−2.27∗	−1.89∗	−2.63∗	−1.17
	(−6.44)	(−2.99)	(−1.54)	(−8.76)	(−7.85)	(−12.21)	(−1.49)
Language	1.15∗	0.25	−2.38∗	−1.96∗	−0.26	−1.98∗	−5.67∗
	(3.03)	(0.41)	(−3.86)	(−3.92)	(−0.76)	(−5.48)	(−4.85)
Language x pre-war	−3.17∗	−3.57∗	−6.84∗	0.96	0.23	−0.58	3.15
	(−3.69)	(−3.19)	(−2.09)	(1.60)	(0.39)	(−1.06)	(2.01)
No-river	−0.27	−1.01∗	−0.57∗	−0.74∗	−0.81∗	−0.37∗	−0.94∗
	(−1.54)	(−3.94)	(−3.85)	(−7.77)	(−8.96)	(−4.62)	(−4.93)
No-river x pre-war	0.27	0.43	−1.01∗	−0.04	−0.31∗	−0.21	−0.68∗
	(1.24)	(1.24)	(−4.41)	(−0.31)	(−2.63)	(−1.70)	(−2.52)
Imp, exp effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
# of Obs.	7724	7882	7762	7675	7423	7482	7528
Adj R^2	0.94	0.89	0.87	0.86	0.79	0.88	0.97

Notes: ∗ denotes the 5% level of statistical significance. PPML Estimation based of balanced sample using robust standard errors.

and steel products and paper and related products) we can explain all of it.[27] To summarise, regions that were more heterogeneous in terms of their population and more affected by geographical barriers to trade did trade less than others at any point in our sample. For the same set of reasons, these regions were more likely to be separated by a border after 1918, as ethnicity, language and geography featured prominently among the arguments and motives of the peacemakers at Paris. Together this explains why we find less of a treatment effect of the new borders than the conventional literature suggests.

[27]We also tested extensively for changes in these effects over time. The results in table 8 report changes after the war, especially those related to the great depression. We also checked whether the trade frictions prior to 1914 increased. In most cases we find that the new border fixed effect is stable over the years 1885, 1910 and 1913. Only for iron and steel products do we find a significant increase between 1885 and 1913. Notably for rye we don't find any change over time. This is in line with our earlier findings on grain price dynamics for the Habsburg Empire 1878-1910 (Schulze and Wolf 2009). In that case we estimated a disintegration along the future borders along which the Habsburg Empire was split after 1918 between 1878 and about 1890, followed by a slight decline of that effect in about 1895 that stabilized afterwards. This suggests that our data set on railway shipments in 1885, 1910 and 1913 starts too late and includes too few points in time to pick up this kind of dynamics.

9. Conclusion

This study explored the impact of the border changes after the First World War on trade across Central and East-Central Europe. The new borders, imposed at the Paris Peace Conferences, did indeed create new barriers to trade. However, the treatment effect of these new borders tends to be much smaller than the pure cross-sectional effects. This is so because most of the 1919 border changes followed a pattern of trade relations across the region that was clearly established already before 1914. We conclude that re-drawing the map of Central Europe after the First World War was far less damaging economically than thought so far.

By adopting an arguably narrow trade perspective and limiting attention to a relatively small, if sectorially broadly representative range of commodities, this inquiry leaves aside several issues that ought to be explored in further work on the short- and long-run effects of the post-war settlement. These include, for example, changes in infrastructure after 1918 that we have taken into account only indirectly (via time-varying distance coefficients). We note however that the descriptive evidence on that matter would tend to strengthen our finding that the borders followed pre-existing frictions (Howkins 1999). Further, it might be revealing to analyse explicitly the changes in trade costs during the course of the Great Depression that were related to tariff policy, the introduction of exchange controls, or shifts in relative prices. From a broader perspective, the benefits (rather than the costs) the post-war border changes brought about in terms of access to public goods that correspond better with regional preferences (cf. Alesina and Spolaore 2003) are potentially promising areas of research. Addressing these issues, though, goes beyond the scope of this study.

However, already the limited the results presented here encourage some reinterpretation of Europe's inter-war economic history. The changes in political borders across Central Europe should not be treated as exogenous shocks to the economy, but were systematically related to the region's economic development prior to 1914. Put differently, our results reflect the tensions that existed between the economic relations and the political framework across Central and Eastern Europe prior to 1914. The German, the Habsburg and the Russian Empires of the 19th century were under pressure to accommodate the rising demand for political representation and public goods that would reflect their own political preferences, for example in terms of education. Our findings suggest that these frictions may have been exacerbated by an economic geography of resource endowments and natural barriers to trade that run counter to the existing political boundaries, as in the case of Germany (see Wolf 2009).

Finally, one might question to what extent our results on the effects of borders on trade hold more generally. The new post-war borders were (and still are) considered extreme cases of political barriers to trade. First, the border settlements of the Paris Peace Treaties were widely disputed, not only in the 1920s and 1930s and not only by the losers of the war. The fact that the border changes in 1919 were considered as arbitrary and harmful by so many observers suggests that our results present an upper rather than a lower bound estimate for the treatment effect of borders on trade flows: in many other cases changes in borders might have followed patterns of economic integration in a much more obvious way than in our case. Second, one might argue that it takes time before a change in political borders fully affects trade flows so that we just pick-up the short-run effect of new borders. However, our estimates include 1933, when European countries had just erected massive trade barriers along their borders in a protectionist response to the Great Depression. When estimating the treatment effects for 1933 separately from those for 1925/26, we find that they increased somewhat but they were still smaller than those obtained in a naïve cross-sectional estimation. Given that the new borders codified at Versailles, St. Germain and Trianon can be considered as extreme cases of political barriers to trade, we conclude that our findings should be fairly robust: the impact of re-drawing the map of Central Europe on trade was modest because these changes reflected an economic reality that had emerged already prior to 1914. More generally we conclude that borders matter and are highly persistent, because they follow deeper lines of economic relations.

CHAPTER 4

The Course of the Great Depression. A Consistent Business Cycle Dating Approach

Despite extensive research on the economic crisis of the 1930s, there is little consent about its causes. One exception to this is international economic dependency. As such, it is striking that to date there is no common understanding of the global depression's course, neither of its beginning, nor its end, nor its extent.

This chapter proposes a business cycle chronology of the Great Depression that is based on a comparative dating of turning points in 10 European countries, the United States, and Japan between 1925 and 1936. A set of monthly data on industrial production is evaluated by Markov-regime switching models. The dating derived is then used to analyze economic integration of business cycles during the interwar period. Three findings should be highlighted: First, the degree of comovement of output is high. This is also reflected by the coefficient on business cycle integration, which is high by postwar standards during the entire period. Secondly, the depression evolved in two 'stages' globally, not only in the US. The first stage has characteristics of a 'common' business cycle. It commenced several months before the Great Crash at Wall Street occurred in October 1929. The second stage was of a different type and unusually severe. It began mid 1931 despite the dissolution of the gold standard. The dating indicates that the global depression already ended in 1932 and thus before governments planned to undertake Keynesian measures. This implies, thirdly, that the world economic crisis began and ended earlier than is commonly presumed, probably due to a 'US bias' in research. Difficult to reconcile with many hypotheses, the US economy lagged behind on the international business cycle. The US was among the last 3 countries in the sample to enter recession in 1929 and among the last 3 countries to recover.

1. Introduction: The Great Depression and the business cycle

> *The most difficult challenge for [...] the monetary interpretation of the onset of the Great Depression, is the identification problem.*
> (Eichengreen 2004, p.2)

What was the starting point of the Great Depression, or, as Kindleberger (1973/86) put it, when did the world economy begin to "slide into the abyss"? Was it the Great Crash in October 1929, as suggested in the famous study of Galbraith (1954/2009)? This, at least, has become the dominating popular belief.[1] Many academic economists, by contrast, regard as the defining moment the Federal Reserve's decision to raise interest rates in fast sequence from 350 basis points in January 1928 to 500 basis points in August 1928. Again others emphasize Germany's stepwise default on reparation payments and foreign credit beginning in July 1931. Similar uncertainty prevails about the end of the depression: Did the dissolution of the interwar gold standard mid 1931 mark the beginning of its end? Was it the announcement and introduction of Keynesian measures for lowering unemployment in 1933 and 1934 that ultimately changed expectations?

Much of the dispute about causes and effects of the Great Depression is due to identification issues (cf. Tobin 1965, Eichengreen 2004). I argue that the absence

[1]Cf. the article "Great Depression" in the English Wikipedia edition (retrieved May, 7th 2011).

of any widely accepted dating of the Great Depression is part of this identification issue. At the time of the Great Depression, business cycle research was still in its infancy. Until today, statements regarding the beginning of the Great Depression, its end, or its course, remain arbitrary: Researchers tend to argue with a chronology of the crisis as well as with international co-movement such that it fits the respective hypothesis. Although no dating of the global cycle exists, many studies presume that the US business cycle was driving it. Yet, was the US actually the first major economy to turn down dragging the world into "the abyss"?

Many recent explanations of the world economic crisis attribute an important role to the interdependency of national business cycles before and during the crisis. One branch of the literature claims that monetary integration through the interwar gold standard proved fatal in spreading adverse shocks throughout the world economy (e.g. Eichengreen 1992, Bernanke 1995). Another strand points to strong financial integration through the framework of inter-allied credit and postwar reparation payments and argues that the 1929 recession pushed the German debt to GDP ratio to unsustainable levels (e.g. Borchardt 1979, Ritschl 2002, Ritschl and Sarferaz 2009). However, does the cross-country business cycle pattern fit with theory both during downturn and recovery? Were business cycles developing similarly, if countries followed similar economic policies?

Existing studies of interwar business cycles suffer from several shortcomings: The majority of them investigates business cycles in only one country, whereas comparative studies of international cycles rely on annual data.[2] A major weakness can be seen in the lack of data of sufficient quality and quantity. The interwar period spans only twenty years from 1919 to 1938. This period is effectively further shortened since the aftermath of WWI greatly disturbed European economies in the early twenties. Particularly in Central Europe, political instability, the creation of new borders, and ongoing military conflicts affected economic recovery. As I will demonstrate, low data frequencies are not suited for dating exercises, for they are too imprecise. Hence, both types are of limited value if identification is a major concern. Establishing comparative dating and knowing more about synchronicity would represent important building blocks for any explanation of the crisis. Moreover, it could resolve that part of the disagreement on the origins of the Great Depression —e.g. between monetarists and Keynesians or between economists from Europe and from the US— which is caused by different presumptions of business cycles at that time.

The present study addresses these issues by dating business cycles in the US, Japan, and 10 European countries. It utilizes a widely neglected industrial production and mining index, which was recorded monthly starting from January 1925. Empirical focus is on the the US and Europe. Japan is included to capture developments outside the Atlantic economy. The sample countries represent approximately

[2]This is the case, because they analyze business cycles in the long run, e.g. the entire 20th century, and aim to include as many countries as possible. Important studies include Backus and Kehoe (1992), Bergman, Bordo, and Jonung (1998), and Bordo and Helbling (2003).

50% of world GDP (in 1940) and account for a much larger share in industrial production, for instance about 90% of steel production (in 1925).[3]

One aim is to provide stylized facts on national business cycles that any theory of the depression should comply with. This concerns the chronology of turning points as well as the severity of the downturn. The dating is inferred from Markov-regime switching (MS) models, which have been applied for dating purposes beginning with Hamilton (1989). The dating obtained is then used to analyze the integration of cyclical economic fluctuations during the interwar period. To check robustness, I apply two standard approaches from business cycle analysis: (i) a dating procedure similar to the one followed by the US National Bureau of Economic Research (NBER) and (ii) a trend-cycle decomposition using the filter proposed by Hodrick and Prescott (1980).

The study contributes to the literature on interwar economic history as well as to the literature on business cycle integration. It attempts to reveal systematic similarities and differences in the development of IPM between countries. The focus is narrow: As a basis for further research, this study proposes consistent chronologies during the interwar period on a country-by-country level. Furthermore, it presents inferences on turning points of the international business cycle and examines the evidence for co-movement of cyclical fluctuations, regarded as an indicator of economic integration. The study makes references to important hypotheses, but it does not aim to systematically test whether these are consistent with the dating established.

The analysis proceeds as follows: The next section reviews the role of business cycle fluctuations in hypotheses on the world economic crisis. Section 3 provides results from the univariate regime switching models. The individual results are used in section 4 for making inferences on the course of the global crisis. Section 5 relies on standard approaches for country-by-country dating as a robustness check on section 3 and 4. Section 6 concludes this study.

[3]Based on GDP estimates of Maddison (cf. table 6 in appendix 7.)

2. Motivation and literature review

The extraordinary recession between the world wars still poses a puzzle and various theories have been developed to explain economic behavior during the crisis. Figure 1 depicts not only its magnitude, but also its global dimension.[4] It displays index values of physical IPM in levels and not, as one may suspect, deviations from trend growth. The extreme course of the series between 1929 and 1933 illustrates the world economy's "slide into the abyss" (Kindleberger 1973/86).[5] It implies that most economies virtually stagnated between 1926 and 1936. The whole industrialized world was affected by the crisis, with Central Eastern Europe, Germany, and the US, experiencing the strongest decline in IPM. IPM in Western Europe and in Japan began to decline at the same time as in the above mentioned countries, but at a lower pace. Of the major industrial economies, only Japan managed to escape the crisis early.

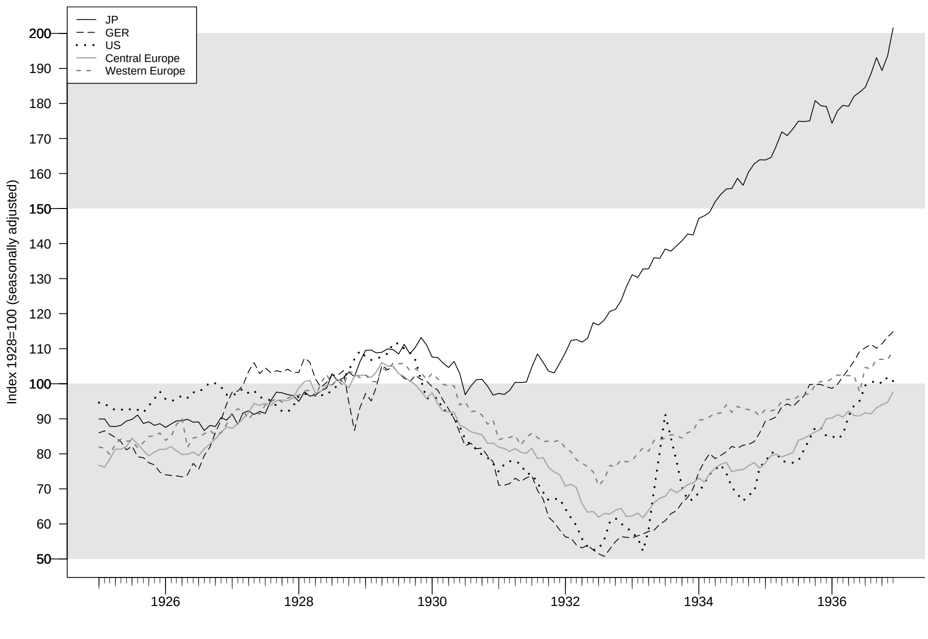

Figure 1: Industrial production in Europe, the US, and Japan, 1925–1936

[4]MS models in section 5 were estimated using the *MSVAR* package v1.31k in *Ox* v3.40.

[5]The data were seasonally adjusted using X11–ARIMA from the *pastecs* package in *R* (Ibanez, Grosjean, and Etienne 2006). In order to check robustness of the adjustment, I applied seasonal dummy variables as well, but without notable change in the results. Still, I consider X–11 to be more appropriate than the dummy variable approach, since I find it unreasonable to assume that seasonal effects remained constant over the course of the crisis.

Instead of investigating this global crisis at global level, hypotheses on cause and effect of the depression have often been developed solely from the analysis of the US economy.[6] Most prominently, Friedman and Schwartz (1963) hold the Federal Reserve's restrictive monetary policy in 1928 and 1929 responsible for a recession in the US, causing the recession elsewhere. Their main contemporary antipode, Temin (1976) argues for an autonomous drop in US demand. However, such 'US centric' research turned out to be unable to convincingly explain the downturn, neither in the US nor internationally (Eichengreen 2004). For some time now, the approach of research has been shifting toward analyzing the Great Depression in an international perspective. Turning away from this orthodoxy has added a further dimension to the debate about money, demand, or real causes of the depression: Location. In his seminal *Golden Fetters*, Eichengreen (1992) argues that the institutional set-up of the gold standard worked as a mechanism to propagate the fluctuations resulting from monetary policy shocks to the international economy as well as to aggravate them. Hence, the Great Depression was only indirectly triggered by the Fed's monetary policy in 1928 and 1929. As monetary policy of central banks around the world was tied to that of the Federal Reserve by the interwar gold standard, the rules forced several countries to pursue a restrictive monetary policy as well. This tightening was inappropriate for the economic situation of these countries, many of which were on the brink of recession at the time. The resulting economic decline outside the US kept the US from generating export surpluses to offset the decline in domestic demand. Similarly, most studies continue to suggest the origin of the crisis in the US, but consider the possibility that international economic dependencies functioned as central mechanism for spreading and aggravating the crisis (cf. Bernanke 1995, Eichengreen 2004). An opposing view rejects that the depression originated in the US and cites the cause of the decline —or, at least, its trigger— in the economic periphery instead (e.g. Ritschl 2002 and, quite early, Kindleberger 1973/86). The question, where this crisis originated is not a patriotic one of who owns the Great Depression (although it sometimes appears to be). Knowing when and where the depression commenced helps to detect the sequence of international spill-overs of the recession. Timing, location, and sequence are in turn necessary information for identifying the initial shock and to understand if particular institutions played a role in the crisis.

In the absence of a comparative dating of national business cycles, a chronology of interwar business cycles has often been presumed, explicitly or implicitly. These business cycles chronologies have played an important role for supporting or criticizing the consistency of explanations, particularly if the rationale itself depends on the chronology. The monetarist hypothesis of Friedman and Schwartz (1963) was weakened because, among other things, it was not possible to identify a large monetary shock in the US prior to the downturn in the US and elsewhere. Bernanke and Carey (1996) criticize Eichengreen and Sachs (1985) for choosing an arbitrary time frame for their analysis. Also Eichengreen's (1992) gold standard theory rests on a particular sequence of events in combination with the course of macroeconomic cycles, most notably in Europe. The same applies to the importance he attributes

[6] Eichengreen (2004) provides a comprehensive overview.

to untying the Golden Fetters, i.e. that the dissolution of the gold standard affected timing and speed of recovery.

I was unable to find an explicit identification in the literature of the beginning of the real economies' depression, but it is usually suspected to have commenced during the year 1930. Eichengreen (1992) denotes the Great Depression as the downturn following the stock market crash and the subsequent collapse of financial institutions, which would imply that it took place in 1930 or 1931. Kindleberger (1986, p.117) notes that "the New York stock market leveled off in the first months of 1930 [...] and so did industrial production, imports, and employment. Employment actually picked up from the December level." A similar dating is implied in Bernanke (1995) and Ritschl and Woitek (2000). Cole, Ohanian, and Leung (2005) assume that the international crisis took place between 1929 and 1933, but effectively investigate the period 1930-1933 only. They state that "average output fell only slightly [in 1930]" (2005, p.27). One the contrary, German economists suggest that the German business cycle peaked in 1928 already (cf. Voth 2003, Ritschl 2002). Meanwhile, some inconsistencies of the existing hypotheses have been acknowledged. Eichengreen (2004) emphasizes that Germany and other countries started to turn down before the United States in 1929, but insists that the US triggered the depression being the first country to cut interest rates.[7] Strikingly, even the study of Bergman, Bordo, and Jonung (1998) on international business cycles is not making inferences on the exact beginning and end of the global crisis. Much more consensus exists concerning the end of the Great Depression, which is commonly dated to 1933 (e.g. Cole, Ohanian, and Leung 2005, Mattesini and Quintieri 1997, Bernanke 1995, Eichengreen 1992).

These examples demonstrate that a consistent and comparative dating of national business cycles during the world economic crisis is yet missing, even though it would be necessary to identify shocks and distinguish their propagation. Moreover, in order to review a hypothesis in detail, it seems crucial to analyze data with a high frequency, ideally recorded monthly. Research has neglected the comparative account of interwar business cycles in the interwar period, probably due to the scarcity of quantitative information: One has to go back to Burns and Mitchell (1946), who conducted the first major dating of turning points of interwar business cycles. They analyzed and compared economic time series of the United States (US), Germany, the United Kingdom (UK), and France. Yet, their approach requires a great amount of data and, as pointed out by Romer (1994, p.574), "[statistical] techniques and the understanding of economic fluctuations have advanced greatly since *Measuring Business Cycles*." Burns and Mitchell's (1946) interest was to understand general, long-term business cycle patterns, not to scrutinize the Great Depression. So far, no one has made inferences on a chronology of the global business cycle during the interwar period and the Great Depression in particular.

Yet, this approach seems promising: Figure 1 suggests interdependencies in the global recession for several reasons: Many countries experienced the extraordinary slump in IPM. The crisis caused an unusual level shift of IPM across countries.

[7]Eichengreen (2004, p.5) refers also to the "problem noted by Temin (1971), that the German economy started turning down even before capital outflows from the United States were curtailed."

Moreover, turning points coincided both at the beginning and at the end of the crisis. These observations imply co-movement in the development of IPM across the selected regions, though less for Japan. Secondly, it appears to be difficult to restrict the Great Depression to the period 1929–1933. IPM in Germany stagnated already in 1928, and recovery after 1932 was sluggish both in the US and Europe. The simplest approach to assessing the extent of the crisis is to compute turning points directly from the data. The calculation is given in table 1. They are in line with the common claim that the US and Germany experienced the largest drop in industrial production during the Great Depression. But also in Poland and Belgium the drop in IPM from its pre-Depression peak to the subsequent trough amounted to over 50%. All of the remaining countries reached their respective peak production levels between August 1929 and February 1930. All the countries in this group reached the lower turning point between July 1932 and March 1933. These stylized facts are not exceptional, but they still challenge the traditional and the more recent explanations of the crisis. At the upper turning point, the US is not leading the cycle and the lower turning point appears much behind the dissolution of the gold standard. Yet, from descriptive approaches —such as figure 1 or table 1— it is not possible to derive a business cycle chronology and to detect patterns. This, in turn, makes it difficult to evaluate the evidence of a significant positive response in IPM to monetary policy shocks in the US in 1928. It also makes it difficult to see evidence of the dissolution of the gold standard in 1931 in most participating countries.

Table 1: Drop in industrial production between peak and trough level

Country	Peak date	Peak level	Trough date	Trough level	Change
Poland	Apr 29	107.9	Mar 33	47.9	-56%
United States	Jun 29	111.5	Mar 33	52.2	-53%
Germany	Apr 29	105.3	Aug 32	50.7	-52%
Belgium	Jun 29	104.0	Jul 32	50.2	-52%
Austria	Jun 29	109.3	Dec 32	58.5	-46%
Czechoslovakia	May 29	107.7	Mar 33	61.3	-43%
Hungary	May 29	106.9	May 32	66.5	-38%
France	Feb 30	113.3	Jul 32	72.7	-36%
Sweden	Jan 30	107.6	Jul 32	70.2	-35%
Britain	Aug 29	112.6	May 31	78.7	-30%
Finland	Aug 29	102.4	Mar 32	74.6	-27%
Japan	Nov 29	113.2	Dec 30	96.8	-15%

Notes: The figures are calculated on the basis of the seasonally adjusted data. The base year of each index is 1928=100.

In order to systematically evaluate the evidence, I will draw on approaches from the literature on business cycle dating and co-movement. In fact, business cycles research and economic history are overlapping regarding the Great Depression. The goal of business cycle research is to explain business cycles in principle. It is central to business cycle theory to study what kind of shock produces fluctuations and how fluctuations are diffused through an economy. For instance, in the aggregate supply–aggregate demand (AS–AD) framework, cycles result from domestic shocks to demand or supply. These can be either real or monetary shocks. It is also possible that fluctuations stem from the transmission of foreign shocks.[8] In order to produce stylized facts about these phenomena, economic research has developed various dating procedures. The analysis needs to apply empirical approaches developed in business cycle research, which are both appropriate in dealing with the unusual economic event and in describing the chronology of turning points across countries as well as synchronicity in business cycles. The above observations raise the question, if business cycle parameters are sensible means of describing the Great Depression at all. More precisely, was the depression merely a particularly strong or durable recessionary period or was it something fundamentally different from a business cycle downturn? After all, Basu and Taylor (1999, p.5) raise the question, whether "the Great Depression should be treated as a singular, anomalous event or a litmus test for any general theory of business cycles." Quantitative studies that use higher frequency data have examined business cycles in one or two countries at most (e.g. Ritschl and Sarferaz 2009, Mattesini and Quintieri 1997, Sims 1980). The likely presence of structural breaks and the short period of time require non-standard approaches. Ritschl and Woitek (2000), who resort to Bayesian methods of coping with these problems, note that

> Since Perron's (1989) critique of the unit root hypothesis and Hamilton's (1989) work on regime switches, there has been widespread skepticism about the correct way of modeling economic time series in the presence of apparent structural breaks. Together with the shortness of the available time series this appears to have impeded time series work on the interwar period. (pp.2–3)

Keeping this in mind, the absence of studies of business cycle co-movement during the Great Depression is not surprising. By contrast, there are many empirical studies on business cycle synchronization after World War II, and still a couple of studies that cover the late 19th century up until WWI. A'Hearn and Woitek (2001) conduct spectral analysis on a sample of 13 countries with prewar IP data. They detect regular long and short term cycles in most countries and find a core-periphery pattern with higher correlations in cycle patterns between core countries, e.g. the UK and Germany. They show that trading partners and members of fixed exchange rate regimes exhibit higher synchronicity than other economies. Backus and Kehoe (1992) compare means and variances of HP filtered real output series from the mid 19th century through the mid 1980s. In a sample of 10 countries, they find very low contemporaneous correlations prior to WWI. During the interwar period correlations for some countries are high —in a few cases, correlations are even higher than in the post-WWII period. Bergman, Bordo, and Jonung (1998) and Bordo

[8]Modern business cycle research has also established the conception of non-deterministic cycles that are caused by a continuous flow of stochastic shocks. The economic system turns these shocks into fluctuations, i.e. it functions as an impuls-propagation mechanism. The resulting quasi-cycles are characterized by variable frequencies and amplitudes.

and Helbling (2003) examine co-movement in GDP of 16 countries between 1880 and 2001. However, all these studies rely on annual data and cover the interwar period along the way, i.e. they do not aim to understand the Great Depression. Artis, Krolzig, and Toro (AKT) (2004) analyze co-movement in changes of industrial production after WWII. By dating business cycles in 10 European countries using Markov switching models, they make inferences on the existence of a common European business cycle.

Hence, one contribution of my study is that the new data set allows the use of regime switching models, which outperform other approaches under the circumstances seen in the interwar period. Furthermore, the model produces stylized facts that are easily comparable and, as such, has become increasingly popular for characterizing fluctuations in aggregate economic activity (see e.g. AKT 2004). A sufficient amount of data facilitates the use of advanced methods of business cycle analysis. Modern econometric approaches, such as filtering methods or Markov switching and factor models, are otherwise not as powerful or cannot be applied at all. This in turn, produces results, which are directly comparable to results from the postwar period. Considering more data is not merely a technical point; monthly data contain much more information on fluctuations than annual data.

3. Data issues

The empirical approach outlined in the previous section requires data of sufficient quality and quantity. With regard to the interwar period, this requirement is not easy to meet. The data sample compiled for this study consists of a monthly index of physical production in industry and mining from 10 European countries, the US, and Japan. All IPM data used in this paper were taken from two volumes of *Statistisches Handbuch der Weltwirtschaft*, published by Statistisches Reichsamt in 1936 and 1937. In contrast to most studies analyzing log difference, the monthly data is being transformed into year-on-year (yoy) differences in percent. Because of large jumps in the series, log differences are not a good approximation of the percentage change.[9]

The analysis requires time series for each country for an identical time period and of the same frequency. Because of missing data, four series were partly interpolated or extrapolated in order to obtain identical frequencies and time periods, specifically for the UK, Japan, Hungary, and Finland. For each of these countries I computed correlations between the monthly change in IPM and monthly changes in physical production in certain industries of the respective country. I investigate preceding, contemporaneous, and subsequent correlations in order to identify (a combination) of time series with high contemporaneous correlation and low correlation at preceding and subsequent lags. The selected series are used to interpolate or extrapolate, respectively, the missing data. Details of this manipulation are provided in appendix 1.

The data is chosen as a proxy for aggregate economic activity, because the IPM index has been used earlier (e.g. Bernanke and James, 1991, analyze US and German IPM data) and has several compelling features: It is internationally comparable regarding its composition and recording (see Mattesini and Quintieri 1997, FN 5). Moreover, one does not need to price-adjust or convert the data. Moreover, it is one of few broad indicators related to aggregate economic activity that was recorded at higher than annual frequency during the interwar period.

The frequency of the data is a central issue in business cycle research. In Burns and Mitchell's (1946) standard work *Measuring Business Cycles* several sections are devoted to the question why time units matter. They demonstrate that low frequency observations of economic time series can lead wrong inferences on economic developments. If the worst comes to the worst, an entire business cycle may not show up in annual data. Another issue is the application of econometric models to data with low frequency. Taylor's (2001) study points to problems in identification of adjustment processes if the data have inadequate frequencies.[10] His caution must

[9]The results presented in this section stem from the estimation of the first differences of each series computed as percentage change on the month earlier. Using the augmented Dickey-Fuller (ADF) test, the presence of a unit root could not be rejected at the 5% significance level for each series. After taking first differences, the null of a unit root was rejected at the 5% significance level in each case, which indicates stationarity.

[10]He shows that estimating responses in price adjustments —within days— using low frequency data —usually monthly or even quarterly— combined with insufficient model specification can substantially bias the estimator.

be taken seriously, because the analysis of business cycles draws on adjustment processes. Figure 2 depicts that annual data are not sufficient to measure the reaction of economies to domestic or foreign shocks during the interwar period. It compares the year-on-year change in US industrial production taken from my sample to the annual year-on-year figures used by Bernanke and James (1991, p.45).[11]

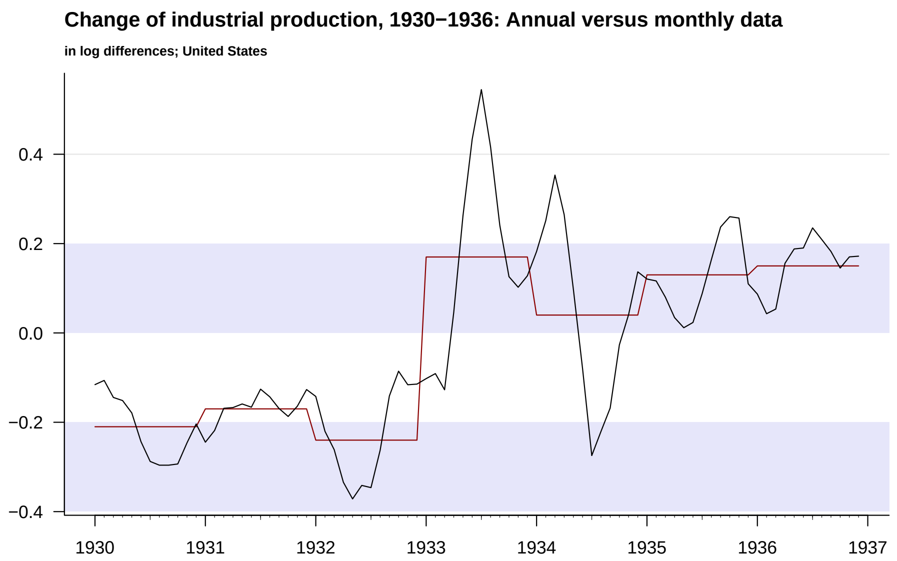

Figure 2: Great Depression in the US: annual vs. monthly IP data

The graph shows three notable differences between annual and monthly data: Whereas the annual data imply one big positive shift between 1933 and 1934, the monthly data show a stepwise improvement of the situation between mid 1932 (potentially a turning point), spring 1933 (another potential turning point), and mid 1933. Looking only at annual data, one would not discover the positive spikes in mid-1933 and early 1934 as well as the subsequent steep decline in 1934. In fact, the annual figure for 1934 remains positive (+4%) in spite of a potential recession suggested by a decline of 10%–20% over several months. Similar differences can be found for Great Britain, Germany, France, and Japan (respective figures can be found in appendix 2).

The question remains though, whether the IPM index is in fact a good indicator of overall economic activity for all countries in the sample. Several economies in the sample were still dominated by agriculture, and industry and mining accounted only for a minor share of output. Countries that largely depended on agriculture might well have experienced a weaker crisis than suggested by the IPM index, since

[11] The reason for using log differences here is that Bernanke and James (1991) provide their annual data accordingly.

unemployment occurred mainly outside agriculture. The example of Poland shows that changes in IPM can be expected to reflect fluctuations in overall economic activity sufficiently well, nevertheless. During the interwar period, only one third of the workforce was employed outside agriculture (cf. table 6 in appendix 7). Landau and Tomaszewski (1982, 1986) contrast these figures with the importance of the Polish industry for the whole economy. On the one hand, Polish industry was producing manufactured goods to a large part for Poland's agricultural sector. On the other hand, they argue that in interwar Poland "[...] the business cycle determines not only the level of industrial production, but in practice the entire life" (Landau and Tomaszewski 1986, p.120). Under intense international competition, Polish agriculture was extremely dependent on the country's industry, since miners and workers were the most important consumers of its products. Hence, the downturn in Polish industrial activity depressed demand for agricultural products (Landau and Tomaszewski 1986, p.123). Such an interdependency between agriculture and industry can be expected to hold for other countries in the sample as Poland is presumed to have had the largest agricultural sector at the time.

An additional feature of the data is a large variance of the time series. The data show that economies around the globe were growing strongly in the months before the commencement of the crisis: Already in April 1929, Polish IPM was 7.9% above the annual average in 1928. The respective figures for other countries were 9.3% in Austria (June 1929), 11.5% in the United States (US) (June 1929), and 13.2% in Japan (November 1929). Table 1 shows that after these peak dates, a massive decline in IPM occured in Western and Central Eastern European countries as well as in the US and even Japan.[12] Interwar French IPM appears to be smoother than in other countries.[13] During the interwar period, both negative and positive growth rates were generally much larger than during the postwar period. In addition, there is evidence of exceptionally adverse recessionary regimes, i.e. outlier states, in several economies during the Great Depression. Nothing similar has been detected for economies after WWII.[14] The advantage of Markov regime switching models, which will be used for the subsequent analysis, is that they can explicitely model exceptional economic growth rates and heterogeneity.

[12]Central Eastern Europe: Austria, Czechoslovakia, Hungary, and Poland; Western (and Northern) Europe: Belgium, Britain, France, Sweden, and Finland. Unweighted averages.

[13]The reason for this may be that this series was seasonally adjusted by the French statistical office and not by the author. The office did not specify the method used for adjustment.

[14]Cf. Hamilton (1989, 2005), Krolzig (1997), and AKT (2004).

4. Empirical approach

Empirical evidence suggests that many macroeconomic variables behave differently during economic upswings and downturns, i.e. the underlying data generating process (DGP) is subject to non-linearities (Hamilton 1989). One potential reason is that utilization of factors of production in an economy changes with different states of growth of that economy, e.g. during booms and recessions. Hamilton (1989) proposes Markov regime switching (MS) models for dating purposes, which formalize these observations in economic states. The state is one of several factors affecting output dynamics. The evidence on regimes and regime shifts can then be used to make inferences on growth rates, persistence, and turning points. Hamilton (1989) shows that MS models do well in capturing non-linearities. The MS model was chosen, because a major shift in factor utilization is most likely during an event as disruptive as the Great Depression. Moreover, the approach is well suited to deal with business cycle anomalies and structural breaks (Krolzig 1997). This is important as figure 1 suggests the presence of structural breaks in the IPM series.

In Hamilton (1989), the MS model consists of the following autoregressive (AR) process of order one or n, subsequently denoted as AR(n), which models fluctuations in a series y_t, which in my case is the IPM index at time t.

$$y_t = c_{s_t} + \phi y_{t-1} + \epsilon_t \text{ , where } \epsilon_t \sim N(0, \sigma^2) \tag{4.1}$$

c is a constant, which takes on different values depending on the state of the economy s_t. c changes when the economy is shifting from one state $s = 1$ into another state $s = 2$. Put differently, c is time-invariant, but state-dependent, while s is time-dependent. The states themselves are not observable.

MS models are based on such AR processes with variable dynamics. The approach has proved to be well suited for the analysis of macroeconomic fluctuations in the postwar period (see Krolzig 1997, Hamilton 2005) and should be particularly advantageous for the interwar period, because the approach can account for various non-linearities as it allows for time-varying AR parameters. Following Hamilton, I will estimate a two-state model for each country. Based on this, I will select appropriate country-specific models. The resulting turning points will then used to make inferences about business cycle co-movement in section 6.

As a initial specification, I assume the existence of two states of the economy. Thus, the DGP is driven by two regimes: During regime 1 the economy is in recession, during regime 2 it is expanding. Let regime 1 prevail from time $t = 1$ up until $t = t_1$, i.e. in period $t_1 + 1$ a regime shift occurs in the stochastic DGP. The AR process under regime 1 with $s_t = 1$ is defined as

$$y_t = c_1 + \phi y_{t-1} + \epsilon_t \tag{4.2}$$

for $t = 2, ..., t_1$. As the model is applied to the first difference of the IPM index, the coefficients c_1 and c_2 reflect the mean growth rate during state 1 and state 2, i.e. during recessions and booms, respectively. The complementary model with $s_t = 2$ for $t = t_1 + 1, t_1 + 2, ..., t_2$ gives

$$y_t = c_2 + \phi y_{t-1} + \epsilon_t \tag{4.3}$$

Note that s_t itself is a random variable. When estimating the model, one infers on the probability that one regime —which leads to the respective state— prevails, conditional on the available set of information about the likely state in the previous period. The simplest specification is that s_t is the realization of a two-state Markov chain with

$$\begin{aligned} Pr(s_t = j \mid s_{t-1} = i, s_{t-2} = n, ..., y_{t-1}, y_{t-2}, ...) \\ = Pr(s_t = j \mid s_{t-1} = i) = p_{ij} \end{aligned} \tag{4.4}$$

This way of modeling business cycles implies that if the inferred state of the economy is expansionary and growth regimes have proved to be highly persistent (p_{ij} close to 1), output can temporarily decline without changing the inferences on the state. The probability that the economy is in state 1 depending on being in state 1 in the previous period p_{11} can also be used to compute the expected duration of a recession in the economy.

Krolzig (1997) emphasizes that model selection within the MS framework is not always straightforward. Since the estimation relies on numerical and iterative procedures, one runs the risk of obtaining local rather than absolute maxima. As a solution, he recommends to begin with a prior on the DGP as well as on potential regime shifts and to apply general-to-specific modeling (Krolzig 1997, chapter 7). My approach is to use regime probabilities obtained from a parsimonious two-state model as a prior about DGPs, specifically a MSM(2)-AR(7). This is a MS model with switches in the mean (M) and a seventh order AR process. It is a slight modification of the MS model in Hamilton (1989).[15] The fit of this model as well as the resulting AR-coefficients serve as starting point for more complex models.

Table 2 presents the results.[16] Most importantly, the Davies test rejects the null of a linear model in all cases. In similar studies of the postwar period, parsimonious two-state models mostly perform very well (cf. Hamilton 1989, Krolzig 1997). For the interwar period, the two-state model yields meaningful regime switches for the course of IPM in Germany, Japan, Sweden, Austria, Czechoslovakia, and Finland, but not for the US, the UK, France, Belgium, Poland, and Hungary. For Germany, the switching model provides evidence of evidence of a short recession in mid-1925 and of a W-shape recession from the spring of 1929 to the fall of 1932. The estimation gives switching means with coefficients $\mu_1 = -2.29$ and

[15]Hamilton applied a MSM(2)-AR(4) to quarterly US GDP. However, models with higher AR-order than 7, like the analogous MSM(2)-AR(12), did not converge for all countries in the sample probably due to the short time period.

[16]Table 1 in appendix 4 gives results for every country in the sample. Appendix 4 contains the corresponding plots of regime probabilities for every country in the sample (cf. figures 3a-4f as well as table 1 in appendix 4).

$\mu_2 = 1.41$. These growth rates relate to the following periods, where the probability of the German economy being in recession is above 50%: From September 1925 (1925:9) until January 1926 (1926:1), from 1928:9 until 1928:11, from 1929:7 until 1931:1, and from 1931:8 until 1932:7.[17] The coefficient μ_1 implies an annualized growth rate of German IPM during recessions of –24.3%, which is computed as $((1 + (-0.0229))^{12} - 1) * 100\%$. The corresponding annualized growth rate during booms is 18.3%. The corresponding postwar growth rates for Germany are much lower with +1.6% during expansions and −2.3% during contractions in industrial production between 1970 and 2000 (AKT 2004). The persistence of recessions — given by the probability of remaining in regime 1 conditional on being in regime 1— is $p_{11} = 0.895$ and hence only slightly lower than between 1970 and 2000. From p_{11} one can infer on the expected duration of a recession in Germany, which is nearly 10 months $(1 - p_{11})^{-1} = 9.5$.

Table 2: Regime coefficients obtained from an MSM(2)-AR(7)

Coefficient	**Germany**	**Austria**	**Japan**	**Finland**	**US**	**Sweden**	**CSR**
Regime-dependent means							
μ_1	-2.29	-1.82	-0.74	-0.86	-0.02	-3.71	-0.78
μ_2	1.41	1.99	1.19	0.82	1.15	0.73	2.21
AR coefficients							
α_1	0.233	-0.694	-0.464	-0.414	0.660	-0.501	0.431
α_2	-0.015	-0.427	-0.390	-0.147	-0.183	-0.284	0.197
α_3	0.039	-0.456	-0.183	-0.163	0.033	-0.061	-0.213
α_4	0.110	-0.493	0.077	-0.192	-0.139	-0.006	0.364
α_5	-0.138	-0.261	0.230	-0.097	-0.029	0.152	0.211
α_6	0.106	-0.265	0.433	-0.146	-0.103	0.134	-0.158
α_7	0.097	-0.072	0.268	-0.062	0.207	-0.007	-0.148
Persistence of recessions: Transition probability and expected duration in months							
p_{11}	0.895	0.795	0.703	0.945	0.984	0.635	0.871
duration	10	5	3	18	62	3	8
Variances and number of months in recession between 1925 and 1936							
σ^2	5.02	8.27	1.76	10.85	4.71	8.70	1.21
# month rec.	40	66	41	30	131	12	94
# obs.	136	136	136	136	136	136	136
$\ln L$	308.84	253.13	362.39	266.13	312.65	270.83	372.38
LM rejected	yes	yes	yes	yes	yes	yes	yes

Notes: "LM rejected" refers to the rejection of the H_0 of a linear model using the Davies test. "# month rec." gives the number of months for which the probability of recession is above 50%.

The results for the remaining countries are similar. Average growth rates are comparably high. Persistence of regimes is low, reflecting great economic instability at the time. IPM in Austria and Czechoslovakia was subject to frequent regime

[17] This dating is similar to the one yielded by the pseudo-NBER approach, namely from the second quarter 1925 (1925–II) to the first quarter 1926 (1926–I) as well as from 1927–III to 1927–IV, 1928–II to 1928–IV, 1929–III to 1931–I, and from 1931–III to 1932–III.

shifts even after 1933. In Austria recessions were rather short and strong, whereas in Czechoslovakia they were less severe and lasted longer. For Czechoslovakia, the most industrialized part of the former Austro-Hungarian empire, I find a high probability of recession during almost 70% of the sample period. Finland and Sweden show the fewest number of months in recession. The Swedish economy was subject to few, very short and strong contractions. In Finland there was only one modest, but extended recession. Japan was subject to frequent regime shifts only until 1931 and recessions were both shorter and less severe than anywhere else: the annualized growth rate of IPM during recessions was only –8.5%.

In general, though, the MSM(2) struggles to capture the development of IPM in several countries during the interwar period appropriately. The graphs indicate that the initial two-state model does perform very well in discriminating between periods of growth and periods of decline. In some cases, the model indicates that one regime prevails all the time. In other cases, the probability of one regime's dominance is not clearly higher than that of the other regime.[18] The incomplete success of the MSM(2)-AR(7) model suggests that the economic development during the interwar period requires more complex MS models.

Both Krolzig (1997) and AKT (2004) show that allowing for more than two states can improve the performance of MS models in capturing business cycles. The applicability of MS models depends on amplitudes and exceptional states of growth in the respective time series, e.g. very high growth rates in Japan during the 1970s. Such features are likely to be the reason for the above results as well. Take the graphs from the pseudo NBER approach (appendix 3) as an example: The IPM series of countries for which the MS model fails to produce sensible estimates, indeed they have exceptional amplitudes. Within a short period of time, the amplitude is larger than 30% in the US (1933) and Sweden (1925) and larger than 60% in Belgium (1935) and Britain (1926). Another reason, why the MSM struggles to identify regimes, is a low persistency of regimes, i.e. very high frequencies of economic fluctuations. This is the case for French, Polish, and Hungarian IPM.

One way to extend equation (4.1) is to allow for state-dependent heterogeneity as well as for state-dependency of AR coefficients. In addition, it is possible to relax the restriction on the number of states, i.e. to allow for three or more regimes.

$$y_t = c_{s_t} + \phi_{s_t} y_{t-1} + \epsilon_t \tag{4.5}$$

For each state I assume $\epsilon_t \sim N(0, \sigma^2_{s_t})$. It is possible to test, whether a model with fewer restrictions, like (4.5), is superior to a more parsimonious one such as (4.1). Model choice is difficult, but it can be crucial for results. The aim is to select a suitable MS-model for each economy in the sample drawing on the MSM(2) results. Since more complex set-ups are necessary, MS models with shifts in the

[18] The former is the case for the US, Poland, France, and Sweden; the latter is the case for the UK, Hungary, Czechoslovakia, and Belgium.

intercept (MSI) are applied.[19] MSI are not as computationally demanding as MSM and better suited for the analysis of less restrictive and as such less parsimonious models. Krolzig (1997) shows that for dating purposes, both approaches lead to comparable results for simple specifications with two regimes. In order to indicate the differences between estimation outputs, I use different notation for the respective state parameters, namely ν in the case of MSI.

Based on the initial MSM(2)-AR(7) model and on the rejection of the linear model, I adopt a three step strategy:[20]

(1) If MSM(2) and MSI(2) yield regime probabilities indicating that a two-state model is not appropriate, I relax restrictions on the number of regimes incrementally up to $n = 4$.
(2) Once the estimation output indicates that the number of states suffices to capture the fundamental development of IPM, I use diagnostic statistics of the residuals to check whether restrictions ought to be extended or relaxed (a) on the number of AR terms, (b) on the state-dependency of heterogeneity, i.e. error variances σ_{s_t}, as well as (c) on state-dependency of the AR-terms.
(3) Using the likelihood ratio (LR) test, I decide whether additional restrictions are necessary or dispensable.[21] As the estimation results are quite sensitive to the model specification, I apply the Akaike (AIC) as well as the Bayesian Information Criterion (BIC) as further test statistics. Results from alternative specifications are mentioned if sensible. Finally, if several models are appropriate the most parsimonious model is chosen.

I exemplify the empirical strategy using the case of the United States, for which both MSM(2) and MSI(2) suggest that interwar IPM is modeled more appropriately by more than two growth states. Figure 3 presents a MSI(3)-AR(7) to model month-to-month percent differences in US IPM. In the upper row, the fitted model is plotted against the actual data (dotted vs. bold line). The shifts in the intercept are well recognizable. In the rows below the conditional probability for each regime is plotted over time (vertical line) together with two modifications. The 'smoothed' probability (dotted line) takes into account the information of the entire data set, whereas the Kalman filtered, 'unsmoothed' series (horizontal line) indicates the state probability of the economy at time t based only on the information available at time t (for details see AKT 2004). The filtered probability can be interpreted as

[19] In general, MSI models allow for smooth regime shift, whereas MSM models assume regime shifts within one period. For a conceptional description and a detailed comparison of the MSM and MSI approach, cf. Krolzig (1997, section 11).

[20] The LR test on the number of states does not have a standard asymptotic distribution. Thus, in general it is not possible to test $H_0\colon \mu_1 = \mu_2$ vs. $H_1\colon \mu_1 \neq \mu_2$ or $H_0\colon \nu_1 = \nu_2$ vs. $H_1\colon \nu_1 \neq \nu_2$, respectively (Krolzig 1997, p.247). However, a general test on the linearity of the model can be performed using the Davies test, which also belongs to the class of likelihood ratio tests. It compares a model with n = 1 regimes, i.e. a linear model, to more complex models. The null is given by $H_0\colon n = 1$ vs. $H_1\colon n \geq 2$ regimes. Cf. Davies (1987).

[21] The LR test can be based on the LR statistic $LR = 2(lnL(\tilde{\lambda}) - lnL(\tilde{\lambda}_0))$, where $(\tilde{\lambda}_0)$ denotes the restricted ML estimate of the parameter vector λ. Under the null, LR has an asymptotic χ^2 distribution with r degrees of freedom. r represents the number of restrictions.

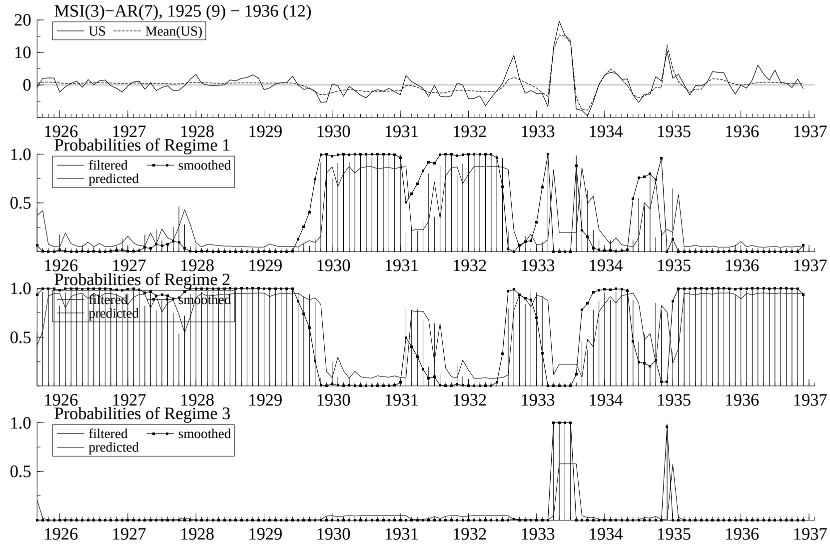

Figure 3: US IPM: MS model with 3 states, MSI(3)-AR(7)

the inferences of a well-informed contemporary observer, e.g. the central bank, and the smoothed probability represent the retrospective view. The business cycles are dated based on the smoothed probabilities; Policy, by contrast, should be evaluated using the filtered series.

Although most of the variation in the time series can be explained by two regimes, the fit of the model is much improved by allowing for a third regime. The third regime leads to an economic "outlier state" (Krolzig 1997, p. 240). An outlier state is an exceptionally negative or positive state of the economy. However, even allowing for a third state, the model is still unable to account for the strong contractions observed during the first half of 1932, in 1933, and in the second half of 1934. Allowing for two outlier states, I apply a MSI(4)-AR(7) model, which accounts for periods of extreme contraction and of extreme expansion. The results are given in the second column of table 3. The estimation identifies two regimes of moderate growth and decline, ν_3 and ν_2, as well as two regimes that entail an extremely positive and an extremely negative economic state, ν_4 and ν_1, respectively. However, a plot of the residuals still suggests the presence of heterogeneity. Using the LR test the MSI(4) model is compared to a less parsimonious MSIH(4) model, which accounts for state-dependent heterogeneity (H). The test statistic is computed as $LR = 2(lnL(\tilde{\lambda}) - lnL(\tilde{\lambda}_0)) = 2\,[(-282.42) - (-294.89)] = 24.94$. At the 1% significance level, the critical value is given by $\chi^2_{0.99}(4) = 13.28$. Table 3 also shows that relaxing the assumptions on heterogeneity substantially changes the estimated intercepts. The rejection of the null-hypothesis implies that the MSIH

Table 3: Estimation of different MSI(n)-AR(p) for US IPM

Coefficient	MSI(3)-AR(7)	MSI(4)-AR(7)	MSIH(4)-AR(7)
Regime-dependent intercepts			
ν_1	-2.498	-3.529	-2.704
ν_2	0.790	-0.160	-0.697
ν_3	11.472	1.989	0.786
ν_4		11.532	4.911
Regime-independent AR coefficients			
α_1	0.275	0.258	0.228
α_2	-0.151	-0.209	-0.134
α_3	-0.109	-0.105	-0.112
α_4	-0.177	-0.210	-0.072
α_5	-0.053	-0.130	-0.110
α_6	-0.136	-0.177	-0.179
α_7	0.045	0.017	0.181
Persistence of regimes: transition probabilities			
p_{11}	0.875	0.829	0.907
p_{22}	0.954	0.890	0.502
p_{33}	0.577	0.865	0.965
p_{44}		0.596	0.506
Variances, regime-dependent and independent			
σ^2	3.27	2.28	
σ_1^2			2.28
σ_2^2			0.02
σ_3^2			2.85
σ_4^2			40.32
Number of months in recession, 1925–1936			
# rec. month	42	86(31)	48(34)
# obs.	136	136	136
ln$L$	-302.34	-294.89	-282.42
LM rejected	yes	yes	yes

Notes: 'LM rejected': H_0 of a linear model was rejected using the Davies test. '# rec.': Number of months in recession (due to regime 1).

should be adopted.[22] Both the AIC and the BIC corroberate the selection of the MSIH in place of the MSI model. After comparing the MSIH(4)-AR(7) model with other specifications, the MSIH(4)-AR(7) is selected as appropriate specification.[23]

[22] These tests implicitly require the regime-preserving hypothesis $\nu_1 \neq \nu_2 \neq \nu_3 \neq \nu_4$. The number of degrees of freedom is given by the difference in restrictions. It is equal to four, because in the case of MSI(4) homogeneity is assumed in four states.

[23] The MSIH(4)-AR(7) was tested against the following specifications: MSIH(4)-AR(6), MSIH(4)-AR(8), and MSIH allowing for state-dependent AR terms MSIAH(4)-AR(7). MSIH(4)-AR(6) was rejected by LR, AIC, and BIC. The algorithm of the program was unable to obtain numerically stable estimates for the MSIH(4)-AR(8). On the MSIAH(4)-AR(7), the resulting test statistics were contradictory: The LR test rejected the null of state-independent AR coefficients at the 5% level, whereas both AIC and BIC yielded lower values for the MSIH than for the MSIAH.

5. Results I: Dating of national business cycles

5.1. United States. The MSIH approach identifies four regimes, of which two regimes exhibit moderate growth and two regimes exhibit extraordinary growth (cf. figure 4). The estimation output of the MSIH model is provided in column 'United States' of table 4 (p.103). Regime switches and estimated growth rates appear reasonable. The intercepts of the moderate regimes (ν_2, ν_3) imply annualized growth rates of about –8.1% during recessions and 9.8% during upturns.[24] The annualized growth rate implied by ν_1 reflects an outlier state with an average growth of –28.0% p.a. These coefficients are still reasonable though, since industrial production is much more volatile than GDP. The estimation yields AR coefficients that are mostly negative, which accounts for the fluctuation of the series around the conditional intercepts. Allowing for state-dependent heteroskedasticity reveals huge differences in error variances σ_s between the states.

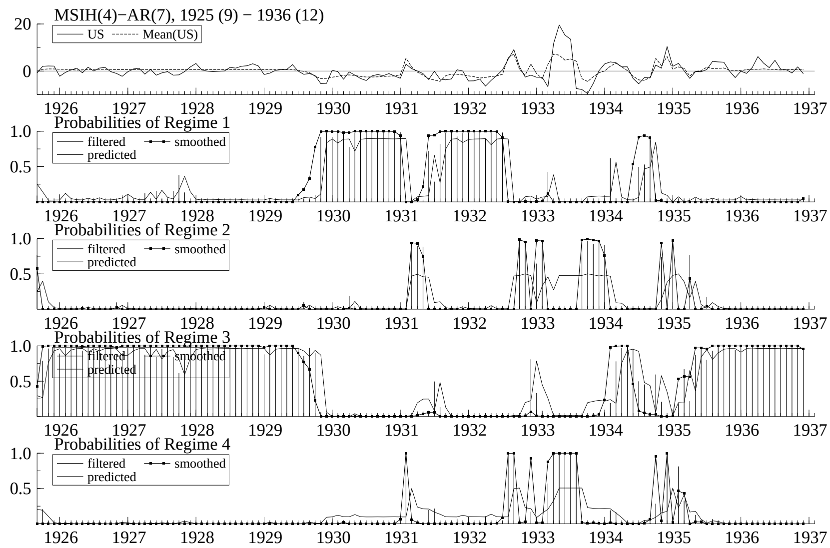

Figure 4: US struggled with a double-dip recession and fragile recovery

The business cycle model indicates that a moderate growth regime prevailed uninterrupted from the beginning of the period of the study to September 1929. The commencement of the Great Depression is dated to October 1929. At this point in time, the smoothed conditional probability of being in regime 1 was above 0.5 for the first time. This dating accords with the quarterly dating provided by the NBER. The fitted probability, by contrast, indicates that the downturn became

[24] In order to compute the annualized growth rates for each state from the conditional intercept estimates, one needs to calculate the mean of the AR processes. Under the assumption that $s_t = s_{t-1} = \ldots = i$, one can compute the mean for state i analogously to means of standard AR processes, i.e. $\mu_i = \frac{\nu_i}{1-\alpha_i}$. I am grateful to Hans-Martin Krolzig for pointing this out to me.

apparent for contemporary observers first by November or even December 1929. It is an interesting feature of the MS approach to formalize this divergence between the actual and the perceived state of the economy, which is likely to have added to the severity of the Great Crash on Wall Street occurring in the last week of October 1929. Furthermore, the model detects the supposed two stages, or W-shape, of the recession. The end of the first stage of the depression is dated to January 1931; its second stage is estimated to have lasted from May 1931 to June 1932.[25]

Recovery in the US remained fragile afterward. At first, the index increased strongly, but dropped again strongly in October 1932. Although industrial production rose shortly after the US had left gold in March 1933, the economy remained on the brink of recession until January 1935. The interruption of recovery in September 1933 and frequent regime shifts thereafter characterize a period, in which economic recovery of the US fell behind recovery of other countries. Only from March 1935, the probability of being in an expansion is found to be persistently above 50%, which is a notable contradiction to the official NBER dating. The conditional means ν_1 and ν_3 accord with the views that the downturn was harsh, whereas growth, even after 1933, remained only modest. The low value of p_{22} —the probability of remaining in regime 2 conditional on being in regime 2— reflects the low persistence of this state. Apparently, regime 2 leads to a transitory state. Two findings add to this impression: First, the corresponding switching probabilities p_{21}, p_{23}, and p_{24} are comparably high (cf. table 4). Second, figure 4 shows that the US economy was driven by regime 2 primarily in the process of switching from regime 1 back into growth. This is the case in early 1931 and late 1932. The fact that the high growth regime immediately follows on the recessionary regime shows that Sichel's (1994) observation of fast recovery after postwar recessions also applies to the interwar period. Contrary to the postwar period though, the slumps appear too large and regime 4 too short-lived to boost output back to pre-recession levels.

5.2. Japan. Figure 5 shows the estimated regimes for the Japanese interwar economy. There are several notable differences to the development in the US. The most obvious one is the existence of stable regimes in a regular two-state pattern similar to the one observed for postwar economies (see e.g. Krolzig 1997). The beginning of the recession in 1929 is dated to August 1929. However, the smoothed probability rises only gradually and is close to 100% first in December 1929. It drops again below 50% in December 1930. While the fitted probability indicates that a return to the recessionary regime was possible in late 1931, the smoothed probability is unambiguous: The economy remained in the expansionary regime beginning in January 1931. The estimation suggests that Japan experienced prolonged growth thereafter as opposed to the frequent regime shifts characterizing the development of US IPM between 1931 and 1935. The early end of the recession in Japan implies that the country was affected by the world economic crisis for a shorter period of time than any other country in the sample. Like other countries, Japan experienced

[25]These turning points are obtained independent from using models with three or four states.

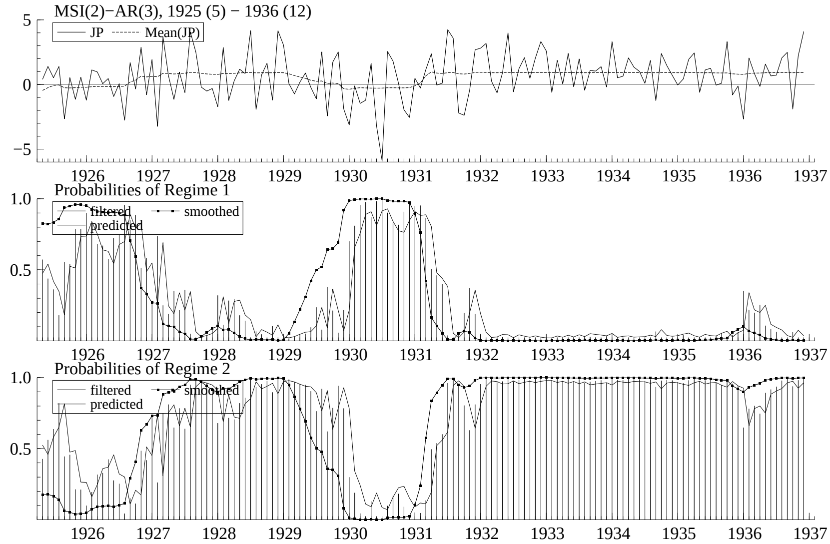

Figure 5: Japan avoided a second slump during the Great Depression

a recession in 1926. This finding suggests a global dimension even of this earlier contraction, although evidence for a US recession is mixed.[26]

5.3. Germany. The algorithm leads to the adoption of either a MSIAH(4)-AR(7) or a MSIH(4)-AR(3) model.[27] I adopted the MSIH(4)-AR(3), because it is more parsimonious (cf. figure 6). Independent from the model chosen, the filtered and smoothed regime probabilities suggest that negative innovations depressed the German economy repeatedly between 1925 and 1928.[28] These innovations could be varying reparation and transfer schemes during the 1920s, which Ritschl (2002) suspects to have induced particular German business cycles (*Sonderkonjunkturen*). Moreover, the temporary rise in Germany's IPM was probably fueled by the inflow of American capital at that time (cf. Ritschl 2002). One circumstance that positively affected Germany's IPM, is that Britain ceased to export in the world coal market during the miner's strike in 1926. Subject to frequent regime shifts over the late 1920s, the German economy was likely to have been weakened at the time the Great Depression commenced.

The MSIH model dates the onset of the Great Depression in Germany to May 1929. Like in the US, there is evidence of two stages of the recession, even at about the same time. The beginning of the first stage of the depression in Germany is

[26]In case of the US, I obtain evidence of a recession between 1925 and 1929 only from the pseudo-NBER approach, but not from the MS-AR estimation.

[27]Whereas the LR test and AIC prefer the MSIAH, the BIC suggests the use of the MSIH since it penalizes the use of additional parameters more strongly.

[28]One recession, beginning in May 1927, can be observed only in Germany. The dating coincides with the crash of the Berlin stock market.

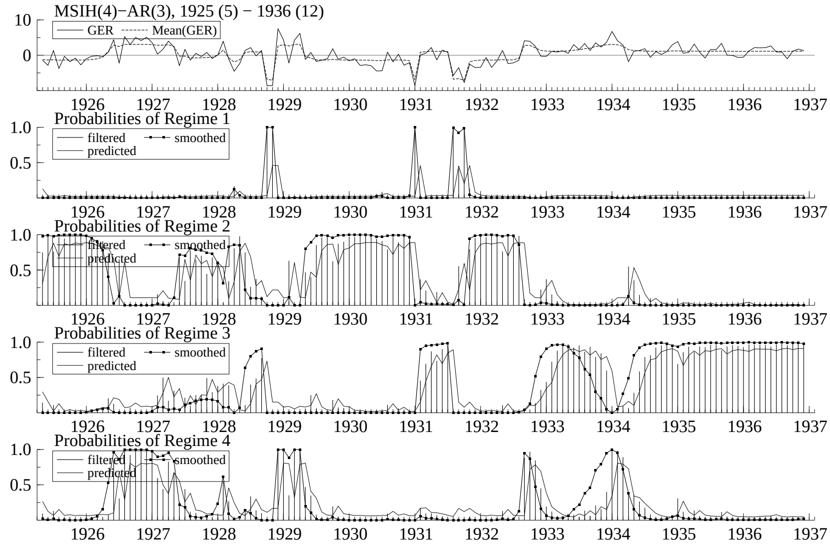

Figure 6: German depression consisted of usual recession and few extreme slumps

dated to May 1929, that is three months ahead of Japan and five months ahead of the US. It is worth noting that the coefficient of the conditional intercept of the recessionary regime is estimated as $\nu_2 = -1.307$. The implied annualized growth rate of which is –14.6% and thus merely half the negative rate inferred for the US. The probability of recession in Germany is estimated to have dropped below 50% after 21 months of recession. Beginning in February 1931 the economy appears to have started to recover, at least in retrospect. The upturn was not just a blip in IP growth rates; the smoothed probability of being in regime 3 indicates that it lasted for several months.[29] The likelihood of an economic recovery even slightly increased throughout the months until July 1931. Yet, the expansion ended abruptly, when IPM plunged by about 16% between August and October 1931. Besides October 1928 and January 1931, this was the third time, the German interwar economy was exposed to a major shock reflected in the outlier regime 1. Its conditional intercept has an estimated coefficient of $\nu_1 = -6.843$ implying an annualized growth rate of –58.7%. It marks the beginning of the second stage of the Great Depression in Germany, with a lag of several months on a similar development in the US. Only in November 1931, the German economy shifted back into regime 2, i.e. to the usual recessionary state. The dating of turning points coincides with the mounting currency crisis. In June 1931, the gold reserves of the German Reichsbank plunged to the statutory minimum. When reserves melted down further, the monetary authorities were forced to introduce exchange controls and Germany *de facto* left the

[29]This conclusion might depend on the way of seasonal adjustment (s.a.). Monthly percent differences differ as follows, March to July 1931: 0.6/5.7, 2.2/6.1, –1.3/–0.7, 1.5/1.6, 0.9/–1.6 (s.a. / unadjusted).

Gold Standard in July 1931. From September 1932 onward, the German economy was recovering faster than other countries, switching between two expansionary regimes. The estimation yields reasonable values for both coefficients related to expansions, $\nu_3 = 1.081$ reflects a normal expansion and $\nu_4 = 2.961$ reflects an outlier state. These coefficients imply annualized growth rates of 13.8%, and 41.9%, respectively. The last figure seems unrealistic at first, but it represents the boost to economic growth in September 1932 and at the turn of the year 1933/1934.

The MS approach reveals that the pattern of the Great Depression in Germany diverges from the pattern in the US. The US experienced an extraordinary strong decline during the entire recession, whereas the Great Depression in Germany stemmed from few extreme slumps in IPM in combination with the otherwise usual recessionary regime of a business cycle. Furthermore, the dating provides further evidence that economic recovery in Germany was not induced by Nazi economic policy. The recovery started well ahead of the Nazi take-over and coincided with strong recovery in the US and in other economies.

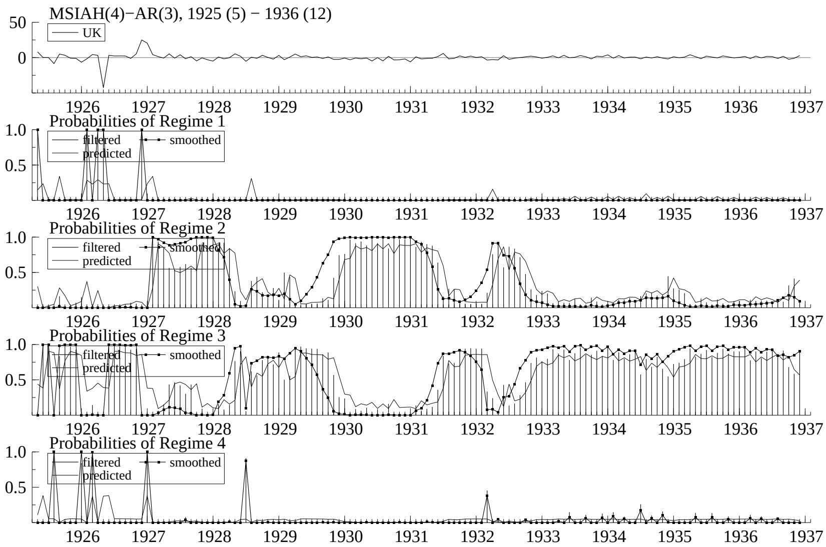

Figure 7: Second stage of the crisis in the UK lasted only 6 months

5.4. Great Britain. The impact of the British miners' strike in 1926 complicates the analysis of interwar IPM in the UK. The 42% decline in IPM in May 1926 and the subsequent recovery with an increase of about 20% per month in December 1926 and January 1927 dominate the identification of regimes. The algorithm leads to the adoption of a MSIH(4)-AR(7) model. However, this model delivers unreasonable regimes and dates the beginning of the Great Depression in Britain to March 1927 and its end to July 1932. In addition, the MSIH model does not yield sensible conditional intercepts for the recessionary state. A MSIAH(4)-AR(3) process is estimated as alternative model (see figure 7). This model identifies three separate contractions between 1927 and 1932. The test statistics remain inconclusive on which model to prefer. Neither model fully captures the outlier states. The solution is to estimate a MSI model excluding the exceptional year of 1926.[30] This model yields three separate recessions between 1927 and 1932 as well. The dating of turning points is very similar to the one provided by the MSIAH(4)-AR(3) for the period 1925-1936 (cf. figure 5c in appendix 5). As such, I adopted the latter specification, even though it is less parsimonious than the MSIH.

The commencement of the Great Depression in Britain is dated to August 1929, hence later than in Germany and earlier than in the US. Just as these two countries, the British business cycle exhibits a double-dip recession. Different from the US and Germany, the recessionary regime that prevailed in Britain during the entire Great Depression was the moderate regime 2. Its conditional intercept was estimated $\nu_2 = -0.907$, which implies an annualized growth rate of –10.4%. Between June 1931 and February 1932 British IPM recovered substantially, again lagging on the recovery in Germany and Japan by about 5 months. Average growth during expansions was driven by a regime with $\nu_3 = 1.09$ and an annualized growth rate of 13.9%, respectively. The second stage of the depression in Britain took place between March and August 1932. That the second stage lasted only 6 months and did not include extreme slumps suggests that the UK was less exposed than other countries to shocks that the second stage of the depression precipitated.

For Eichengreen (2004) Britain's departure from the gold standard in September 1931 was the prerequisite for eventual recovery.[31] My results provide ambiguous evidence for his hypothesis even concerning Britain: A fact in favor of the hypothesis is that Britain does not show a recessionary outlier state during the second stage of the depression like other countries. Yet, the pound was devalued against gold when economic recovery was already underway in Britain, though the fitted probability suggests that this was not apparent for the contemporary observer. Moreover, departing from the gold standard did not protect the British economy against contagion from the second stage of the international crisis. A related issue for the gold standard is posed by the finding that the recessions in Britain between 1929 and 1932 were far less severe than in other countries that adhered to the gold standard as long and left it at about the same time.

5.5. France and Belgium. After the dissolution of the interwar gold standard, several countries continued to fix their currencies against gold. They formed the "Gold Bloc", led by France. Among these countries were Poland, Belgium, and Czechoslovakia. Membership in the Gold Bloc is cited as the main reason why these countries suffered from sluggish recovery during the 1930s (Eichengreen 2004). In fact, the analysis shows that Gold Bloc members were subject to frequent regime shifts even after 1932, while other countries had already returned to stable

[30] If the sample period is restricted to 1927-1936, a MSI(2)-AR(7) model is sufficient to capture the course of the British business cycle.

[31] In addition to leaving gold it "was also necessary also to abandon the ethos of the gold standard that encouraged the continued pursuit of restrictive policies" and thereby prevented the resumption of rapid growth (Eichengreen 1992, p.289).

Table 4: (a) Regimes obtained from individual MSI(n)-AR(p)

Coefficient	**Germany** MSIH(4)-AR(3)	**Austria** MSI(2)-AR(7)	**Japan** MSI(2)-AR(3)	**Finland** MSIAH(2)-AR(3)	**Unites States** MSIH(4)-AR(7)	**Sweden** MSIH(2)-AR(5)
Intercepts and AR coefficients depending on regime 1						
ν_1	-6.843	-2.891	-0.424	0.388	-2.704	-0.315
α_{11}				-0.273		
α_{12}				0.020		
α_{13}				0.130		
Intercepts and AR coefficients depending on regime 2						
ν_2	-1.307	2.148	1.419	3.223	-0.697	0.983
α_{21}				-0.098		
α_{22}				0.598		
α_{23}				-2.127		
Intercepts and AR coefficients depending on regime 3						
ν_3	1.081				0.786	
α_{31}						
α_{32}						
α_{33}						
Intercepts and AR coefficients depending on regime 4						
ν_4	2.961				4.910	
α_{41}						
α_{42}						
α_{43}						
Regime-independent AR coefficients						
α_1	0.031	-0.424	-0.188		0.228	-0.195
α_2	0.014	-0.104	-0.202		-0.134	-0.220
α_3	-0.006	-0.156	-0.147		-0.112	0.077
α_4		-0.202			-0.072	-0.050
α_5		-0.030			-0.110	0.093
α_6		-0.177			-0.179	
α_7		-0.045			0.181	
Transition probabilities and expected duration of recessions in months						
p_{11}	0.461	0.909	0.930	0.959	0.908	0.972
duration	2	11	14	24	11	36
p_{12}	0.025	0.047	0.019	0.472	0.075	0.018
p_{13}	0.039				0.028	
p_{14}	0.000				0.000	
p_{21}	0.218	0.091	0.070	0.041	0.000	0.028
p_{22}	0.897	0.953	0.981	0.528	0.503	0.982
duration	10				2	
p_{23}	0.000				0.006	
p_{24}	0.110				0.478	
p_{31}	0.153				0.000	
p_{32}	0.020				0.199	
p_{33}	0.932				0.966	
p_{34}	0.082				0.015	
p_{41}	0.169				0.092	
p_{42}	0.058				0.224	
p_{43}	0.029				0.000	
p_{44}	0.809				0.507	
Variances, dependent and independent of regime						
σ^2		11.62	2.76			
σ_1^2	4.17			8.68	2.28	12.50
σ_2^2	1.65			1.21	0.02	3.45
σ_3^2	2.72				2.85	
σ_4^2	6.10				40.32	
# obs.	140	136	140	140	136	138
ln$L$	-312.34	-375.94	-277.21	-358.56	-282.42	-343.23
Recessions	1925:5-26:4(2)	1925:9-25:10	1925:5-26:10	1925:5-28:3	1925:9-25:9(2)	1928:4-32:9
identified	1927:6-28:1(2)	1926:3-26:9	1929:8-31:2	1928:5-31:11	1929:10-31:1(1)	
	1928:3-28:5(2)	1929:9-33:1		1932:6-33:12	1931:3-31:5(2)	
	1928:10-28:11(1)	1936:5-36:6		1934:3-36:6	1931:6-32:7(1)	
	1929:5-30:12(2)			1936:8-36:12	1932:10-32:11(2)	
	1931:1-31:1(1)				1933:1-33:2(2)	
	1931:8-31:10(1)				1933:9-34:1(2)	
	1931:11-32:8(2)				1934:6-34:9(1)	
					1934:11-34:11(2)	
					1935:1-35:1(2)	

Notes: MS(I)(A)(H)-AR denotes a Markov switching model allowing for changes in the (i)ntercept, in the (a)utoregressive component, and for (h)eterogeneity of the error term. 'Recession identified' refers to the prevailance of one of the recessionary states.

Table 5: (b) Regimes obtained from individual MSI(n)-AR(p)

Coefficient	**Czechoslovakia** MSIH(4)-AR(9)	**Belgium** MSIH(3)-AR(5)	**Poland** MSIH(3)-AR(1)	**Hungary**	**France** MSIH(4)-AR(7)	**Britain** MSIAH(4)-AR(3)
Intercepts and AR coefficients depending on regime 1						
ν_1	-1.616	-2.030	-1.858		-1.842	0.249
α_{11}						1.417
α_{12}						-9.521
α_{13}						3.078
Intercepts and AR coefficients depending on regime 2						
ν_2	-0.831	0.560	0.916		-0.781	-0.907
α_{21}						-0.106
α_{22}						0.297
α_{23}						-0.042
Intercepts and AR coefficients depending on regime 3						
ν_3	1.329	2.563	4.328		0.379	1.097
α_{31}						-0.090
α_{32}						-0.063
α_{33}						-0.023
Intercepts and AR coefficients depending on regime 4						
ν_4	3.542				1.477	-2.014
α_{41}						1.105
α_{42}						-1.100
α_{43}						-0.836
Regime-independent AR coefficients						
α_1	0.098	0.055	-0.155		0.148	
α_2	0.115	0.017			0.118	
α_3	-0.035	0.008			0.123	
α_4	0.207	-0.002			-0.128	
α_5	-0.008	-0.028			-0.089	
α_6	-0.036				0.127	
α_7	-0.012				0.044	
α_8	-0.016					
α_9	0.192					
Transition probabilities						
p_{11}	0.818	0.754	0.940		0.846	0.236
duration	5	4	17		6	1
p_{12}	0.000	0.063	0.001		0.000	0.000
p_{13}	0.072	0.010	0.185		0.000	0.012
p_{14}	0.182				0.015	0.340
p_{21}	0.000	0.016	0.000		0.040	0.000
p_{22}	0.843	0.936	0.960		0.931	0.908
duraction	6				14	11
p_{23}	0.114	0.382	0.173		0.029	0.028
p_{24}	0.211				0.002	0.281
p_{31}	0.000	0.231	0.060		0.035	0.383
p_{32}	0.157	0.002	0.039		0.059	0.092
p_{33}	0.814	0.607	0.642		0.868	0.904
p_{34}	0.187				0.038	0.379
p_{41}	0.182				0.000	0.381
p_{42}	0.000				0.000	0.000
p_{43}	0.000				0.173	0.055
p_{44}	0.419				0.827	0.000
Regime-dependent variances						
σ_1^2	1.90	1.74	5.71		1.98	0.00
σ_2^2	0.43	4.66	2.13		0.49	4.75
σ_3^2	0.70	295.84	3.24		0.57	3.42
σ_4^2	2.16				0.29	0.44
# obs.	134	138	142		136	140
ln$L$	-235.48	-357.13	-322.97		-161.44	-304.79
Recessions	1925:11-26:7(2)	1925:12-26:1	1925:3-26:1		1926:10-26:10(2)	1925:8-25:8(4)
identified	1927:9-27:11(1)	1929:10-30:5	1928:4-28:10		1926:11-27:4(1)	1926:1-26:1(4)
	1928:2-28:6(2)	1931:4-31:4	1929:1-33:3		1930:5-31:9(2)	1926:3-26:3(4)
	1928:8-28:10(2)	1931:9-32:6			1931:10-32:7(1)	1927:1-27:1(4)
	1929:6-31:3(2)	1936:3-36:5			1933:8-35:3(2)	1927:2-28:3(2)
	1931:8-32:7(1)				1936:6-36:9(1)	1928:7-28:7(4)
	1933:1-33:3(2)					1929:9-31:5(2)
	1933:9-33:9(1)					1932:3-32:8(2)
	1933:11-34:1(1)					
	1934:5-34:12(1)					
	1935:3-35:4(2)					
	1936:1-36:6(2)					

Notes: MS(I)(A)(H)-AR denotes a Markov switching model allowing for changes in the (i)ntercept, in the (a)utoregressive component, and for (h)eterogeneity of the error term. 'Recession identified' refers to the prevailance of one of the recessionary states.

growth regimes. In addition, the initial MSM(2) models are neither appropriate for the French, nor the Belgian, nor the Polish economy. If allowing for more than two states, however, the MS approach reveals that business cycles of Gold Bloc members differed from each other at least as much as they resembled each other.

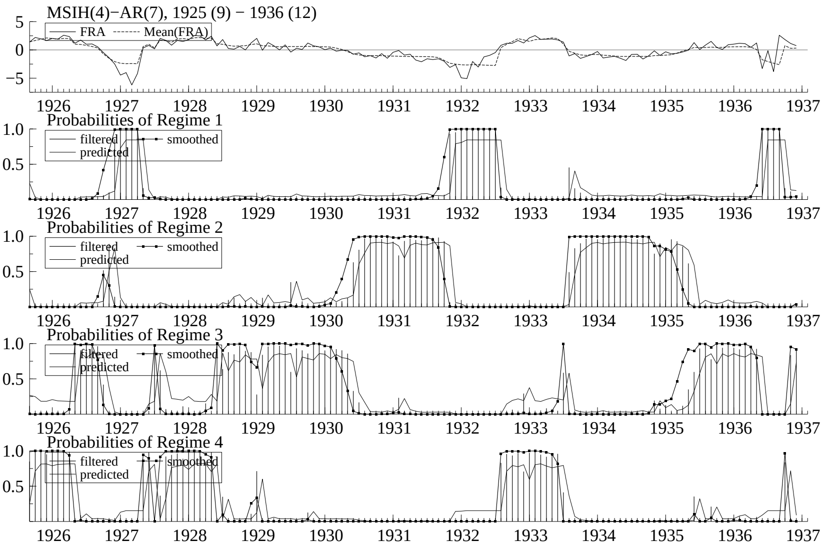

Figure 8: France strongly recovers in 1932 despite being a Gold Bloc member

The selected MSIH(4)-AR(7) indicates a high probability of the French economy being in one of two recessionary states between May 1930 and July 1932.[32] The conditional mean $\nu_2 = -0.781$ implies a moderate annualized rate of -13.5%. It has been stressed earlier that the French economy was the last to show decline in the Great Depression. Nevertheless, the analysis of monthly data suggests a larger degree of co-movement with other economies than is usually presumed. Not only do these results imply a much earlier start of the depression in France than the common narrative dictates.[33] The model also dates a shift into the outlier economic regime 1 in October 1931. One may interpret this shift as evidence that France experienced a second stage of the depression just as other countries, but without the temporary upturn. Co-movement during the second stage with Germany's business cycle is striking: the economic situation deteriorated in October 1931 and recovered from July 1932 on. However, regime 1 and 2 show a fall-back to recession, which is ultimately over first in March 1935. This prolonged recession has made France the literature's favored example of the adverse effects of remaining fettered to gold (e.g.

[32] The AIC favors a model with state-dependent AR terms, i.e. a MSIAH. However, this model is rejected by the LR test and the BIC.

[33] Eichengreen (1992, p.255), for instance, writes that "[t]hrough the end of 1930 [...] France remained a prosperous island in a sea of depression." The MS model with monthly data clarifies that this 'island' was flooded already in the first half of that year.

Bernanke 1995, Eichengreen 1992, Choudhri and Kochin 1980). Such an evaluation however, ignores the fact that the French economy had begun to recover just as other economies. Within only 12 months, following August 1932, French IPM grew by 21.4%. Though growth remained below the annualized growth rate of 31.1% implied by the conditional mean $\nu_4 = 1.477$, this recovery was unexpectedly strong keeping in mind that France had not left the gold standard. Growth during regime 3, with an annualized rate of only 7.2%, was sluggish by comparison. Moreover, while the dating of regime switches matches recovery in Germany and Britain, it does not concur with turning points in Gold Bloc countries such as the Poland and Czechoslovakia.

It is noteworthy that French IPM had been governed by the outlier regime 1 between September 1926 and February 1927 already. To some extent internal factors may explain this slump in output, particularly the Poincare stabilization in September 1926 that raised real interest rates. Internationally, however, one observes an upturn of the business cycle at that time. It appears that the French economy had been 'out of phase' with the international business cycle already before its decision to remain on gold and was getting more and more 'in phase' with the progression of the Great Depression.

IPM in Belgium is commonly regarded to have followed the development of IPM in France. Not only trade links were strong, but also political ties with Belgium joining the Gold Bloc. Nonetheless, the dating suggests that the course of the Belgian business cycle was closer to the pattern in Britain than in France (cf. figure 6a in appendix 5). The MSIH(3)-AR(5) model identifies two stages of Great Depression in Belgium also. The double-dip recessions in Belgium between 1929 and 1933 are quite short, but very sharp, with an estimated intercept of $\nu_1 = -2.030$ or 22.8% p.a. The model yields that the first stage commenced in October 1929 and ended in May 1930. Hence, the point in time Belgium experienced a temporary upturn marks the onset of the depression in France. The second stage of the recession took place between September 1931 and June 1932. Henceforth, Belgian IPM was growing moderately by 7.3% p.a., determined by the expansionary regime 2. The only exception is one major drop in IPM between March and May of 1936, which immediately precedes the slump in French IPM in 1936.

5.6. Central Eastern Europe. It appears that countries in Central Eastern Europe were affected particularly negatively. In comparison to most other countries in the sample, the economic downturn in Poland, Czechoslovakia, and Austria was larger, prevailed longer, and recovery was sluggish. The Republic of Poland provides the prime example of the economic malaise in interwar Central Europe. The MSIH(3)-AR(1) model selected yields that Poland was subject to the longest lasting recession in the sample. As the onset of the Great Depression is dated to January 1929 and its end to March 1933, the Polish economy was in recession for about 40 consecutive months (cf. figure 9). Different from Germany and the US, there is no evidence of an outlier contractionary state in Poland, but the conditional intercept of the recessionary regime $\nu_1 = -1.858$ is one of the lowest across the sample. It implies an annualized growth rate of the Polish economy of –17.7% during this state. Economic recovery began about 9 months later than in Germany,

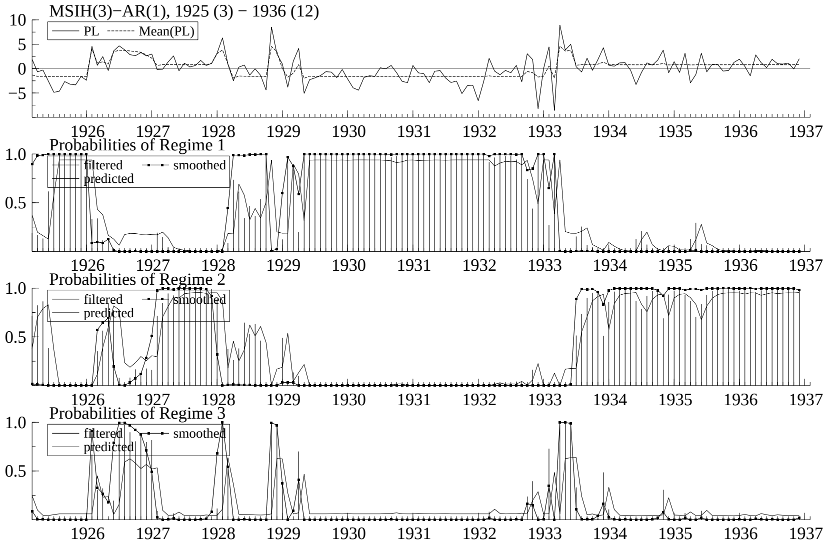

Figure 9: Poland was subject to the longest lasting recession

France, and the UK. Post-depression growth in Poland was less dynamic than in Germany or Britain with an annualized rate of 9.9%. It is hence quite notable that recovery in Poland remained uninterrupted, even when France turned again into recession in August 1933. Nonetheless, Poland first reached its pre-depression level of IPM as late as 1938 (cf. Landau and Tomaszewski 1986). The business cycle of Polish industry appears to have been driven by Germany's economy, even despite the economic conflict between both countries from 1925 to 1934 and Polish attempts to reduce economic dependency on Germany. The difference from the French cycle is even more obvious in case of Belgium. Both the dating of pre-depression fluctuations and the prolonged boom after 1932 are very similar to the findings on Britain.

The course of IPM in the Republic of Austria can be modeled by a MSI(2)-AR(7) set-up. Apart from an early and short contraction in 1926, the model yields only few regime shifts. It dates the beginning of the Depression in Austria to September 1929 (cf. figure 6d in appendix 5). Evaluated by the smoothed probabilities, Austria's economy declined without interruption to January 1933. The conditional intercept ν_1 is estimated to equal −2.891. The estimate implies an annualized growth rate of −15.1%. While the smoothed probabilities do not indicate a double-dip recession, the filtered probabilities suggest that the probability of economic recovery increased steadily between January and July 1931, and hence at the same time it actually materialized in other countries. I can only speculate here what prevented temporary recovery in Austria, but it seems likely to have been affected by the failure of Creditanstalt, then the largest Austrian bank, in May 1931, i.e. just when recovery was gaining momentum. The event represented a major blow to the Central European banking system as a whole at the time and real effects are plausible (e.g. Wandschneider 2005).

The Czechoslovakian business cycle developed similarly to that of Poland and Austria, until at least 1934. This is of particular interest to economic historians as the three countries had been unified in the Austro-Hungarian empire earlier on and had existed as independent states for little more than a decade. Czechoslovakia was the successor state that comprised the most industrialized part of the former empire. According to an MSIH(4)-AR(9) model, the country experienced a long-lasting economic decline beginning in June 1929 (cf. figure 6c in appendix 5). Moreover, the depression was sharp with regime 2 implying an annualized growth rate of −18.4%. The model provides evidence that Czechoslovakia took part in the worldwide upturn in the midst of the Great Depression, because there is a high probability that regime 3 prevailed between April and July 1931. The subsequent contraction during the second stage of the recession was still harsher, represented by regime 1 in the model. Recovery of the Czechoslovakian economy was slow, because the economy remained subject to frequent regime shifts even after the recession ended in March 1933.

As far as the remaining countries are concerned, I find the following results: The algorithm leads to the adoption of a MSIH(2)-AR(5) model for Sweden. This two-state model suggests that the world economic crisis did just superficially affect the Swedish economy. An annualized growth rate during recessions of 2.9% implies stagnation, which is much less negative than even in Japan, though it lasted for a longer period of time. In case of Hungary and Finland, the EM algorithm did not converge when fitting the likelihood function. Thus, it was not possible to obtain an appropriate MS model for these countries.

6. Results II: Business cycle integration and the global cycle

In order to make inferences on the global depression, the business cycle dating for all countries is being aggregated. I chose to transform regime probabilities from the MS-AR estimation into binary time series: To this end, I made a dichotomous distinction between expansion and contraction dependent on whether smoothed regime probabilities are greater than 0.5. If, at a certain point in time, the country was likely to be in recession —or in an outlier recession state— the respective time series observation is set to 1, otherwise it is set to 0. Accordingly, 0 indicates that an expansionary regime prevailed. The resulting binary time series are then summed up over all countries.[34]

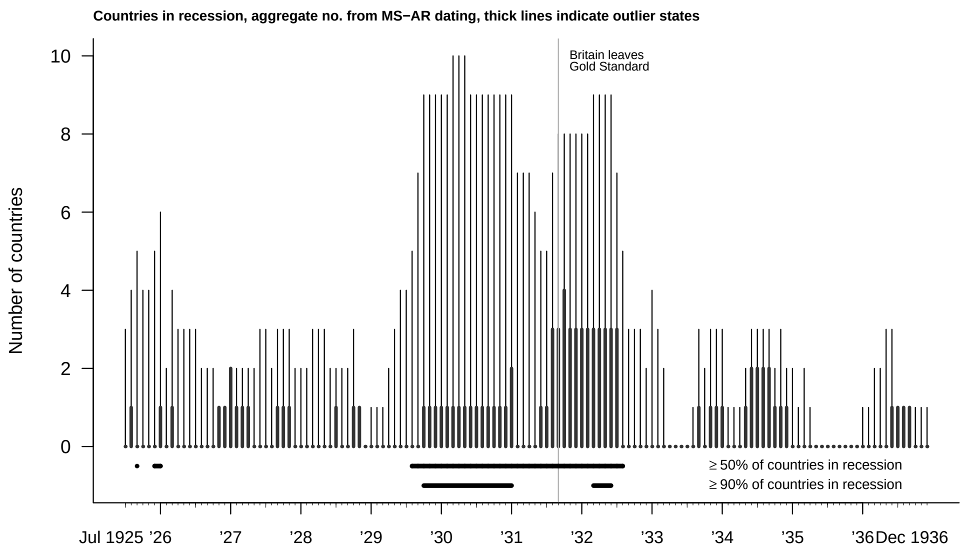

Figure 10: Scope and scale of worldwide downturn (from MS-AR dating)

Figure 10 gives the number of countries in recession in a particular month.[35] The thick lines indicate the number of countries in a recessionary outlier state in a particular month. Whereas the commencement of the Great Depression is not immediately visible from the individual business cycle dating, the aggregate series makes it easy to distinguish the Great Depression from the 'common' business cycle

[34]A more accurate way of inferring on the global cycle was to estimate regime probabilities within a MS-*Vector* Auto Regression embracing all IPM series. This approach is not feasible, because the number of sample countries as well as the number of potential regimes is too large given the short time period in consideration.

[35]Finland and Hungary were excluded from this overview. Thus, the total number of countries is ten.

downturn in 1925. The graph illustrates the magnitude, duration, and range of the global downturn during the Great Depression. I consider it representative, because the sample countries accounted for most of the world's industrial production (cf. table 6 in appendix 7.).

During most of the interwar period, between 0% and 30% of countries were in a recessionary state. To determine the international business cycle, a share of 50% of countries in recession is defined as threshold for an international recession. According to this rule, the analysis suggests an international downturn at the beginning of the sample period. In the second half of 1925, the estimations yield a high probability of recession in 40%–60% of countries in the sample. Most likely, this observation represents the last part of a precedent recession. In April 1926, the share of countries in recession decreased to 30% and fluctuated between 10% and 30% afterwards. Only at one time, in December 1928, the share fell to 0%. Even as the evidence of a recession in the US at that time is mixed (as will be discussed in the next section), it appears to be appropriate to speak of an international cycle starting with a trough in 1925 or early 1926 and a robust global expansion thereafter. Every country in the sample, except Sweden, was in recession at least once between 1925 and 1929. Evidence of a recession in the US during this period is mixed. One can even observe a second negative shock to the German, Polish, and Czechoslovakian economy in the late 1920s. However, the global boom remained robust and these countries always returned to economic expansion after a few months already.

Beginning in April 1929, the share of countries in recession rose sharply within a few months only. Being at 10% in March 1929, the proportion surpassed 50% in August and reached 90% in October 1929.[36] By the above definition, the onset of the international business cycle downturn, and hence of the Great Depression, is dated to August 1929. Between October 1929 and January 1931, during 16 consecutive months, the economies of at least 90% of the sample countries were contracting without interruption. The trough of the international business cycle was likely reached between March and May 1930, when the probability of recession is estimated to have been high for all ten sample countries.

In the first half of 1931, the odds for international economic recovery were increasing. Recovery was immanent, when the share of countries in recession had fallen from 90% in January to 50% in June 1931. And even though the proportion did not fall below 50% ultimately, it is evident that the temporary recovery in several countries during the Great Depression reflected a turning point of the international business cycle. The aggregation yields that immediately after the short-lived recovery, 80% to 90% of the countries in the sample again entered recession in a short sequence. However, recovery lasted for quite different periods of time in the sample countries. Recovery in the US was short-lived, whereas IPM in the UK expanded for almost one year. As the only country in the sample, Japan even managed to escape the second recessionary stage of the Great Depression completely.

[36]The following countries were in recession by August 1929: Germany, Japan, Sweden, Czechoslovakia, and Poland. Austria and Britain followed in September, Belgium and the US in October 1929.

This improvement of the economic situation has not attracted much attention in the literature, despite the fact that it can be observed in many countries.

The MS approach reveals that, at global level, the second stage of the Depression differed substantially from the first. The first stage of the Depression affected every country in the sample and lasted comparably longer. Despite its scope and duration, it appears to still be similar to a typical business cycle downturn. By contrast, the second stage of the depression did not last as long, but had a stronger negative impact. The number of economies being simultaneously in an outlier recession state tripled as compared to the first stage (cf. figure 10). Taken together, the global downturn lasted for 36 months surpassing by far any postwar recession. For comparison, Krolzig's (1997) study of the international business cycle after WWII yields that the longest global recession took place after the first oil price shock and lasted for eight quarters.[37]

With a huge variation in time, IPM began to pick up in most countries in 1932. Figure 10 indicates that recovery of the global economy was underway beginning in August 1932, when the share of sample countries in recessions fell below 50%. In many countries, recovery from the crisis proved to be instable. Between September 1932 and the end of the sample, the share fluctuated between 0% and 30%. Several countries, including the US, only recovered in 1933 and some, even later. Two different groups of countries can be classified: In one group, e.g. in the US and France, the depression ended relatively early, but recovery was subsequently frequently interrupted. The other group, e.g. Czechoslovakia, experienced a continued decline, i.e. the absence of fundamental recovery. In this regard also the international business cycle differed from the postwar cycle in Europe and the US (cf. Hamilton 1989 and AKT 2004): In some countries, recurrent setbacks from recovery were second only in importance to the downturn itself for the poor economic record of these countries during the interwar period. Besides the US, this primarily concerned France. Both the setbacks from recovery as well as the double-dip pattern at national level and in the international cycle have not attracted much attention in the literature. Yet, it appears essential to integrate them into a comprehensive explanation of the global depression.

The decline in 1925, the subsequent boom, the Great Depression, and recovery from it may it well represent an international business cycle. In order to systematically explore synchronicity of interwar business cycles, Pearson's contingency coefficient (PCC) is computed for each country pair using the binary time series. The corrected PCC provides a straightforward, non-parametric measure of contemporaneous co-movement of business cycles. The degree to which recessionary regimes were synchronized, is inferred from the number of coincidental months in recession for a pair of countries.[38]

The analysis shows that business cycles of the sample countries were in general highly synchronized. Table 6 indicates that the degree of co-movement of business

[37]His sample comprises Australia, Britain, Canada, Japan, Germany, and the US.

[38]PCC in its corrected form can be read in percentage terms. Details on the computation of PCC are provided at the end of appendix 5.

Table 6: Pearson's contingency coefficient (corrected) of probabilities of recession

Country pair	**Germany**	**Austria**	**Japan**	**Unites States**	**Sweden**	**Czechoslovakia**	**Belgium**	**Poland**	**France**	**Britain**
				Contingency coefficients for the entire period 1925:7-1936:12						
Germany	100.0									
Austria	47.6	100.0								
Japan	50.9	60.6	100.0							
United States	41.2	70.9	22.2	100.0						
Sweden	69.7	80.2	40.9	72.3	100.0					
Czechoslovakia	47.3	40.7	38.6	50.9	44.9	100.0				
Belgium	54.3	56.1	24.0	54.6	61.8	49.1	100.0			
Poland	68.4	68.6	42.8	58.3	81.2	38.7	49.2	100.0		
France	6.0	18.9	16.3	58.3	35.5	7.2	14.5	10.7	100.0	
Britain	63.6	45.3	44.3	33.8	56.3	23.0	33.4	35.2	18.7	100.0
				Contingency coefficients for the period 1925:7-1929:12						
Germany	100.0									
Austria	9.1	100.0								
Japan	11.9	81.6	100.0							
United.States	37.2	63.5	47.3	100.0						
Sweden	44.5	29.5	21.4	57.3	100.0					
Czechoslovakia	30.7	29.9	36.3	19.8	34.1	100.0				
Belgium	41.6	36.7	52.6	76.4	49.2	43.0	100.0			
Poland	44.9	3.2	20.1	39.8	59.5	30.7	44.5	100.0		
France	41.1	36.7	38.4	18.9	29.5	60.9	21.3	59.0	100.0	
Britain	31.8	6.6	20.9	25.8	2.4	5.3	33.2	41.3	36.1	100.0
				Contingency coefficients for the period 1930:1-1936:12						
Germany	100.0									
Austria	82.6	100.0								
Japan	72.7	61.2	100.0							
United.States	70.2	70.9	47.0	100.0						
Sweden	90.5	92.5	68.7	73.0	100.0					
Czechoslovakia	63.2	44.3	48.9	62.9	48.5	100.0				
Belgium	69.2	59.8	19.6	44.6	63.0	51.1	100.0			
Poland	82.6	94.9	61.2	75.2	92.5	44.3	53.6	100.0		
France	36.8	24.2	26.4	55.7	44.0	36.7	13.1	17.4	100.0	
Britain	80.0	77.9	83.3	59.6	85.8	45.9	41.4	77.9	30.6	100.0

Notes: Pearson's contingency coefficient in corrected form, i.e. normalized to the interval 0 to 100. PCC below 23.3 indicate that the null hypothesis that the country pair's business cycles were unassociated cannot be rejected at the 5% significance level (cf. appendix 5 for details).

cycles in interwar Europe was similar to the degree of co-movement among European countries between 1970 and 2001 estimated by AKT (2004). AKT (2004) consider a $PCC_{corr} \geq 50$ to indicate high synchronicity. They find that 22 out of their 36 country pairs, or 61%, surpass this threshold. As AKT (2004) is based on smoothed data, a lower threshold seems appropriate to indicate high synchronicity within my sample. If I define a corrected $PCC_{corr} \geq 40$ as threshold, 17 out of 28 European pairs, or 61%, can be said to have been highly integrated. Even if the threshold for high synchronicity is raised to $PCC_{corr} \geq 50$, this group still comprises 36% of the European pairs examined. Compared to the postwar period, the commonality of the French business cycle with other European countries was much lower during the interwar period, whereas the British business cycle shows a higher degree of integration into the European cycle. Business cycle integration between Europe, the US, and even Japan is as high as the intra-European business cycle integration: If Japan and the US are taken into account, 64% of country pairs have a $PCC_{corr} \geq 40$. However, this is largely due to France; omitting France from the sample, the intra-European share rises to 81% at $PCC_{corr} \geq 40$. A remarkable difference to the postwar period is that business cycle integration between the US and Germany as well as between the US and Britain is relatively low. It is even more important that the US cycle lagged behind international developments all along the sample period. The US was not only among the last 3 countries in the sample to enter recession, it was also among the last 3 countries to recover from the crisis. The turning point of the global cycle in 1929 preceded the US business cycle's turning point by half a year.[39] This finding provides new support to studies, which suspect the origin of the crisis outside the US such as Ritschl (2002), Kindleberger (1986), Borchardt (1979), and others. Moreover, the results are in line with long-run correlations in Ahern and Woitek (2001), e.g. the finding that business cycles were well integrated between the US and Sweden, or between the UK and Germany, but not so much between the US and Germany or Britain.

In order to analyze whether the Great Depression affected synchronicity, the sample of binary time series is being split in December 1929. Corrected PCC of all country pairs are then computed for each subperiod. The resulting coefficients suggest that the crisis had an integrating effect, since co-movement was much higher in the 1930s than in the second half of the 1920s (see table 6). For the subperiod 1925 to 1929, only 38% of country pairs have a $PCC_{corr} \geq 40$. During the period between 1930 and 1936, by contrast, the proportion amounts to 82%. However, the integration increases unevenly across countries. Synchronicity of business cycles increased for Britain, Germany, Poland, and Austria with respect to almost every other economy. In case of the US, Belgium, and Czechoslovakia, and Sweden business cycle co-movement grew stronger with respect to many other economies. France is the only country that became less well integrated with the other sample countries in the 1930s, meaning that the synchronization during the depression did not last. The strong increase in integration of business cycles in the 1930s contradicts to some extent the idea of economic disintegration and de-globalisation at the time (e.g. Eichengreen and Irwin 1995, Kindleberger 1986, Thomas 1996). It

[39]The US turning point occured in March 1933 as indicated in table 1, table 4, and the official NBER dating.

is however in line with Backus and Kehoe's (1992) analysis of interwar GDP correlations.[40] They conclude that "correlations were highest in the interwar period, reflecting the common experience of the 1930's." (p.877)

In order to exemplify how the dating can add to the discussions concerning origin, cause, and effect of the Great Depression, I compared my findings to predictions of the Gold Standard Theory. The dating can be used to evaluate the relationship between adherence to the gold standard and the course of the business cycle. Two issues are being reviewed: Firstly, if tightening of monetary policy coincided with a certain sequence of the commencement of the crisis. Secondly, if countries that abandoned the gold standard early on did successfully reduce the probability of recession thereafter. For the sake of simplicity, I will tackle these issues only based on graphical evidence.

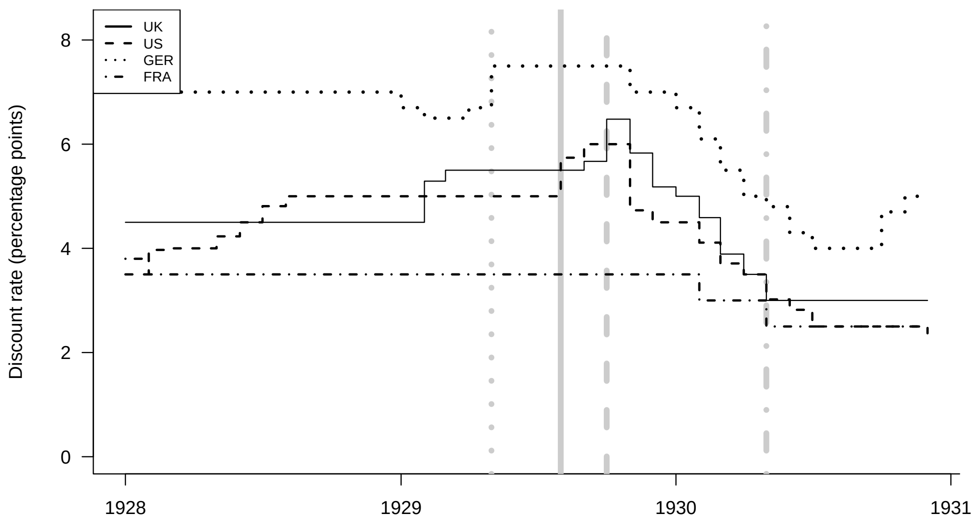

Figure 11: Monetary tightening coincides with beginning of depression

As pointed out above, proponents of the GST often emphasize the importance of monetary tightening and subsequent monetary contraction in inducing the downturn in the real economy in 1929 (cf. Eichengreen 2004, Bernanke 1995). Consequently, the tightening of monetary policy by central banks is expected to have preceded the beginning of the recession. Following the increase of the discount rate by the Fed, other central banks raised discount rates to prevent capital outflows. Since IPM can be expected to react to the change only with delay, the sequence

[40]They examined correlations of HP filtered series for Canada, Denmark, Germany, Italy, Japan, Norway, Sweden, the UK, and the US between 1870 and 1986.

of recessions should by and large match the sequence of changes in interest rates. In figure 11 business cycle turning points (thick, vertical lines) are plotted against discount rates in the US, the UK, Germany, and France.[41] Evidence of a systematic relation between monetary tightening and the start of the Great Depression is mixed. The expected pattern is found in the US, i.e. the crisis commenced several months after interest rates were raised in 1928 and again in August 1929. The Fed raised the discount rate from 3.5% in January 1928 in eight steps to 6% in September 1929. This increase was by no means extraordinary.[42] However, the US case cannot be generalized: The interest rate in Germany was raised in two steps from 6.5% in March to 7.5% in Mai 1929. Again, this increase was marginal as the discount rate was at 10% in 1926 and still at 7% through 1928. Furthermore, the increase coincided with the business cycle turning point in Germany. Similarly, monetary policy in the UK was tightened in two steps from 4.5% to 5.5% in early 1929 and further to 6.48% in October 1929. Hence, the Bank of England raised the discount rate for the second time after the beginning of the recession in the UK. The increase in interest rates was certainly not helpful for dealing with the upcoming crisis. Nevertheless, the dating challenges the timeline of events put forward in the Gold Standard Theory.

The poor economic record of France after 1931 is usually ascribed to the country's decision to adhere to the rules of the gold standard beyond 1931. Countries, which left gold and devaluated their currencies, were shown to have benefited from depreciation through faster recovery (Eichengreen and Sachs 1985). Bergman, Bordo, and Jonung (1998, p.71) state that "only when the links with gold were cut did recovery take place". However, the chronology of the global business cycle in figure 10 does not show the stated positive reaction of global IPM to the change of the gold standard regime. Instead, the factual dissolution of the gold standard due to Britain's departure from the currency regime in August 1931 is followed by the second, the worse, stage of the depression. The dissolution of the gold standard was apparently ineffective in preventing or attenuating this part of the crisis.

Of course, it could be possible that departing from gold still had a positive effect for single countries. I evaluated if and how the timing of departure from the gold standard was related to the probability of recession after mid-1931.[43] Figure 12 shows the duration of the Great Depression after 1931 plotted against the exit date from the gold standard. The observations are scattered unsystematically adding to the impression that the variation in the probability of recession after 1931 can hardly be explained by adherence to the gold standard: Ex post, the decision to depart from the gold standard had little or no relevance for the duration of the

[41]Data was taken from the NBER's Macrohistory Database: discount rates of the Federal Reserve Bank of New York (NBER series no. m13009), the Bank of England's minimum rate of discount (m13013), the Bank of France's discount rate (m13014), and the Reichsbank's official bank discount rate in Germany (m13015).

[42]Eichengreen (2004, p.2) provides that "[the] Fed has typically (since 1951) raised the discount rate by 425 basis points between the trough and the peak of the business cycle (as dated by the NBER)."

[43]The exit dates were taken from Wolf and Yousef (2005). Among the countries leaving the gold standard in 1931 were Austria, Britain, Czechoslovakia, Finland, Germany, Sweden, and Japan.

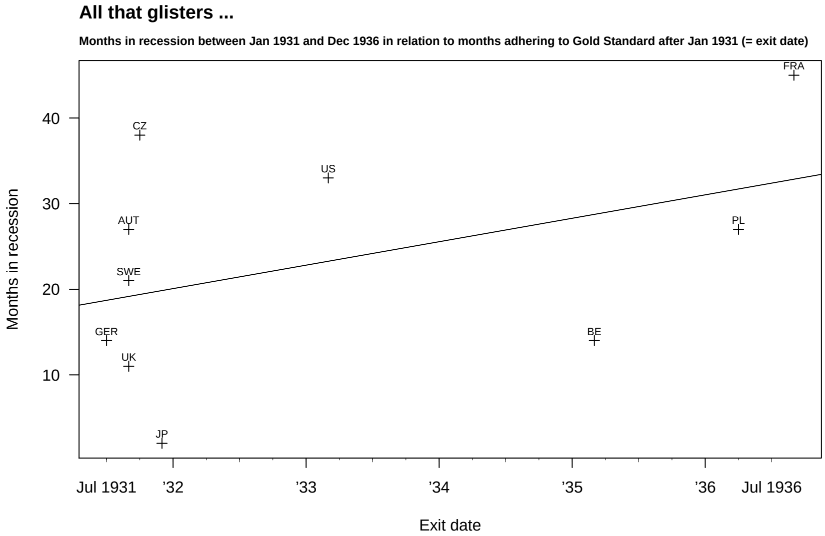

Figure 12: Exit from gold and duration of recovery in the 1930's

crisis.[44] At least the fact that the lower left corner contains the most observations is in accordance with Eichengreen and Sachs' (1985) hypothesis: It was slightly more likely that a country experienced recovery early if it left gold early. Nevertheless, the case of Japan and Belgium shows that staying longer on gold did not necessarily imply a longer contraction. Correspondingly, Czechoslovakia and Austria left gold almost as early as Germany and Britain, but remained in recession almost as long as the US and France.

The results on business cycle co-movement strongly suggest that the economic performance of France during the 1930s should not solely be attributed to the country's membership in the Gold Bloc. The corrected PCC between France and other Gold Bloc members remains low. After France's slide into even deeper recession in 1931, which coincided with the second stage of the global depression, the French economy followed a special path. This deviation poses the questions, which factor differentiated France from the rest of the world, and in particular from other gold bloc members. That the particular developments in France were apparently not related to the gold standard becomes obvious from considering Poland and Belgium, the two other Gold Bloc member countries in the sample. Their economic performance is much better than that of France and they shared the international business cycle. Regarding the business cycle chronology however, there is little evidence that Gold Bloc countries shared a common business cycle. The Polish

[44] These findings at first seem to contradict the results of a similar regression in Eichengreen and Sachs (1985, fig. 1). The reason for the discrepancy is not the selection of sample countries, but that Eichengreen and Sachs (1985) compare the percentage change in IP between 1930 and 1935 to the devaluation of currencies during this particular time period. By contrast, I considered the time series perspective.

economy developed much more closely to German or British cycles than to the French business cycle. Apparently, the impact of the common currency regime was inferior to business cycle integration.

7. Alternative dating approaches

Dating the interwar business cycle by two alternative approaches has the main objective to prove that the above results can be integrated straightforward into existing research, as both of them are widely applied in business cycle research. It is also supposed to cross-check my findings and to add further evidence in case of ambiguous results. To this end, turning points are identified using a rule that is often associated with the dating procedure of the US National Bureau of Economic Research (NBER). In addition, trend-cycle decomposition is performed.[45] The first dating procedure defines a recession as two consecutive quarters of declining industrial production. It is subsequently denoted as pseudo-NBER dating, since the actual NBER dating procedure is more flexible and considers several time series including GDP (Hall et al. 2003). In the following this rule is applied to quarterly averages of the time series in my sample.

The recessions identified by the dating rule, are depicted in figure 2a–2l in appendix 3. The dating is broadly consistent with the chronology inferred from the MS-AR approach. It shows that IPM declined in many countries at the turn 1925 and 1926. Different from the MS-AR approach, the development of IPM in the US during the 1920s is not interpreted as sluggish growth through the 1920s, but as two subsequent recessions in 1925 and 1927.[46] Just as the MS-AR approach the pseudo-NBER rule reveals that almost all countries in the sample experienced a persistent decline of IPM beginning in 1929. Only Sweden, Finland, and Japan were subject to comparably short recessions. Hence, the approach dates the commencement of the Great Depression to 1929, when all economies in the sample but the French turned down. Again, the stock market crash on Wall Street in late October 1929 appears to have lagged behind on the looming downturn of the world economy. The pseudo-NBER dating also finds that turning points, marking the end of the depression, varied a lot over time, contrary to the initial downturn. Large differences across countries can be also observed for the time period after 1931. What is more, the approach confirms that a temporary recovery took place contemporaneously in most countries. However, the approach does not find evidence of a double-dip in the US. It mistakes the ambiguous progression of US industrial production giving that the recession ended already in 1932, i.e. a year earlier than provided by table 1 and by the official NBER dating.[47] These findings illustrate that the pseudo-NBER approach is of limited value for comparative business cycle dating. It ignores differences in the extent and persistence of the slump and potentially underestimates the duration of the recessions being very sensitive to temporary jumps in IPM.

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[45] Spectral analysis is not a feasible approach for measuring synchronization in this case, since there were at most two complete cycles between 1925 and 1936.

[46] The dating of recessions in the US accords with the recessions acknowledged by the NBER's Business Cycle Dating Committee, except for the contraction in 1925. Dates are provided on the NBER's website (nber.org/cycles.html).

[47] The deviation is likely to have been caused by two short, but strong positive shocks, which shift the quarterly averages.

A Hodrick-Prescott (HP) filter is applied as the second approach to cross-checking the business cycle chronology established further above. The filter separates a cyclical component of a time series y from its trend component. It is a means of removing higher frequency fluctuations from the series. The resulting smoothed series can be interpreted as the long run growth trend and hence provide an insight of fundamental similarities and differences across countries. To this end, it is assumed that industrial production y_t consists of a secular component x_t, cyclical fluctuations c_t, seasonal fluctuations s_t, and random fluctuations ϵ_t.[48] The relationship is formalized as

$$y_t = x_t + c_t + s_t + \epsilon_t \tag{7.1}$$

After the series has been seasonally adjusted, it is possible to isolate the growth trend by applying the HP filter to the series. This filter relies on a smoothing parameter λ.[49] Its objective function is given by

$$\max_{x_t} \sum_{t=1}^{T} (y_t - x_t)^2 + \lambda \sum_{t=2}^{T} [(x_{t+1} - x_t) - (x_t - x_{t-1})]^2 \tag{7.2}$$

Maximizing equation (7.2) yields an estimate $\hat{x}_t$ of the growth component. Figure 13 depicts the resulting growth components for all sample countries. Whereas postwar recessions hardly affected the secular component, the Great Depression shows up as a dent in the growth path of most economies, which underlines the advantage that the HP filter allows for non-linearity. The removal of higher-frequency fluctuations reveals that the secular trend developed similarly in most countries until 1928. Only in the US, IPM stagnated from the beginning of the sample period. The subsequent turning point in 1928 marks the beginning of the Great Depression. While it is not possible to derive a precise sequence of turning points, the isolated growth components suggest that two groups of countries can be distinguished by the course of $\hat{x}_t$ between 1928 and 1934: In group B, consisting of the US, Belgium, and most Central European countries, IPM declined much more than in group A, consisting of the UK, the Northern European countries, and Hungary. For instance, British trend growth was pushed downward much less than German trend growth. France and Japan, by contrast, are neither part of the one nor of the other group of countries. In France, the crisis had a long-term impact, at least to the extent that one holds the Great Depression responsible for the prolonged decline after 1933. Only the estimated trend of Japanese IPM displays the curve progression as it is known from the postwar period. The decomposition using HP filters reveals that trend growth in group B countries developed highly irregularly and suggests that the Great Depression was the consequence of an exceptional shift in their growth paths.

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[48] $y_t = log(Y_t)$, x_t, c_t, s_t, and ϵ_t refer to log transformations of the original variables.

[49] The filter was proposed by Hodrick and Prescott (1980). λ is set to $129600 = 1600 \times 3^4$ according to Ravn and Uhlig (2002). Though HP filtering represents measurement without an

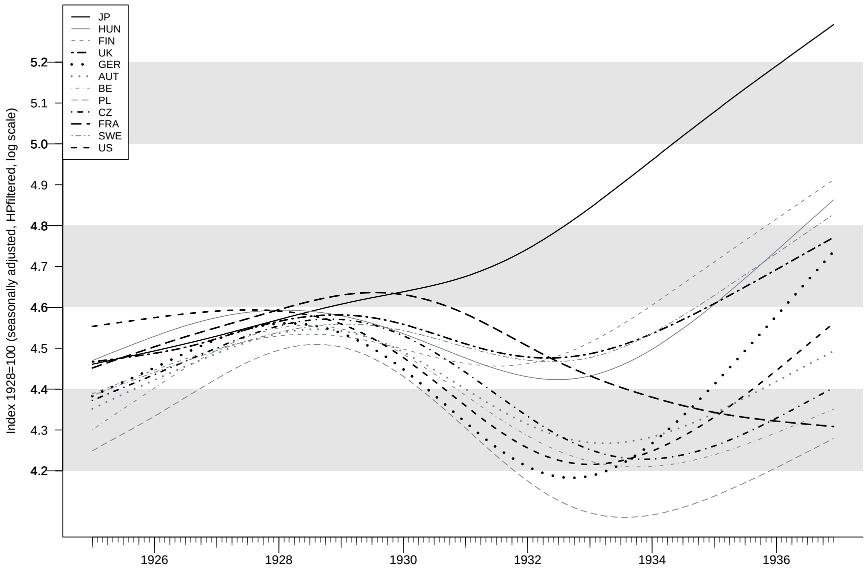


Figure 13: Trend components of IPM obtained from decomposition

The secular components also display huge differences in the speed of recovery. Evaluated by the HP decomposition, the fact that Germany was the first country of group B that overtook its pre-depression level of IPM was due to strong trend growth. By contrast, trend growth in most countries of group B remained weak after 1932. Hence, the economic recovery observed in these countries was mainly driven by the business cycle. This includes all four countries in the sample that border on Germany, which had been affected as negatively as Germany during the crisis, but recovered much more slowly. The secular components are largely consistent with the findings from the MS analysis. It confirms that the slump was truly global. It suggests, too, that the Great Depression commenced earlier than acknowledged by most of the literature. It corroborates that Japan, France, and the US represented special cases: The US prior to the Great Depression, Japan during its second stage, and France from 1929 on.

Having obtained $\hat{x}_t$, one can subtract $\hat{x}_t$ from y_t in order to compute the residual component, which consists of $c_t + \epsilon_t + (x_t - \hat{x}_t)$.[50] This sum cannot be further decomposed. However, one can use the term as a proxy for the cyclical component c_t under the assumption that $E\left[c_t + \epsilon_t + (x_t - \hat{x}_t)\right] = c_t$. This residual component is thus usually interpreted as the business cycle component.

underpinning in economic theory, the approach is based on fundamental assumptions of many business cycle theories (cf. Canova 1998).

[50]It actually consists of $c_t + \epsilon_t + (x_t - \hat{x}_t) + (s_t - \hat{s}_t)$.

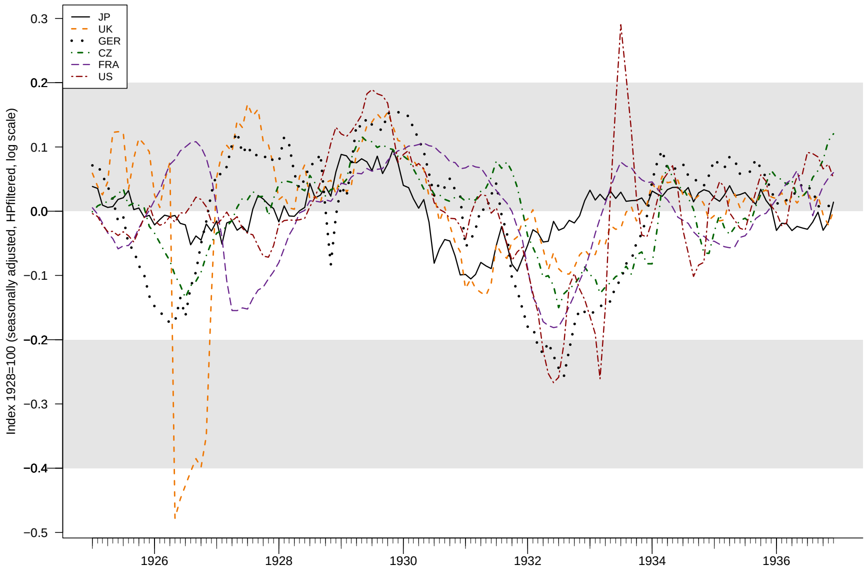


Figure 14: Cyclical component of IPM for important economies

The residual components depicted in figure 14 suggest that two global business cycles were completed during the period 1925–1936 (for the sake of clarity some small economies were omitted). The individual cycles were quite synchronized and appear to have been even more so from 1930 on. Before 1930, particularly the French cycle ran contrary to the rest of the world. These observations are supported by the contemporaneous cross correlations for c_t. The coefficients, given in table 5 in appendix 6, are broadly consistent with contingency coefficients obtained for the binary MS series in table 6 above. They indicate statistically and economically significant conformity of business cycles among most European countries during the interwar period. They add to the evidence that the French business cycle was out of phase particularly with the rest of Europe. In fact, the highest correlation in the sample is calculated between the Gold Bloc member state Poland and its antagonist, Germany. Trend-cycle decomposition adds to the evidence of low co-movement between the US and the UK. Unlike the MS approach, a high conformity between the US and Germany is detected. In this respect, the results of the decomposition are similar to Backus and Kehoe's (1992) findings.

The filtering approach emphasizes that the Great Depression was not simply a business cycle phenomenon, but a combination of a downward shift in growth trends and a contemporaneous global business cycle downturn. It adds to the evidence of an international business cycle that even encompassed the Japanese economy. Unlike the MS model, particular German business cycles during the late 1920s as suggested by Ritschl (2002) do not show up in trend-cycle decomposition, neither in the growth trend nor in the business cycle. This finding stresses the main weakness of trend-cycle decomposition in comparison with MS models, namely that an integrated analysis of both components is difficult. Though being able to capture the unusual developments, it remains artificial. Although the HP filter distinguishes countries by different degrees of the downturn, the large decline between 1929 and 1933 does not necessarily relate to economic outlier states identified by the MS approach, e.g. in case of Austria, Czechoslovakia, and Belgium. Furthermore, as with any other filter, one may doubt whether the HP filter accurately reproduces the 'true' growth pattern. Using a HP filter, one can discriminate between slight and major downturns. Yet, one may obtain inconclusive results on turning points, because secular and cyclical components overlap and can be contradictory. The NBER dating rule is equally unable to simultaneously do both, cope with structural breaks, derive a clear-cut business cycle chronology, and be useful for making inferences on co-movement. It results in imprecise turning points (only in quarters) and is unable to discriminate between slight downturns, depressions, and structural breaks. Moreover, it is unhistoric, since it applies an inflexible dating rule that may be appropriate for one country but not for the other.

8. Conclusion

This study approaches the troubled interwar period, and in particular the Great Depression, from the viewpoint of business cycle analysis. Identification of cause and effect looms large in any hypothesis of the Great Depression. The comparative dating of cyclical macroeconomic fluctuations by this study is meant to take one unknown out of the equation.

The MS approach formalizes the description of the Great Depression and makes it easier to distinguish it from 'common' business cycles. Applied to high frequency data, the approach dates business cycle inflection points much more precisely than any earlier study. I checked the robustness of results applying a HP filter and using a dating rule similar to the one employed by the NBER. These approaches corroborate the following course of events with a few differences in the exact dating and sequencing of countries' entry into recession: Economic growth in the second half of the 1920s was internationally instable and often weak. Particularly in the US data, I did not find evidence that would reflect the Roaring Twenties, i.e. the presumed real effect of the stock market boom on consumption. I presented unambiguous evidence that the business cycle downturn that was to become the Great Depression, started in 1929. At first, the crisis spread all over Central Europe and then encroached on Japan and the US. At the height of global depression, ten out of ten sample countries were in recession. Contrary to popular wisdom, the stock market crashes on Wall Street occurring in October 1929 preceded neither the Great Depression in the US nor the global crisis. Instead, my findings raise the question why the crashes took place first after the commencement of the crisis in the real economy.

My findings point to a 'W' shape of the global business cycle due to two consecutive recessions. After a short-lived recovery in many countries in 1931, the second recession started between March and August 1931. In contrast to the first recession, the second recession was atypically short and severe. Within the MS framework, this is reflected by extremely negative outlier states of economic growth in the US, Germany, France, and Czechoslovakia. The secular component from HP filtering reveals a massive downward shift in the growth path of countries. The second recession appears to have been a spill-over from the US, hitting Europe but not reaching Japan: While European nations were the first to turn down in the first recession, the US spearheaded the second downturn after experiencing a short-lived upturn in IPM. The end of depression in the sample stretches over a much longer time span than its beginning: It ranges from March 1931 (Japan) to March 1933 (Poland). In general, impeded growth after 1932 and not a persistent downturn was the reason why in some countries the 1930s are considered as a 'lost decade' (e.g. in Landau and Tomaszewski 1982). Poland recovered, but only from mid-1933. French IPM declined again from August 1933, while the US and Czechoslovakia were subject to recurrent contractions after May 1934. My findings raise the question if the sluggish recovery after March 1933 should be taken into account as the aftermath of the Great Depression.

The analysis of co-movement implies that business cycle integration increased strongly after the commencement of the first stage of the crisis. The exception is

France, which became decoupled from the international cycle. I measured strong co-movement of business cycles during the 1920s within Europe. Contingency coefficients of global business cycle synchronicity are high even by postwar standards. There is little evidence of desintegration among European economies as well as between European economies and Japan as well as the US. Solely, the French business cycle is out of phase with the global cycle. In general, international co-movement of business cycles is more pronounced in the 1930s than in the 1920s.

These findings have various implications for research. Firstly, the world economic crisis began and ended earlier than is commonly presumed. This is probably due to a 'US bias' in research. The US business cycle lagged behind on the international development. The US was among the last 3 countries in the sample to enter recession in 1929 and among the last 3 countries to get out of the recession and recover. US-centricity has led to the widely-held belief that the Great Depression began in 1930 and ended in 1933. Yet, even in France the slide into recession is dated as early as March 1930. Hence, the dating of the the global cycle strongly suggests that future studies should preferably select 1928 as pre-crisis reference year instead of 1929, which is the standard now.

Secondly, the observation that the Great Depression consisted of two recessions, or at least of two 'stages' of the global depression —in the wake of research about the depression in the US (cf. Friedman and Schwartz 1967, Temin 1998, Eichengreen 1992)—, is reflected in the global data. What is more, my dating shows that this pattern was even more pronounced in other parts of the world. Viewing the Great Depression as two separate, highly different global business cycles has far reaching consequences: For instance, the question is no longer how Japan got out of the recession one year earlier than other countries, but why it did not follow others into the second recession. After all, Germany or Britain had got out of the first recession in early 1931 along with Japan. Put differently, it is a key to understanding what killed the global upturn beginning in 1931 in order to understand the Great Depression as a whole.

With respect to the "Golden Fetters hypothesis", an immediate effect of the dissolution of the gold standard on recession probability is neither visible on the level of a country, e.g. the UK or Poland, nor the global level. Instead, it appears that the gold standard was dissolved at the beginning of the second stage of the depression. The course of the crisis in Japan provides additional evidence for this hypothesis: Japan had been "fettered" to gold only from 1930 to 1931, yet it entered recession in 1929. Similar to other countries, Japan got out of recession in 1931 —before it abandoned gold— and was not involved in the second downturn within the Great Depression.

In fact, the time sequence of the two recessions is hard to reconcile with the gold standard theory of the Great Depression: Japan was infected by the depression before it became a member of the gold standard. In addition, there are several countries, in which the depression temporarily ended by early 1931, i.e. a couple of months before the gold standard was dissolved. What is more, all of the sample countries that had left gold by mid-1931, nevertheless entered the second recession of 1931/1932. Japan, by contrast, stayed on gold up until December 1931 and did not turn down. Neither do my results provide support that the timing or scope of monetary tightening is linked to the business cycle dates. Evidence remains sketchy on whether monetary tightening in the US and subsequent restrictive monetary policy around the world triggered the first wave of recessions. Rather, my findings imply that a 'one size fits it all' theory of the Great Depression must be treated with caution. Yet, it is likely that the two parts of the depression were interrelated: The first recession further weakened fragile economies in Europe. The depression of the Atlantic economy between 1931 and 1932 was, as such, a consequence of a preceding 'usual' business cycle downturn.

Thirdly, and accordingly, the fact that international business cycles were highly integrated during the interwar period stresses the importance of cross-country studies and comparative analysis. My findings emphasize that it is necessary to integrate the study of fluctuations within the real economy into the investigation of the transmission of real and monetary shocks, particularly with regard to co-movement in business cycles. They suggest that the international business cycle contributed to the second recession of the Great Depression. During the second recession, on the contrary, business cycles were much more integrated making the depression spread quickly. Evidence on co-movement further questions the gold standard theory: there were large differences in business cycles among gold bloc members, notably France, Poland, and Belgium. These differences are reflected in low co-movement in Polish and Belgian IPM with respect to French IPM and high co-movement with respect to IPM of countries that were not part of the gold bloc.

The study also contributes to our understanding of the agricultural crisis that preceded the Great Depression. My findings show that IPM declined in every country in the sample regardless of whether or not it belonged to the core of industrial activity or to its periphery. The adverse impact of the crisis on an economy does not correlate with its degree of industrialization; highly industrialized countries belonged to both the groups of worst, and least affected countries, e.g. the US as well as the UK. Considering the case of Poland and France or Sweden, the same appears to be true for less industrialized countries as well. Although Poland, Austria, and Czechoslovakia pursued highly different currency and trade policies, these Central European nations - with the exception of Hungary - suffered generally more than Western Europe and Scandinavia during the Great Depression. While the severity of recession in Eastern Europe was similar to Western European countries, the crisis was much more persistent in Central Europe. The likely cause of this persistence was that these economies were weakened so strongly during the 1920s (cf. Landau and Tomaszewski 1986) that they depended entirely on the recovery of Western economies.

CHAPTER 5

The Persistence of the Great Depression: How Large Were Nominal Wage Rigidities in Europe, the US, and Japan?

Is wage rigidity key to explaining the Great Depression? A recent debate about the role of wage inertia during the interwar period suggests that it is. At its heart is the puzzle left by Eichengreen's (1992) Gold Standard Theory of the Great Depression. The theory states that sticky wages incurred persistent money non-neutrality, and global monetary tightening caused the Great Depression. A high level of rigidity appears implausible, though, in the face of mass unemployment and weak unions. Hence, is US President Herbert Hoover to be blamed for keeping industry leaders from cutting wages at the onset the 1929 recession as stated by Ohanian (2009)?

The ongoing controversy over the role of wages seems to be caused by different perceptions of wage rigidity at the time, while empirical knowledge of wage stickiness is lacking. This study analyzes nominal wage rigidity during the interwar period based on time series models for the first time. It considers about 20 times as many observations per country than previous studies, which allows for estimating country-specific rigidity parameters. This study only partially supports Eichengreen's (1992) synthesis. The findings suggest that the decrease in wages represented a massive internal devaluation that took place internationally about two years prior to the external devaluation that followed the dissolution of the gold standard. There is time series evidence of a (Keynesian) inverse relationship between real wages and industrial production. My main finding is that during the Great Depression two aspects of wage rigidity must be considered separately: Wage inflation, i.e. trend growth in nominal wages, as well as the speed of adjustment, i.e. persistence of nominal wage changes. The analysis discovers structural breaks in wage trend growth, which remain hidden in lower frequency data. If structural breaks are taken into account, the level of persistence is shown to be moderate, similar to the postwar period, and similar across countries; If not, persistence estimates would suggest almost perfect wage stickiness in most countries. Since the speed of adjustment in nominal wages remained unchanged in the Great Depression, high pressure on wages was built up due to exceptional price deflation in 1930 and 1931. Structural breaks in wage inflation —in a technical sense— released this pressure, yet only discretely and insufficiently. This is the reason why the internal devaluation left room for nominal wage rigidity sufficient to rationalize money non-neutrality. A further result is that shifts in wage trend growth closely followed business cycles. Only in the US, changes in wage inflation coincided with political measures, such as the NIRA.

1. Introduction: Wage rigidity during the interwar period

Interest in the link between wages and the Great Depression has risen in the wake of the 2008 international finance crisis and the 2009 global recession in output and trade. A debate among US economists recently started about the role of President Hoover's labor market policies in worsening the crisis and in protracting recovery (see Cole and Ohanian 2004, Krugman 2008, Ohanian 2009, deLong 2009, and Rose 2010). The contradictory assessment highlights that the empirical knowledge about the wage rigidity during the interwar period remains very limited. In fact, ever since Friedman and Schwarz (1963) have attributed the central role in causing the Great Depression to monetary policy failure, economists have discussed the following puzzle: Why did the slightly restrictive monetary policy in the United States (US) in 1927 and 1928 trigger a sharp deflation not only in the US but around the world? How did a monetary shock trigger the Great Depression? Why did deflation result in an unprecedented recession? Eichengreen's (1992) Gold Standard Theory provides the most widely accepted explanation of this "aggregate demand puzzle"[1]: At the end of the 1920s, the Federal Reserve pursued a contractionary monetary policy to curb stock market speculation. The interwar gold standard forced other central banks to act accordingly, whereby it transmitted contractionary shocks to other countries. Hence, downward shifts in aggregate demand occurred synchronously, mutually aggravating themselves so as to cause a global recession.

It remains a major challenge for research, though, to rationalize the extent and the persistence of monetary non-neutrality (Ohanian 2009, Lucas 2007). In contrast to economic theory, supply side forces did not balance out the extraordinary negative shock to aggregate demand. This "aggregate supply puzzle" (Bernanke and Carey 1996) originates in the fact that, except in special circumstances, money is thought to affect output in the short run only. If output declines due to a negative monetary shock, prices and nominal wages will fall along with output. The decrease in wages and in other costs of production curbs the real effect of the monetary contraction. However, during the Great Depression the self-equilibrating forces of the markets remained inactive (Eichengreen 1992).

The Gold Standard Theory explains money non-neutrality with sticky wages: wages are seen as the main transmission channel for money to affect output during the depression. The combination of demand side shocks and supply side rigidities at national level resulted in a global depression, since economies were tied together through the interwar gold standard (cf. Eichengreen 1992, 2004, Bernanke and Carey 1996). As there are only few, general references to sticky wages in Eichengreen's (1992) seminal study, many studies in the wake of it make assumptions on the level of wage rigidity. To explain the scale of the Great Depression requires models that assume (nearly) perfect nominal wage rigidity across countries. However, a glance at monthly nominal wage series contradicts the downward rigidity story: In stark contrast to postwar recessions —during which wage cuts have been extremely rare (Fehr and Goette 2005, Bewley 1999)— nominal wages proved very flexible during the Great Depression: Between 1929 and 1934, nominal wages fell by more than 20% in Austria, Germany, the US, and Poland.

The absence of an empirical underpinning of the wage rigidity hypothesis in Eichengreen (1992) and others is puzzling. Most likely, Eichengreen considered the hypothesis to be justified by the finding of Eichengreen and Sachs (1985) that real wages and output were strongly negatively correlated during the depression. In fact, Eichengreen and Sachs conclude that sticky wages kept up real wages. However, their study was criticized for its narrow empirical basis and arbitrary assumptions (Bernanke and Carey 1996). Bernanke and Carey (1996) conduct the

[1] There are numerous studies supporting this theory. Eichengreen (2004) provides a comprehensive overview of the most important contributions.

only analysis I am aware of that explores the international evidence, whether wage stickiness kept markets from responding to negative shocks to aggregate demand at the beginning of the 1930's. They estimate a single rigidity parameter for 22 countries and find evidence of substantial nominal wage stickiness at the time. Due to data restrictions, their estimate is imprecise and leaves room for either interpretation: Wage rigidity could have been large enough to have played a decisive role in transmitting the monetary shock to the real economy, but it could also have played only a secondary role.

Despite of being a central building block of the gold standard theory, the actual extent of wage rigidity is unknown. Empirical research in the Great Depression is almost exclusively concerned with demand side factors. Studies of wage rigidity after WWII cannot be used to make inferences, because they consider a period of high inflation in prices and wages. Such periods imply a behavior of nominal wages that differs from a low inflation environment, such as that of the 1930s (Fehr and Goette 2005). Despite the lack of reliable rigidity estimates, the majority of studies on the Great Depression assumes —and often implicitly— that wage stickiness of an unknown degree was responsible for inertia on the supply side (see Cole, Ohanian, and Leung 2005, Bordo, Erceg, and Evans 2000). The closely related assumption that wage rigidity was equal across countries at that time is common, but questionable and has not been verified empirically. After all, wage rigidity depends on many factors that typically vary from country to country (cf. Taylor 1998) and differences in rigidity would affect the economy's capacity for adjustment.

The main contribution of my study is to learn more about wage rigidity during the Great Depression by addressing the following questions: First, to what extent were nominal wages "sticky" during the interwar period? Second, can the identifying assumption that wage rigidity was equal across countries be justified empirically? Thirdly, if and how wage rigidity was related to the time pattern of downturns and recoveries? If wages were not as rigid as commonly assumed, why didn't they adapt to price changes earlier and more strongly such as to balance out growing deflationary pressure? Put differently, what determined the timing and scale of the fall in nominal wages? Thereby, my study picks up Bernanke and Carey's (1996, p.881) request that

> "[more] attention needs to be paid to the performance of the sticky-wage hypothesis in the time-series dimension —by using the higher-frequency data that are available for some countries, for example."

To this end, I have compiled a set of monthly data of industrial wages for the US, Japan, and eight European countries[2]. The time series analysis covers the period between 1926/1929 and 1936 and draws on recent econometric advances in the measurement of price rigidities (Andrews and Chen 1994, Levin and Piger 2004). This review of the sticky wage hypothesis uses data of much higher frequency than earlier studies, which allows to obtain more precise and meaningful rigidity estimates as well as further economic insights, e.g. variation between countries.

[2]Austria, Belgium, Czechoslovakia, France, Germany, Italy, Poland, and the UK.

The remainder of this chapter is organized as follows: In section 2, I will review the literature related to wage rigidity and the Great Depression. The data and the empirical strategy will be discussed in section 3. In section 4, I will present the estimation results and check their robustness in section 5. I will conclude my findings in section 6. Additional graphs and tables will be presented in the appendix.

2. Literature review

Most explanations of the Great Depression, particularly the more recent ones, postulate that rigidity in nominal wages was a prerequisite of the crisis. The reason is that, even though economists have reached a certain consensus that monetary shocks were the driving force of the interwar Great Depression (Bordo, Erceg, and Evans 2000, Bernanke 1995, Eichengreen 1992, Friedman and Schwartz 1963), it remains difficult to explain the persistent departure from economic equilibrium in many countries during the Great Depression (Bernanke and Carey 1996, Eichengreen and Sachs 1985, 1986).[3]

In the initial hypothesis by Friedman and Schwartz (1963) wage stickiness does not matter. The authors claim that the crisis originated in inappropriate policy of the US monetary authorities, when the Fed chose a restrictive stance in monetary policy in 1927 and failed to deal with deflation and the downturn of the real economy in the US at the beginning of the 1930s. Nominal wages should have declined in order to adjust to the resulting pressure on the costs of production, because the introduction of the interwar gold standard and, in particular, the restauration of prewar parities exerted deflationary pressure on many economies after 1925. However, this hypothesis cannot satisfyingly explain the "aggregate demand puzzle of the Great Depression" (Eichengreen 2004): How could a slightly restrictive US monetary policy in 1927 and 1928 result in a harsh deflation and trigger an unprecedent slump of the US and of the world economy? Contrary to predictions from equilibrium models, the market's self-equilibrating forces did not balance out adverse effects of recurrent monetary policy shocks. One would expect that

> "[after] two years of decline, the market's self-equilibrating tendencies should have asserted themselves. [...] Far from improving, however, the situation deteriorated markedly [...]." (Eichengreen 1992, pp.258–259)

Eichengreen's (1992) Gold Standard Theory of the Great Depression (henceforth, GST) tackles this puzzle by introducing two innovations: On the supply side, it replaces the 'US-centric' approach by taking the Great Depression as a global economic crisis and by considering international economic interdependencies. On the demand side, it argues that Keynesian-type sticky wages, neglected by Friedman and Schwartz (1963), played a central role. Hence, the GST is a synthesis (Eichengreen 2004).

The GST is based on the AS-AD framework, yet Eichengreen does not formalize it in a structural model. In line with Friedman and Schwartz, the theory restates that the initial deflationary impulse originated in the US. It was transmitted internationally, because many countries stuck to the rules of the interwar gold standard. The theory stresses that the international currency regime was structurally flawed: The participating countries did not prevent large imbalances in the accumulation of reserves and they were unable to coordinate expansionary

[3] An overview of the literature and of competing explanations is provided by Eichengreen (2004).

monetary policies when the crisis began to mount. The contractionary monetary policy in the US and elsewhere triggered deflation and caused a downward shift in aggregate demand throughout the world at the end of the 1920s. If the supply curve is non-vertical, the AS-AD model predicts that such a shift leads to a fall in prices and in output. This is what one observes in the data as well (cf. figures 1–9 in appendix E). According to the GST, the rules of the gold standard amplified the initial minor monetary contraction in the US and the subsequent synchronous drop in international demand worsened the recession in the US: While the US economy needed exports to compensate for struggling domestic demand, international demand plunged due to a synchronous monetary contraction in many countries. However, wages did not adapt to falling prices. Real wages jumped, which increased the downward pressure on output. Because this malfunctioning of labor markets is attributed to slow adjustment in nominal wages, wage stickiness is held responsible for the massive and persistent non-neutrality of money (Eichengreen 2004). Eichengreen (1992) regards wages to be the main transmission channel for money to affect the real economy. Also Bernanke (1995) ascribes the prolonged non-neutrality of money both to deflation-induced financial crisis and to increases in real wages above market-clearing levels.

The wage rigidity hypothesis, though a key assumption of the GST, is impaired by theoretical and empirical weaknesses. First, political and economic circumstances for highly rigid wages were absent at the turn of the decade. Bernanke and Carey (henceforth, BC) (1996) stress that the government's role in labor markets was limited and "price declines were too large and well publicized for money illusion to be widespread" (p.855). The fact that nevertheless wages were sufficiently sticky to cause persistent money non-neutrality poses the "aggregate supply puzzle" of the Great Depression (BC 1996). Second, it is not clear if inferences on wage rigidity from the post-WWII period with high inflation can simply be extrapolated to the interwar period, in which *de*flation prevailed. Thirdly, the empirical knowledge of the extent of wage rigidity during the depression is quite weak. Thus, assumptions made about stickiness must be viewed with caution. Most prominently, Eichengreen (1992) does not discuss any empirical evidence for the sticky-wage hypothesis in his 400-page volume. BC (1996, p.854) note that

> "[explicitly] or implicitly, most proponents of the Gold Standard Theory have invoked 'sticky' nominal wages as the reason for the protracted real impact of the monetary contraction. However, in contrast to the attention paid to the determinants of aggregate demand, recent research on the Depression has included very little analysis of aggregate supply in general or the sticky-wage assumption in particular."

Research in labor markets offers various explanations of nominal inertia. Prominent explanations include misperceptions about market wages due to informational frictions (Lucas 1972), staggering of contracts (Taylor 1979), costly adjustment of prices (Mankiw 1985), moral aversion against wage cuts (Akerlof and Yellen 1988), insider-outsider models relating to unionization (Lindback and Snower 1988), and not the least money illusion (Fisher 1928, Keynes 1936). The latter has been somewhat discredited in modern economics, but recent research provides evidence for its existence, e.g. Fehr and Tyran (2001) show that after a fully anticipated negative nominal shock, long lasting nominal inertia prevails.

If it was proven that these mechanisms were universal and time-invariant, the idea would suggest itself to refer to the postwar period. After all, wage rigidity has been intensely studied after 1945, often based on large microdata panels (e.g. Bewley 1999, Hamermesh 1993). Different from the interwar period, however, the postwar period is characterized by strong price and wage inflation in most countries. As stressed by Fehr and Goette (2005), the behavior of nominal wages in a low inflation environment is not well understood. It is far from obvious that wages remain rigid during deflation. They argue that this "question is important because there is little need to cut nominal wages in an environment with high wage inflation [...]. [Whether] real effects are associated with widespread nominal wage rigidity is ultimately an empirical question." One reason for the uncritical acceptance of the sticky wage hypothesis in spite of these issues may be strong evidence of wage rigidity in the postwar period. Taking the cautioning of Fehr and Goette (2005) seriously, however, implies that we need to know more about wage rigidity in the environment of low inflation (or deflation) that prevailed during the interwar period:

> "If the macroeconomic environment were different, microeconomic behavior would be different. Nominal wage reductions would no longer be seen as unusual if the average nominal wage was not growing. Workers would not see them as unfair, and firms would not shy away from imposing them." (Gordon 1996, p. 62, cited in Fehr and Goette 2005).

Eichengreen and Sachs (1985, henceforth ES 1985) presented one of the first econometric studies of the supply side puzzle. In order to analyze whether exit from the gold standard and devaluation were preconditions for recovery, they regress the change in output on the change in real wages based on a cross-section of ten countries. They present a plot showing that the change in real wages was negatively related to the change in output between 1930 and 1935: countries which left gold in 1931 scatter the low real wage-high output region of the plot. Countries that continued to adhere to gold scatter the opposite high real wage-low output region. ES (1985) conclude that the former group recovered faster, because countries reflated earlier and were thus less affected by nominal wage rigidities.

This result has been criticized for various reasons: Above all, the actual role of wage stickiness remains unclear, because ES (1985) do not separate the real wage into its price level and nominal wage component and they take perfect wage rigidity as given. This is particularly relevant as empirical evidence for the postwar period suggests that real wages are a-cyclical rather than countercyclical (e.g. Romer 1996). BC (1996) criticize ES (1985) for arbitrarily choosing 1935 and 1929 as reference years. They demonstrate that the strong output-wage correlation vanishes if one chooses 1932 instead of 1935 and also if one includes further countries in the sample. Cole, Ohanian, and Leung (2005, henceforth COL 2005) object that the correlation between output deviations and real wages was actually positive in the

1930s. Nonetheless, Ohanian (2009) argues that US President Hoover pushed up wage rigidity by convincing industry leaders not to cut wages. This hypothesis would explain BC's (1996) aggregate supply puzzle, yet it represents a return to the narrow US-centered approach of the 1980s and before. Moreover, Rose (2010) opposes Ohanian's (2009) conclusions and demonstrates that Hoover's talks had little to no effect on wage cuts.

In the light of such contradictions, it is striking that BC (1996) remains one of very few studies which estimate nominal wage rigidity during the Great Depression, i.e. the nominal component, separately from prices. BC (1996) analyze a panel with 22 countries, which comprises six observations per country (1931-1936).[4] They obtain a common rigidity estimate with a small positive coefficient. They conclude that changes in nominal wages were persistent, but neither determined entirely by past wages nor entirely by prices during the 1930s. Hence, they reject the hypothesis that wages responded fully to aggregate demand shocks. They regard the level of wage rigidity to be high in the light of mass unemployment, but they find that in most specifications, the unemployment variable is economically and statistically insignificant for wage formation.[5] The authors do not take into account evidence from other studies, which suggests that high unemployment can have positive effects on labor union organizing (Freeman 1997). At least in the US, the UK, and Germany, though, unionization rose strongly during the Great Depression (see figure 7 in appendix E). Bernanke and Carey themselves point out severe shortcomings of their analysis: Inferences from the study are limited, because the data set comprises annual observations for only a few years. Estimates are imprecise due to the small amount of data. The imprecision is expressed by a large lower and upper bound of estimates, which leaves room for either conclusion: The upper bounds would suggest that wage rigidity was large enough to provide a transmission channel for monetary shocks, whereas the lower bound would suggest that rigidity was too low, which is consistent with the exceptional decline in money wages. Using a single parameter for all sample countries assumes that rigidity did not vary across countries. However, any existing variation at the time needs to be detected in order to correctly identify cause and effect of wage rigidity. This makes it virtually impossible to validate the competing explanations. Moreover, BC (1996) rule out that purely real factors affected the IPM/real wage relationship. Yet, the authors stress that they focus solely on the cross-sectional relationship between output and real wages.

One of the few further econometric studies is Madsen (2004). He estimates a supply equation similar to BC (1996), also based on annual data. He finds that wages adapted by about 40% to contemporaneous shocks in prices, whereas past wages had no or little impact on current wages. Hence, in his model simulations, he assumes that nominal wages were perfectly flexible and sets the coefficient of

[4] *De facto* they omit 1930, because they lose one observation per country by introducing lagged variables. Their estimation in differences includes only four years, 1933–1936.

[5] They introduce instruments to avoid simultaneity bias. Moreover, they test further supply shifters and allow for dynamic effects. As pointed out by BC (1996), estimation of (2.5) by ordinary least squares requires the implausible assumption that cross-sectional variation is entirely due to aggregate demand factors.

lagged wages to zero. The majority of studies, by contrast, is country-specific and uses descriptive methods, making it difficult to integrate their findings e.g. in order to parameterize theoretical or empirical models: They do not quantify the level of wage rigidity, but consider indicators of inflexibility, such as unionization or political interference (e.g. Eichengreen and Hatton 1988, Gourevitch 1984). Dighe and Schmitt (2010) estimate the effect of changes in employment and in wholesale prices on nominal wages in the US between 1920 and 1939. Furthermore, they test for a potential breakpoint in nominal wage formation in 1929 and 1933. Their results suggest that nominal wages were almost perfectly sticky, i.e. changes in employment and prices hardly affected wage formation. They do not find evidence of a change in the degree of rigity throughout the interwar period.

The uncertainty about the role of nominal wage rigidity is an immediate consequence of the dearth of empirical studies concerned with the supply side during the interwar years. Studies, which consider labor markets during the interwar period in some way, need to make (brave) assumptions on wage rigidity due to the lack of empirical knowledge. Often they consider scenarios: Bordo, Erceg, and Evans (2000) analyze a dynamic stochastic general equilibrium model based on staggered labor contracts. In this model, nominal wages are sensitive to the level of employment. Monetary shocks are largely unanticipated. The study evaluates the effect of two different wage rigidity parameters: One parameter reflects perfectly flexible wages, the other one reflects a two quarter half-life in wage adjustment. COL (2005) feed a DSGE model with a simple dichotomous assumption on the state of wage or price stickiness, i.e. wages are either sticky ($\rho = 1$) or flexible ($\rho = 0$). When COL (2005) compare the model outcome to historical data, they find that deflation alone cannot fully explain the variance in the decline of output across countries. In many other studies such assumptions are used only implicitly. Similarly, few studies discuss the implicit assumptions that wage rigidity was equal across countries and over time.[6] To see why such implicit assumptions are problematic, let us suppose that wage rigidity was not uniform across countries, but that variation in rigidity remained unaccounted for, as usually. In this case, the impact of deflation on the real economy would differ due to the variation in wage rigidity. This, in turn, could explain the contradicting conclusions presented above. In this case, the differential impact of deflation on output would be attributed to other factors. Put differently, the reason for greater or longer lasting negative allocative effects in some countries may have been a stronger monetary contraction in these countries —or a higher level of wage rigidity. Differences in the level of wage rigidity across economies would explain the lack of a systematic relationship between output and deflation. In fact, the literature on wage rigidity after 1945 shows that wage rigidity depends on many factors that typically vary across countries (see Taylor 1998 for an overview and also Hamermesh 1993).

Yet the Great Depression represents a unique situation for analyzing the adjustment of wages under wage deflation. There is no second situation during the 20th century, in which massive wage cuts occurred across industries and countries. Because deflation in prices and wages was common and well publicized during the

[6] They usually make explicit assumptions on the underlying state of wage rigidity.

Depression, various explanations for wage rigidity can be ruled out right away, such as misperception of market wages (Lucas 1972). Classical economic 'voluntary' unemployment is improbable as well, because mass unemployment and insufficient unemployment benefits limited bargaining power. Keynes' (1936) suggestion that wages are downward rigid, because employers would have to coordinate wage cuts —which he considers impossible without state intervention— is refuted by the simple fact that massive wage cuts took place during the Great Depression without any such agreements. Empirical evidence for the moral wage theory (Akerlof and Yellen 1988) refers to the usual scenario of positive wage inflation during the post-war period.

Finally, it should be noted that some authors question the deflation plus sticky wages story altogether. COL (2005, p.4) note that "many countries had similar deflation rates, but had very different output changes" and conclude a "lack of a strong and systematic positive relationship between output and deflation". Their study argues that, beyond the monetary shock, a productivity shock was responsible for the international depression. Ritschl and Woitek (2000) demonstrate, based on a VAR approach, that money had little predictive power during the Great Depression, at least for the US economy.

The literature review suggests that, in spite of its importance for understanding the Great Depression, no substantial progress on the issue of international nominal wage stickiness during the 1920s and 1930s has been made after BC (1996). What is more, the debate has returned in recent years to a US-centric evaluation of the crisis (e.g. Bordo, Erceg and Evans 2000, Cole 2009, Rose 2010, Dighe and Schmitt 2010). However, such a debate neglects the global dimension of the depression, particularly as the level of wage rigidity is determined not least by different country-specific institutions.[7]

[7] E.g. a comparably low level of unionization in the US (cf. Taylor 1998).

3. Data and empirical strategy

Besides ideology, the lack of research in nominal wage rigidity during the interwar period is due to both insufficient and inappropriate data used in the literature. Whereas postwar studies examine large micro data panels, studies of the interwar period usually consider aggregate macroeconomic data. Since wage rigidity is indirectly observed from the inertia of nominal wages in adapting to shocks —e.g. in labor demand, in the price level, or in wages itself— one needs information that allows to detect this inertia. In order to measure rigidity during the postwar period, studies rely on thousands of observations of individual company-worker contracts.[8] However, such data are not available for the interwar period.

My analysis follows Andrews and Chen (henceforth AC) (1994), who present an approach to measuring persistence based on macroeconomic time series. The approach represents an improvement on previous research —e.g. by Bernanke and Carey (1996), Madsen (2004), or Dighe and Schmitt (2010)— along the following lines: (i) Nominal wage rigidity is estimated for 10 countries including four Gold Bloc member states (France, Poland, Italy, and Belgium). The analysis yields estimates for each sample country, which are still immediately comparable. (ii) My analysis considers a much longer time period and does not depend on an arbitrary reference year such as ES (1985). (iii) The approach requires a lot of data. Thus, more information is taken into account than in earlier studies leading to greater precision of estimates. Additionally, estimating rigidity from monthly data minimizes aggregation bias in the time domain (cf. Taylor 2001).

The empirical approach utilizes impulse response functions (IRF) to reveal persistence properties of the time series. The level of wage rigidity is given by the IRF's half-life, i.e. on the speed of adjustment in wage growth after innovations. Because a scalar measure is desirable in order to compare persistence in different countries, AC (1994) calculate the cumulative impulse response (CIR). The CIR is the sum of the IRF over all time horizons. Because the IRF are estimated using autoregressive (AR) models, their estimation requires a lot more observations in the time domain than is provided by the League of Nation's sample. These data, which have been analyzed by most of the existing cross-country studies (cf. BC 1996, Madsen 2004, and others), comprise only seven annual observations per country. What is more, the sample only covers the period from 1930 to 1936. Thus, the studies ignore wage rigidity at the onset of the Great Depression and before (cf. the business cycle dating in chapter 4).

Therefore, I have compiled a new sample of monthly wage index data from the US, Japan, and eight European countries. These aggregate time series have been reported by a couple of countries for most of the interwar period. The sample begins in January 1925 and ends in December 1936. The sample is incomplete in the time domain in the case of Austria, Belgium, Czechoslovakia, and Italy. The raw sample data, re-based to the 1929 annual average, are plotted in figure 1. The sample effectively comprises as many as 20 times more observations per country

[8] Taylor (1998) provides an overview of approaches to evaluating wage rigidity at the micro and macro level with an emphasis on the staggered contracts model.

than the League of Nation sample. Even after taking annual differences, up to 132 observations per country remain for empirical analysis (for details see table 1).

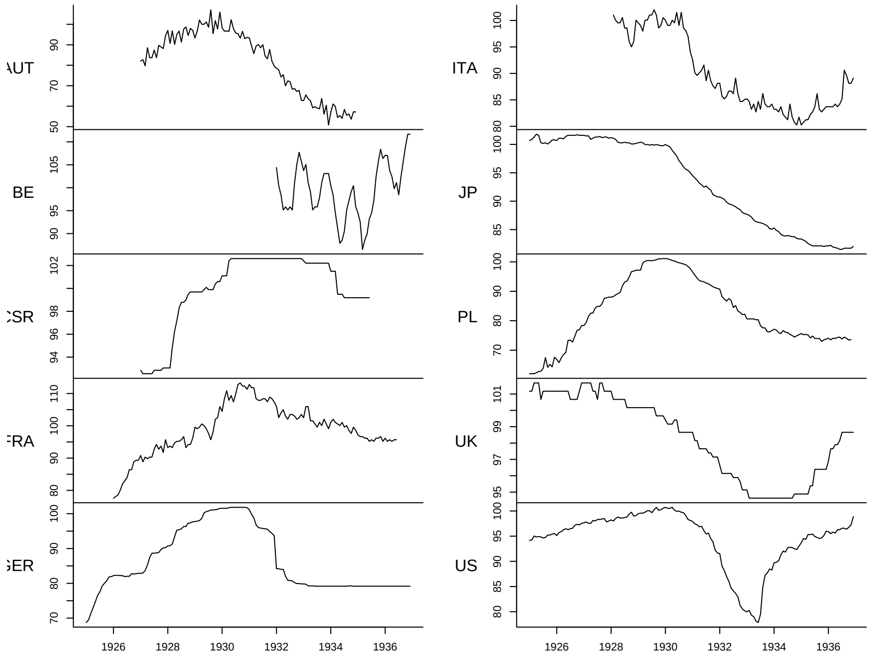


Figure 1: Nominal wage levels, 1925–1936 (by country, 1929=100)

The data were taken from various sources: Data on wages, industrial production, and prices for most countries were taken from *Statistisches Handbuch der Weltwirtschaft.* This publication contains a collection of economic indicators that were published by statistical offices all over the world. Nominal wage data for the UK and the US were downloaded from the NBER's Macrohistory Database website (NBER series no. m08048b and m08061c, respectively). Wage data for France are taken from the Foreman-Peck, Hallett, and Ma (2000) data set.[9] Price data for Japan were downloaded from the Bank of Japan's website.[10]

The wage index data are based on average hourly or weekly wages either covering overall manufacturing or important industrial sectors.[11] Further documentation

[9]This monthly wage index consists of interpolated annual data by Foreman-Peck, Hallett, and Ma (2000) using monthly *Indices des salaires horaires etablis par le syndicat des entrepreneurs de reseaux centrales electriques.*

[10]http://www.imes.boj.or.jp/english/hstat/data/prices.html

[11]Austria: Weekly wage rates in Vienna; Czechoslovakia: Hourly industry wages; Germany: Avg. hourly tariff wages; Italy: Average industrial wages; UK: New index of average weekly wages; US: Index of composite (average hourly) wages.

in the secondary sources is incomplete, i.e. the original sources are seldom indicated. Similar uncertainty prevails over the composition of these aggregate data and adjustment issues. For instance in case of US weekly wage rates, it is not clear if the data were corrected for reduced work schedule taking place in the US in 1930 (cf. Ohanian 2009). If they were not corrected, nominal wages would appear more flexible than they actually were. The comparative approach of my study allows to check robustness of results with respect to this issue as some countries do not report an index of hourly rates. Moreover, all wage series seem to be consistent in the sense that there is no sign of changes in the composition over time.

Comparing the graphical evidence in figure 1, three common sequences can be distinguished: At the beginning of the sample period, nominal wages were growing strongly in most countries. However, wages grew very slowly in the US and decreased even slightly in Japan and in the UK. During the second period, beginning approximately in 1930, industry wages decreased strongly in all sample countries, but Czechoslovakia. During the third part, beginning in 1932, wage levels stagnated at low level in most countries. Only in the US, in the UK, and in Italy wage levels recovered that quickly that, in 1936, US wages came close to their pre-crisis level.

Year-on-year (yoy) differences are calculated in order to obtain seasonally adjusted and trend-stationary growth rates (depicted in figure 2 in appendix E). Summary statistics of the resulting time series provide mixed evidence of nominal wage rigidity at the time: In the majority of countries, negative wage growth rates were close to or even surpassed maximum positive growth rates (see table 1). Yet, autocorrelations of order one are close to unity, hinting at a high dependency of current wages from past wages and, hence, at persistence of wage changes. Moreover, the figures suggest two different groups of countries with respect to extreme wage changes: One group is characterized by two-digit annualized growth of nominal wage rates. The reduction in nominal wages exceeded –20% in Austria and was still more than –10% in Germany, Italy, the US, and Poland. By contrast, in Japan, Czechoslovakia or the UK, negative growth rates did not exceed –6%, –2%, and –3%, yoy, respectively. Wages fell fastest between March, 1931 (Japan) and January, 1933 (in Poland, neglecting Belgium and Czechoslovakia). Readers familiar with the study of BC (1996) may be surprised by the high volatility of the wage series. This, in turn, implies that information is usually lost through the aggregation in the time domain.

Table 1: Year on year change in nominal monthly wages: data coverage and summary statistics

	AUT	BE	CSR	FRA	GER	ITA	JP	PL	UK	US
Start of series	*Jan* 1928	*Jan* 1933	*Jan* 1928	*Jan* 1927	*Jan* 1926	*Feb* 1929	*Jan* 1926	*Jan* 1926	*Jan* 1926	*Jan* 1926
End of series	*Dez* 1934	*Dez* 1936	*Jun* 1935	*Jun* 1936	*Dez* 1936	*Dez* 1936	*Dez* 1936	*Nov* 1936	*Dez* 1936	*Dez* 1936
# observations	84	48	90	114	132	95	132	131	132	132
Maximum and minimum values										
Minimum reached in	*Okt*1932	*Mrz*1935	*Jul*1934	*Feb*1932	*Jan*1932	*Aug*1931	*Mrz*1931	*Jan*1933	*Nov*1932	*Okt*1932
Minimum value, %	−23.5	−8.4	−3.0	−8.3	−16.8	−10.8	−5.9	−11.1	−2.1	−13.5
Maximum reached in	*Mrz*1928	*Mrz*1936	*Okt*1928	*Aug*1930	*Jan*1926	*Sep*1936	*Sep*1926	*Mrz*1927	*Apr*1936	*Jun*1934
Maximum value, %	22.0	19.8	7.2	17.9	19.6	7.7	1.5	20.9	3.2	18.0
Total decrease, %	−52.5	–	−0.4	−12.5	−22.3	−18.9	−15.3	−24.7	−5.5	−22.7
Gold standard entry and exit dates										
Exit	*Sep* 1931	*Mar* 1935	*Oct* 1931	*Sep* 1936	*Jul* 1931	*Mar* 1934	*Dec* 1931	*Apr* 1936	*Sep* 1931	*Mar* 1933
Entry (Year)	1923	1925	1926	1928	1924	1927	1930	1927	1925	1919
Descriptive statistics										
Mean	−5.2	1.5	0.9	1.6	0.6	−1.6	−1.9	1.7	−0.3	0.3
Standard deviation	10.8	6.8	2.6	5.9	7.1	4.2	1.8	8.7	1.3	6.1
Autocorrelation (1)	0.8	0.9	1.0	0.9	1.0	0.9	1.0	1.0	0.9	1.0
ADF test for non-stationarity										
p-value	> 0.99	0.445	0.032	0.582	0.603	0.754	0.707	0.962	0.959	0.342

Sources: Statistisches Handbuch der Weltwirtschaft (1936, 1937). US, UK: NBER Macrohistory Database website (series no. m08048b and m08061c). France: Interpolated quarterly data from Foreman-Peck, Hallett, and Ma (2000). Gold standard entry and exit dates: Wolf and Yousef (2005).
Notes: Lines 4-7 indicate the lowest and highest annual growth in nominal wages between 1926 and 1936 (annual rates, monthly basis).

Compared to the development of nominal wages, prices declined earlier and even more strongly than wages: Between 1929 and 1933, prices dropped more than 30% in all countries but Austria (cf. figure 3 in appendix E). Nevertheless, the total decrease in wages during the crisis remains impressive, too: From 1929 to 1933, nominal wages fell by about 52% (!) in Austria, more than 20% in Germany, the US, and Poland, and still by 13%–19% in Italy, Japan, and France (all in local currency, cf. table 1). In some countries, nominal wage levels were lower at the end of the sample period than they had been at its beginning. In the light of this drop in wage payments, it is difficult to speak of downward nominal wage rigidity. Yet, also in eight out of ten countries, price levels were lower in 1936 than in 1925. Taken together, the decrease in nominal wages did not suffice to keep real wages stable, it only slowed down the increase of real wages. These observations raise the question why wages, being in the 'state' of falling, did not decrease further such as to balance out the drop in prices.

With the respect to the Gold Standard Theory, there is no visual evidence of a correlation of mean growth rates or standard deviations with exit from gold. It must also be noted that the observations of wholesale prices contradict ES' (1985) conclusion that membership in the gold standard exposed a country to the deflationary impulse beginning in 1927/1928, and that exit from gold stopped deflation and allowed reflation. The expected price movements can be observed in the US, Belgium, Italy, and Poland and thus in less than half the sample countries.[12]

The autoregressive model applied subsequently represents an a-theoretical approach to the issue of wage rigidity. It is still possible to motivate the approach by the AS-AD model (cf. section 2 in appendix E), in which aggregate supply depends on real labor costs. In practice, by contrast, wage contracts refer to nominal wage rates. Thus, the AS curve depends on a function of both nominal wages and prices. For instance, ES (1985) assume a functional form for the AS curve, in which output q_t at time t depends negatively on the real wage, i.e. on the nominal wage w_t and positively on the price level p_t. The nominal wage is assumed to be perfectly sticky ($w_t \equiv \bar{w}$), hence (all variables in logs)

$$q_t = -\alpha\left(\bar{w} - p_t\right) \tag{3.1}$$

There are many alternative formulations of (3.1), yet the central issue to all of them remains, whether the assumption $w_t \equiv \bar{w}$ is justified during the interwar period. The empirical model used here to estimate country-specific wage rigidity makes use of recent advances in the measurement of price rigidities at the macro

[12]Cf. figure 3 in app. 1. In Japan, deflation started in early 1929 although the country became a gold standard member first in 1930. In the UK, where the end of deflation coincided with exit from gold in August 1931, reflation can be observed first in 1935. Although Germany and Czechoslovakia were among the first countries to leave the gold standard, deflation in both countries continued until early 1933. In Austria, price deflation ended already in early 1931, i.e. half a year before the country left gold, and reflation is observed beginning in December 1936, only.

level. Andrews and Chen (1994) advocate a measure of persistence in price formation ρ, which is calculated as the sum over the coefficients of an appropriate AR model (given in its most general form by)[13]

$$w_t = \mu_0 + \sum_{k=1}^{K} \alpha_k w_{t-k} + e_t \tag{3.2}$$

The rigidity measure ρ is equivalent to $\sum_{k=1}^{K} \alpha_k$. ρ indicates the speed of adjustment of nominal wages to shocks, ranging from immediate adjustment ($\rho = 0$) to perfect stickiness ($\rho = 1$), in case the shock does not die out. The rigidity measure indicates how strongly and for how long innovations affect a time series. Estimating $\hat{\rho}$ requires the researcher to determine an appropriate lag order K in advance. Because the available data set is small, it is preferable to select a parsimonious model. Therefore, I apply an algorithm for model selection that restricts K stepwise, starting with $K = 12$. The fit of the resulting models is evaluated by Akaike's information criterion (AIC, henceforth). The algorithm continues to further restrict k until the AIC signals that the loss in explanatory power outweighs the gain from making the model more parsimonious.

An econometric issue, which plagues models such as (3.2), arises from the potential presence of structural breaks in price or wage formation (Levin and Piger 2004). Structural breaks in the data generating process (DGP) strongly bias the estimation of autoregressive models (Perron 1990a). If the mean μ_0 of the series is subject to a structural break, it will exhibit characteristics of a unit root process. The reason is that deviations from the mean are highly persistent. As a consequence, the level shift in the series biases the estimator $\hat{\rho}$ upward, i.e. it will understate the speed of adjustment toward the (new) mean. One can imagine various economic reasons that caused structural breaks in wage formation during the interwar period: The combination of extraordinary economic crisis, high unemployment, and a low inflation environment set stage for fundamental changes in wage setting. Another potential reason are institutional changes on labor markets.[14]

In order to detect structural breaks in the DGP, I make use of the fact that the augmented Dickey-Fuller (ADF) test tends to fail to reject the hypothesis of non-stationarity of a time series if structural breaks are present (Perron 1990a). Applying the test to each of the wage growth series, I find that the ADF test statistic rejects the null hypothesis of non-stationarity for only one sample country. It is improbable that such an overwhelming acceptance of the null of non-stationarity of wage changes is actually due to unit root processes: In theory, one would expect wage formation to be mean-reverting in the medium term. Technically, the test results are most likely due to the 'V' pattern that is observed in most of the time series (figure 2, app. 1). Yet, such a pattern is produced by unit root processes coincidentally making it highly unlikely that a similar pattern occurs across countries,

[13]This approach has become standard in the literature on price rigidity (cf. Levin and Piger 2004).

[14]E.g. Ebell and Ritschl's (2007) discussion of fundamental changes in labor market institutions in the US suggests to consider the presence of structural breaks in wage formation.

as is the case with the sample of nominal wages. As discussed above, the adoption of the unit root hypothesis by the ADF test can result from structural breaks as well. The systematic appearance across countries suggests that the mean of all time series is subject to structural breaks rather than to a random walk process. In this case, it would be necessary to remove their effect in order to obtain unbiased estimates of wage rigity.

The Chow test is the most common test of structural breaks in a time series, if the underlying DGP is supposed to be linear.[15] The Chow test requires the researcher to determine the potential break dates *a priori*, such that the means of the resulting two sub-samples can be tested for equality. In practice, such break points are seldom known in advance. Thus, Quandt (1960) proposes an agnostic approach to detecting the break date instead of assuming it from the beginning: The idea is to compute Chow's test statistic at each date of the time series and, in case the F-test rejects equality at several potential breakpoints, to select the most probable break date as indicated by the supremum F-statistic.

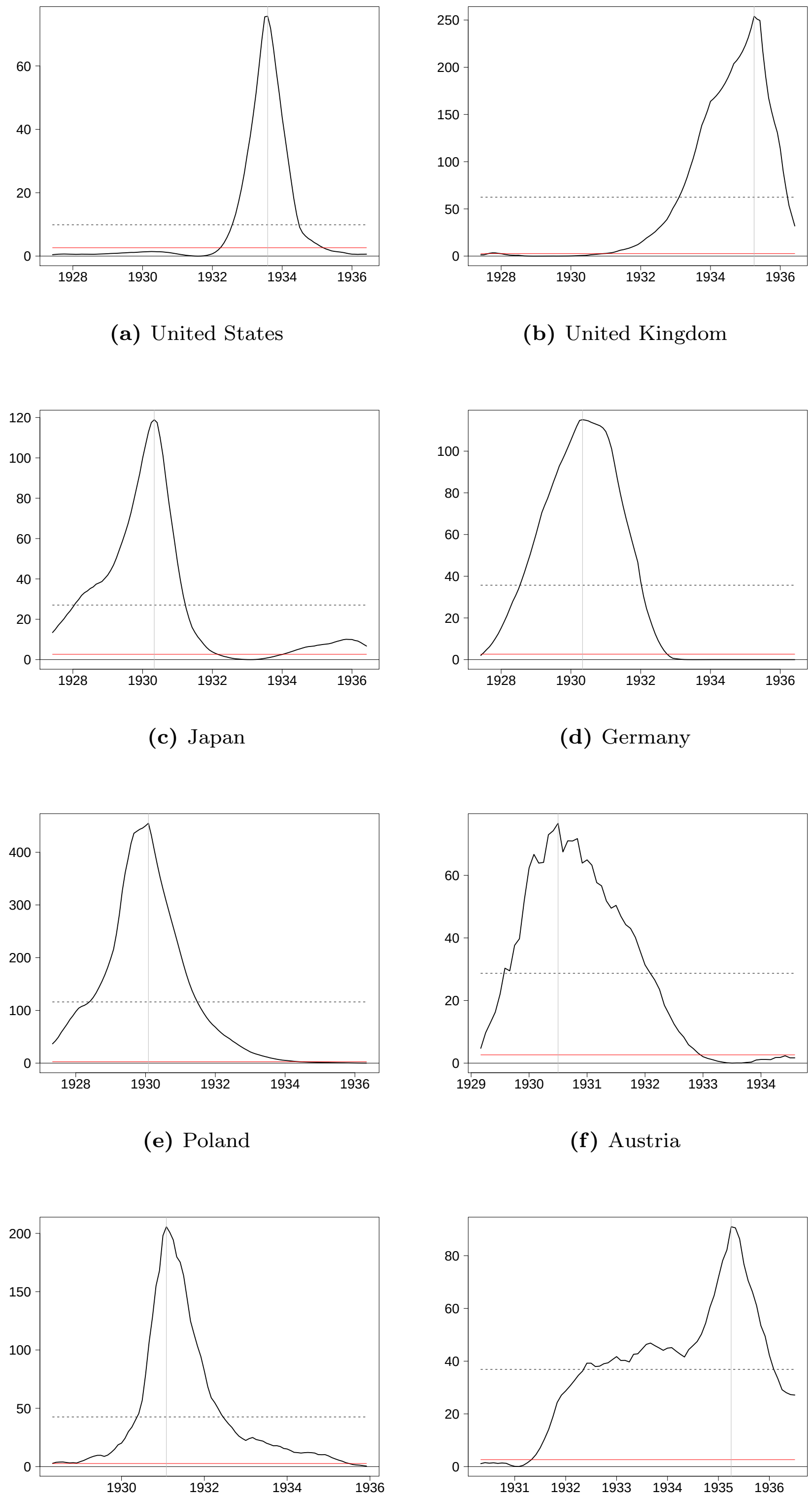
Table 2: Quandt test for structural breaks in wage formation

	AUT	BE	CSR	FRA	GER	ITA	JP	PL	UK	US
F stat	84.4	92.9	-	205.7	115.4	91.0	118.9	455.0	254.0	75.7
p-value	0	0	-	0	0	0	0	0	0	0
Date	1930(7)	1935(4)	-	1931(2)	1930(5)	1935(4)	1930(5)	1930(2)	1935(4)	1933(8)

Notes: Based on supremum F-statistic. A p-value of 0 indicates $p < 0.0005$. Dates are indicated as YEAR(MONTH).

The null hypothesis of the Quandt (1960) test states that the DGP of wage growth between 1926 and 1936 was not subject to a structural break in the intercept. It is evaluated by a F test for equality of the estimated coefficient μ before and after each potential break date. A series of such test statistics, evaluated at each point in time, is plotted in figure 2. Table 2 provides the corresponding supremum F-statistic (*supF*, subsequently). They provide strong evidence of a structural break in all 10 sample countries. The null that no break occurred during the sample period, is rejected in all cases. If one assumes that only a one major shift in wage inflation occurred in each country during the interwar period, the *SupF* statistics can be used to infer on the most likely break date. The last line in table 2 gives the inferred break dates. It indicates that structural breaks in Poland, Germany, Japan, Austria, and France occurred between February 1930 and February 1931 (countries in chronological order). The structural break in US wage formation occurred most likely in August 1933, while in the UK and in Italy it is dated to April 1935. It is difficult to use this dating for making inferences on causality between wages, prices, and output. Yet, the results of the Quandt test clearly show that in many countries the way wages were set, changed fundamentally at the beginning of the depression.

[15] The Chow test is based on an F-test of the null hypothesis that the means of two subsets of the time series are equal. They are estimated by fitting an ordinary LS model to the sub-sample before and after the potential change point.



(a) United States **(b)** United Kingdom

(c) Japan **(d)** Germany

(e) Poland **(f)** Austria

(g) France **(h)** Italy

Figure 2: Plots of F-test statistics for a single structural break in nominal wage formation. The red line indicates the boundary of the supF test. The dashed line indicates the mean of the F statistics.

The Quandt test is used to investigate single structural breaks; it is not suited to testing for multiple structural breaks in the mean. However, both the Quandt statistic (figure 2) and yoy wage growth (cf. figure 2 in appendix E) suggest considering the possibility of more than one structural break in wage formation during the interwar period. In most countries, wage growth shows a 'V' or 'Λ' pattern. It is not well captured by models of wage formation that allow for a single shift only (cf. figure 2). Since the business cycle dating in chapter 4 finds extraordinary economic instability between the wars, it would hardly be surprising if the DGP of aggregate nominal wages had been subject to multiple breaks despite the short sample period.

In order to deal with multiple structural breaks at an unknown breakdate Bai and Perron (1998, 2003) propose the following multiple linear regression with m breaks representing the structural change model (in its most general form)

$$w_t = x_t'\beta + z_t'\delta_j + \epsilon_t, \quad j = 1, \ldots, m \tag{3.3}$$

Segment j is defined as $t = T_{j-1} + 1, T_{j-1} + 2, \ldots, T_j$, for $j = 1, \ldots, m + 1$. Consequently, the number of segments is determined by the actual number of break points m plus one. During each segment j the coefficient of mean μ_j remains stable. The error term ϵ_t does not need to be iid., i.e. its variance is allowed to differ between segments j.[16] In order to date m jointly, a triangular matrix of sums of squared residuals is constructed based on all possible segments.[17] Let $SSR(Tm, t)$ be the sum of squared residuals associated with the optimal partition containing m breaks using t observations. T is the total number of observations. The optimal partition solves the following recursive problem (for details cf. Bai and Perron 2003):

$$SSR(\{T_{m,T}\}) = \min_{mh \leq j \leq T-h} [SSR(\{T_{m-1,j}\}) + SSR(j+1, T)] \tag{3.4}$$

h is some minimum distance imposed between break points. Bai and Perron's (2003) approach is used to test the null hypothesis of no structural breaks in the series against the alternative hypothesis of an unknown number of breaks up to a maximum of M. The approach determines the exact number and location of breaks by minimizing the sum of squared residuals from the linear model.[18]

As argued above, comparative analysis is the prerequisite for obtaining a rigidity estimator $\hat{\rho}_i$ that is comparable across countries. This does not mean applying an identical model to each countries' data, but to developing a consistent approach. To this end, the following algorithm is applied to each wage series in the sample.

[16]The only assumption necessary is that the breaks in variances and coefficients occur at the same time.

[17]This matrix is of size $T(T+1)/2$, where T is the number of observations.

[18]Levin and Piger (2004) apply a partial structural change model based on this approach for measuring inflation persistence.

(1) Stepwise selection of an appropriate lag length k for (3.2), i.e. the AR(k) model of wage growth, starting with $K = 12$. The model fit is evaluated using AIC.
(2) Based on the appropriate AR(k), persistence is estimated as $\sum_{k=1}^{K} \alpha_k$ regardless of potential structural breaks. This yields the unconditional persistence parameter of nominal wages $\hat{\rho}_i$.
(3) Presence of multiple structural breaks in the intercept is examined using the approach of Bai and Perron (2003). This yields the number of breaks $m = 1, \ldots, M$. The maximum number of breaks M is restricted to three due to the length of the time series. As a cross-check, an alternative restriction is tested, which leaves M unrestricted but requires that any partition j must at least consist of 15% of the number of observations.
(4) Data are conditioned on the structural breaks identified beforehand, following Bataa et al. (2007).
(5) Steps (1) and (2) are repeated based on the conditional data, which yields a conditional AR(k) model and conditional rigidity parameter $\hat{\rho}^*$.

4. Results

At first, the high frequency data are used to review the ES (1985, figure 2) regression, from which the wage rigidity hypothesis has initially been inferred. As discussed in the literature review, their finding has been criticized for various reasons. Plots of time series data in section 3 demonstrate as well that nominal wages were way more flexible than annual data suggest. Not even the weaker condition of downward nominal rigidity is justified since negative yoy growth rates in nominal wages matched or even exceeded positive growth rates in Austria, Germany, Italy, Japan, and the US during the 1920s boom and the 1930s recovery (cf. table 1, rows 4-7). Despite this high degree of flexibility, wage cuts remained insufficient to balance out the absolute fall of the price level (cf. figure of price levels and real wages in appendix E).

The sample I have put together allows me to examine the negative relationship between IPM and real wages separately for each country. Monthly data of WPI, nominal wages, and IPM, provide sufficient observations to compute cross-correlations between yoy percent differences of output and real wages. Table 3 presents strong evidence of an inverse relationship between real wages and output: In 9 out of 10 countries, contemporaneous correlation is negative and the coefficient is statistically significantly different from zero. The correlograms (figure 3 in appendix E) show that correlation coefficients are highest in the interval of lag [0; -4], i.e. changes in real wages either coincided with changes in output or slightly preceded them.[19] The findings confirm that real wages increased either contemporaneously with the decrease in output or slightly beforehand.

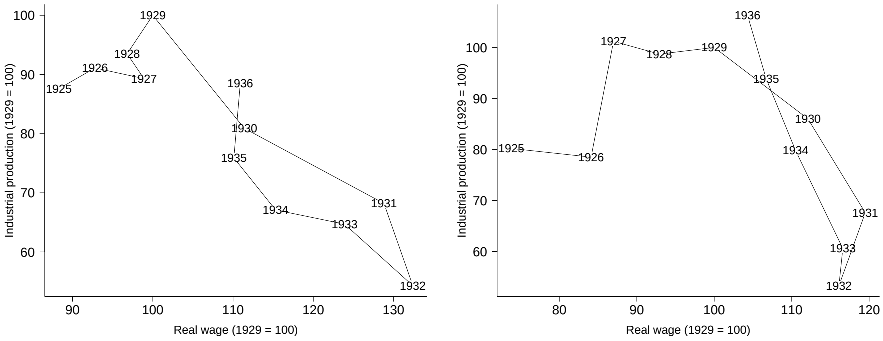
Table 3: Negative correlation between changes in real wages and in industrial production

	AUT	BE	CSR	FRA	GER	ITA	JP	PL	UK	US
Coefficient	0.15	-0.33	-0.55	-0.39	-0.50	-0.65	-0.86	-0.21	-0.26	-0.69
H_0 rejected	NO	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: One-sided test for negative contemporaneous cross correlation of yoy changes based on Pearson's product moment correlation coefficient. The H_0 that the variables are uncorrelated, is evaluated at the 5% significance level.

However, phase plots of changes in IPM against changes in real wages show that the expected inverse relationship does not hold during the entire interwar period: It is present only from 1929 and disappears in several countries after the end of the Great Depression (figure 3, for the remaining countries see figure 10 in appendix E). During the 1920s, by contrast, IPM was uncorrelated or even positively correlated with real wages. This pattern would be evidence of downward nominal wage rigidity, if the negative relationship was present during deflation of prices and absent during inflation of prices. Yet, IPM was not negatively related to real wages before 1929, even if countries experienced deflation at that time.

[19] Lag k is used in the calculation as follows $cor\left\{\left(\frac{w}{p}\right)_{t+k}, q_t\right\}$.



(a) United States **(b)** Germany

Figure 3: Phase plots of real wages versus IPM (1929=100)

On the one hand, the evidence of an inverse relationship between IPM and real wages I present is more robust than the findings in ES (1985) and corroborates the sticky wage hypothesis. On the other hand, my findings raise the question, why wage rigidity was effective in some years, but not in others. How can the findings be reconciled with the great flexibility of nominal wages discussed further above? What determined the fall of wages in 1929/1930, if it was not to follow the decrease in prices, which started years before? And, if wages were able to fall as strongly as prices, why did wages not fall more strongly to keep real wages more or less stable?

4.1. Evidence of structural breaks. As discussed in the previous section, structural breaks can strongly bias estimation of autoregressive (AR) models, such as equation (3.2). The economic intuition of a structural break in wage formation is that the process of wage setting is subject to fundamental, long term change. Time periods with high and low wage inflation represent one potential source of such breaks (cf. Fehr and Goette 2005): Transition from high to low wage inflation, and vice versa, is reflected as structural break in the data. The model captures the prevailing state in the intercept, i.e. as average wage growth rate, or trend component, respectively. As such, structural breaks also reflect the effectiveness of policy. A structural break in US wage formation in 1929 would put doubt of the effectiveness of President Hoover's attempt to keep industry leaders from cutting wages. The evidence of successful intervention would be particularly strong, if structural breaks took place in other countries at that time, but not in the US.

In order to simultaneously test for multiple structural breaks in the intercept, the approach of Bai and Perron (BP) (1998, 2003) is applied. In its most general form, this test looks out for breaks in both intercept and lag parameters of the AR model, i.e. in wage inflation and in the speed of adjustment, respectively. Specifically, I examine the presence of structural changes in the intercept $w_t = \mu_j + \epsilon_t$, but not in AR coefficients. It is a pure 'structural change' version of (3.3): z_t has dimension (1×1) and x_t has dimension (0×1). As my sample covers 11 years only, I restrict m to three and thereby the number of segments to $j \leq 4$. In order to derive the break points, equation (3.4) is solved, testing for $m \leq 3$ whether the null hypothesis

$$H_0\colon \mu_j = \mu_1, \quad (j = 2, \ , \dots \ , m+1) \tag{4.1}$$

can be rejected at the 5% significance level.[20] Table 4 reports the resulting break date estimates as well as upper and lower bounds of the respective 90% confidence interval. Corresponding plots of breaks in wage formation are depicted in figure 12 in appendix E.

The test yields that wage formation in every sample country was subject to multiple shifts in the intercept between 1926 and 1936 (neglecting Belgium). The sequence of structural breaks followed a similar course across countries: The first series of breaks, which shifted the intercept of wage growth models downward, took place between February 1930 and August 1931 (break dates highlighted by bold figures).[21] In most countries, this shift turned wage inflation strongly negative, i.e. one observes wage deflation. In Germany, for instance, average wage growth dropped from a rate of 6.0% to -2.2%; the corresponding break is dated to April 1930. In autumn 1931, wage inflation dropped further to -12.3%. The contraction in nominal wages continued at this average rate up until April 1933, when the intercept jumped to -0.3% finally stabilizing wage inflation.[22] Also in other sample countries, wage inflation shifted upward in 1933 and 1934 (cf. table 4, dates in italics). However, these shifts did not incur strong growth of nominal wages after the Great Depression. There is no country in which wages adjusted strongly enough to make up the preceding decline. In many countries, average wage growth even remained slighly negative until 1936 at least.

In contrast to Eichengreen (1992), my findings suggest that the self-equilibrating forces of labor markets remained largely active during the Great Depression. Yet, the results also highlight that the US case deviated from the international pattern: The downward shift in US wage inflation took place first in August 1931, i.e. a couple of months later than elsewhere. Even though this lag supports Cole's hypothesis of the effect of Hoover's interventionist policy, it implies that this policy could only delay but not prevent downward adjustment of nominal wages. A second deviation is that wage inflation grew by 9.7% for almost two consecutive years after the depression, implied by the estimate $\hat{\mu}_{US,3}$. There is no other country with such a high wage inflation between 1933 and 1936. The findings suggest that the NIRA as well as union policy in the US were effective in pushing up nominal wage inflation in 1933 and thereafter.

[20]Computations were performed in GNU R using the 'strucchange' package, specifically methods related to *breakpoints()* (cf. Zeileis et al. 2002, 2003).

[21]Selected either if growth rates were pushed from positive to negative or, being negative already, to more negative growth.

[22]It is important to note that wage growth in Germany after 1933 must be evaluated carefully. Beginning in 1934, wage growth remained limited and displays almost no variance, which probably reflects the increase of state control after the Nazi *Machtergreifung* in January 1933.

Table 4: Testing for multiple shifts in mean wage growth

	AUT	BE	CSR	FRA	GER	ITA	JP	PL	UK	US
Break date estimates and 90% confidence intervals										
No. breaks, m	3	1	2	2	3	3	3	3	3	3
	1929(12)	1935(2)	NA	1929(5)	1930(2)	1930(12)	1930(5)	1929(5)	1930(5)	1931(7)
m_1	**1930(2)**	*1935(4)*	**1931(3)**	1929(9)	**1930(4)**	**1931(1)**	**1930(6)**	1929(6)	**1930(6)**	**1931(8)**
	1930(4)	1935(5)	NA	1929(11)	1930(5)	1931(3)	1930(7)	1929(8)	1930(10)	1931(9)
	1931(8)		1933(12)	1930(12)	1931(7)	1931(12)	1931(12)	1930(10)	1933(9)	1933(6)
m_2	1931(10)		1934(3)	**1931(1)**	1931(10)	1932(1)	1932(1)	**1930(11)**	*1933(10)*	*1933(7)*
	1931(12)		1934(4)	1931(2)	1931(11)	1932(2)	1932(5)	1931(1)	1933(11)	1933(8)
	1933(8)				1933(3)	1935(3)	1934(7)	1934(5)	1935(5)	1934(12)
m_3	*1933(12)*				*1933(4)*	*1935(5)*	*1934(9)*	*1934(6)*	1935(6)	1935(1)
	1934(2)				1933(6)	1935(6)	1934(10)	1934(7)	1935(7)	1935(7)
Coefficients of intercept during subperiod j										
$\hat{\mu}_1$	4.6	-5.5	2.4	3.7	6.4	-1.7	-0.5	12.9	-0.4	0.2
$\hat{\mu}_2$	-6.9	7.7	-0.2	9.8	-2.2	-9.2	-4.7	2.1	-1.5	-9.6
$\hat{\mu}_3$	-16.4		-2.2	-2.6	-12.3	-2.7	-3.2	-7.1	0.1	9.7
$\hat{\mu}_4$	-6.1				-0.3	3.6	-1.4	-1.5	2.3	2.5

Notes: Breaks dates are given as YEAR(MONTH). Confidence intervals of break dates are constructed following Bai and Perron (2003, section 4.2) and allow for heterogeneous errors. Bold dates mark the beginning of (strong) wage deflation, italics the end of (strong) wage deflation.

Structural breaks in wage formation are not a well-established issue in the literature, which raises the question how they need to be interpreted. The international coincidence of structural breaks in wage formation shown above, suggests that some factor fundamentally changed wage setting across industrial sectors in all sample countries. I consider three possibilities: Changes in the monetary regime, i.e. in the interwar Gold Standard, political intervention in wage negotiation, such as in the US, and real factors, meaning the business cycle. I exclude the possibility that structural breaks were simply caused by the duration of general union wage contracts, because the breaks do not occur in a regular interval.[23]

The gold standard theory argues that nominal wages followed prices in the 1930s, but that deflation in wages lagged behind deflation in prices due to nominal rigidities. If this was reflected in structural breaks, one would expect that structural breaks in wage inflation systematically followed changes in price inflation. This includes changes in the monetary regime, in particular. However, changes in WPIs do not match the sequence of structural breaks. Major changes did neither coincide in the cross-section nor country-wise in the time domain. Price deflation increasingly diverged among countries beginning in 1925. This divergence is not mirrored in the state of wage inflation either: The first downward shift in the sample took place first in June 1929, which implies that DGPs of nominal wages were stable at least until then. Contrary to annual observations, monthly data reveal that the evolution of interwar wages and prices did not simply differ due their respective ability to decrease. Instead, the pattern of their decrease differed: Prices were falling throughout the entire sample period, whereas wages fell only during a certain period of time (figure 3 in appendix E). Moreover, one would expect to observe a change in wage setting across countries following the dissolution of the gold standard due to the shock to expectations about price inflation. Yet, there is no evidence that wage setting was fundamentally changed in countries that went off gold after 1931.

Political intervention in wage negotiations, as discussed by Rose and Ohanian (2009), represents an alternative reason for structural breaks. Politicians were interfering in wage negotiations not only in the US but also elsewhere at certain times during the interwar period. However, I was not able to find anecdotal evidence of policies, neither coordinated or uncoordinated nor formal or informal, that would imply fundamental changes in wage formation with similar effect across countries at the same point in time. On the contrary, political decision makers were guided by "orthodoxy" and hesitated to take action for a long time (cf. Eichengreen and Hatton 1988, Gourevitch 1984). A further alternative is that breaks reflected a reaction of workers triggered by unionization or mounting unemployment. Monthly data on unemployment is available for most countries during the interwar period, whereas unionization data is only available on an annual basis.[24] One would expect

[23]In addition, the variance of growth rates during subperiods remains high, whereas one would expect low variances, if most contracts were agreed on at certain intervals.

[24]The data is plotted in figure 6 and figure 7 in appendix E. The change in unemployment is calculated here as the yoy change in absolute unemployment figures, because data on employment, needed to calculate unemployment rates, is not available.

that either a strong decrease in the unionization rate or a strong increase in unemployment could have lead to a downward shift in nominal wage inflation. Although unionization increased during the crisis, there is no dramatic change in unionization rates such that it would rationalize structural breaks in wage formation.[25] The evidence concerning changes in unemployment is less clear. One observes that unemployment grew at an increasing rate over the course of the year 1930. The growth rates peaked in mid-1931, and hence after the shift to wage deflation. However, a similar or even stronger increase in unemployment took place in many sample countries during the 1920s as well, but did not result in structural breaks in wage formation anywhere.

The correlation of structural breaks in wage formation across countries appears, in fact, to be linked to the synchronization of business cycles during the interwar period. Fundamental changes in expectations about the real economy are a potential explanation for this relationship. Figure 4 shows breaks in wage formation plotted against the business cycle dates that were established in chapter 4 (Plots for the remaining countries are given in figure 14 in appendix E). In most countries, the first wave of shifts to negative wage growth (bold figures) occurred right after the beginning of the depression. What is more, only the exceptional business cycle linked to the Great Depression corresponded to shifts in wage inflation. The coincidence is most obvious during the beginning of the depression. Yet, also the upward shifts in wages, which accounts for the 'V' pattern, paralleled the upturn of IPM. Averages indicate how long shifts in wage formation were lagging the business cycle, particularly at the depression's end.[26] The downward shift in wage inflation lagged behind the business cycle downturn on average by 8 months, with a standard deviation of 3 months. The lag of the upward shift amounted on average to 10 months, with a standard deviation of 4 months. These findings suggest a relationship between the real economy and nominal wage formation for all countries but the US. Wage formation in the US deviated from these stylized facts with the downward shift lagging behind the business cycle by 22 months.

In order to prove that this pattern reflected fundamental changes in the DGP and did not only mirror the business cycle, I examine whether further business cycle induced structural breaks in nominal wages existed. Specifically, the robustness of the results is checked by dropping the explicit restriction of m. Instead of m, subperiods j are required to contain a minimum number of 30 observations. If structural breaks in nominal wages did only fluctuate due to upswing and downturn of business cycles, one would expect that these breaks occurred more frequently due to the higher number of recessions. The estimation yields that the number, the dating, and the extent of breaks do not substantially differ from the results above if the restriction of m is relaxed.[27] In addition, the breaks do neither exhibit a double dip pattern, nor do they coincide with recessions that took place in some

[25]It is improbable that strong changes in unionization remain hidden in annual figures.

[26]The downward shifts in wages lag the business cycle by less than 12 months in Poland, the UK, Germany, Austria, and Japan and by more than 20 months in Czechoslovakia and the US. The upward shift occurs with a lag of up to 14 months in Germany, the US, Austria, the UK, and Poland, but after more than 20 months in Czechoslovakia and Japan.

[27]For details see table 2 in appendix E.

(a) United States **(b)** Germany

Figure 4: Structural breaks in wage formation (growth rate in %) versus business cycle turning points during the Great Depression (grey area).

countries in 1926 and 1935. Again, these findings are robust, because the dating of structural breaks and of business cycles are derived from different analytical approaches applied to different data. Taken together, the findings suggest that the structural breaks, identified by the Bai and Perron approach, reflected a fundamental change in DGPs of wage formation and were independent from the business cycle. Nevertheless, the sequence of break dates coincided with the depression in output.

The idea that these two extraordinary events —the depression of 1929-1932 as well as structural breaks in wage formation— coincided largely unaffected by money and prices is difficult to reconcile with the AS-AD rationale, which is behind ES (1985) and BC (1996). The latter assumes that either nominal wages were determined by prices if flexible, or mainly determined by themselves if sticky. However, the findings above suggest that it is appropriate to think of wage formation to be governed by a 'state' of the DGP among other factors. The massive change in the DGP of output during the Great Depression was then directly affecting wage setting through shifting the DGP into another state.

The most plausible interpretation of the cascade of shifts into wage deflation is to regard them as internal devaluations similar to a spiral of external devaluations induced by beggar-thy-neighbor policies. This downward spiral itself increased the pressure on prices and worsened negative shocks to demand. This mechanism would also explain why wage growth turned negative that quickly in many countries. After all, the exceptional extent of the crisis was hardly foreseeable at the turn of the year 1929 to 1930. This implies that the coincidence of structural breaks resulted to some extent from a 'spill-over' effect: The internal devaluation in one country enforced changes in wage setting in other countries, while the Gold Standard prevented explicit devaluation. The fact that structural breaks in the US occurred much later than in other countries is evidence in favor of Ohanian (2009). Yet, the idea of spill-over effects cannot explain the observation that nominal wage inflation in the US jumped so strongly at the end of the depression. One can only speculate that the US either relied on its large internal market and import restrictions to protect it from the loss of competitiveness or that it expected other countries to follow.

However, more and more European governments became authoritarian, which put them in a position to impede increases in industry nominal wages.

4.2. Rigidity estimates. Structural breaks in the DGP cause persistence estimates to be upward biased. In order to obtain unbiased estimates of the rigidity measure ρ, the wage data need to be conditioned on the breaks and level shifts presented in the previous section (cf. table 4). To this end, each time series i was split at the break dates and the unconditional mean $\hat{\mu}_{i,j}$ was subtracted from the data of the corresponding sub-period j (cf. Bataa et al. 2007, Ng and Vogelsang 2002).[28] Since structural breaks in the intercept would cause the data to appear non-stationary, I applied the augmented Dickey-Fuller (ADF) test in order to verify that the data were successfully conditioned on the structural breaks.[29] Prior to conditioning, the ADF test yields the expected result: The null hypothesis that wage growth followed a unit root, can be rejected in only one out of ten countries (at the 10% significance level). If the ADF test is applied to the conditional data, the null is rejected in eight out of ten countries. I consider this outcome to be reasonable evidence that the conditioning was effective.

The estimation of rigidities from the conditional data yields the unbiased estimate $\hat{\rho}_i^*$ for each country i. Additionally, the estimation was performed using the unconditional, original data in order to obtain "naïve" estimates $\hat{\rho}_i$, which are used to evaluate the extent of the bias. In order to obtain unbiased estimates, it is not sufficient to subtract level shifts from the time series, because the estimation of standard errors is biased as well. Confidence intervals, which are constructed on the basis of an asymptotic normal distribution for ρ, tend to be incorrect (cf. Levin and Piger 2004). In order to obtain correct confidence intervals for $\hat{\rho}_i^*$, a parametric bootstrap approach was applied.[30]

Table 5 presents the conditional, i.e. bias-corrected, parameter estimates of ρ, specifically the 0.05, 0.50, and 0.95 quantiles.[31] The median of parameter estimates ranges from 0.42 in Italy to 0.78 in Czechoslovakia. The unweighted average of the parameter estimates $\bar{\hat{\rho}}^* = \frac{1}{N}\sum_{i=1}^{N} \hat{\rho}_i^*$ equals 0.63. These figures provide strong evidence of nominal inertia: Estimates of wage rigidity are statistically significantly different from zero for all countries, even if structural breaks in intercepts are removed from the data. Changes in wage growth were in fact persistent during the interwar period, which is evidence in favor of the wage rigidity hypothesis. Yet, the results show that wages were far from being perfectly rigid, since none of the

[28]The resulting sample of conditional growth rates is depicted in figure 13, page 256.

[29]In the presence of a unit root, wage growth is determined by past values and the effect of shocks does not vanish over time. The ADF test evaluates H_0 of the presence of a unit root vs. H_1 that the time series is stationary. A series exhibits characteristics of a unit root process if the root of the AR process' characteristic equation is statistically insignificantly different from one. In addition, an AR(1) process shows the close relation with ρ, because ρ is equal to the root of the characteristic equation of the process. Hence, $H_0\colon \delta = 0$, where $\delta = \rho + 1$.

[30]Levin and Piger (2004) apply a grid bootstrap procedure in their analysis of inflation persistence.

[31]I relied on Akaike's information criterion (AIC) in order to select lag order K. A sensitivity analysis applying Schwartz' information criterion (SIC) instead of AIC leaded to more parsimonious AR models, as expected. Yet, estimates $\hat{\rho}_i$ were almost identical.

confidence intervals includes unity. Confidence intervals given in table 5, can also be used to make inferences about the equality of coefficients. The estimates support the assumption that the level of wage rigidity was similar internationally. Equality of parameters cannot be rejected for most combinations of countries. Even in Italy and France, where the level of rigidity was by a third lower than in other countries, the deviation is statistically significant only with respect to Czechoslovakia and Poland.

Table 5: Conditional rigidity estimates

	Median estimate	$Perc_{lower}$	$Perc_{upper}$
CSR	0.78	0.71	0.83
PL	0.77	0.63	0.85
JP	0.71	0.56	0.80
UK	0.71	0.57	0.81
GER	0.69	0.54	0.79
US	0.69	0.53	0.80
BE	0.47	0.12	0.69
FRA	0.45	0.18	0.61
ITA	0.42	0.19	0.60

Notes: Rigidity estimates based on conditional yoy growth rate of nominal wages. Median and confidence bands (0.05; 0.95) obtained from a parametric bootstrap with 300 replications.

Which degree of wage rigidity is implied by the coefficients, i.e. should it be regarded as high or low? A direct comparison of $\bar{\hat{\rho}}^*$ with the estimate of λ_w by BC (1996), the closest reference, is not meaningful, because both data and estimation methods differ strongly.[32] I refer to Levin and Piger's (2004) study of postwar inflation persistence in 12 countries, since I am not aware of studies that present estimates of wage rigidity to which $\bar{\hat{\rho}}^*$ is directly comparable. They provide estimates that are derived from a similar approach and based on monthly data as well. Compared with their results, the level of my estimates appear plausible: The median of estimates is ranging from −0.24 to 1.05, the unweighted average of parameters is 0.52. The variance of estimates is greater than in my analysis, which stresses the above finding that differences in wage stickiness between countries were small.[33] Levin and Piger regard a persistence parameter as "low" if $\hat{\rho}_i^* \leq 0.8$. Evaluated using this threshold, my estimates $\hat{\rho}_i^*$ imply that nominal wages can be regarded as quite flexible during the interwar period, all the more so because prices are generally expected to be more flexible than wages.

In order to understand how such moderate rigidity affected nominal wages, I generated univariate impulse responses of each AR model of wage formation. Figure 5 depicts the effect of a shock of one standard deviation to nominal wages over time. It shows that the deviation due to the shock lasted for much less than

[32] λ_w in BC (1996) is a pooled estimate, based on annual data, and the assumption that λ_w is equal across countries.

[33] In 5 out of 12 countries, the confidence interval includes 1 implying that $\hat{\rho}_i^*$ was statistically insignificantly different from 1. In 3 out of the remaining 7 countries the confidence interval includes 0, implying perfect flexibility (Levin and Piger 2004, table 5).

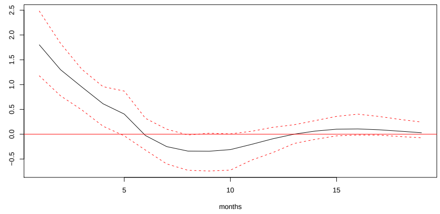
a year. Evaluated by the lower confidence interval, the effect vanished in the US, the UK and Germany within 5 months and within 3 – 8 months in the rest of the sample. By contrast, the estimate presented in BC (1996) suggests that shocks substantially affected wages still in the subsequent year.[34]

The much higher rigidity implied by BC's (1996) estimate of λ_w could be due to two kinds of bias: Either an upward bias caused by aggregation in the time domain (cf. Taylor 2001), or a bias caused by the fact that BC (1996) do not consider breaks in wage formation. In order to examine the potential extent of the latter, I re-estimated rigidity parameters from data on wage growth, which were not conditioned on structural breaks. Table 5 presents the resulting unconditional parameter estimates of ρ. All estimates are close to unity in all sample countries, which implies a much higher level of rigidity in wage formation than the conditional data: The unweighted average of estimates $\bar{\hat{\rho}}$ equals 0.91, which would imply that the variance in nominal wages was almost exclusively determined by past wages.

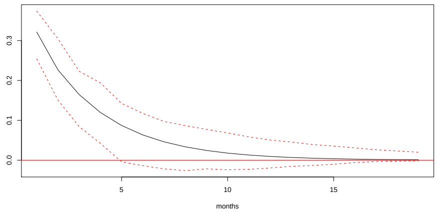
The unconditional parameter estimates demonstrate that a considerable part of the bias is due to structural breaks in the data generating process. Each bias-corrected parameter $\hat{\rho}_i^*$ is statistically significantly lower than its unconditional counterpart $\hat{\rho}_i$, which demonstrates that $\hat{\rho}_i$ is strongly upward biased. This finding implies that the few existing estimates of (real or nominal) wage stickiness are likely to suffer from this bias, unless they account for the possibility of structural changes in wage formation. Depending on the extent of the bias, it is possible that wages were not particularly sticky even in those countries, for which studies have estimated or suggested $\hat{\rho}$ to be close to unity. Even more so, as the least-squares estimator of the persistence parameter tends to be biased downward, particularly if ρ approaches unity (cf. Levin and Piger 2004). These results are somewhat problematic for the Gold Standard Theory, which would fit much better with the high degree of wage stickiness suggested by the unconditional data. The moderate degree of rigidity is much less suited to explain the protracted real impact of deflation beginning in 1930. The comparison of conditional and unconditional parameter estimates once again highlights the necessity of using monthly data in any analysis of the interwar period, because data of lower frequency make it difficult to detect and control such bias.

Eichengreen (1992) does not use an equational model to reach his conclusions. Therefore, I cannot re-parameterize his model and compare the outcome to his results. Similarly, the data necessary to re-estimate the model of BC (1996) are not all available at monthly frequency. Thus, I assess implications of the Gold Standard Theory using a structural vector autoregression (VAR). It includes the baseline variables of Eichengreen's and BC's models, i.e. output, prices, and nominal wages, each depending on its own past growth rates as well as on the other variables's current and past growth rates (referring to equation (5) and (6) in BC 1996). The estimation is based on the conditional data derived above. The results are consistent with the findings of the univariate IRF (cf. figure 15). It is striking that the output of the VAR is nevertheless broadly consistent with predictions of the

[34]Depending on the empirical model, estimates of λ_w by BC (1996) are in the range of 0.2 to 0.6 and estimates of λ_p are ranging from 0.2 to 0.4.



(a) United States



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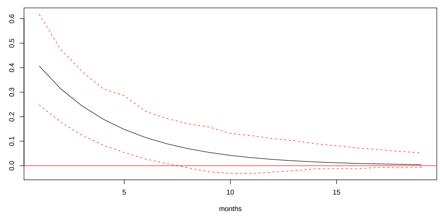

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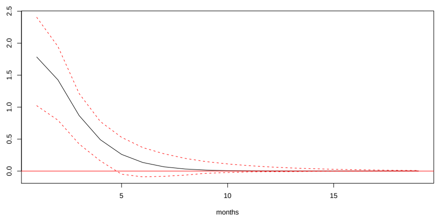

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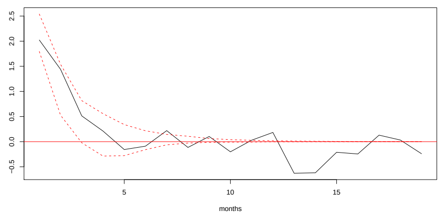

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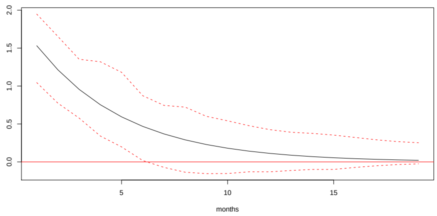
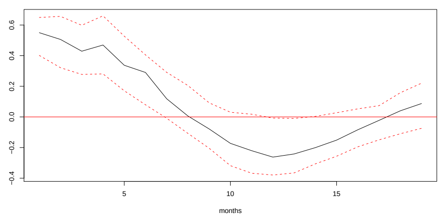
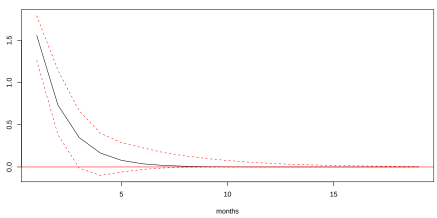


(f) Poland



(g) Czechoslovakia



(h) Italy

Figure 5: Impulse response functions showing speed of adjustment of nominal wages. Red dashed lines indicate upper and lower 95% confidence intervall.

GST: I find that deflationary shocks in prices led to contractions in output in the US, UK, Germany, and Japan. Price shocks had little or no effect on conditional nominal wages in most countries. The impact of prices on wage inflation was

Table 6: Unconditional rigidity estimates

	Median estimate	$Perc_{lower}$	$Perc_{upper}$
GER	0.95	0.89	0.98
JP	0.95	0.89	0.98
PL	0.94	0.87	0.97
US	0.94	0.88	0.97
UK	0.93	0.84	0.97
CSR	0.91	0.85	0.95
ITA	0.90	0.79	0.95
FRA	0.87	0.68	0.95
AUT	0.85	0.63	0.94
BE	0.81	0.59	0.92

Notes: Rigidity estimates based on unconditional yoy growth rate of nominal wages. Median and confidence bands (0.05; 0.95) obtained from a parametric bootstrap with 300 replications.

limited in most countries.[35] In spite of lower than expected nominal wage rigidity, deflation triggered contractions in output and reflation was a necessary condition for economic recovery. The intermediate degree of nominal wage rigidity appears to have been sufficient to keep nominal wages from adapting quickly to price changes. An explanation to this puzzle could be that temporal correlation in the international deflation of wages accelerated the deflation of prices. It remains a task for further research to understand if wage reduction itself propelled deflation at that time.

[35]Only in Germany, the VAR indicates that shocks to prices were highly persistent and strongly affected nominal wages.

5. Sensitivity check: Stability of AR parameters

Throughout the above analysis, I have assumed that the AR coefficients of each wage formation model remained stable over the sample period. To ensure that the results presented above are not sensitive to this assumption, I analyze the evidence regarding structural breaks in the dynamic behavior of wage formation.

To this end, I modify the structural change model (3.3). This time, z_t contains lagged dependent variables, where the number of lags k is determined by an appropriate AR model selected using AIC. Two restrictions are imposed on the model: First, it is assumed that breaks in coefficients of all explanatory variables occur at the same time. Second, the lag length of the AR model is assumed to be identical across subperiods j, i.e. it does not change due to the structural break in coefficients. Both restrictions are necessary due to the limited number of degrees of freedom.

The results from the estimation of the extended version of (3.3) are presented in table 7. Probability values of the respective SupF-test show that the null of a structural break in all coefficients of the AR process cannot be rejected at the 5% level of statistical significance for Japan, Poland, the US, and Germany. For all the remaining countries, the null hypothesis of a structural break is rejected. In order to obtain m_i, I compare wage formation models with $M = 0, 1$ using the BIC.[36] BIC favors a model with no breaks in case of Japan and Poland, and a model containing one structural break in case of the US and Germany. The results show that parameter estimates in the previous section are generally robust. It is sufficient for most countries to remove breaks in the intercept, since a model with no breaks in coefficients of lagged dependent variables is preferred in all but two countries.

Table 7: Sensitivity Check Allowing for Breaks in all Variables

	JP	PL	US	UK	BE	GER	AUT	CSR	ITA
Lag order	6	12	7	11	1	5	12	4	1
F-test, pval	0.01	<0.01	<0.01	0.44	0.16	<0.01	0.10	0.07	0.14
m	0	0	1	0	0	1	0	na	0
breakdate	—	—	1934(7)	—	—	1932(12)	—	na	—
$\hat{\rho}_1^*$			0.94			0.99			
$\hat{\rho}_2^*$			0.56			0.33			

Notes: Number of break points m determined by BIC evaluating $M = 0, \ldots, 3$. For Czechoslovakia no structural change model could be obtained.

The robustness check suggest that low rigidity parameter estimates for Germany and the US presented in section 4 are potentially misleading. They represent some sort of average over subperiods with differences in wage formation due to structural changes. The break in the US is dated to July 1934. In Germany, it is likely to have occurred in December 1932. The difference in estimates of the rigidity

[36] In case of Czechoslovakia, equation (3.4) could not be solved, because the Cholesky decomposition of the triangular matrix of SSRs failed.

parameter before and after the break point is large. Both in the US and in Germany, the persistence parameter $\hat{\rho}_1^*$, which measures rigidity before the structural break, is close or equal to unity. $\hat{\rho}_2^*$, which represents rigidity following the break point, is much lower. The interpretation of this change in parameters is difficult, because the model is technically not able to detect more than one break. However, as the structural break in all AR coefficients is dated at the end of the Great Depression, the high level of rigidity prior to this break point is likely due to further structural breaks at the beginning of the depression.

It is important to note that in both countries, the US and Germany, the change in wage formation coincided with crucial changes in labor market policies: In Germany, the Nazi party took power in January 1933. It instantaneously destroyed free and independent unions replacing them by a single government-controlled union, *Deutsche Arbeitsfront* in the course of the *Gleichschaltung*. In May 1933, a new law, the *Treuhändergesetz*, abolished state-independent wage negotiations, instead nominal wages were effectively fixed to the level in January 1933 (cf. Englberger 1995, James 1988). In the US, the structural break coincided with the introduction of the New Deal. Pressure on companies not to cut wages had already been exerted under president Hoover, but the National Industrial Recovery Act (NIRA) restricted wages from falling lower than a certain threshold and the National Labor Relations Act in 1935 further strengthened union bargaining power (Cole and Ohanian 1983).

At least for the US case though, the finding that wage formation became more flexible at the end of the crisis, would explain the different response of the economy to price changes during and after the depression. After all, the US was among the countries suffering most during the depression, not the least because recovery failed after 1933. One could hypothesize that prior to 1933, wage formation was relatively inflexible, leaving price deflation to unfold its full negative dynamics. In contrast to most other countries, the US reflated strongly after leaving gold in 1933. However, nominal wage formation became more flexible at the same time, keeping inflation from unfolding its full effect in the recovery. By contrast, Japan was benefiting much more from reflation after the depression, because wage rigidity remained high.

6. Conclusion

The role of nominal wages constitutes one major puzzle of the Gold Standard Theory of the 1930s Great Depression: Nominal wages must have been both flexible in order to explain time series observations and, at the same time, inflexible in order to explain persistent non-neutrality of money. My study suggests that this puzzle can be partly resolved by considering two components of nominal wage growth separately: short to medium-term inertia, which is denoted in this study as 'stickiness' or 'rigidity', and the long term trend, here referred to as wage inflation.

My findings indicate that the massive decline of wages during the crisis was driven mainly by significant drops in wage inflation in almost all sample countries. The level of wage stickiness, on the contrary, remained stable through the entire sample period. Shifts in wage inflation are reflected in the data as structural breaks in the intercept of the respective time series model. Some evidence of structural changes in rigidity is presented as well, but this phenomenon is observed only in the US and Germany and probably due to extraordinary changes in economic policy. By accounting for long term and short term components of nominal wage formation separately, I show that the contradiction between strong decreases in nominal wages and the insufficient adaption of wages to deflation can be explained by the coincidence of nominal rigidities and structural breaks in wage inflation.

Structural changes in wage inflation are particularly interesting as they are unknown from the postwar period. Breaks reflect the shift from wage inflation to wage deflation that occurred in most countries at the beginning of the crisis — and, vice versa, back to wage inflation at the end of the crisis. The classification and explanation of breaks in this study is based on coincidence in time, because an econometric analysis is not possible. The shifts itself can be used to analyze the difference between the inflationary and the deflationary state. Two patterns of structural breaks can be identified in nominal wages: The first pattern is quite regular and shows similarities with inflection points of the business cycle. The other pattern is rather irregular, which I take as evidence that these breaks followed political events.

Wage inflation became strongly negative at the beginning of the crisis. Yet, structural shifts exerted a one-time effect on growth rates of nominal wages only. Wage rigidity, on the contrary, was not affected. Although inertia of wages was as low during the interwar period as during the postwar period, it proved to be too high during the exceptional economic downturn: When the downward spiral in prices began to accelerate at the beginning of the crisis, wage stickiness represented a weak but binding restriction. When deflationary tendencies gained pace at global level, the real economy went into recession being no longer able to bear the burden of increasing real wages. Nominal wages fell, but prices fell faster. Increased competitiveness through external devaluation would have been a way to substitute domestic demand in the face of price deflation and a binding restriction in nominal wages. However, an external devaluation was impossible due to the rules of the gold standard. In this situation, structural breaks acted as a valve that temporarily relieved the increasing pressure. From this point of view, the massive downward shift in wage inflation acted as an internal devaluation. This implicit beggar-thy-neighbor approach fits with the finding that breaks in wage formation coincided across the sample. Wage inflation remained closely linked internationally even after the end of the interwar gold standard in 1931 showing the limited effect of prices.

As such, my study addresses some important issues discussed in the literature, e.g. concerning the inverse relationship between changes in output and in real wages as well as the uncertainty about the level of wage rigidity. Although the results hold some support for Eichengreen and Sachs' (1985) sticky wage hypothesis, my findings add to doubts that monetary shocks through the sticky-wage channel alone can account for the extent of the Great Depression: Time series evidence is presented of a (Keynesian) inverse relationship between real wages and industrial production. Yet, the time series evidence suggests that this relationship held only during the Great Depression, i.e. independent from price deflation as predicted by theory. Wage rigidity was sufficiently high to induce increases in real wages above market-clearing levels; it certainly affected the recession beginning in 1929. The coincidence of wage rigidity, strong declines in wages, and structural breaks fits with Eichengreen's synthetic explanation of the depression: In Keynesian thinking, nominal rigidity keeps wages from adapting to business cycles or price changes. The fact that structural breaks existed besides that, can be interpreted as adaption of expectations: With a certain time lag, people realized that the depression was an unusual recession and that the demand shock would be stronger and last longer. Such time lags are similar in different sample countries, which suggests that expectations were formed based on the national perspective rather than taking into account what happened abroad.

The study holds some advice for future research. Empirical models of the Great Depression need to account for regime shifts in output, wages, and probably in prices, too. Otherwise, they can result in severe estimation bias. Furthermore, my study demonstrates the advantages of using high-frequency data and of multi-country analysis. Among others, the dating of breaks in wage formation suggests that developments in the US deviated systematically from other sample countries, which highlights the problems of research limited to the US: It suggests that wage dynamics in the US lagged behind on the global economy, similarly to the dating of business cycles in the previous chapter. Nonetheless, the common assumption of identical wage rigidity was not rejected outright, because equality of parameters across the sample could be rejected at reasonable values of statistical significance. However, some differences in parameter estimates are economically significant, which implies that the assumption of identical wage rigidity needs to be applied with caution. It remains for further research to understand the interplay of persistences in wage and price inflation during the interwar period, e.g. in how far concerted wage reduction itself propelled deflation. To this end, it would certainly be valuable to look at firm level data not only in the US and compare findings in different countries.

CHAPTER 6

Conclusion and final remarks

This thesis is concerned with European economic integration between 1880 and 1939 and in particular with WWI and the Great Depression, the political and economic focal points of this period and, perhaps, of the entire 20th century. I presented evidence that the war, national break ups which succeeded it, and the implosion of the world economy about 10 years thereafter did not destroy the economic integration of Europe.

In the first two chapters, economic integration was traced in regional trade data. In the latter two chapters, I examined macro-economic variables at national level with respect to interdependencies of business cycles. The nature of the sample of trade data allowed me to separate the actual 'treatment' effect of borders from the average deviation of trade across borders from internal trade. Naturally, the multiplication of borders in Europe after WWI —the share of cross-border trade increased by a third in my sample— hampered interwar trade substantially. Nonetheless, the 'treatment' effect of the new borders on trade was lower than the average effect of existing borders. First, economic integration of areas which became separated by new borders, was below average before the war already: Borders created by peacemakers at the Versailles conference appear to have been economically sound to some extent —perhaps even intentionally. Secondly, regions were forced to trade with each other despite opposing political goals.

The comparison of national business cycles with the global business cycle, in the third chapter, shows a similar pattern of depressions in the entire sample. This finding suggests to reconsider the assumption that demand shocks were sent out only by the US. What is rather needed, is a global explanatory approach in place of the 'pseudo' global approach, which takes the US a exogenous factor. In this view, the increasing pace of deflation appears as the consequence of higher interest rates in combination with a looming recession. Assuming that both factors aggravated each other would explain that a massive deflation followed a comparably small rise in interest rates. At the time the monetary shock was spread by the interwar gold standard, the negative real shock in output had already started to spread due to the high level international economic integration.

In the fourth chapter, wage changes appear highly persistent in all countries during the interwar period —at first blush. I show that multiple structural breaks can be identified in the wage formation process . They occurred quite synchronically across countries, probably due to a common (real) shock not to policy. Once these breaks are properly controlled for, the results indicate that wage changes have been similarly persistent as after WWII. Moreover, the degree of inertia seems quite stable over time and is similar in most countries. Differences across countries in the estimated degree of wage stickiness are economically significant, but in most cases statistically insignificant. The main difference with the interwar period: I show that wages fell during the depression, even strongly and persistently. This results stresses that the economic conditions are crucial for the effect of wage rigidity, much in line with Fehr and Goette (2005).

Some questions and remarks are raised for future research. Above all, how reasonable is it to use equilibrium models and the steady state assumption for explaining a period such as the Great Depression? After all, many economies apparently departed far from economic equilibrium between 1930 and 1932, such as France, Germany, or the US. As another issue, the study demonstrates that the US case did not necessarily represent global economic developments. This insight highlights the risk if economic research, concerned with the Great Depression, is focusing (in isolation) on the US.

CHAPTER 7

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2. Data Sources

This sections mentions only data sources, which are referred to in the text. For further data sources see appendix A.

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APPENDIX A

A new Data Set for the Analysis of Central European Trade

1. Data

We have compiled a large data-set that comprises well above 50,000 observations of regional trade flows in Europe before and after World War One (WWI). The data-set covers six years, namely three years (1885, 1910 and 1913) before and three years (1925, 1926, and 1933) after WWI. The main share of the data is given by railway shipments, which accounted for about 85 per cent of total trade in the region at the time.

The definition of exporting and importing regions in the data set follows that of the German railway statistics, which are the starting point and largest single source. The original railway statistics report trade of about 60 entities, which we have consolidated. As a result, our data-set comprises in total 43 consolidated transport districts (CTD) (cf. table 1 in appendix A). 31 out of these 43 CTD in the data-set are both 'exporters' (marked gray in table 1) and 'importers'. Additional 12 CTD are only 'importers', i.e. we observe exports from 31 CTD into 43 CTD, including themselves. The German statistics would allow to have Austria and Bohemia as separate exporters in the data set, i.e. yielding 32 exporters and 44 importers. Yet, data on internal trade between Austria and Bohemia before WWI are incomplete compared to internal data for Cisleithania as a whole. In order to loose as few observations from balancing the sample as possible, we opt for aggregating the two TD as CTD #34.

For each of the resulting 1,333 (= 31 × 43) bilateral trade pairs, the data-set contains observations of trade in seven commodity groups in metric tons. The goods selected represent a wide array of sectors of the economy: rye - an important agricultural product; brown coal and hard coal - natural resources used for power generation in industry and transport and for domestic heating, as well as coke, which is a key input in iron and steel making. Furthermore, the data-set covers three groups of processed industrial products: iron and steel (semi-) manufactures, cardboard and paper-products, and finally chemical products.

The main original sources are the following annual series *Statistik der Güterbewegung auf Deutschen Eisenbahnen* before WWI and *Die Güterbewegung auf Deutschen Eisenbahnen* after WWI as well as *Rocznik Statystyczny Przewozu Towarów na Polskich Kolejach Państwowych* for Poland.[1]

The special feature of these German and Polish sources is that they provide this information at the level of administrative transport districts (TD). Particularly the fact that the definition of regions is maintained after WWI makes the data unique. A second feature is the report of intra-area trade, i.e. the area both 'exports' and receives the traded goods. The reason is that the Polish (and also the Saargebiet's) sources have been modeled on the German example. The Russian, Austro-Hungarian, and French authorities did not provide any comparable statistics on railway shipments broken down by district or region. They usually report aggregate data. Yet, the German statistics can substitute such records in case of

[1]Titles in English (in the order of appearance): *Statistics of the Movement of Goods on German Railways*, *Movement of Goods on German Railways*, and *Statistical Yearbook of the Movement of Goods on Polish State Railways*.

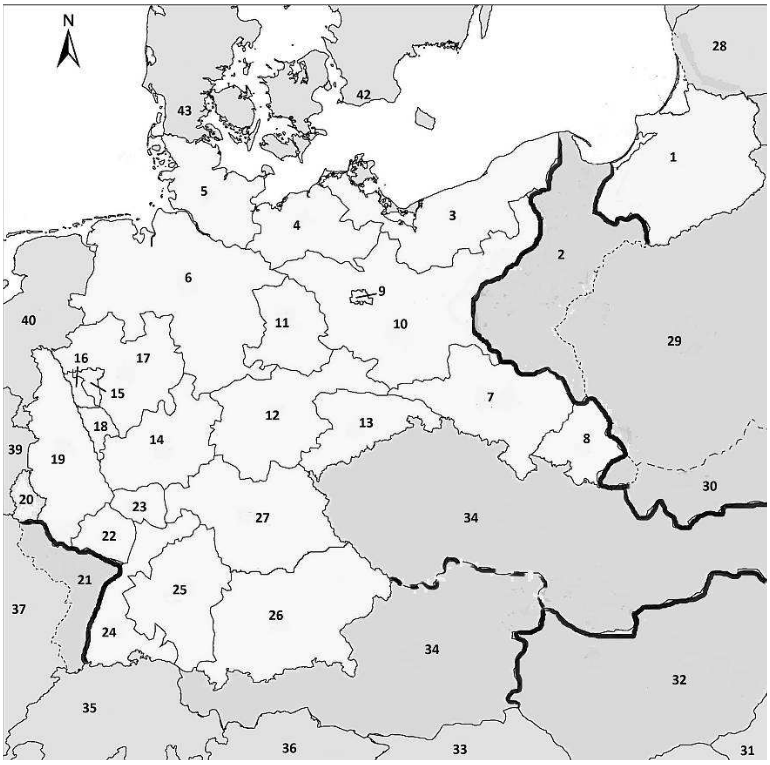

Figure 1: Central Europe, consolidated transport districts in the data set

bilateral trade between foreign regions and German TDs taking the imports of the latter as exports of the former. Nonetheless, goods-specific data on the residual foreign trade of the foreign regions is needed as well as on their internal trade. For the cases where information on internal trade was unavailable, internal trade was proxied by subtracting exports from production following Wei's (1996) procedure. In some very rare cases, where this procedure was not feasible, used circumstantial evidence on the absence of certain trade relations was used, if the sources could be regarded as reliable. Where neither of these approaches was feasible or sensible, observations were entered as missing. All details concerning the sources used and means of approximation are described in table 2 and table 3 in appendix A). The reconstruction of data is feasible because the definition of product groups is well described in the German statistics and quite narrow, with the exception of paper and cardboard. Moreover, it follows the categories of the international trade statistics. For instance, the Polish trade statistics do not report data of all chemical products commonly, but report separately acids, soda and chemical salts (*kwasy*, *soda wszelka*, and *sol chemiczny*). Hence, the solution was to construct an artificial Polish basket of chemical products comparable to the one of the German trade statistics using the figures on chemical salts from the Polish 1926 record, supplemented by information of the Polish 1931 record, which gives movements of chemical goods in greater detail.

An overview of the data by cross-section is provided by table 4 and table 5 in appendix A. The cross-sections are nearly complete, the share of missing observations is mostly between 0% and 5%. Only for 1885, the share of missing observations is higher, namely 100% in case of coke, and about 10% in case of other commodities. In later years, only for trade in chemicals the share of missing observations is consistently higher, ranging from 5% to 10%. It is also notable that many of the observations indicate the absence of any trade at all. This is not surprising as the data is highly disaggregated. The share of observations equalling zero is much lower for industrial goods (25%–50%) than for raw materials (63%–84%). The quantile measures indicate that the distribution of trade in each of the goods is strongly positively skewed. The skewness of the data is illustrated by figures 2 to 8 in appendix A. These figures show the total number of observations per weight interval, ignoring zero observations (in logs).

The considerable change in Germany's, Poland's, and Austria's territories can be accounted for strikingly well by the data-set. Territorial changes between Germany and Poland, Germany and France, Poland and Russia, or Austria und Hungary took place along the boundaries of the areas defined as TDs (cf. map 1).[2] Still, some disruptions in the statistical records cannot be captured, i.e. the post-WWI data are not fully comparable to the pre-WWI records. Most importantly though, the German postwar statistics kept up the definition of TDs if possible and accounted separately for Alsace-Lorraine as well as for the different Polish regions by introducing separate, new TDs. One of these new TDs results from the division of the former Upper Silesia (Oberschlesien), into a German and a Polish part, namely Upper Silesia and East Upper Silesia (Ost-Oberschlesien), respectively. Another new TD results from the division of the districts of Posen and of West-Prussia. A small part of each district remained part of Germany. Both of these parts were merged to become the Borderland of Posen-West Prussia (TD 12: Grenzmark Posen-Westpreußen) after the war. The larger part of each former TD merged to form a new district, West Poland (TD 47), that was part of postwar Poland. Since the number of regions in each cross-section of the panel must be the same, one basically has two options. One may opt either for non-ambiguous demarcation, i.e. to compile regions alongside the actual postwar barriers, or for comparable shape of regions before and after the war, i.e. to construct bi-national regions. In this case consistency of the cross section is preferred over the splitting of the region.

A second problem is that the postwar TDs of "East Poland" and "Galicia" are not identical to prewar "Kingdom of Poland" and "Galicia". The postwar TD "East Poland" corresponded to both the former Kingdom of Poland and to the region east of it, i.e. the Eastern Borderlands (*kresy*). Thus, the postwar area of East Poland is larger than the former Kingdom. In contrast, the TD "Galicia" was smaller

[2]That is the case with the Saargebiet, West Prussia as well as Alsace and Lorraine. The reshaping of TDs in the cases of Northern Schleswig, Eupen and Memel is unproblematic because of their negligible economic influence.

after WWI, because it had included the Bukovina beforehand.[3] I presume that both issues only pose a minor problem for the empirical analysis, since the changes occurred in terms of geography rather than in terms of economic activity. Both the kresy and the Bukovina were sparsely populated and purely agricultural areas.

[3]Data were taken for Galicia *and* the Bukovina from the Austrian statistics as to make them comparable to the German definition of the TD Galicia.

2. Compilation of regions; Definition of central cities

Table 1: Consolidated transport districts (CTD) in the data-set (exporters and importers)

Prewar TDs acc. to German statistics		**Postwar TDs acc. to German statistics**		**Region in the data set, CTD**			
Name of TD	#TD	Name of TD	# TD	**#CTD**	#prewar	# postwar	Central city
East Prussia	1a	East Prussia	1	1	1a	1	Kaliningrad
West Prussia	1b						
Posen	12	West Poland (former German Territories, excl. East Upper Silesia)	47	2	1b + 12	47	Poznan
Pomerania	3	Pomerania	3	3	3	3	Szczecin
Mecklenburg	5	Mecklenburg	5	4	5	5	Güstrow
Schleswig-Holstein, Lübeck	7	Schleswig-Holstein, Lübeck	7	5	7	7	Kiel
Hannover, Braunschweig, Oldenburg, Schaumburg-Lippe	11	Oldenburg, Stade	11a				
		Hannover, Braunschweig, Schaumburg-Lippe	11b	6	11	11a + b	Verden
		Upper Silesia	13				
City of Breslau	14	Stadt Breslau	14				
Province of Lower Silesia	15	Province of Lower Silesia	15	7	14 + 15	13 + 14 + 15	Wrocław
Oppeln	13	East Upper Silesia	47a	8	13	47a	Gliwice
Berlin	16	Berlin, center	16a				
Berlin suburbs	16a	Berlin, periphery	16b	9	16 + 16a	16a + 16b	Berlin
		Borderland of Posen-West Prussia	12				
Brandenburg	17	Brandenburg	17	10	17	17 + 12	Fürstenwalde
Anhalt und Magdeburg	18	Anhalt and Magdeburg	18	11	18	18	Magdeburg
		Merseburg, Erfurt	19a				
Thuringia and the administrative districts of Merseburg and Erfurt	19	Thuringia and the administrative districts of Merseburg and Erfurt	19b	12	19	19a + b	Rudolstadt
Greater Leipzig	20a	Leipzig	20a				
Saxony and Leipzig	20	Saxony and Leipzig	20	13	20 + 20a	20 + 20a	Dresden
		Frankfurt a.M.	21a				
Hesse-Nassau, Upper Hesse	21	Hesse-Nassau, Upper Hesse	21	14	21	21 + 21a	Gießen
Ruhr basin (Westphalia)	22	Ruhr basin (Westphalia)	22	15	22	22	Dortmund
Ruhr basin (Rhine province)	23	Ruhr basin (Rhine province)	23	16	23	23	Oberhausen
Westphalia, Lippe, Waldeck	24	Westphalia, Lippe (and Waldeck)	24	17	24	24	Lippstadt
Rhine province right of the river Rhine	25	Rhine province right of the river Rhine	25	18	25	25	Mülheim
		Cologne	26a				
Rhine province left of the river Rhine and Cologne	26	Rhine province left of the river Rhine	26	19	26	26 + 26a	Cologne
Saar	27	Saar	27	20	27	27	Saarbrücken
Lorraine	29						
Alsace	30	Alsace + Lorraine	44	21	29 + 30	44	Strasbourg
Ludwigshafen, Mannheim	34	Ludwigshafen, Mannheim	34				
Bavarian Palatine (excl. Ludwigshafen)	31	Bavarian Palatine (excl. Ludwigshafen)	31	22	31 + 34	31 + 34	Neustadt a.d. Weinstr.
Hesse (excl. Upper Hesse)	32	Hesse (excl. Upper Hesse)	32	23	32	32	Darmstadt

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Name of TD	#TD	Name of TD	# TD	**#CTD**	#prewar	# postwar	Central city
Baden	33	Baden	33	24	33	33	Karlsruhe
Württemberg, Hohenzollern	35	Württemberg, Hohenzollern	35	25	35	35	Stuttgart
		Munich	36a				
South Bavaria	36	South Bavaria	36	26	36	36 + 36a	Munich
North Bavaria	37	North Bavaria	37	27	37	37	Nuremberg
Russia	50	Memel	45				
		Latvia, Lithuania, Estonia, Finland	50b/d				
		Russia	50a	28	50	50a + 50b/d + 45	Moscow
Kingdom of Poland	51	East Poland	51	29	51	51	Warsaw
Galicia, Bukovina	52	Galicia (Poland)	52	30	52	52	Przemysl
Romania	52a	Romania	52a	31	52a	52a	Bukarest
Hungary, Slavonia, Croatia	53	Hungary	53	32	53	53	Budapest
Serbia, Bulgaria, Turkey, Greece	53a	Yugoslavia	53a	33	53a	53a	Beograd
Bohemia	54	Czechoslovakia	54				
Austria (without Bohemia, Galicia)	55	Austria	55				
Cisleithania		Cisleithania		34	54 + 55	54 + 55	Vienna
Switzerland	56	Switzerland	56	35	56	56	Bern
Italy	57	Italy	57	36	57	57	Rome
France	58	France (without Alsace, Lorraine)	58	37	58	58	Paris
Luxemburg	59	Luxembourg	59	38	59	59	Luxemburg
Belgium	60	Belgium	60	39	60	60	Brussels
Netherlands	61	Netherlands	61	40	61	61	Utrecht
Great Britain	62	Great Britain	62	41	62	62	London
Sweden, Norway	63	Sweden, Norway	63	42	63	63	Gothenburg
		Northern Schleswig	48				
Denmark	64	Denmark (without Schleswig)	64	43	64	48 + 64	Kopenhagen

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Notes: Center cities were chosen either because they were most important economic center of the TD or because they were centrally situated between several important economic centers of the TD (see text). Gray rows indicate exporting regions in the data-set.

3. New Data set: Sources and Methods

Prewar trade flows unrelated to German trade: Data sources and methods of compilation of exports, imports, internal trade

Good(s), goods' group	Region	Year	Source(s)	Source(s) provide(s) data on	Computation, assumptions
Brown coal	Bohemia	1885	1; 7	Production; sales of Bohemian coal	Sales within Cisleithania minus local and railways consumption as well as exports to Austria and Galicia
Brown coal	Bohemia	1885	1; 7	Exports across custom border; Shipments into other crownlands	Same distribution of exports as in 1910. Galicia did not receive coal from Bohemia.
Brown coal	Bohemia	1910; 1913	1; 19	Exports across custom border; Shipments into other crownlands	Galicia did not receive coal from Bohemia.
Brown coal	Bohemia	1910; 1913	1; 19	Production; Internal shipments	Sales within Cisleithania minus local and railways consumption as well as exports to Austria and Galicia
Hard coal	Bohemia	1885	1; 7	Production; Sales of Bohemian coal	Sales within Bohemia plus sales within Cisleithania minus local and railways consumption as well as exports to Austria and Galicia.
Hard coal	Bohemia	1885	1; 7	Shipments into other crownlands	Galicia did not receive coal from Bohemia.
Hard coal	Bohemia	1910; 1913	1; 7	Production; Internal shipments	Sales within Cisleithania minus exports to Austria.
Hard coal	Bohemia	1910; 1913	1; 7	Production; Shipments into other crownlands in 1907	Galicia did not receive coal from Bohemia; Export to Austria assumed to take the same export to production share as in 1907.
Hard coal	Bohemia	1885-1913	1; 3; 4	Exports across custom border	Exports of Cisleithania minus exports across the custom border of all other Cisleithanian crownlands than Bohemia.
Coke	Bohemia	1885-1913	1; 2	Exports across custom border	Exports in 1910 were entirely attributed to Germany
Coke	Bohemia	1885-1913	1	Shipments into other crownlands	
Coke	Bohemia	1885-1913	1; 7	Production; exports	Production minus exports
Brown coal	Galicia	1885	1	Production; local consumption	Internal trade equal to production minus local consumption* and exports.
Brown coal	Galicia	1910; 1913	1	Internal trade	
Brown coal	Galicia	1885-1913	1; 2; 15	Foreign trade	Exports negligible.
Hard coal	Galicia	1885-1913	1; 12	Internal shipments; railway consumption	Sales within Cisleithania minus local consumption, exports, and railway consumption, assumed to constantly take the 1908 share (based on [12]).
Hard coal	Galicia	1885-1913	1; 10; 13; 15	Exports across custom border; imports of Poland	Exports to the Russian Empire attributed entirely to Poland (based on the information in [10, 13, 15]).
Hard coal	Galicia	1885	1; 7	Shipments within Cisleithania; imports of Bohemia	Exports to Austria in 1885 equal to zero.
Hard coal	Galicia	1910	1; 15	Shipments within Cisleithania	Share of sales to Austria in total sales within Cisleithania to be the same as in 1913. Exports to Bohemia negligible.
Hard coal	Galicia	1913	1; 15	Shipments within Cisleithania; exports to other crownlands	Only exports from Western Galicia considered for exports to Austria. Exports to Bohemia negligible.
Coke	Galicia	1885-1913	1	Production; exports	Internal trade negligible but non-zero (set to 1t).
Iron+steel s.m.	Galicia	1885	8	Production	Production of on fasson iron and plates is taken as total production. Internal trade equal to production minus exports. Exports equal to zero.
Iron+steel s.m.	Galicia	1910; 1913	15	Production; exports	Production in 1913 to be the same as in 1910 (based on [15]). Exports in 1910 to be the same as in 1913. Internal trade equal to production minus exports.
Iron+steel s.m.	Galicia	1910	10	Exports	Exports assumed to be zero (valid for all countries but: A countries). Exports through custom station "Granica" in 1908 taken as proxy for exports to Poland in 1910.

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Good(s) (cont.)	**Region** (cont.)	**Year** (cont.)	**Source(s)** (cont.)	**Data** (cont.)	**Computation, assumptions** (cont.)
Iron+steel s.m.	Galicia	1913	15	Exports	Exports from Eastern Galicia negligible (based on [15]).
Rye	Galicia	1885-1913	5	Production	Internal trade equal to production minus exports to Germany and seed grain*. Exports to other regions negligible for the calculation of internal trade.
Paper+cardboard	Galicia	1910	16; 17; 18	Production 1908; Exports by largest producers 1908; Production 1908-1910 in Cisleithania	Growth rate of Galician production between 1908 and 1910 the same as of overall paper production in Cisleithania. Internal trade equal to production minus exports. Only exports by the largest producers taken into account. Export share constant between 1908 and 1910.
Paper+cardboard	Galicia	1885; 1913	6; 18	Production	Internal trade equal to production minus exports. Export/production share in 1885 and 1913 assumed to be the same as in 1910.
Brown coal	Austria	1885	1	Production; local consumption	In case data na. internal trade equal to production minus local and railway consumption*, and exports.
Brown coal	Austria	1910-1913	1; 7	Internal shipments; partly shipments within ”inlandŤ (e.g. Moravia, Silesia)	”InlandŤ shipments are taken as internal shipments, i.e. no exports to Bohemia and Galicia (based on [7]).
Brown coal	Austria	1885	1; 7	Exports (n.a. for Moravia, Silesia); Imports Bohemia	Moravian and Silesian exports abroad and into Galicia are negligible.
Brown coal	Austria	1910-1913	1; 7	Exports; shipments to crownlands (n.a. for Moravia, Silesia); Imports Bohemia	”InlandŤ shipments are taken as internal shipments, i.e. no exports to Bohemia and Galicia (based on [7]).
Hard coal	Austria	1885-1913	1	Production; local consumption; internal trade exc. for Moravia, Silesia	In case data na. - i.e. Moravia, Silesia - internal trade is equal to production minus local and railway consumption*, and exports.
Hard coal	Austria	1885	1; 7	Exports; shipments to crownlands (n.a. for Moravia, Silesia); Bohemian imports in 1880; 1890	Bohemian imports from other crownlands attributed entirely to Austria as exports. Linear interpolation between 1880 and 1890 for 1885; Use for coking at mines in Moravia assumed to be 80% of overall use for coking.
Hard coal	Austria	1910-1913	1; 13; 15; 7	Exports; shipments to crownlands (n.a. for Moravia, Silesia); Bohemian imports 1885-1907; Polish imports 1908/10	Export (to Bohemia) to production relation the same in 1910, 1913 as in 1907; Use for coking at mines in Moravia in 1910 assumed to be 80% of overall use for coking. Exports to Galicia assumed to have been 500 tons in 1910, i.e. 1% of its joint import from Poland and Moravia in 1908, which is known. Exports to the Russian Empire attributed entirely to Poland.
Coke	Austria	1885-1913	1	Production at mines / ironworks	Internal trade equal to production at mines minus exports. Share of production of coke at ironworks in Moravia in 1910 assumed to be equal to 1913. No shipments to other crownlands.
Coke	Austria	1885-1913	1; 11; 13; 15; 7	Exports across customs, Bohemian imports in 1885-1907, Polish imports 1910, 1912	Bohemian imports from other crownlands attributed entirely to Austria as exports. Export (to Bohemia) to production relation the same in 1910, 1913 as in 1907. Export to Poland in 1910 is residual of total Polish import and imports from Germany. 1912 Share of exports to Poland in exports to Russia applied to 1913. Export to Russia in 1885 entirely attributed to Poland.
Brown coal, hard coal, coke	Cisleithania	1885-1913	1	Internal trade; shipments into other crownlands	Internal trade of coals and coke is given by the internal trade of the individual regions (see above) plus their bilateral trade.
Iron+steel s.m.	Cisleithania	1885	6	Production	Production of steel products and iron and steel plates is regarded as total production. Internal trade equal to production minus exports.

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Good(s) (cont.)	**Region** (cont.)	**Year** (cont.)	**Source(s)** (cont.)	**Data** (cont.)	**Computation, assumptions** (cont.)
Iron+steel s.m.	Cisleithania	1910; 1913	17	Internal sales	Data is restricted to sales of cartelized producers. The share of cartelized production in total production was between 85% and 100%, depending on the specific product. Data on certain products is n.a. in 1913 (corresponding to 15% of the production in 1910). Its share was assumed to have developed similar to the residual production since 1910.
Paper+cardboard	Cisleithania	1885	6	Production	Internal trade equal to production minus exports.
Paper+cardboard	Cisleithania	1910; 1913	16; 17; 18	Production	Internal trade equal to production minus exports.
Rye	Cisleithania	1885-1913	5	Production	Internal trade equal to production minus exports and seed grain*.
All (excl. chemicals)	Cisleithania	1885-1913	3; 4	Foreign trade of Austria-Hungary; Exports of Hungary	Exports of Austria-Hungary minus exports of Hungary. Exports from Cisleithania to Hungary given by Hungarian imports from Cisleithania.
Brown coal, hard coal, coke	Cisleithania (excl. Galicia)	1885-1913	1	Internal trade; shipments into other crownlands	Internal trade of coals and coke is given by the internal trade of the individual regions (see above) plus their bilateral trade.
Iron+steel s.m.	Cisleithania (excl. Galicia)	1885	6	Production Cisleithania	Production of steel products and iron and steel plates is regarded as total production. Internal trade equal to production minus exports. Production of Galicia subtracted. Exports into Galicia negligible.
Iron+steel s.m.	Cisleithania (excl. Galicia)	1910; 1913	17	Internal sales	Data is restricted to sales of cartelized producers. The share of cartelized production in total production was between 85% and 100%, depending on the specific product. Data on certain products is n.a. in 1913 (corresponding to 15% of the production in 1910). Its share was assumed to have developed similar to the residual production since 1910. Internal trade of Galicia subtracted. Imports from and exports into Galicia negligible.
Paper+cardboard	Cisleithania (excl. Galicia)	1885	6	Production	Internal trade equal to production minus exports. Internal trade of Galicia subtracted. Imports from and exports into Galicia negligible.
Paper+cardboard	Cisleithania (excl. Galicia)	1910; 1913	16; 17; 18	Production	Internal trade equal to production minus exports. Internal trade of Galicia subtracted. Imports from and exports into Galicia negligible.
Rye	Cisleithania (excl. Galicia)	1885-1913	5	Production	Internal trade equal to production minus exports and seed grain*. Internal trade of Galicia subtracted. Imports from and exports into Galicia negligible.
All (excl. hard coal, chemicals)	Cisleithania (excl. Galicia)	1885-1913	3;4	Foreign trade of Austria-Hungary; Exports of Hungary	Exports of Austria-Hungary minus exports of Hungary. Exports from Cisleithania to Hungary given by Hungarian imports from Cisleithania. Exports of the Galician part negligible for total exports.
Hard coal	Cisleithania (excl. Galicia)	1885-1913	1	Exports	Accumulated exports of Bohemia and Austria.
Brown coal, Hard coal, rye	Hungary	1885-1913	21	Production	Internal Trade equal to production minus exports and installation consumption* or seed grain*, respectively. Coal used for the production of briquettes and coke is not considered.
Coke	Hungary	1885	24	Production 1903	Production in 1885 assumed to be the same as in 1903 (reasonable because of the amount of exports in 1885). Internal trade given by production minus exports.
Coke	Hungary	1910; 1913	21	Production	Exports to Austria assumed to be negligible.

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Good(s) (cont.)	**Region** (cont.)	**Year** (cont.)	**Source(s)** (cont.)	**Data** (cont.)	**Computation, assumptions** (cont.)
Iron+steel s.m.	Hungary	1910; 1913	20; 22; 23	Production in 1898, 1913; # of workers in industry 1910-1900	Production determined by linearly interpolating the relation of production in 1898/workers in 1900 and the production/worker relation in 1913. Internal trade given by production minus exports.
Paper+cardboard	Hungary	1885-1913	16; 18; 22; 24	Production in 1898, 1908, 1909, 1913; # of workers in industry 1910-1900	Rate of production increase between 1898 and 1908 linearly extrapolated to 1885. Production increase between 1908 and 1909 linearly extrapolated to 1910. Internal trade given by production minus exports.
Hard coal	Hungary	1885-1913	3; 7; 15	Exports; imports of Bohemia, Galicia	No exports of hard coal to Bohemia. No exports to Galicia in 1910 and 1913 and negligible n 1885.
Brown coal	Hungary	1885-1913	3; 7; 15	Exports; imports of Bohemia, Galicia	Exports to Galicia very small (= 1t) in 1913. No exports of brown coal to Bohemia in 1910 and 1913.
Coke	Hungary	1885-1913	3; 7; 15	Exports; imports of Bohemia, Galicia	Exports to Galicia about twice the exports to Western Galicia. No exports of coke to Bohemia in 1910 and 1913.
Iron+steel s.m., paper + cardboard, rye	Hungary	1885-1913	3	Exports	No exports into the Kingdom of Poland
Hard coal	Kingdom of Poland (KP)	1885	14	Production	Internal trade equal to production minus installation consumption*. Exports negligible.
Hard coal, brown coal	KP	1910; 1913	28	Production	Internal trade equal to production minus installation consumption*.
Coke	KP	1910; 1913	9	Production 1910, 1912	Production in 1885 negligible, i.e. set equal to 1t. Rate of production increase between 1910 and 1912 linearly extrapolated to 1913. Internal trade equal to production minus exports. Exports assumed to be about the same in 1913 as in 1910.
Chemicals, paper	KP	1910; 1913	9	Production 1910, 1912	Rate of production increase between 1910 and 1912 linearly extrapolated to 1913. Internal trade equal to production minus exports.
Iron+steel s.m.	KP	1885-1913	27	Production in 1890-1913 of all total production, Details for 1910-1912	Share of specific semi-manufactures in the total production of semi-manufactures is the same in 1913 as in 1912 and the same in 1885 as in 1910. The value for 1885 is extrapolated from the rate of growth between 1890 and 1895. Internal trade equal to production minus exports.
Rye	KP	1910; 1913	20	Production 1913	Production in 1910 assumed to be the same as in 1913. Grain seed assumed to be 15% of production. Exports in 1913 assumed to be negligible.
Brown coal, coke	KP	1910; 1913	9; 14; 15; 29	Exports 1909-1911, Import data of other regions	Export to other countries than Germany and Russia negligible.
Hard coal	KP	1910; 1913	9; 13; 25; 26; 28	Exports 1908, Exports to Russia and abroad, 1910-1913	Exports in 1910 (residual from those to Russia and Germany) attributed to Austria and Galicia Ũ their respective share determined by the share in 1908. Exports to Austria in 1913 assumed to be the same share in Austrian imports from Russia as in 1910. Exports to following countries negligible: Bohemia, France, Benelux, Great Britain, and Denmark. (Assumptions based on the information given in [13]). The Russian source [25] gives higher exports, the British source [26] confirms the figures from [9, 13, 28]
Paper+cardboard	KP	1885	14	Exports into Russia	Assuming the same share as in 1910 for specific papers in total paper exports.

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Good(s) (cont.)	**Region** (cont.)	**Year** (cont.)	**Source(s)** (cont.)	**Data** (cont.)	**Computation, assumptions** (cont.)
Chemicals, paper	KP	1910-1913	9; 26	Exports 1909-1911	Extrapolation of exports using the growth rate of production. Exports to other countries than Germany and Russia assumed to be negligible in 1913.
Iron+steel s.m.	KP	1885-1913	9; 13; 14	Foreign trade 1885, 1910-1912	Export share in production of 1912 applied to 1913. Share of goods in consideration in overall exports assumed to be constant over time. Exports to other countries than Germany and Russia assumed to be negligible in 1885 and 1913.
Rye	KP	1910	9; 13	Exports 1908, Exports to Russia and "other" countries, 1909-1911	Distribution of exports (residual from those to Russia and Germany) following the same shares as in 1908.

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Postwar trade flows unrelated to German trade: Data sources and methods of compilation of exports, imports, internal trade

Good(s), goods' group	Region	Year	Source(s)	Source(s) provide(s) data on	Computation, assumptions
Hard coal	Saargebiet	1925; 1926	37	Internal sales of Saargebiet's pits	
Brown coal	Saargebiet	1925; 1926	35; 37	Internal shipments by rail in 1928; Internal sales 1925-1928	1925 and 1926 data is approximated by assuming that good-specific shipments developed proportionately to total internal trade with coals and coke between 1925 and 1928.
Coke	Saargebiet	1925; 1926	37	Production of Saargebiet's pits	Internal trade given by the sales quantity of the Saargebiet's mines and ironworks (approximated) minus exports.
Brown coal	Saargebiet	1925; 1926	35; 37	Export of coals and coke 1925-1928 to France, Germany, and "other countries"; Detailed export by rail 1928	Exports of brown coal by rail developed proportionately to the overall exports of coals and coke to France and other countries (excl. Germany) between 1925 and 1928.
Hard coal	Saargebiet	1925; 1926	35	Export of hard coals of the Saargebiet's pits 1925, 1926	Exports to French harbors were substracted from exports to France.
Coke	Saargebiet	1925; 1926	35; 37	Sales abroad of the Saargebiet's coking plant "Kokerei Heinitz"; Detailed export by rail 1928	Constant share of Kokerei Heinitz in the Saargebiet's total exports of coke between 1925 and 1928.
Iron + steel	Saargebiet	1925; 1926	36; 37	Sales of ironworks abroad 1925-1928 to France, Germany, and "other countries"; Detailed export by rail 1928	For France and other countries (excl. Germany), the respective share was the same in 1925 and 1926 imports by rail as in 1928.
Chemicals	Saargebiet	1925; 1926	35; 37	Export of chemicals 1925-1928 to France, Germany, and "other countries"; Detailed export by rail 1928	Exports in 1925 and 1926 by rail developed proportionately to the total exports of chemical products between 1925-1928 to France and other countries (excl. Germany), respectively.
Paper + cardboard	Saargebiet	1925; 1926	35; 37	Export of paper and its products 1925-1928 to France, Germany, and "other countries"; Detailed export by rail 1928	Exports in 1925 and 1926 by rail developed proportionately to the total exports of paper and paper products between 1925-1928 to France and other countries (excl. Germany), respectively.
Rye	Saargebiet	1925; 1926	35; 37	Export of grain and flour 1925-1928 to France, Germany, and "other countries"; Detailed export by rail 1928	Exports in 1925 and 1926 by rail developed proportionately to the total exports of grain and flour between 1925-1928 to France and other countries (excl. Germany), respectively.
All	Saargebiet	1933	37	Internal shipments by rail 1933	
All	Saargebiet	1933	37	Exports by rail 1933	
Hard coal	Alsace-Lorraine (AL)	1925; 1926	38	Internal sales 1925-1928	
Hard coal	Alsace-Lorraine (AL)	1933	38; 39	Production 1925-1933; Internal sales 1925-1928	Sales in 1933 approximated by production times sales/production share in 1928.
Iron + steel	AL	1925; 1926	38	Production 1924-1926; Internal sales 1924	1925 share of sales within AL in overall sales within France to be the same as in 1924. 1926 share of sales within AL in overall production to be the same as in 1925.
Rye	AL	1925-1933	39; 40	Production	Internal trade equal to production minus seed grain* and exports to Germany and Saargebiet. Exports to other regions were assumed small relative to production.
Coke	AL	1933	39	Production	Exports in 1933 assumed to be negligible. Internal trade equal to production minus exports.

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Good(s) (cont.)	**Region** (cont.)	**Year** (cont.)	**Source(s)** (cont.)	**Data** (cont.)	**Computation, assumptions** (cont.)
Hard coal	AL	1925; 1926	19	Exports 1924-1926	Exports to be zero (assumption valid except for A and B countries, cf. notes)
Hard coal	AL	1933	23	Imports of other regions, esp. Saargebiet	Exports to be zero (assumption valid except for A and B countries)
Iron + steel	AL	1925-1926	19	Exports 1924-1925	Each destinationŚs share in production in 1926 the same as in 1925. Exports to be zero (assumption valid except for A and B countries as well as Sweden and Denmark)
Iron + steel	AL	1933	23;30	Imports of other regions, esp. Saargebiet	Exports to be zero (assumption valid except for A and B countries as well as Great Britain, Sweden, and Denmark)
Rye	AL	1925-1933	23	Imports of other regions, esp. Saargebiet	Exports to be zero (assumption valid except for A and B countries)
Brown coal, Paper, Chemicals	AL	1925-1933	23;30	Imports of other regions, esp. Saargebiet	Exports to be zero (assumption valid except for A and B countries as well as Great Britain)
Coke	AL	1925-1933	23	Imports of other regions, esp. Saargebiet	Exports to be zero (assumption valid except for A and B countries)
All (except rye)	Czechoslovakia (CSR)	1925; 1926	41	Shipments by rail 1925-1926; Exports, imports, and transit in 1926	Local shipments 1926 are computed as overall shipments minus exports, imports, and transit. Data for 1925 was approximated by the 1926 share of local shipments in overall shipments.
Brown coal	CSR	1933	41; 42	Shipments of coals (hard coal and brown coal accumulated) 1931 and 1933; Exports, imports, and transit of coals in 1931	Share of locally shipped brown coals in locally shipped coals assumed to be constant between 1926 and 1933 as well as a constant share of local shipments of (hard and brown) coals in overall shipments of coals between 1931 and 1933.
Hard coal	CSR	1933	41; 42	Shipments of coals (hard coal and brown coal accumulated) 1931 and 1933; Exports, imports, and transit of coals in 1931	Share of locally shipped hard coals in locally shipped coals assumed to be constant between 1926 and 1933 as well as a constant share of local shipments of (hard and brown) coals in overall shipments of coals between 1931 and 1933.
Paper + cardboard, iron + steel, coke	CSR	1933	41; 42	Internal trade by rail 1931; overall good-specific shipments by rail 1931, 1933	Share of internal trade by rail in overall shipments by rail the same in 1933 as in 1931.
Rye	CSR	1925-1933	41; 42	Internal trade of grain by rail 1926, 1931; overall shipments of grain by rail 1925-1933, production of rye and grain	Share of internal trade by rail in overall shipments by rail the same in 1925 as in 1926 and the same in 1933 as in 1931. Share of rye in railway shipments proportional to its share in grain production.
Chemicals	CSR	1925-1933	43	Foreign trade statistics	
Iron + steel, rye, hard coals, coke	CSR	1925-1933	43; 49	Foreign trade statistics; Polish imports 1925, 1926 by Polish TD	Total export to Polish TD in 1925 and 1926 proportionate to each TD's share in imports by rail (based on [48]). Import share of each Polish TD in 1933 the same as in 1926.
Brown coals	CSR	1925-1933	43; 49	Foreign trade statistics; Polish imports 1925 by Polish TD	Total export to Polish TD in 1925 proportionate to each TD's share in imports by rail (based on [48]). Import share of each Polish TD in 1926 and 1933 the same as in 1925.
Paper + cardboard	CSR	1925-1933	43; 49	Foreign trade statistics; Polish imports 1926 by Polish TD	Total export to Polish TD in 1926 proportionate to each TD's share in imports by rail (based on [48]). Import share of each Polish TD in 1925 and 1933 the same as in 1926.
Brown coal, hard coal	Hungary	1925-1933	21	Production; share of local consumption in total coal production	Internal trade equal to production minus exports and local consumption. Share in overall local consumption assumed to be proportionate to share in total coal production.
Rye	Hungary	1925-1933	21	Production	Internal Trade equal to production minus exports and seed grain*.

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Good(s) (cont.)	**Region** (cont.)	**Year** (cont.)	**Source(s)** (cont.)	**Data** (cont.)	**Computation, assumptions** (cont.)
Iron + steel	Hungary	1925-1933	21; 23; 34; 44; 45	Production in 1913, 1929; # workers in 1913, 1924-1926, 1933	Internal trade given by production minus exports. Production determined by the number of workers in the industry proportionate to the production/worker relation in 1913. Despite the strong assumption, cross checking with the industry's consumption of coals, an index of T-iron production for 1927-1933, and the value of production indicate reasonable results.
Paper + cardboard	Hungary	1925-1933	44; 46	Share in demand covered by domestic production of writing paper (1927, 1933), packing paper (1925-1933), and cardboard (1925-1933)	Share in demand covered by production of writing paper in 1926 is the same as in 1927. Share in demand covered by overall paper production in 1925 is the same as in 1926. Internal trade given by production minus exports.
Brown coal, hard coal, coke, Paper + cardboard	Hungary	1925-1933	46	Exports	
Iron + steel, rye	Hungary	1925-1933	37; 46; 48; 49	Exports; Polish imports by TD	Exports to Polish TD proportional to each TD's share in imports by rail. Import share of each Polish TD in 1933 the same as in 1926. Exports of iron and steel to Alsace-Lorraine to be zero.
Chemicals	Hungary	1925-1933	2; 37; 48; 49	Imports of other regions; transit shipments through Germany 1926-1933	Exports to be zero (assumption valid except for A countries as well as Yugoslavia, Russia, and Italy)
Brown coal, hard coal, coke, rye	Republic of Austria (RA)	1925-1933	47	Production	Internal Trade equal to production minus exports and installation consumption* or seed grain*, respectively.
Iron + steel	RA	1925-1933	47	Production	Production of "Walz- und Schmiedewaren" is assumed to embrace total production of subgroup. Internal trade equal to production minus exports.
Paper + cardboard	RA	1925-1933	47	Production (exc. for cardboard in 1926)	Cardboard production in 1926 assumed to have the same proportion relative to paper production as in 1925. Internal trade equal to production minus exports.
Brown coal, hard coal, coke	RA	1925-1933	37; 48; 49	Exports; Polish imports 1925, 1926 by TD	Exports to Poland in 1926 distributed evenly across Polish districts as no information available in the Polish statistics because of thresholds to reporting
Chemicals	RA	1925-1933	37; 48	Exports	
Iron + steel	RA	1925-1933	37; 48; 49	Foreign trade statistics; Polish imports 1925, 1926 by TD	Total export to Polish TD in 1925, 1926 proportional to each TD's share in imports by rail. Import share of each Polish TD in 1933 the same as in 1926.
Rye	RA	1925-1933	37; 48; 49	Foreign trade statistics; Polish imports 1925, 1926 by TD	Export to Poland in 1925 (1 ton) attributed to Galicia.
Paper + cardboard	RA	1925-1933	37; 48; 49	Foreign trade statistics; Polish imports 1926 by TD	Total export to Polish TD in 1926 proportional to each TD's share in imports by rail. Import share of each Polish TD in 1925 and 1933 the same as in 1926.
All (except paper + cardboard, chemicals)	Republic of Poland (RP)	1925-1933	49	Internal trade by rail within and among TD	
Paper + cardboard	RP	1926; 1933	49	Internal trade by rail within and among TD	
Chemicals	RP	1933	49	Internal trade by rail within and among TD	

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Good(s) (cont.)	**Region** (cont.)	**Year** (cont.)	**Source(s)** (cont.)	**Data** (cont.)	**Computation, assumptions** (cont.)
All (except paper + cardboard, chemicals)	RP	1925-1933	49; 50	Exports by rail 1925-1926, foreign trade statistics 1926-1933	Development of Polish exports by rail between 1926 and 1933 proportional to the development of total exports to each country between 1926 and 1933. Exports in 1933 are set equal to zero once no exports are given in the foreign trade statistics of 1933.
Paper + cardboard	RP	1925-1933	49; 50	Exports by rail 1926, foreign trade statistics 1925-1933	Development of Polish exports by rail proportional to the development of total exports to each country between 1925 and 1926 as well as between 1926 and 1933. Exports in 1933 and 1925 are set equal to zero once no exports are given for the respective year in the foreign trade statistics.

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4. Distribution of total trade of goods

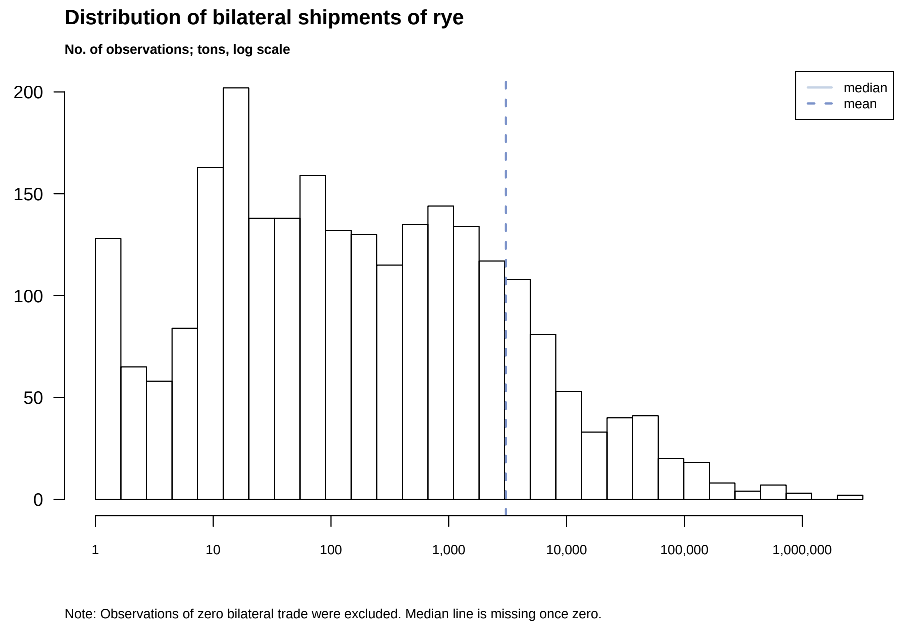


Figure 2: Distribution of observations of rye trade

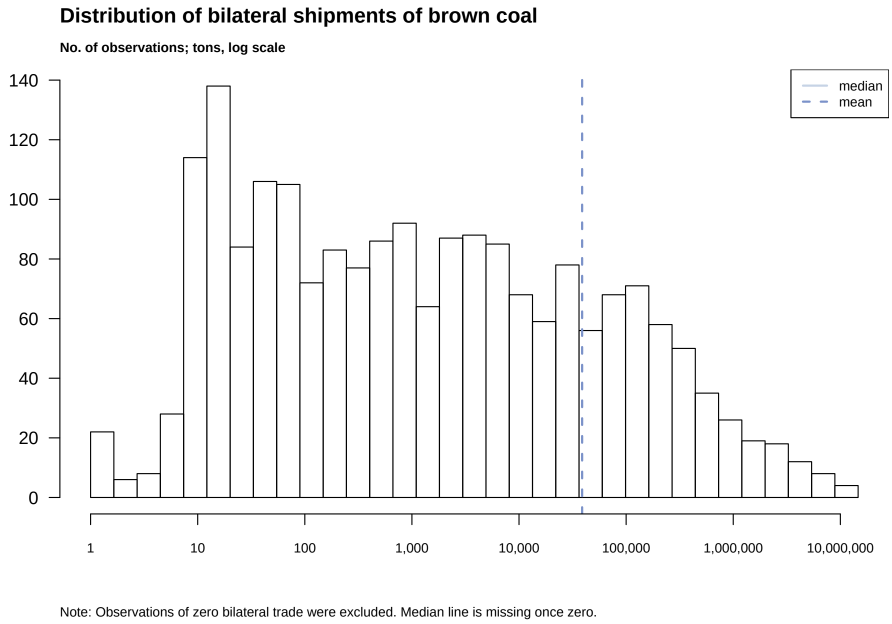


Figure 3: Distribution of observations of trade of brown coal

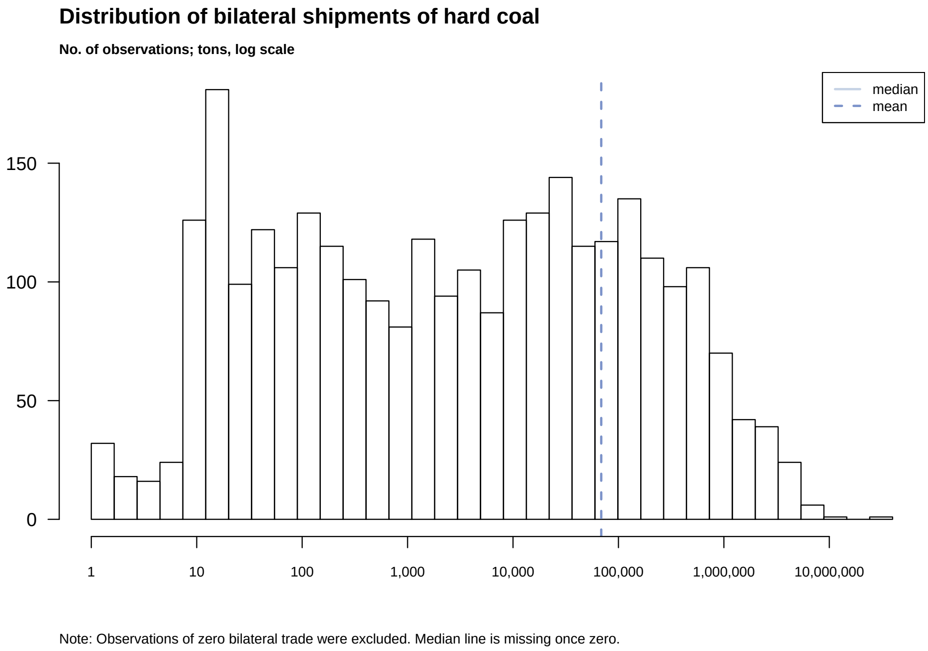


Figure 4: Distribution of observations of hardcoal trade

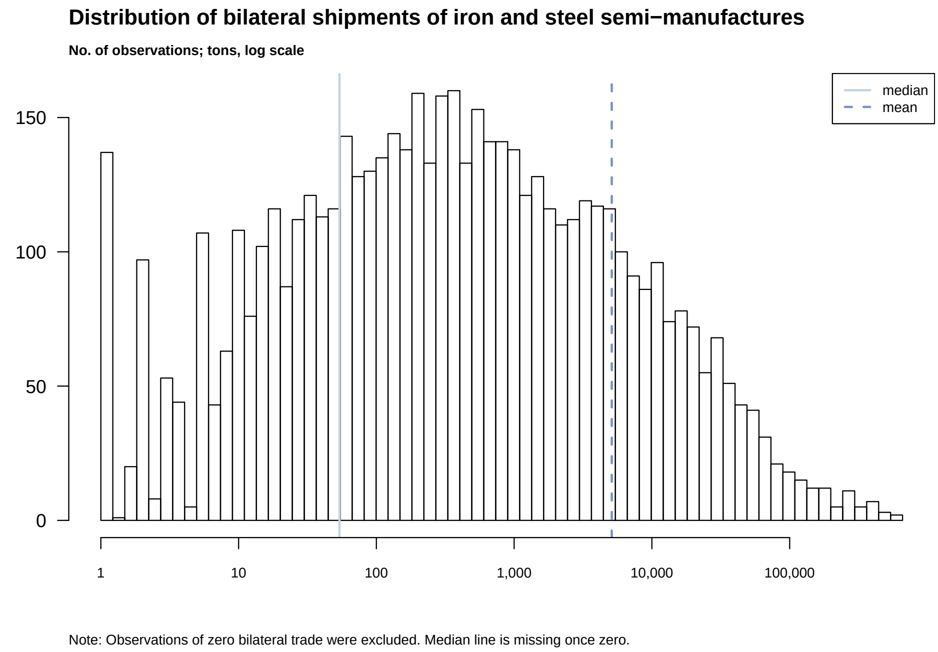


Figure 5: Distribution of observations of trade of iron and steel semi-manufactures

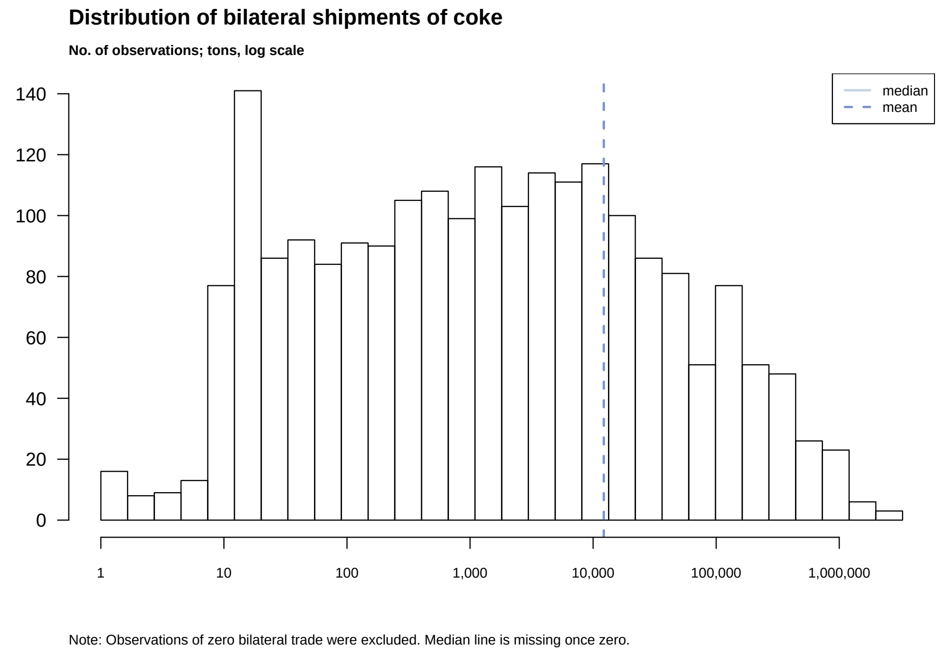


Figure 6: Distribution of observations of coke trade

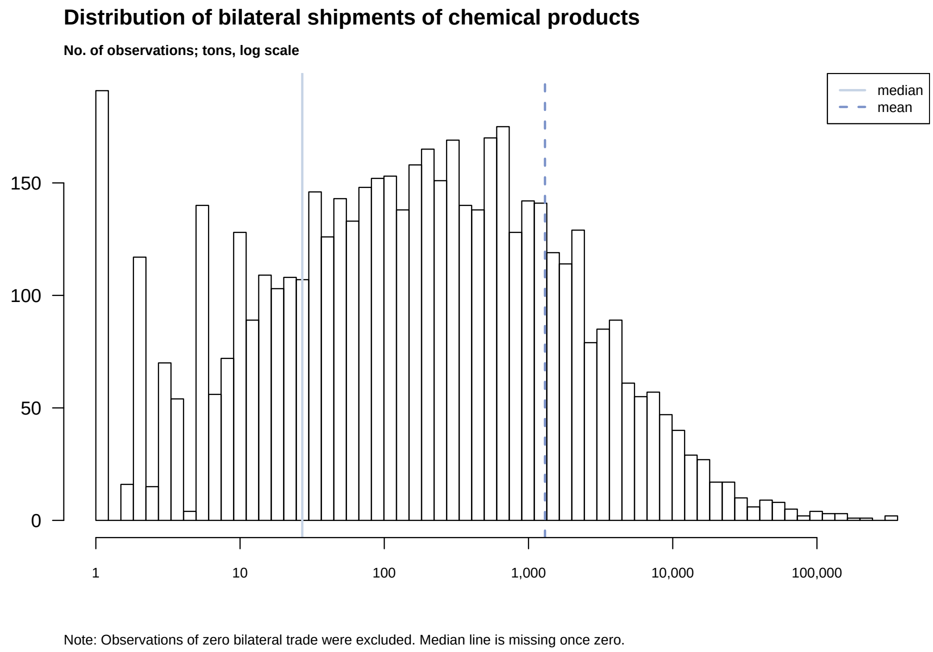


Figure 7: Distribution of observations of trade of chemicals

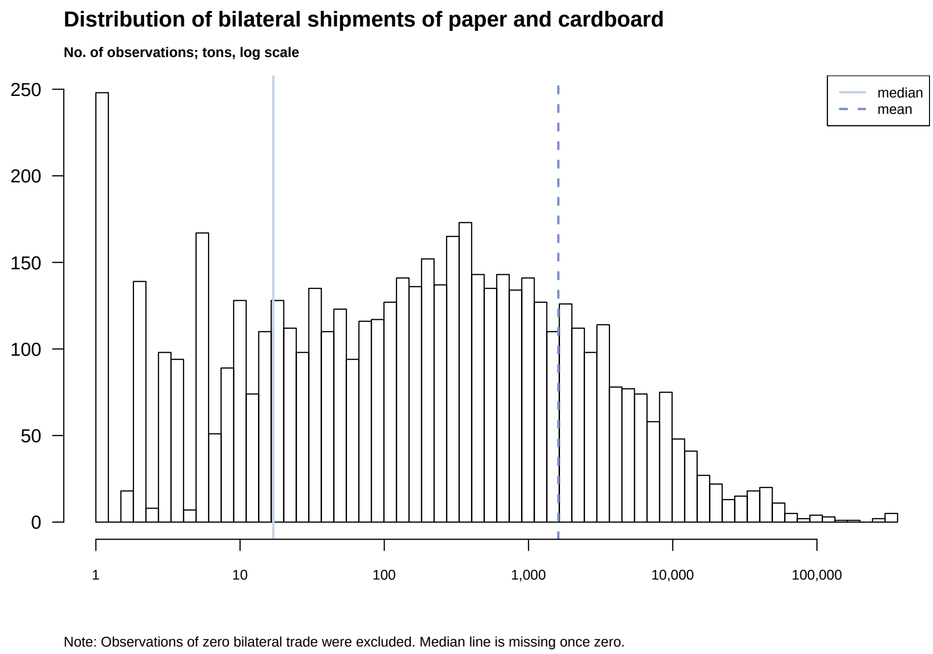


Figure 8: Distribution of observations of trade of paper and cardboard

Table 4: Descriptive statistics: Trade of brown coal, chemicals, and iron

Parameter	**1885**	**1910**	**1913**	**1925**	**1926**	**1933**	**all years**
Brown coal and brown coal briquettes							
Minimum value	0	0	0	0	0	0	0
1st Quartile	0	0	0	0	0	0	0
Median	0	0	0	0	0	0	0
Mean	11274	37019	45737	49325	48536	38202	38830
3rd Quartile	0	0	0	0	20	6	0
Maximum value	3657980	9852316	11959887	9519504	9468640	8024539	11960000
No. of NA obs.	153	4	3	8	8	9	185
Standard deviation	135973	400875	481972	423420	422568	340094	387163
No. of zeros	1051	1073	1055	1067	1001	1037	6284
No. of observations	1255	1404	1405	1400	1400	1399	8263
% share of zeros	84	76	75	76	72	74	76
Chemical products							
Minimum value	0	0	0	0	0	0	0
1st Quartile	0	0	0	0	0	0	0
Median	0	34	56	45	52	38	27
Mean	101	1227	1441	1733	1687	1514	1301
3rd Quartile	30	402	591	606	631	408	386
Maximum value	11343	125032	143611	355182	360114	228330	360100
No. of NA obs.	212	68	68	109	111	97	665
Standard deviation	519	6059	6733	11714	12274	9623	8832
No. of zeros	594	398	338	423	406	375	2534
No. of observations	1196	1340	1340	1299	1297	1311	7783
% share of zeros	50	30	25	33	31	29	33
Iron and steel semi-manufactures							
Minimum value	0	0	0	0	0	0	0
1st Quartile	0	0	0	0	0	0	0
Median	6	92	152	82	78	21	54
Mean	992	5730	6774	6412	6058	4233	5112
3rd Quartile	222	1195	1561	1215	1184	671	946
Maximum value	45743	582608	582608	527092	450052	422556	582600
No. of NA obs.	185	43	38	11	8	19	304
Standard deviation	3517	27806	31672	29499	29994	22262	26304
No. of zeros	496	342	348	417	416	503	2522
No. of observations	1223	1365	1370	1397	1400	1389	8144
% share of zeros	41	25	25	30	30	36	31

Notes: none.

Table 5: Descriptive statistics: Trade of rye, paper, hard coal, and coke

Parameter	1885	1910	1913	1925	1926	1933	all years
Rye							
Minimum value	0	0	0	0	0	0	0
1st Quartile	0	0	0	0	0	0	0
Median	0	0	0	0	0	0	0
Mean	1798	4516	4884	2325	2212	2489	3406
3rd Quartile	9	10	16	18	15	0	10
Maximum value	1045000	2319456	2329923	617980	557242	671606	2330000
No. of NA obs.	200	53	69	5	6	35	368
Standard deviation	31341	71642	73025	23343	21341	26413	46728
No. of zeros	852	913	863	957	973	1044	5602
No. of observations	1208	1355	1339	1403	1402	1373	8080
% share of zeros	71	67	64	68	69	76	69
Paper and cardboard							
Minimum value	0	0	0	0	0	0	0
1st Quartile	0	0	0	0	0	0	0
Median	1	29	34	18	22	22	17
Mean	250	1487	1781	2178	2101	1701	1610
3rd Quartile	58	374	453	656	708	499	390
Maximum value	51018	272964	307532	349214	336323	273577	349200
No. of NA obs.	199	53	53	54	19	23	401
Standard deviation	1719	9206	10526	14899	14002	10295	11095
No. of zeros	541	405	375	481	479	442	2723
No. of observations	1209	1355	1355	1354	1389	1385	8047
% share of zeros	45	30	28	36	34	32	34
Hard coal and hard coal briquettes							
Minimum value	0	0	0	0	0	0	0
1st Quartile	0	0	0	0	0	0	0
Median	0	0	0	0	0	0	0
Mean	38914	71387	110000	67603	67843	53001	68660
3rd Quartile	40	118	168	238	794	74	144
Maximum value	2935173	5360052	9076160	8625213	9076160	7598609	9076160
No. of NA obs.	156	7	8	3	8	10	192
Standard deviation	215215	406856	1072834	396837	385841	327276	548415
No. of zeros	834	893	890	905	877	937	5336
No. of observations	1252	1401	1400	1405	1400	1398	8256
% share of zeros	67	64	64	64	63	67	65
Coke							
Minimum value	0	0	0	0	0	0	0
1st Quartile	0	0	0	0	0	0	0
Median	0	0	0	0	0	0	0
Mean	0	13588	17677	14524	15582	11894	12210
3rd Quartile	0	25	30	49	102	26	3
Maximum value	0	1956727	2622340	2240315	2069603	877322	2622340
No. of NA obs.	1408	1	1	7	9	14	1440
Standard deviation	0	93947	124737	92208	96130	65084	88082
No. of zeros	0	985	977	986	942	986	6284
No. of observations	0	1407	1407	1401	1399	1394	8416
% share of zeros	–	70	69	70	67	71	75

Notes: none.

APPENDIX B

Appendix to Chapter 2

1. Derivation of the gravity equation

Anderson and van Wincoop (2004) formulate the consumer maximization function as follows

$$\max_{c_{ij}^k} U_j = \sum_k \left(\sum_i \left(\beta_i^k\right)^{\frac{1-\sigma_k}{\sigma_k}} \left(c_{ij}^k\right)^{\frac{\sigma_k-1}{\sigma_k}} \right)^{\frac{\sigma_k}{\sigma_k-1}} \tag{1.1}$$

s.t. the budget constraint

$$Y_j = \sum_k E_j^k = \sum_k \sum_i p_{ij}^k c_{ij}^k \tag{1.2}$$

where

c_{ij}^k	denotes region j's consumption of region i's goods in product class k.
σ_k	is the elasticity of substitution between varieties in product class k, with $\sigma_k \neq 1$.
β_i^k	is some positive distribution parameter.
Y_j	is the nominal income of region j's inhabitants.
E_j^k	is the nominal expenditure of region j's inhabitants on product k.
p_{ij}^k	denotes the price charged by region i for exports of product k to region j.

Demand for region i's goods by region j's consumers has to satisfy maximization of (1.1) subject to the budget constraint (1.2). From the Lagrangian we obtain the first order conditions:

$$\left(\sum_i \left(\beta_i^k\right)^{\frac{1-\sigma_k}{\sigma_k}} \left(c_{ij}^k\right)^{\frac{\sigma_k-1}{\sigma_k}} \right)^{\frac{1}{\sigma_k-1}} \left(\beta_i^k\right)^{\frac{1-\sigma_k}{\sigma_k}} \left(c_{ij}^k\right)^{\frac{-1}{\sigma_k}} \frac{\sigma_k-1}{\sigma_k} = \lambda p_{ij}^k \; \forall i \neq j \tag{1.3}$$

$$\left(\sum_i \left(\beta_i^k\right)^{\frac{1-\sigma_k}{\sigma_k}} \left(c_{ij}^k\right)^{\frac{\sigma_k-1}{\sigma_k}} \right)^{\frac{1}{\sigma_k-1}} \left(\beta_j^k\right)^{\frac{1-\sigma_k}{\sigma_k}} \left(c_{jj}^k\right)^{\frac{-1}{\sigma_k}} \frac{\sigma_k-1}{\sigma_k} = \lambda p_{jj}^k \tag{1.4}$$

$$\sum_k \sum_i p_{ij}^k c_{ij}^k = Y_j \tag{1.5}$$

where (1.3) and (1.4) hold for all sectors k. Setting equal (1.3) and (1.4) and rearranging the resulting equation yields

$$p_{ij}^k c_{ij}^k = \left(\frac{\beta_i^k}{\beta_j^k} \right)^{1-\sigma_k} \left(c_{jj}^k\right)^{1-\sigma_k} \left(p_{jj}^k\right)^{\sigma_k} \; \forall i, k \tag{1.6}$$

Summing up (1.6) over all i and using the definition from the budget constraint (1.2) gives

$$\sum_i p_{ij}^k c_{ij}^k = E_j^k = \left(\beta_j^k\right)^{-(1-\sigma_k)} c_{jj}^k \left(p_{jj}^k\right)^{\sigma_k} \sum_i \left(\beta_i^k p_{ij}^k\right)^{1-\sigma_k} \tag{1.7}$$

Now, one replaces the terms in (1.7) that do not depend on i (except for E_j^k) by the terms in (1.6) that do not depend on i. After some rearrangement that yields

$$\frac{\left(\beta_i^k p_{ij}^k\right)^{1-\sigma_k}}{\sum\limits_i \left(\beta_i^k p_{ij}^k\right)^{1-\sigma_k}} E_j^k = p_{ij}^k c_{ij}^k = X_{ij}^k \tag{1.8}$$

where the latter equation stems from the definition that demand X in region i for products k from region j is given by price times quantity, i.e. $X_{ij}^k = p_{ij}^k c_{ij}^k$. One may simplify (1.8) assuming equal weights β_i^k for each region of origin. This assumption yields

$$X_{ij}^k = \left(\frac{p_{ij}^k}{P_j^k}\right)^{1-\sigma_k} E_j^k \tag{1.9}$$

where P_j^k is the CES price index in j defined as

$$P_j^k \equiv \left[\sum_i p_{ik}^{k\ 1-\sigma_k}\right]^{\frac{1}{1-\sigma_k}} \tag{1.10}$$

Prices p_{ij}^k differ between locations due to a mark-up on p_i^k, which is the supply price received by producers of k in region i. To achieve a formulation like equation (1.9) and (1.10), several assumptions were needed. (i) I assumed that the mark-up on prices contains only the ad-valorem tariff equivalent of trade costs t_{ij}^k. (ii) A further assumption is that trade costs are proportional to trade volumes. Taken together, (i) and (ii) imply $p_{ij}^k = p_i^k t_{ij}^k$. (iii) Trade costs are assumed to be borne by the exporter, i.e. formally the exporter incurs export costs equal to $(t_{ij} - 1)$ for each good shipped from i to j. The nominal value of exports from i to j is, thus, the sum of the value of production at the origin $p_i c_{ij}$ plus the trade cost that the exporter passes on to the importer, i.e.

$$X_{ij}^k = p_{ij}^k c_{ij}^k = p_i^k c_{ij}^k + (t_{ij}^k - 1) p_i^k c_{ij}^k \tag{1.11}$$

Imposing market clearing conditions for all regions and sectors

$$Y_i^k = \sum_j X_{ij}^k \ \forall i, k \tag{1.12}$$

and inserting equation (1.9) into (1.12) as well as the assumption on the equivalence of trade costs and trade volumes (ii) into (1.9) gives that

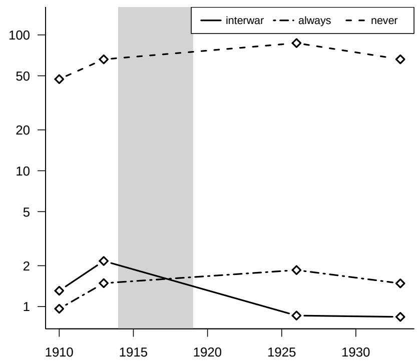
$$Y_i^k = \sum_j \left(\frac{p_{ij}^k}{P_j^k}\right)^{1-\sigma_k} E_j^k = \sum_j \left(\frac{p_i^k t_{ij}^k}{P_j^k}\right)^{1-\sigma_k} E_j^k \tag{1.13}$$

which has to be rearranged in order to solve for p_i^k:

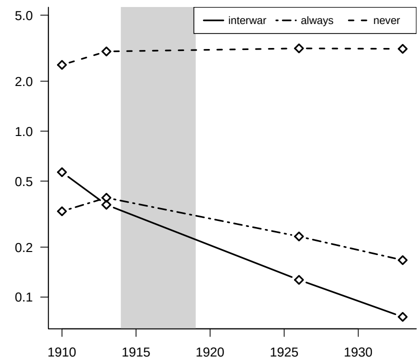
$$\left(p_i^k\right)^{1-\sigma_k} = \frac{Y_i^k}{\sum\limits_j \left(\frac{t_{ij}^k}{P_j^k}\right)^{1-\sigma_k} E_j^k} \; \forall i \tag{1.14}$$

Now, equation (1.14) can be substituted into equations (1.9) and (1.10). The result is the theory-based gravity model, which is described by equations (3.1)-(3.3) in section 3.1.

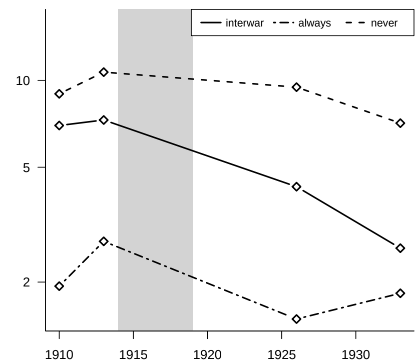
2. Trade between pairs of regions under treatment



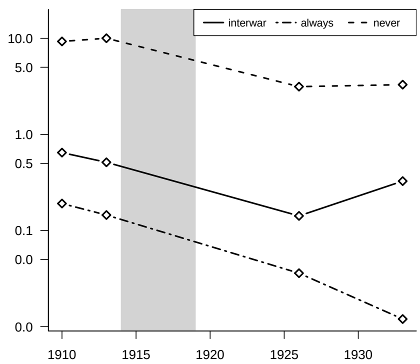
(a) Brown coal



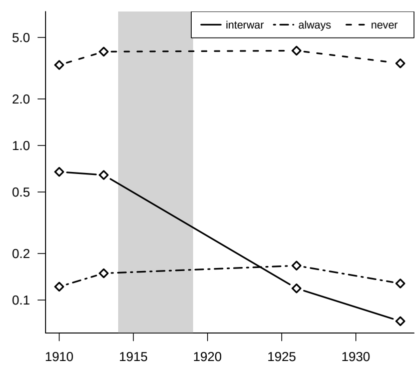
(b) Chemical products



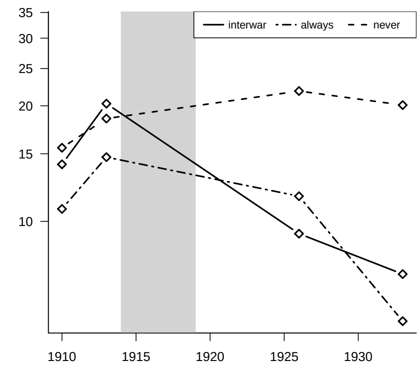
(c) Iron and Steel



(d) Rye



(e) Paper



(f) Coke

Figure 1: Comparison of trade volumes of treatment and control groups.
Notes: Trade in 1000 tons, log scale.

APPENDIX C

Appendix to Chapter 3

1. Total trade of goods, 1885 – 1933

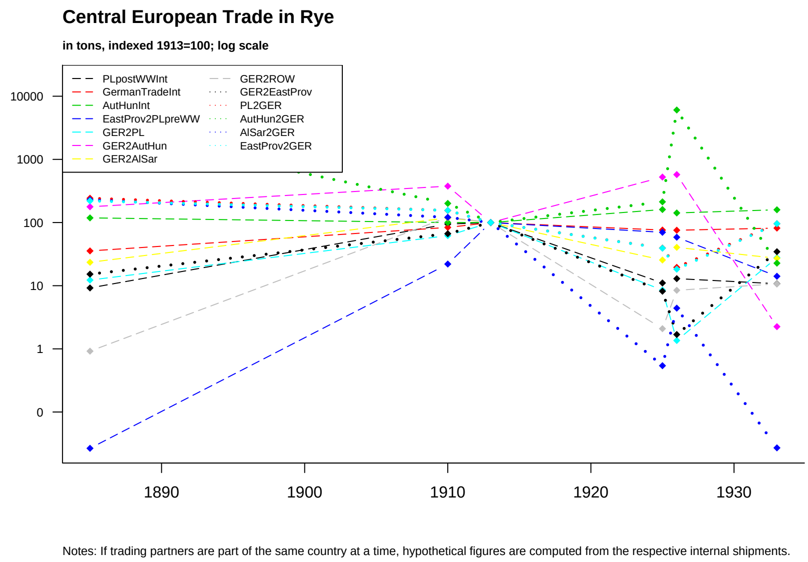


Figure 1: Time series evidence on rye trade

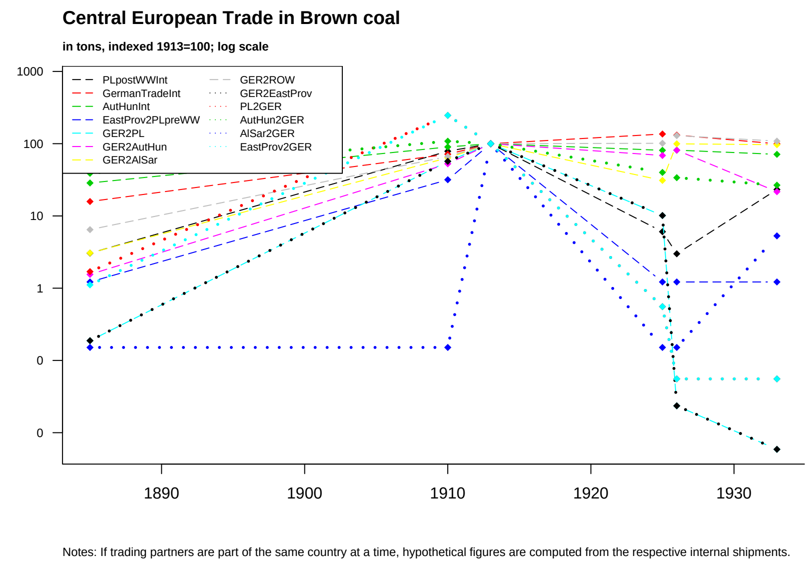


Figure 2: Time series evidence on trade of brown coal

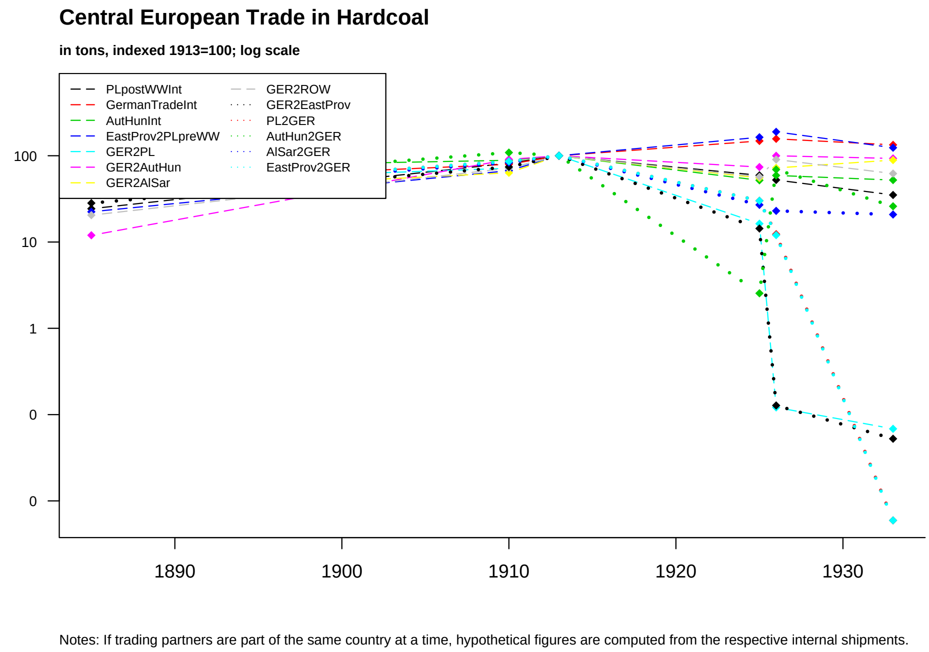


Figure 3: Time series evidence on hardcoal trade

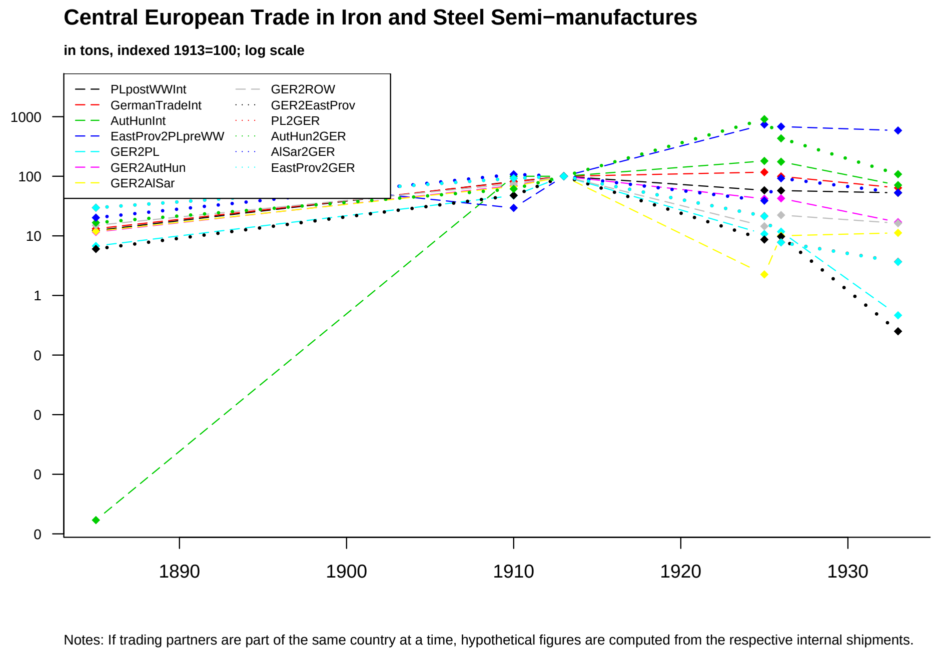
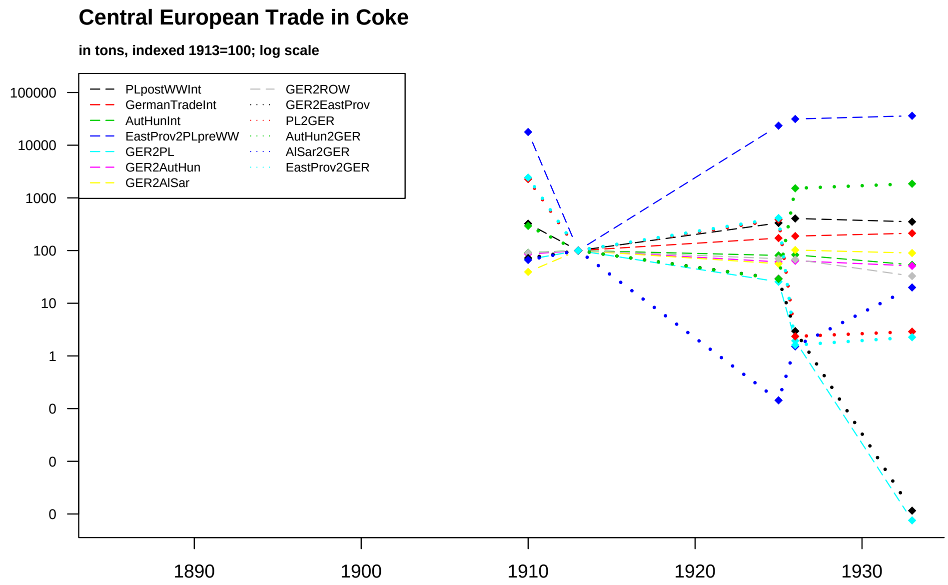
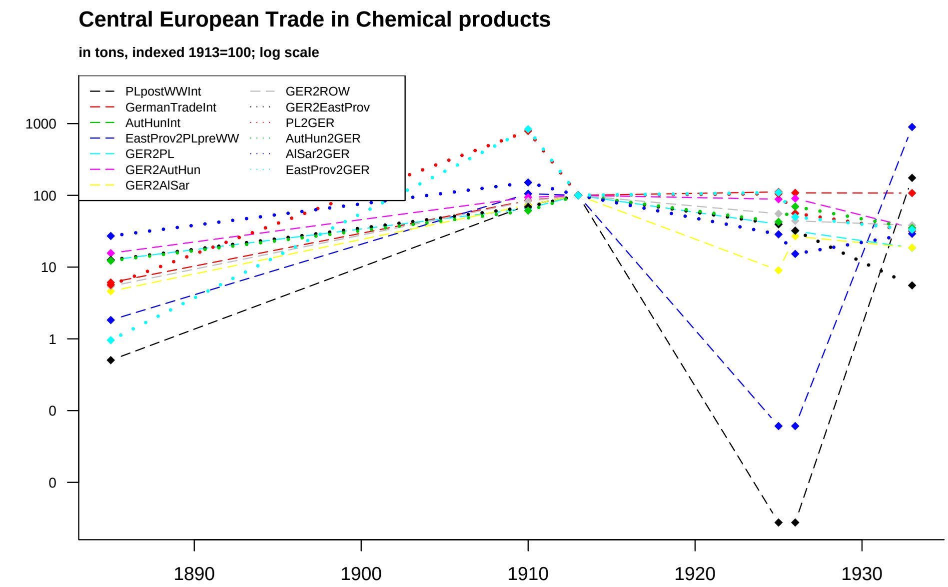


Figure 4: Time series evidence on trade of iron and steel semi-manufactures



Notes: If trading partners are part of the same country at a time, hypothetical figures are computed from the respective internal shipments.

Figure 5: Time series evidence on coke trade



Notes: If trading partners are part of the same country at a time, hypothetical figures are computed from the respective internal shipments.

Figure 6: Time series evidence on trade of chemicals

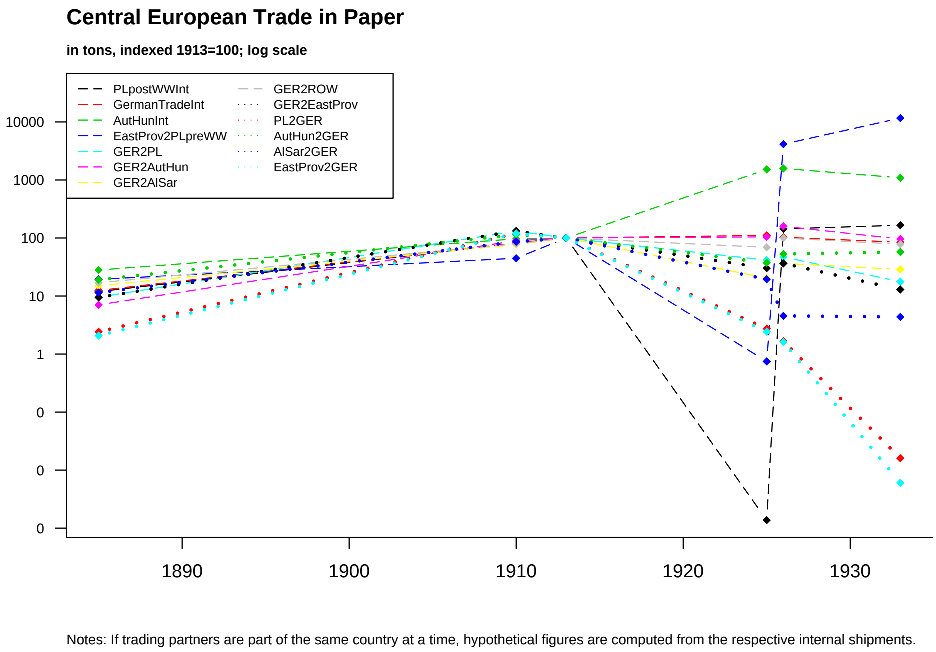
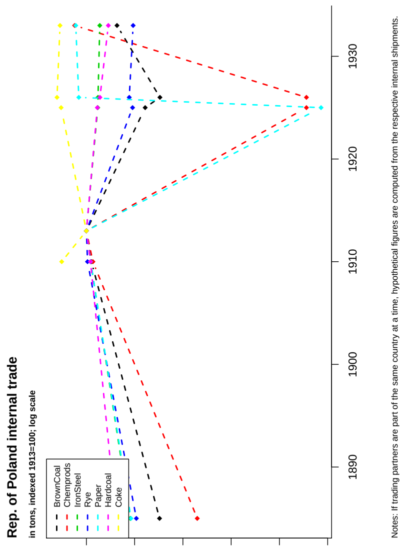
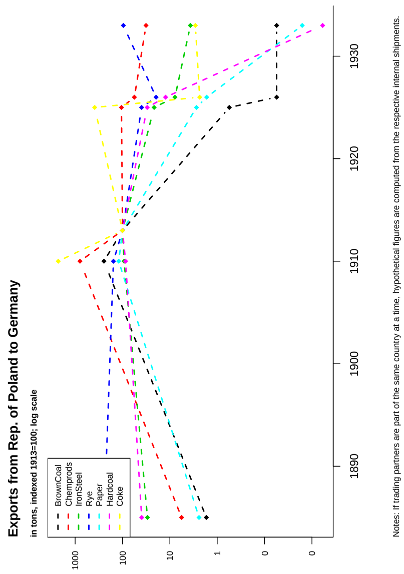


Figure 7: Time series evidence on trade of paper and cardboard

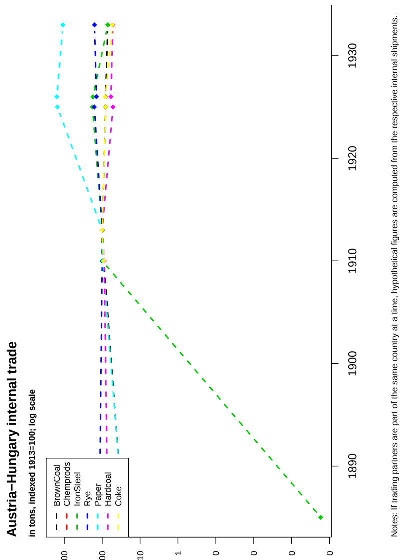
2. Trade flows following identical prewar and interwar borders



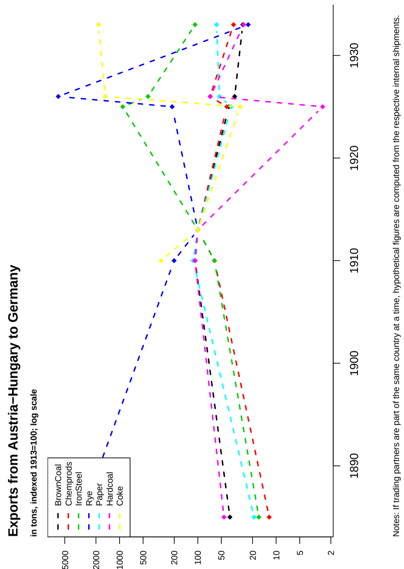
(a)



(b)



(c)



(d)

Figure 8: Trade of regions belonging to Austria-Hungary and interwar Poland

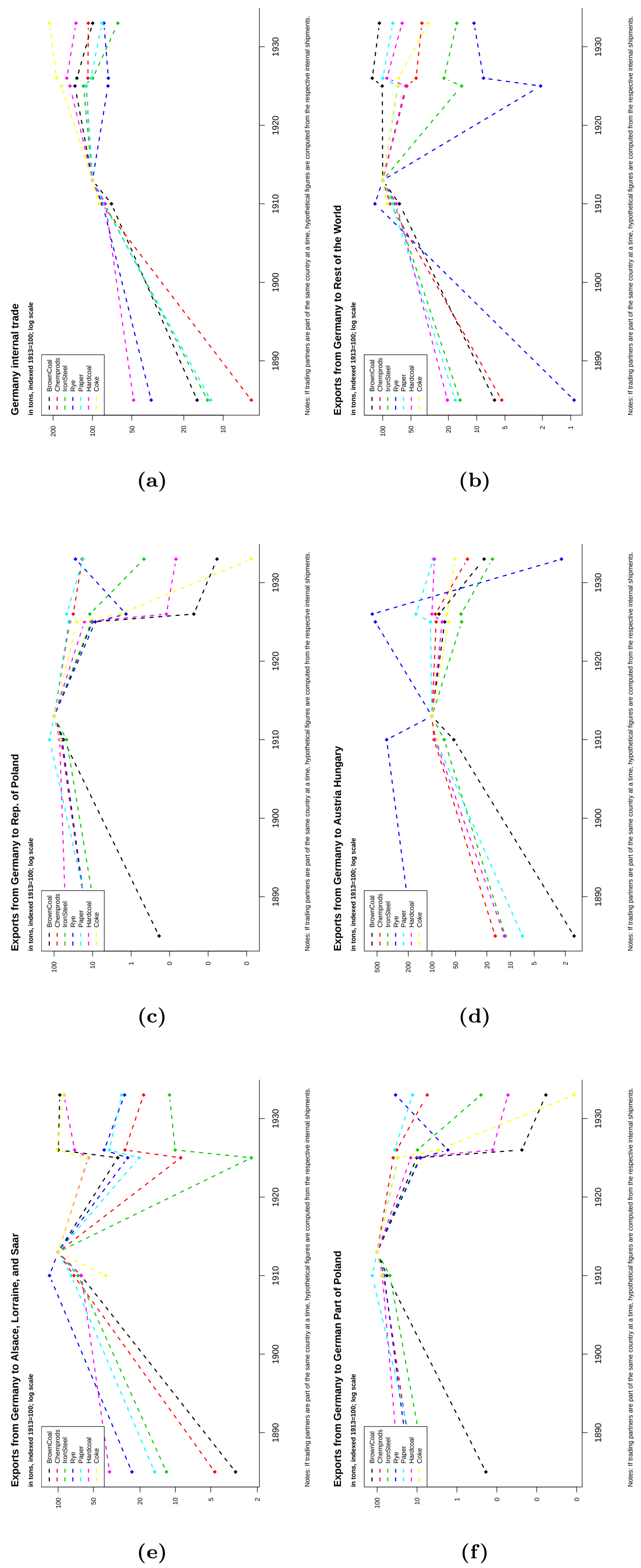


Figure 9: Trade of regions belonging to interwar Germany

3. Import tariff levels in Europe, 1913, 1927, 1931

Table 1: Change in ad-valorem equivalent import tariff rates by product group

Country	Cereals	Chemicals	Paper	Metals
Unweighted average over countries in sample				
1913	25.3	27.7	77.0	46.8
1927	17.1	25.1	26.0	40.4
1931	108.5	32.7	33.6	49.8
1927/31	62.8	28.9	29.8	45.1
Germany				
1913	28.0	17.0	17.5	17.5
1927	28.4	12.8	12.1	22.0
1931	186.0	43.5	15.8	27.0
Russia				
1913	0.0	62.0	247.0	95.5
Poland				
1927	0.0	36.0	25.3	54.5
1931	97.0	32.5	29.5	67.0
France				
1913	30.0	16.0	23.8	41.2
1927	21.6	10.8	33.0	58.0
1931	102.0	11.2	42.6	64.0
Austria-Hungary				
1913	43.0	15.7	19.8	33.0
Czechoslovakia				
1927	22.0	17.0	28.1	39.6
1931	111.0	33.0	33.5	48
Austria				
1927	3.7	17.4	14.4	29.5
1931	96.0	21.7	25.3	37.0
Hungary				
1927	26.7	56.3	43.0	38.6
1931	59.0	54.0	55.0	55.5

Source: Liepmann (1938, pp.60, 118, 383–400).
Notes: For details of computation cf. page 51.

Table 2: German exports by industry: Trade diversion after WWI (% of production)

Year	Cast iron	Electrical equipment	Cotton	Iron and steel goods	Machinery	Chemical products	Paper	Toys
1913	26.6	25.5	21.6	33.2	26.4	35.5	43.3	73.5
1928	27.3	19.1	10.4	31.0	29.2	31.3	37.5	55.9
change	0.7	-6.4	-11.2	-2.2	2.8	-4.2	-5.8	-17.6

Source: Liepmann (1938), p.203.
Notes: German exports by industry as percentage share of production. 'Cast iron' includes rolling works.

Table 3: (a) Mother tongue: language shares before World War I (1905/1910)

Region	# TD	German	Polish	Danish	French	Frisian	Italian	Yiddish	Lithuanian	Dutch	Russian
East Prussia	1	0.810	0.138	0.000	0.000	0.000	0.000	0.000	0.053	0.000	0.000
Pomerania	3	0.984	0.015	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mecklenburg	5	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Schleswig-Holstein, Lübeck	7	0.891	0.008	0.084	0.000	0.011	0.000	0.000	0.000	0.000	0.000
Hanover, Braunschweig, Oldenburg, Schaumburg-Lippe	11	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
West Prussia and Posen	12	0.504	0.495	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Upper Silesia	13	0.424	0.570	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Lower Silesia	15	0.960	0.026	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Greater Berlin	16	0.971	0.020	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Brandenburg	17	0.973	0.016	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Anhalt and Magdeburg	18	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Thuringia (and Merseburg and Erfurt)	19	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Saxony, Leipzig	20	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Hesse-Nassau, Upper Hesse	21	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Ruhr bassin (Westfalia)	22	0.893	0.083	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Ruhr bassin (Rhine province)	23	0.929	0.047	0.000	0.000	0.000	0.005	0.000	0.000	0.020	0.000
Westfalia, Lippe (and Waldeck)	24	0.983	0.009	0.000	0.000	0.000	0.000	0.000	0.000	0.004	0.000
Rhine province right of the river Rhine	25	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Rhine province left of the river Rhine, Cologne	26	0.981	0.003	0.000	0.002	0.000	0.003	0.000	0.000	0.010	0.000
Saar	27	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Alsace-Lorraine	30	0.841	0.000	0.000	0.107	0.000	0.016	0.000	0.000	0.000	0.000
Bavarian Palatinate (incl. Ludwigshafen, Mannheim)	31	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Hesse (excl. Upper Hesse)	32	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Baden	33	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Württemberg, Hohenzollern	35	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
South Bavaria	36	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
North Bavaria	37	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Russia	50	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	1.000
Kingdom of Poland	51	0.055	0.722	0.000	0.000	0.000	0.000	0.149	0.000	0.000	0.047
Galicia	52	0.030	0.537	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Romania	52a	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Hungary	53	0.098	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Yugoslavia	53a	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Bohemia	54	0.368	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Austria	55	0.579	0.021	0.000	0.000	0.000	0.062	0.000	0.000	0.000	0.000
Switzerland	56	0.650	0.000	0.000	0.200	0.000	0.100	0.000	0.000	0.000	0.000
Italy	57	0.000	0.000	0.000	0.000	0.000	1.000	0.000	0.000	0.000	0.000
France (excl. Alsace-Lorraine)	58	0.000	0.000	0.000	1.000	0.000	0.000	0.000	0.000	0.000	0.000
Luxemburg	59	1.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Belgium	60	0.000	0.000	0.000	0.400	0.000	0.000	0.000	0.000	0.600	0.000
Netherlands	61	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	1.000	0.000
Great Britain	62	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sweden, Norway	63	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Denmark (without Schleswig)	64	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Only regions with a share of more than 1.5% of minority languages were taken into account.

Table 4: (b) Mother tongue: language shares before World War I (1905/1910)

Region	#TD	Sorbian	Czech+Slovak	Walloon	Hungarian	Romanian	Ruthenian	Serbo-Croatian	Slovenian
East Prussia	1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Pomerania	3	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mecklenburg	5	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Schleswig-Holstein, Lübeck	7	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Hanover, Braunschweig, Oldenburg	11	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
West Prussia and Posen	12	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Upper Silesia	13	0.000	0.025	0.000	0.000	0.000	0.000	0.000	0.000
Lower Silesia	15	0.008	0.004	0.000	0.000	0.000	0.000	0.000	0.000
Greater Berlin	16	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Brandenburg	17	0.010	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Anhalt and Magdeburg	18	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Thuringia (+ Merseburg and Erfurt)	19	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Saxony, Leipzig	20	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Hesse-Nassau, Upper Hesse	21	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Ruhr bassin (Westfalia)	22	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Ruhr bassin (Rhine province)	23	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Westfalia, Lippe (and Waldeck)	24	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Rhine province right of the river Rhine	25	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Rhine province left of the river Rhine	26	0.000	0.000	0.002	0.000	0.000	0.000	0.000	0.000
Saar	27	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Alsace-Lorraine	30	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Bavarian Palatinate (+ Ludwig., Mannheim)	31	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Hesse (excl. Upper Hesse)	32	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Baden	33	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Württemberg, Hohenzollern	35	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
South Bavaria	36	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
North Bavaria	37	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Russia	50	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Kingdom of Poland	51	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Galicia	52	0.000	0.000	0.000	0.000	0.031	0.400	0.000	0.000
Romania	52a	0.000	0.000	0.000	0.000	1.000	0.000	0.000	0.000
Hungary	53	0.000	0.094	0.000	0.481	0.141	0.023	0.141	0.000
Yugoslavia	53a	0.000	0.000	0.000	0.000	0.000	0.000	1.000	0.000
Bohemia	54	0.000	0.632	0.000	0.000	0.000	0.000	0.000	0.000
Austria	55	0.000	0.175	0.000	0.000	0.000	0.000	0.063	0.100
Switzerland	56	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Italy	57	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
France (excl. Alsace-Lorraine)	58	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Luxemburg	59	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Belgium	60	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Netherlands	61	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Great Britain	62	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Sweden, Norway	63	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Denmark (without Schleswig)	64	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: Only regions with a share of more than 1.5% of minority languages were taken into account.

There are various sources of the language data. The bulk of data is taken from Belzyt (1998), who provides figures for Prussian provinces and municipalities. These were recalculated as to match the geographical boundaries of the transport districts. For the remaining parts of Germany, I verified that none exceeded a share of more than 1.5% of minority languages, except Alsace-Lorraine. The letter figures stem from the statistical office of Alsace-Lorraine and refer to December 1905. Alsace-Lorraine had set up its own body, because as *Reichsland* it was administered directly from Berlin (*reichsunmittelbar*). Figures for Austria-Hungary were taken from Schulze and Wolf (2006). Polish figures were taken from GUS (2003), p.186.

APPENDIX D

Appendix to Chapter 4

1. Data manipulation

All data used to interpolate and extrapolate monthly IPM were taken from Statistisches Reichsamt (ed.) (1936, 1937), *Statistisches Handbuch der Weltwirtschaft*, Berlin: Verlag für Sozialpolitik, Wirtschaft und Statistik.

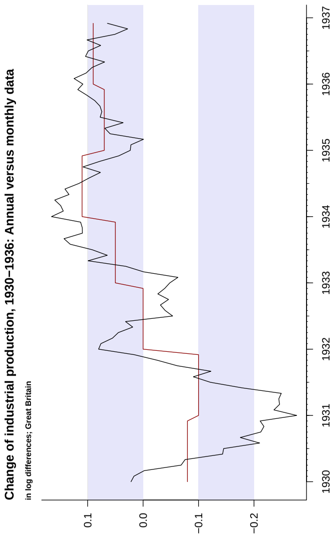
UK: Data on IPM is only available at quarterly frequency. I find the highest contemporaneous correlation between IPM and a composite time series of coal, steel, and iron production indices between 1925 and 1936. The coefficient is 0.83, with insignificant correlations a quarter before and after. The composite index consists of equally weighted time series for each of the above commodities and was averaged over quarters. The variation in the monthly production series was then used to interpolate monthly IPM data.

Japan: Data on IPM is not available for 1925. The contemporaneous correlation between IPM and coal production during the period from 1926 to 1936 was calculated to be 0.63, with no significant correlations at preceding and subsequent lags. The monthly production series was then used to linearly extrapolate monthly IPM in 1925.

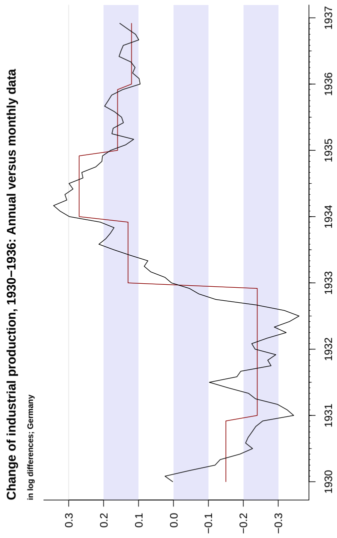
Hungary: There is no IPM data available for the period from 1925 to 1926. The contemporaneous correlation in quarterly figures between IPM and a composite series of brown coal and iron ore production as well as railway shipments was calculated to be only 0.42. In a first step, the composite series for 1925 and 1926 was used to estimate quarterly IPM during this period via a linear regression model. The variation in the monthly production series was then used to interpolate monthly IPM data.

Finland: Data on IPM is not available for 1925 and 1926. In contrast to other countries, there is no data available on production of industrial input and output. Thus, I had to resort to the physical amount of shipments by Finish railways. The contemporaneous correlation in quarterly figures between IPM and shipments was calculated to be 0.41, making it still the most closely correlated series during the period 1927-1936. First the composite series for 1925 and 1926 was used to estimate quarterly IPM during this period via linear regression. The variation in monthly growth rates in railway shipments was then used interpolate monthly IPM data.

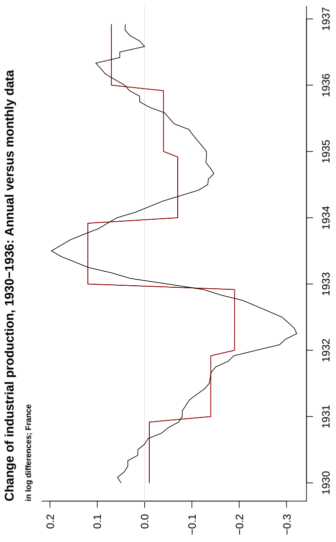
2. Evaluation of the Great Depression by annual and monthly IPM data



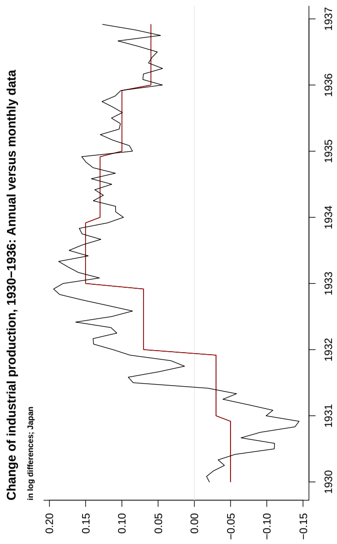
(a) United Kingdom



(b) Germany



(c) France

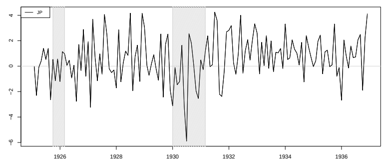


(d) Japan

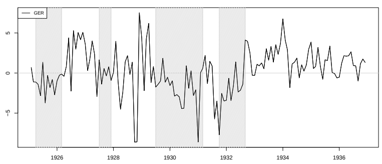
Figure 1: Annual vs. monthly IPM data: UK, Germany, France, and Japan
Sources: Annual data taken from Bernanke and James (1991).

3. Recession dating using the 2-quarters approach

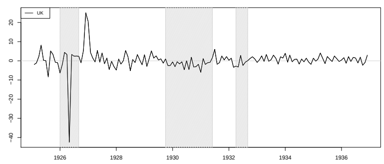
(a) United States



(b) Japan



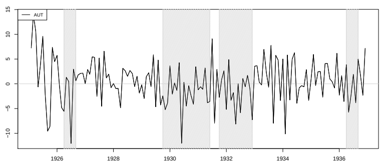
(c) Germany



(d) Britain

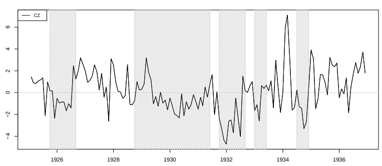
(e) France

(f) Belgium

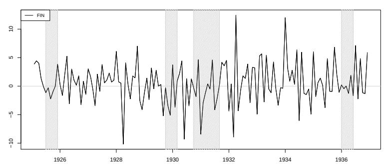


(g) Austria

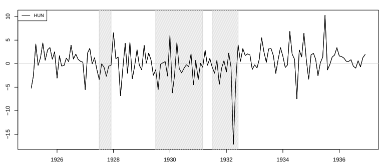
(h) Poland



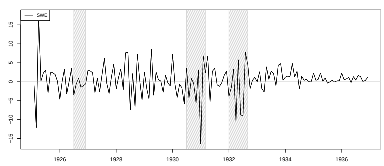
(i) Czechoslovakia



(j) Finland



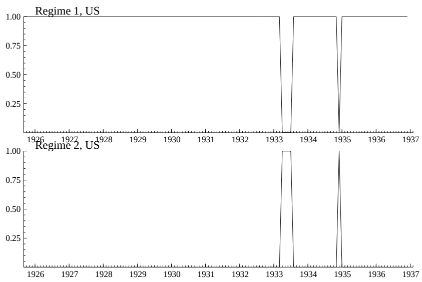
(k) Hungary



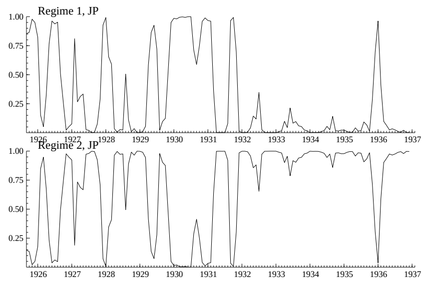
(l) Sweden

Figure 2: Recession dates inferred from 2-quarters approach (% change month to month)

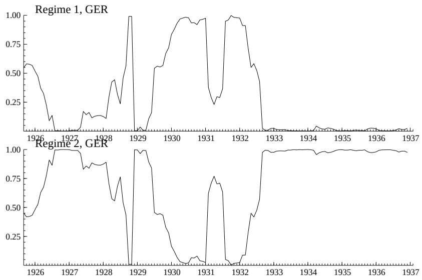
4. Regime probabilities from MSM(2)-AR(7) model



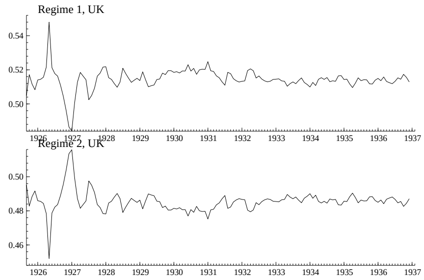
(a) United States



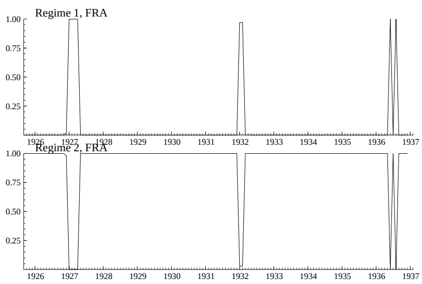
(b) Japan



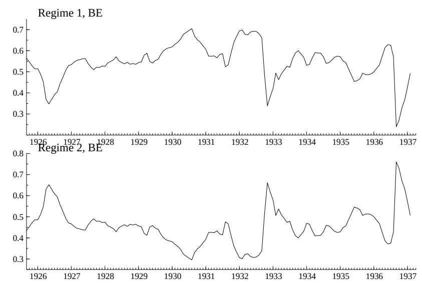
(c) Germany



(d) United Kingdom

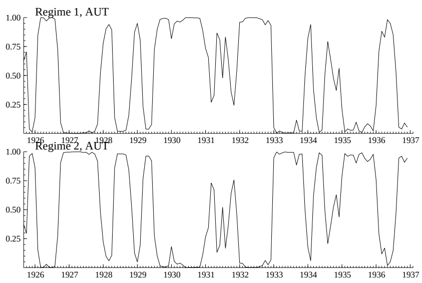


(e) France

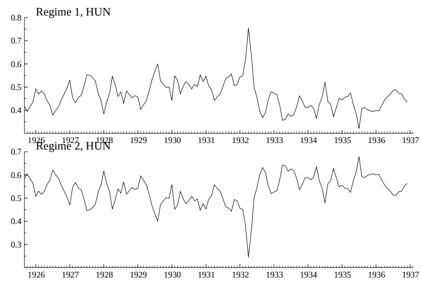


(f) Belgium

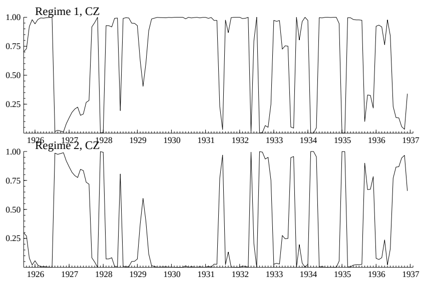
Figure 3: MSM(2)-AR(7) models: US, Japan, and Western European countries



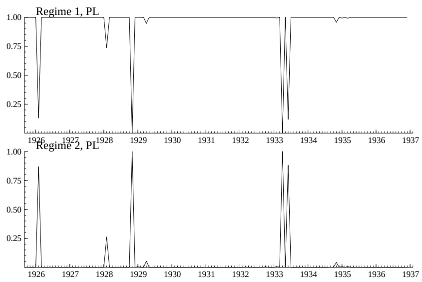
(a) Austria



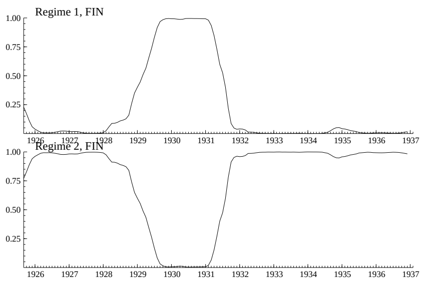
(b) Hungary



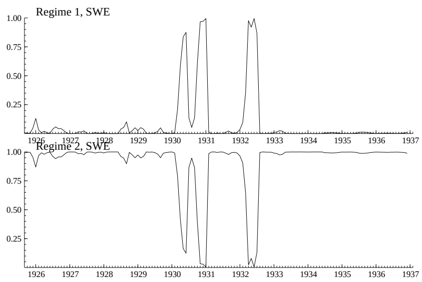
(c) Czechoslovakia



(d) Poland



(e) Finland



(f) Sweden

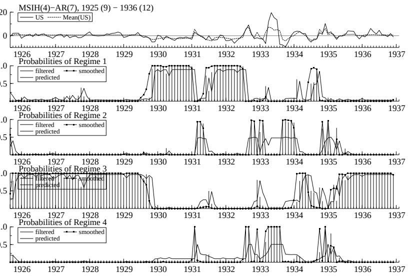
Figure 4: MSM(2)-AR(7) models: Central Eastern European and Scandinavian countries

Table 1: Regimes obtained from an MSM(2)-AR(7)

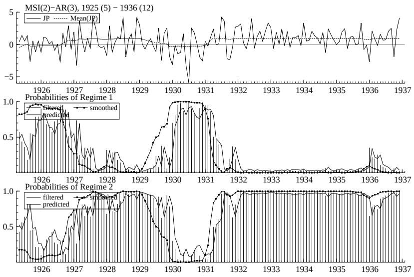
Coefficient	**Germany**	**Austria**	**Japan**	**Finland**	**US**	**Sweden**	**CZ**	**Belgium**	**Poland**	**Hungary**	**France**	**Britain**
Regime-dependent means												
μ_1	-2.29 (1.28)	-1.82	-0.74	-0.86	-0.27	-3.71	-0.78	-0.23	0.04	0.02	-3.53	0.30
		(0.31)	(0.33)	(0.44)	(0.34)	(1.11)	(0.38)	(4.70)	(0.42)	(2.01)	(0.45)	(1.98)
μ_2	1.41 (0.68)	1.99	1.19	0.82	1.15	0.73	2.21	0.97	7.70	0.61	0.16	0.41
		(0.31)	(0.32)	(0.21)	(1.21)	(0.21)	(0.39)	(6.29)	(1.32)	(1.38)	(0.35)	(2.20)
Regime-independent AR coefficients												
α_1	0.23 (0.08)	-0.69	-0.46	-0.41	0.66	-0.50	0.43	-0.26	0.21	0.02	0.57	0.11
		(0.09)	(0.10)	(0.09)	(0.06)	(0.09)	(0.09)	(0.20)	(0.08)	(0.10)	(0.06)	(0.08)
α_2	-0.01 (0.08)	-0.43	-0.39	-0.15	-0.18	-0.28	0.20	-0.11	-0.10	-0.07	0.18	0.03
		(0.10)	(0.11)	(0.09)	(0.08)	(0.10)	(0.09)	(0.19)	(0.09)	(0.09)	(0.07)	(0.08)
α_3	0.04 (0.08)	-0.46	-0.18	-0.16	0.03	-0.06	-0.21	-0.09	0.45	-0.03	0.21	0.06
		(0.09)	(0.10)	(0.09)	(0.08)	(0.09)	(0.07)	(0.16)	(0.10)	(0.09)	(0.08)	(0.08)
α_4	0.11 (0.09)	-0.49	0.08	-0.19	-0.14	-0.01	0.36	-0.04	-0.00	-0.03	-0.01	0.09
		(0.09)	(0.09)	(0.09)	(0.07)	(0.08)	(0.08)	(0.13)	(0.08)	(0.09)	(0.08)	(0.08)
α_5	-0.14 (0.08)	-0.26	0.23	-0.10	-0.03	0.15	0.21	0.05	0.04	0.12	-0.02	0.11
		(0.09)	(0.08)	(0.09)	(0.08)	(0.07)	(0.09)	(0.10)	(0.09)	(0.09)	(0.09)	(0.08)
α_6	0.11 (0.09)	-0.26	0.43	-0.15	-0.10	0.13	-0.16	-	-0.09	0.05	-0.05	0.02
		(0.08)	(0.09)	(0.09)	(0.07)	(0.07)	(0.07)		(0.07)	(0.08)	(0.08)	(0.08)
α_7	0.10 (0.09)	-0.07	0.27	-0.06	0.21	-0.01	-0.15	-	0.03	-0.07	-0.08	-0.31
		(0.07)	(0.09)	(0.09)	(0.06)	(0.07)	(0.07)		(0.07)	(0.08)	(0.07)	(0.08)
Persistence of regimes: transitions probabilities and expected duration of recessions												
p_{11}	0.895	0.795	0.703	0.945	0.984	0.635	0.871	0.778	0.968	0.724	0.492	0.759
duration	10	5	3	18	62	3	8	5	31	4	2	4
p_{22}	0.959	0.810	0.876	0.986	0.598	0.965	0.715	0.734	0.000	0.760	0.968	0.744
σ^2	5.02	8.27	1.76	10.85	4.71	8.70	1.21	35.06	4.53	9.00	0.59	23.23
# obs.	136	136	136	136	136	136	136	136	136	136	136	136
ln$L$	308.84	253.13	362.39	266.13	312.65	270.83	372.38	193.21	313.72	282.95	445.82	219.41
LM rejected	yes	yes	yes	yes	yes	yes	yes	no	yes	no	yes	no

Notes: Standard errors in parenthesis. "LM rejected" refers to the rejection of the H_0 of a linear model using the likelihood ratio test. It was not possible to obtain a model with seven lags for Belgium. Thus, the result of an MSM(2)-AR(5) model is provided, because it was the closest model, for which the algorithm converged.

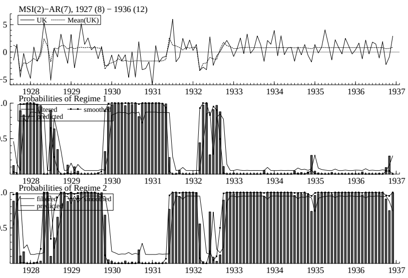
5. Results of country-specific MS-AR models



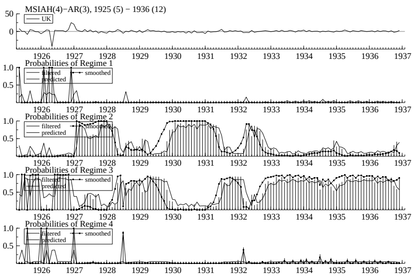
(a) United States



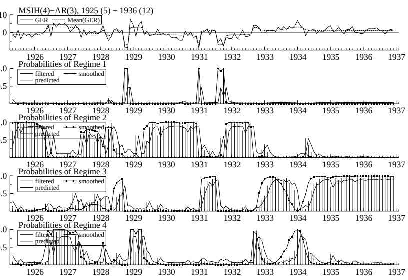
(b) Japan



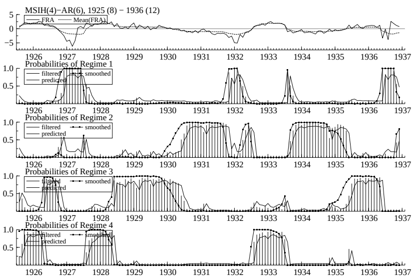
(c) United Kingdom, 1927-1936



(d) United Kingdom

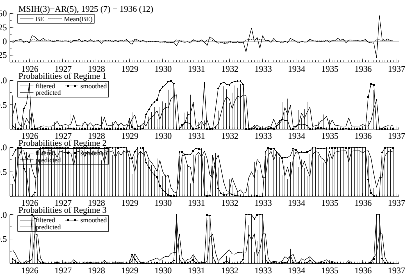


(e) Germany

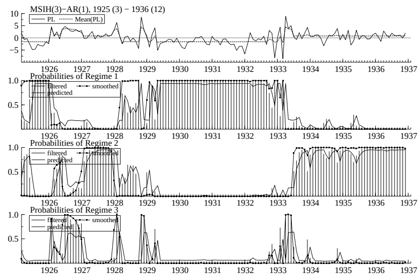


(f) France

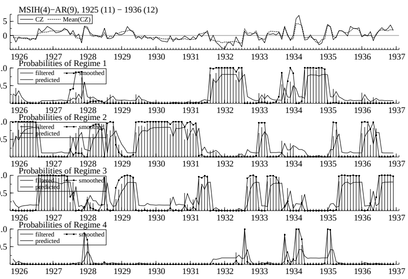
Figure 5: Individual MS-AR models: US, Japan, and Western European countries



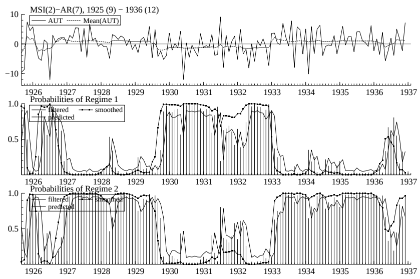
(a) Belgium



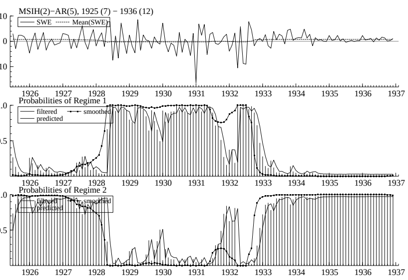
(b) Poland



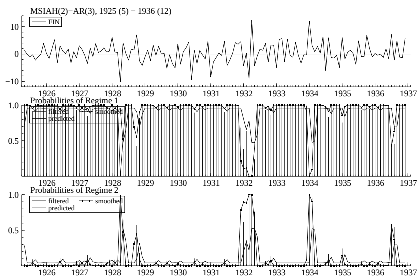
(c) Czechoslovakia



(d) Austria



(e) Sweden



(f) Finland

Figure 6: Individual MS-AR models: Belgium, Eastern European countries and Scandinavia

Table 2: Overview of months in recession from dating

Country	1929											
	1	2	3	4	5	6	7	8	9	10	11	12
Poland	r	r	r	r	r	r	r	r	r	r	r	r
Sweden				r	r	r	r	r	r	r	r	r
Germany					r	r	r	r	r	r	r	r
CSR						r	r	r	r	r	r	r
Japan								r	r	r	r	r
Britain									r	r	r	r
Austria									r	r	r	r
Belgium										r	r	r
US										r	r	r
France												

Country	1930											
	1	2	3	4	5	6	7	8	9	10	11	12
Poland	r	r	r	r	r	r	r	r	r	r	r	r
Sweden	r	r	r	r	r	r	r	r	r	r	r	r
Germany	r	r	r	r	r	r	r	r	r	r	r	r
CSR	r	r	r	r	r	r	r	r	r	r	r	r
Japan	r	r	r	r	r	r	r	r	r	r	r	r
Britain	r	r	r	r	r	r	r	r	r	r	r	r
Austria	r	r	r	r	r	r	r	r	r	r	r	r
Belgium	r	r	r	r	r							
US	r	r	r	r	r	r	r	r	r	r	r	r
France			r	r	r	r	r	r	r	r	r	r

Country	1931											
	1	2	3	4	5	6	7	8	9	10	11	12
Poland	r	r	r	r	r	r	r	r	r	r	r	r
Sweden	r	r	r	r	r	r	r	r	r	r	r	r
Germany	r							r	r	r	r	r
CSR	r	r	r					r	r	r	r	r
Japan	r	r										
Britain	r	r	r	r	r							
Austria	r	r	r	r	r	r	r	r	r	r	r	r
Belgium				r					r	r	r	r
US	r		r	r	r	r	r	r	r	r	r	r
France	r	r	r	r	r	r	r	r	r	r	r	r

Country	1932												1933		
	1	2	3	4	5	6	7	8	9	10	11	12	1	2	3
Poland	r	r	r	r	r	r	r	r	r	r	r	r	r	r	r
Sweden	r	r	r	r	r	r	r	r	r						
Germany	r	r	r	r	r	r	r	r							
CSR	r	r	r	r	r	r	r						r	r	r
Japan															
Britain			r	r	r	r	r	r							
Austria	r	r	r	r	r	r	r	r	r	r	r	r	r		
Belgium	r	r	r	r	r	r									
US	r	r	r	r	r	r	r			r	r		r	r	
France	r	r	r	r	r	r									

Notes: This table shows the dichotomous distinction of recession ('r') and expansion summarizing table 4 and table 5.

Table 3: Contingency table for pair US-Germany

No. months in recessions		**Germany** Boom	Recession	Margin
	Boom	$n_{00} = 64$	$n_{01} = 25$	$n_{0.} = 89$
United States	Recession	$n_{10} = 20$	$n_{11} = 29$	$n_{1.} = 49$
	Margin	$n_{.0} = 84$	$n_{.1} = 54$	$n = 138$

The motivation for using Pearson's contingency coefficient (PCC) is the ability to infer on the regularity of association and not only to test whether two variables are unassociated (by a chi-square test). To exemplify the calculation of PCC I compute the coefficient for Germany and the United States. The PCC is given in percentage terms and normalized to the interval 0 to 100. For a pair of countries (country i, country j) over the sample period, I obtain a 2×2 contingency table, which summarizes the coincidence of economic states (for an example see table 3).

Pearson's contingency coefficient is defined as (figures are calculated on the basis of the example above)

$$CC = \sqrt{\frac{\hat{\chi}^2}{n + \hat{\chi}^2}} = \sqrt{\frac{12.83}{138 + 12.83}} = 0.29 \tag{5.1}$$

where

$$\hat{\chi}^2 = \sum_{i=0}^{1} \sum_{j=0}^{1} \frac{\left(n_{ij} - (n_{i.}n_{.j}/n)\right)^2}{n_{i.}n_{.j}/n} = 12.83 \tag{5.2}$$

represents the chi-square test statistic. The null hypothesis that the German and US chronology was unassociated can thus be rejected at the 5% significance level as the critical value is given by $\chi^2(1) = 3.84$.

In order to obtain the corrected measure CC_{corr} the contingency coefficient is divided by the square root of 0.5 and multiplied by 100. In the case of Germany and the United States this correction yields a contingency coefficient of 41.2. This implies in turn that PCC below 23.3 indicate that the null hypothesis cannot be rejected at the 5% significance level as $CC_{corr}(3.84) = 100 \sqrt{\frac{3.84}{138+3.84}}\, 0.5^{-0.5} = 23.27$.

6. Properties and correlation of HP series

Table 4: International output correlations, 1925–1936

	Hungary	Finland	UK	Germany	Austria	Belgium	Poland	CZ	France	Sweden	US
Japan	0.189	0.664	0.472	0.435	0.431	0.339	0.403	0.202	0.063	0.433	0.424
Hungary		0.519	0.243	0.734	0.617	0.644	0.644	0.678	0.541	0.694	0.747
Finland			0.405	0.641	0.603	0.532	0.598	0.427	0.213	0.608	0.567
United Kingdom				0.568	0.610	0.223	0.497	0.562	−0.104	0.480	0.344
Germany					0.674	0.680	0.899	0.797	0.348	0.708	0.690
Austria						0.528	0.647	0.748	0.289	0.746	0.638
Belgium							0.692	0.620	0.458	0.530	0.648
Poland								0.805	0.385	0.633	0.711
Czechoslovakia									0.434	0.718	0.670
France										0.404	0.670
Sweden											0.621

Notes: Cross correlations based on HP filtered series, i.e. business cycle component.

Table 5: Properties of fluctuations in IPM, 1925–1936

	Japan	Hungary	Finland	UK	Germany	Austria	Belgium	Poland	CZ	France	Sweden	US
Std.Dev.	0.047	0.076	0.072	0.123	0.131	0.097	0.096	0.117	0.087	0.082	0.075	0.110
AC(1)	0.927	0.914	0.864	0.887	0.978	0.865	0.806	0.968	0.964	0.976	0.856	0.940
AC(12)	0.205	0.251	0.412	−0.205	0.391	0.202	0.164	0.372	0.378	−0.123	0.241	0.159

Notes: Series after removing trend component with HP filter.

7. Features of the sample countries

Table 6: Country characteristics

Characteristics	**Germany**	**Austria**	**Japan**	**US**	**Sweden**	**Czechoslovakia**	**Belgium**	**Poland**	**France**	**Britain**
Workforce outside agriculture, 1920										
% of total	77	68	–	–	56	61	80	32	61	90
Share in world production of steel ingots and castings, 1937										
% of total	15.2	0.5	–	38.5	0.8	1.7	3.0	1.2	6.1	10.2
Share in world GDP, 1940 (Geary-Khamis dollars)										
% of total	8.4	0.6	4.7	20.6	0.7	0.9	0.8	1.3	3.7	7.3
Imports relative to exports, 1925-29										
% surplus	+16	–	–	–	+3	-9	–	+11	+2	+55
Financial effect of Hoover moratorium: Net loss / gain										
'000,000 of £	+77.0	+0.3	-0.6	-53.6	–	+1.2	-2.4	–	-16.1	-9.7

Source: Data on workforce and imports taken from Svennilson (1954). Shares in world steel production calculated on the basis of Svennilson (1954). GDP shares calculated on the basis of Maddison (2007). Remaining data taken from Eichengreen (1992).

APPENDIX E

Appendix to Chapter 5

1. Wages and prices in the US, Japan, and Europe, 1925-1936

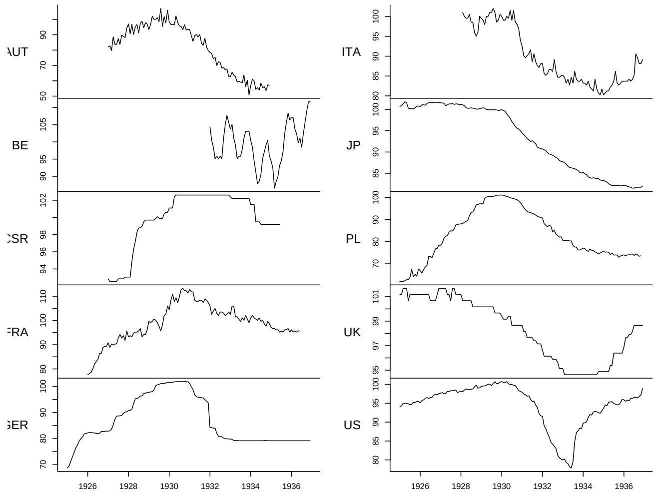


Figure 1: Nominal wages in the US, Japan, and Europe (by country, 1929=100), Belgium (1932=100).

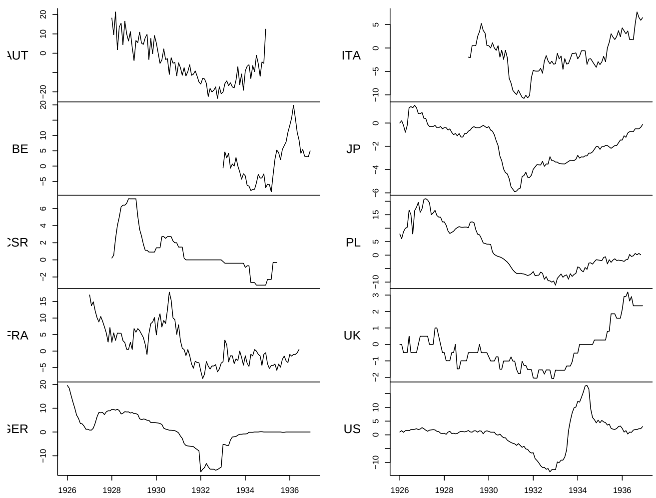


Figure 2: Nominal wages in the US, Japan, and Europe (by country, yoy differences in %)

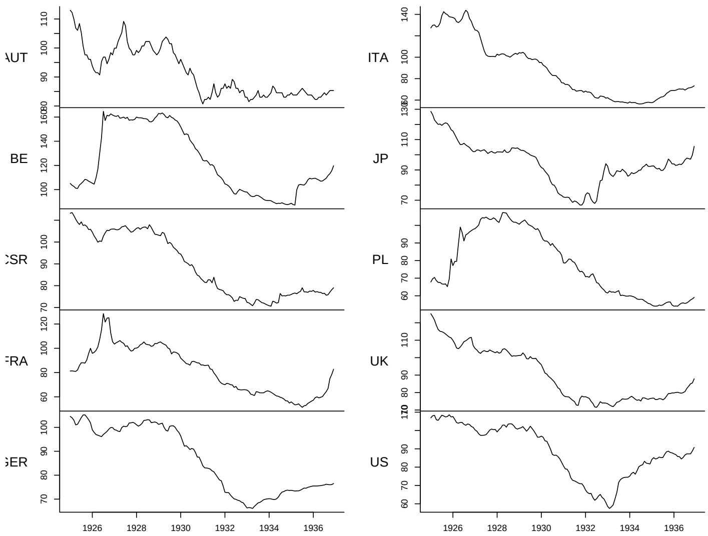


Figure 3: Index data of wholesale prices for the US, Japan, and Europe (by country, 1929=100), Belgium (1932=100)

Table 1: Annual averages of nominal wages

	AUT	BE	CSR	FRA	GER	ITA	PL	UK	US	JP
1925	NA	NA	NA	NA	80.4	NA	76	100.6	98.4	99.4
1926	NA	NA	NA	123.4	86.9	NA	85.7	100.4	100	100
1927	135.8	NA	922.1	135.4	92	NA	100	100.7	101.6	99.9
1928	150.4	NA	967.7	139.6	100	NA	109.9	99.8	102.4	99.1
1929	157.8	NA	994.1	147.6	105.5	101.5	119	99.3	103.7	98.6
1930	151.9	NA	1016.1	162.3	107.3	100.5	118.8	98.3	103.4	96.2
1931	137.2	NA	1020	161	101.8	91	110.9	97	99	91.3
1932	112.4	151.3	1020	152.6	86.2	87.5	102	95.3	88	88.1
1933	95	151.8	1016.2	150.1	83.6	85.5	93.8	94	85.6	85.1
1934	89.4	143.7	992.5	147.6	83.5	83.5	90.2	94.1	95.1	82.9
1935	NA	146.2	NA	141.8	83.5	83.9	88.5	95.1	98.3	81.3
1936	NA	158.9	NA	NA	83.5	87.5	NA	97.5	100	80.7
Total percentage change in 4 consecutive years after peak (in 1929 or 1930). Peak in										
1929	-40%					-16%	-21%	-5%	-17%	-14%
1930				-9%	-22%					

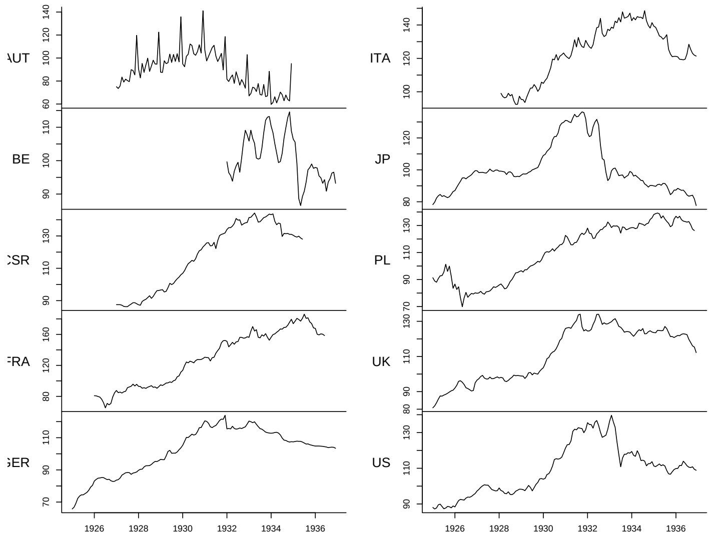


Figure 4: Real wages in the US, Japan, and Europe (by country, 1929=100). Belgium (1932=100). Nominal wages deflated by respective wholesale price index. Japanese wage data deflated by 'Tokyo Wholesale Price Index'.

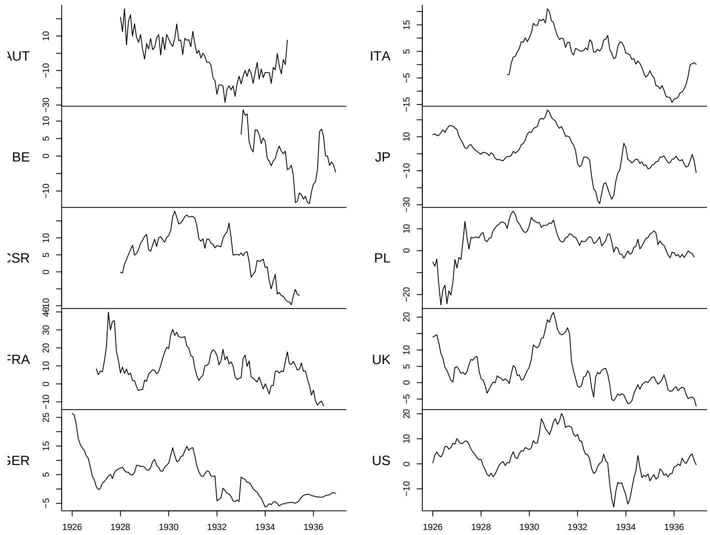


Figure 5: Real wages in the US, Japan, and Europe (by country, yoy differences in %)

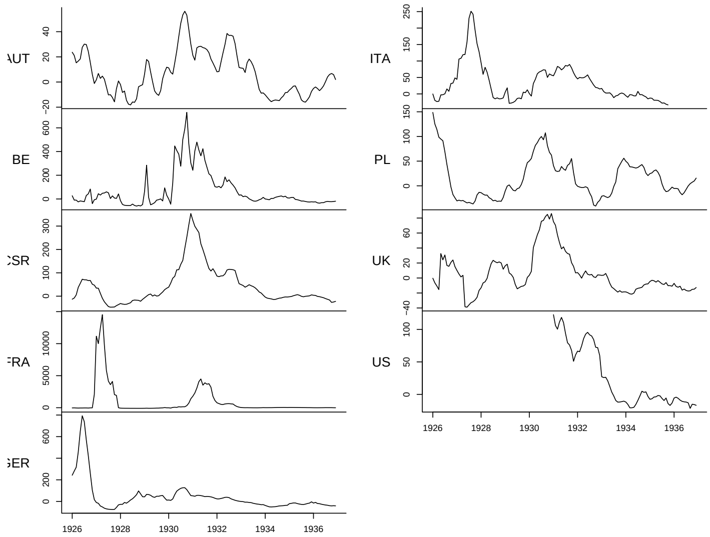


Figure 6: Change in unemployed trade union members in the US and Europe (by country, yoy differences in %)

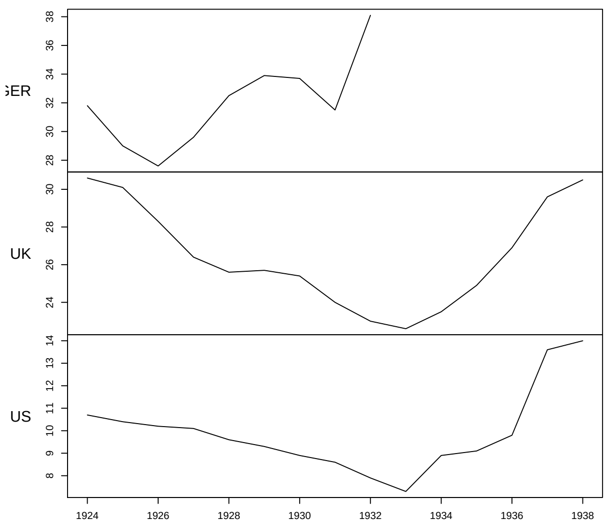
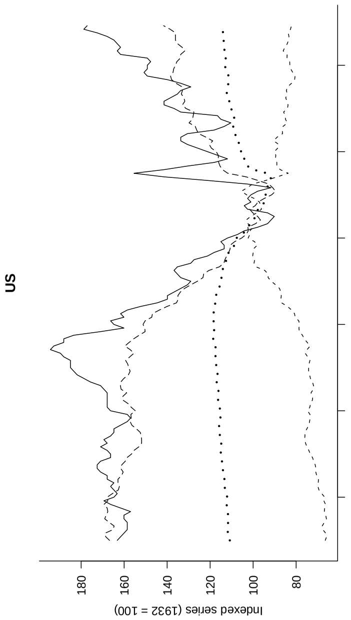
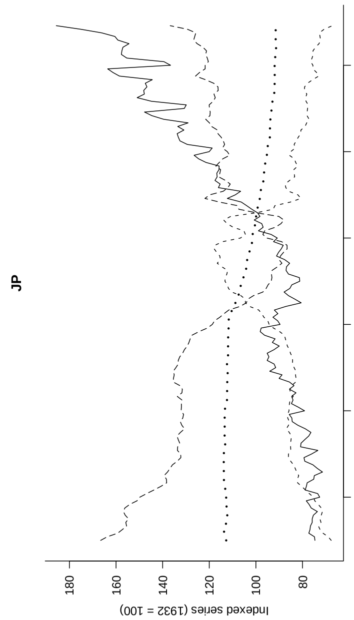


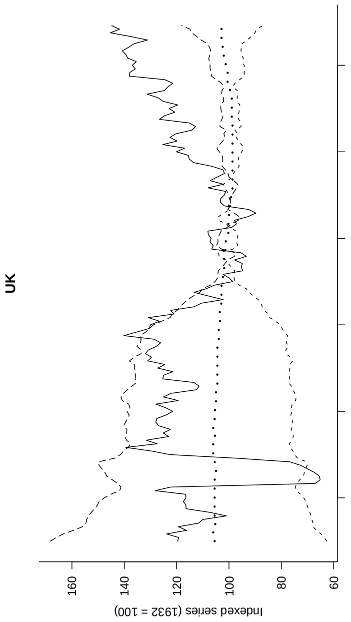
Figure 7: Union density in selected countries (proportion in %)
Source: Eichengreen and Hatton (1988), Table 1.7, corrected with figures from Bain and Price (1978).



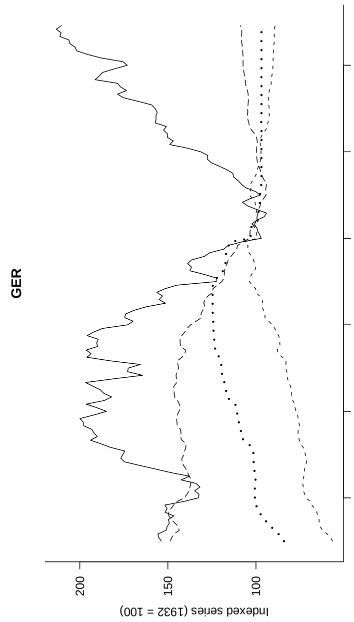
(a) United States



(b) Japan

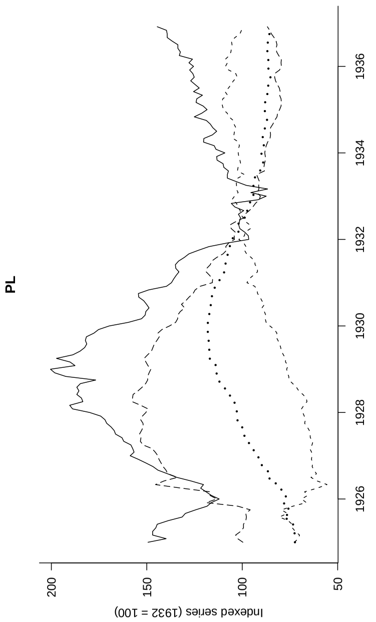


(c) United Kingdom

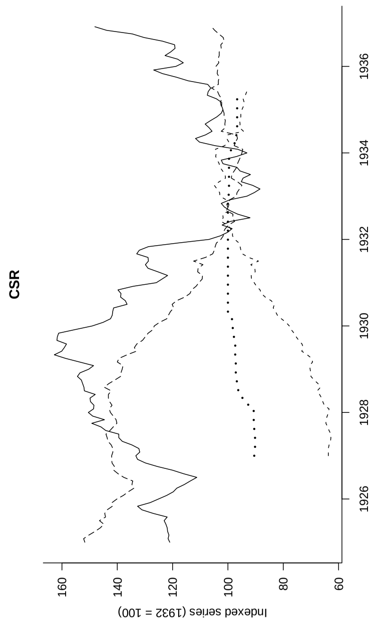


(d) Germany

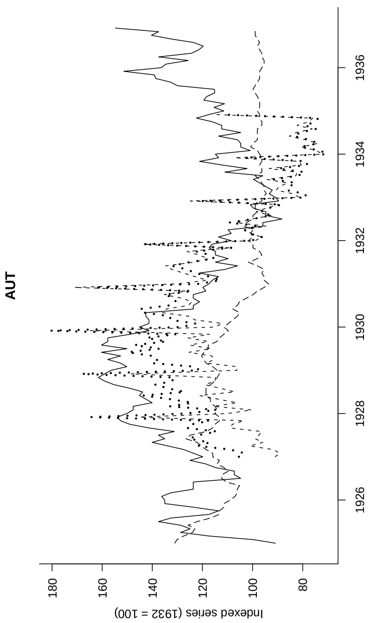
Figure 8: IPM (*solid*), wholesale prices (*longdashed*), nominal wages (*dotted*), and real wages (*dashed*): US, Japan, UK, and Germany.



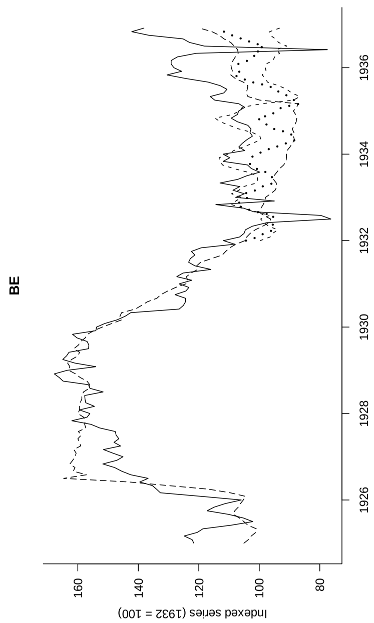
(a) Poland



(b) Czechoslovakia



(c) Austria



(d) Belgium

Figure 9: IPM (*solid*), wholesale prices (*longdashed*), nominal wages (*dotted*), and real wages (*dashed*): Poland, CSR, Austria, and Belgium.

2. Motivation of the autoregressive model

In the AS-AD model, supply is based on an output function of labor.

$$q = f(L) \tag{2.1}$$

Firms will demand labor up to the point where the real wage equals the marginal product of labor. Aggregate supply depends on real labor costs. In practice, wage contracts refer to *nominal* wage rates. Thus, the AS curve depends on a function $H()$ of both nominal wages and prices.

$$q = H\left(\frac{w}{p}\right) \tag{2.2}$$

Changes in wages or prices will affect output. If nominal wages are inflexible or "sticky" and do not accommodate to changes in goods' prices, output depends solely on prices, i.e. monetary shocks have persistent real effects.

The model in ES (1985) assumes a functional form for $H()$ and introduces time to equation 2.2. Suppose that output q depends negatively on the real wage, i.e. in the nominal wage w and positively on the price level p (all variables in logs)

$$q_t = -\alpha\left(w_t - p_t\right) \tag{2.3}$$

The nominal wage is assumed to be perfectly sticky. Hence

$$w_t = \bar{w} \tag{2.4}$$

$$q_t = -\alpha\left(\bar{w} - p_t\right) \tag{2.5}$$

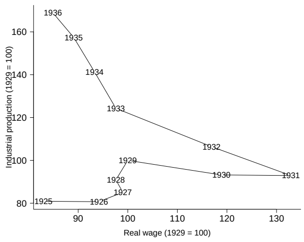
This Keynesian model predicts a negative relationship between real wages and output. The aggregate supply equation (2.5) follows from plugging equation (2.4) into (2.3). In order to justify the Keynesian approach and as a cross-check for equation (2.5),

Because ES (1985) regress output on *real* wages, it is unclear if deflation worked through the nominal wage channel or via prices. Bernanke and Carey (1996) address this identification issue by formulating an aggregate supply equation, in which prices and the *nominal* wage enter separately. In order to tell the respective aggregate supply effects apart, adjustment of the nominal wage w_t is assumed to follow the relationship

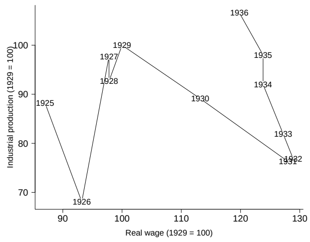
$$w_{i,t} = \lambda_p p_{i,t} + \lambda_w w_{i,t-1} - \gamma u_{i,t} - \theta\left(\Delta u_{i,t}\right) + \epsilon_{i,t}^{w} \tag{2.6}$$

Equation (2.6) explains the current wage by its past values and by current values of the price level p_t and the unemployment rate u_t in country i.

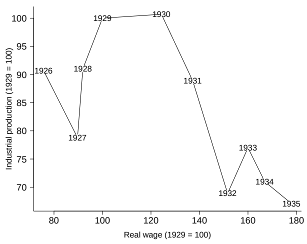
3. Real wages versus real output



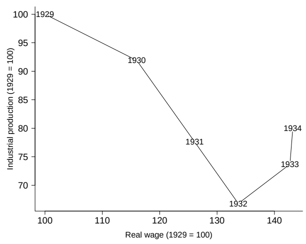
(a) Japan



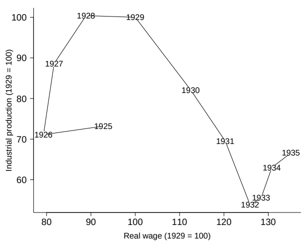
(b) United Kingdom



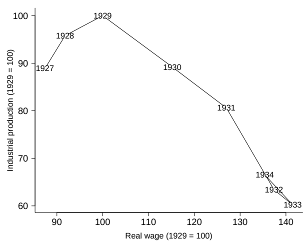
(c) France



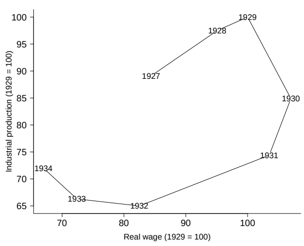
(d) Italy



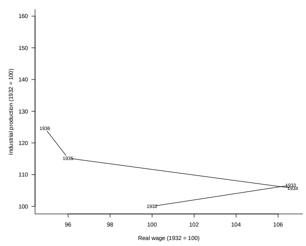
(e) Poland



(f) Czechoslovakia

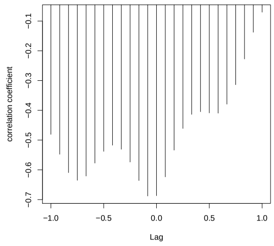


(g) Austria

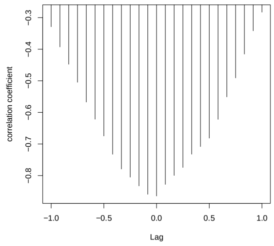


(h) Belgium

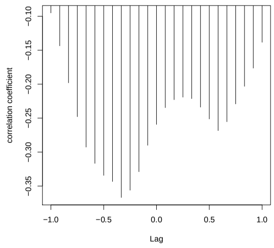
Figure 10: Phase plots of real wages versus IPM (1929=100), Belgium (1932=100)



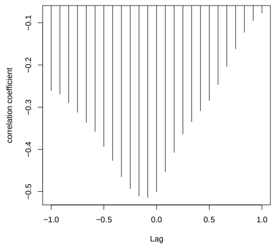
(a) United States



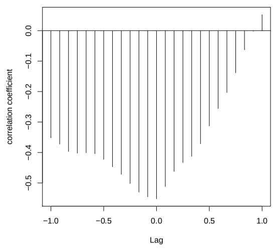
(b) Japan



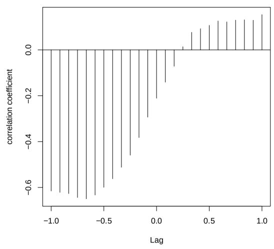
(c) United Kingdom



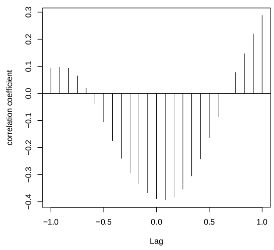
(d) Germany



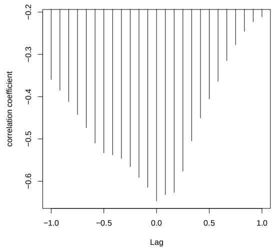
(e) Czechoslovakia



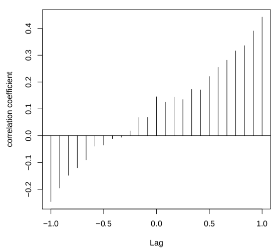
(f) Poland



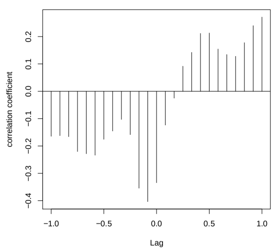
(g) France



(h) Italy



(i) Austria

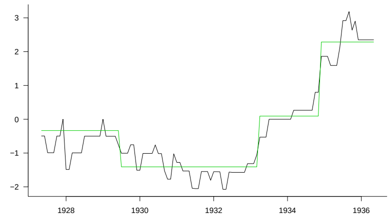


(j) Belgium

Figure 11: Cross correlation of yoy changes in real wage and in IPM (time lag in years)

4. Structural breaks, conditional wage series, and rigidity parameters

(a) United States

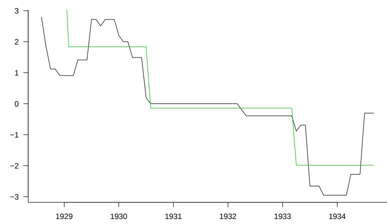


(b) United Kingdom

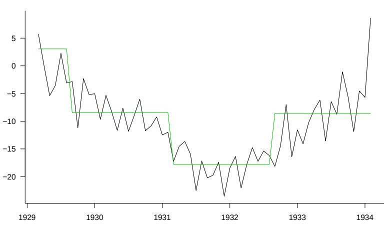
(c) Japan

(d) Germany

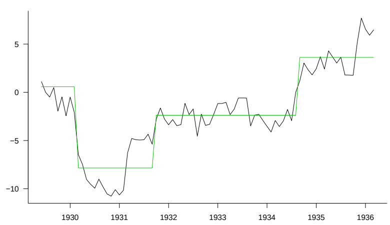
(e) Poland



(f) Czechoslovakia



(g) Austria



(h) Italy

Figure 12: Dating of structural breaks in nominal wage formation

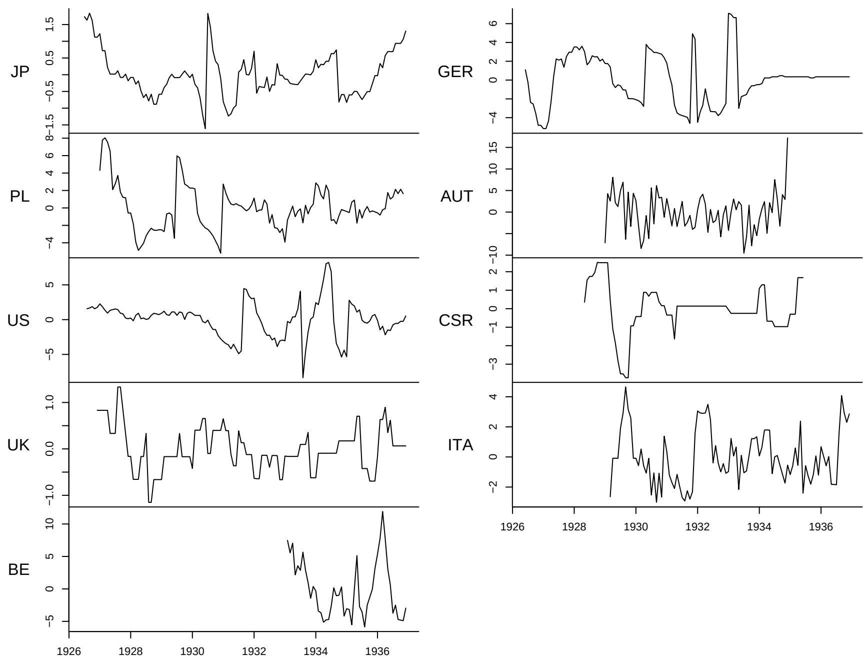


Figure 13: Wage growth by country, conditional %yoy differences

Table 2: Testing for multiple shifts in mean wage growth (minimum sample restriction)

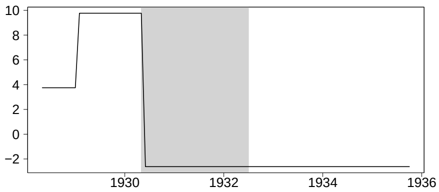
	AUT	BE	CSR	GER	ITA	JP	PL	US	UK
Break date estimates and 90% confidence intervals									
no. breaks, m	2	1	2	3	2	3	3	4	4
	1930(9)	1935(2)	NA	1929(5)	1932(5)	1930(5)	1929(4)	1929(11)	1928(6)
$m = 1$	1930(11)	1935(4)	27	1929(8)	1932(6)	1930(6)	1929(6)	1930(3)	1928(8)
	1931(1)	1935(5)	NA	1929(11)	1934(5)	1930(7)	1929(8)	1930(4)	1929(7)
	1931(10)		52	1931(2)	1935(2)	1932(1)	1931(1)	1931(10)	1931(1)
$m = 2$	1933(4)		58	1931(4)	1935(4)	1932(2)	1931(2)	1931(11)	1931(3)
	1934(3)		59	1931(5)	1935(5)	1932(7)	1931(5)	1931(12)	1931(6)
				1932(11)		1934(7)	1934(5)	1933(6)	1933(6)
$m = 3$				1932(12)		1934(9)	1934(6)	1933(7)	1933(8)
				1933(2)		1934(10)	1934(7)	1933(8)	1933(9)
								1935(2)	1935(3)
$m = 4$								1935(3)	1935(4)
								1935(9)	1935(5)
Means during subperiod									
1	1.2	-5.5	2.4	7	-5.4	-0.5	12.9	1.2	-0.1
2	-14.3	7.7	-0.1	1.4	-2.4	-4.6	1.1	-2.5	-0.8
3	-9.3		-1.8	-11.8	3.4	-3.2	-7.3	-10.2	-1.6
4				-0.8		-1.4	-1.5	9.3	-0.1
5								2.2	2.1

(a) United States

(b) United Kingdom

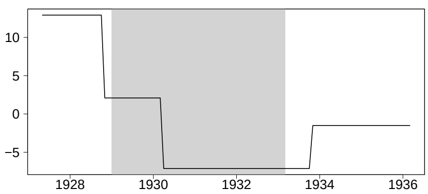
(c) Japan

(d) Germany

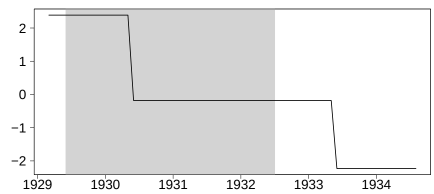


(e) France

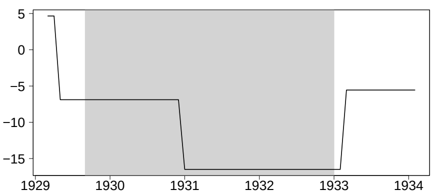
(f) Italy



(g) Poland



(h) Czechoslovakia



(i) Austria

(j) Belgium

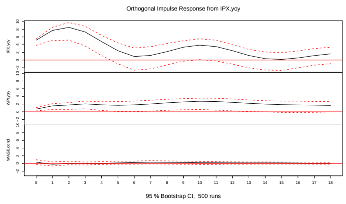
Figure 14: Structural breaks wage formation (growth rate in %) vs the business cycle (grey area).

5. International evidence on the impact of wage rigidity

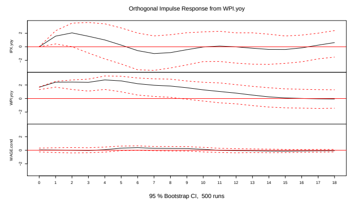
Table 3: Wage rigidity, price changes, recessions, and recovery

	JP	PL	US	UK	BE	GER	AUT	CSR
Wage rigidity estimates								
$\hat{\rho}_i^*$	0.71	0.78	0.65	0.72	0.46	0.60	0.45	0.78
Price changes during the 1930's recession and during recovery								
Beginning of recession	*Aug* 1929	*Jan* 1929	*Oct* 1929	*Sep* 1929	*Oct* 1929	*May* 1929	*Sep* 1929	*Jun* 1929
Beginning of recovery	*Feb* 1931	*Mar* 1933	*Feb* 1933	*Aug* 1932	*Jun* 1932	*Aug* 1932	*Jan* 1933	*Jul* 1932
Prices, yoy, avg. during recession	−14.86	−10.05	−13.00	−9.43	−13.45	−9.26	−4.34	−9.34
Std dev	6.57	4.04	4.46	5.92	4.83	3.82	6.67	2.08
Prices, yoy, avg. during recovery	4.35	−6.05	11.44	0.68	−5.83	−0.07	−0.65	−0.38
Std dev	17.07	4.36	14.63	6.99	6.92	7.11	5.53	5.97
Severity of recession and pace of recovery: State-dependent intercept ν_i, respective MS-AR model								
nu1	−0.42	−1.86	−2.70	0.25	−2.03	*-6.84*	−2.89	−1.62
nu2	1.42	0.92	*-0.70*	−0.91	0.56	−1.31	2.15	−0.83
nu3		4.33	0.79	1.10	2.56	1.08		1.33
nu4			4.91	*-2.01*		2.96		3.54
Duration of recession, # months	19	51	37	27	19	34	41	35

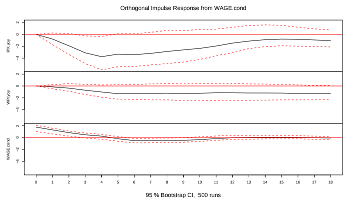
Notes: Price changes are annual rates during the recession dated in chapter 4. The period of recovery is defined as the first 36 months after the end of recession.



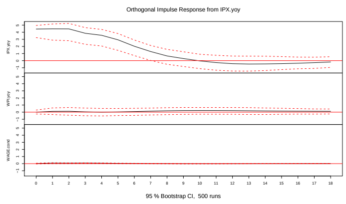
(a) US (IPM)



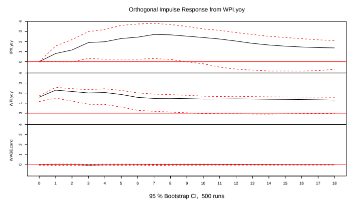
(b) US (WPI)



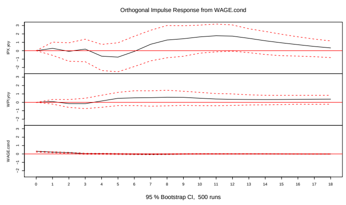
(c) US (WAGE)



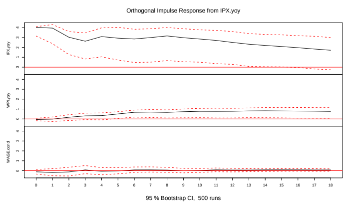
(d) UK (IPM)



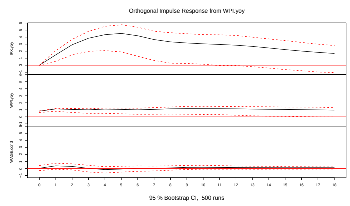
(e) UK (WPI)



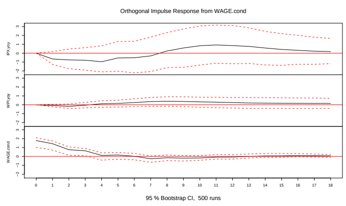
(f) UK (WAGE)



(g) GER (IPM)

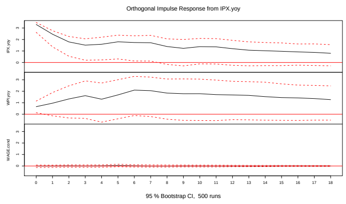


(h) GER (WPI)

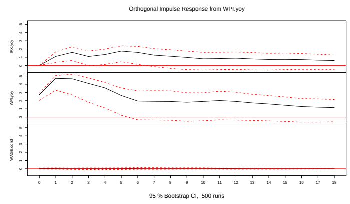


(i) GER (WAGE)

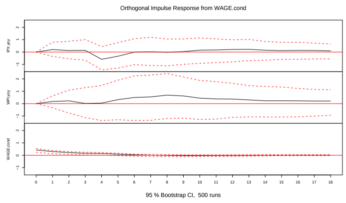
Figure 15: VAR results following shock in output (IPM), nominal wages (WAGE), and prices (WPI)



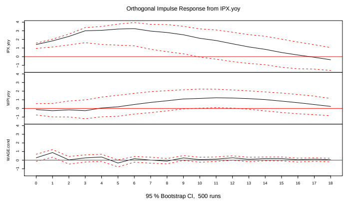
(a) JP (IPM)



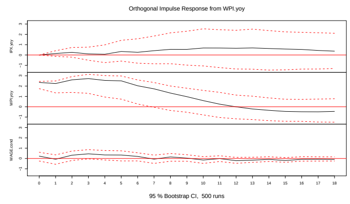
(b) JP (WPI)



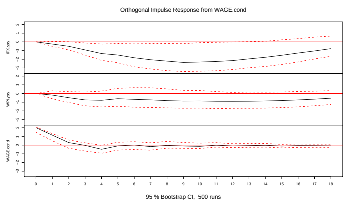
(c) JP (WAGE)



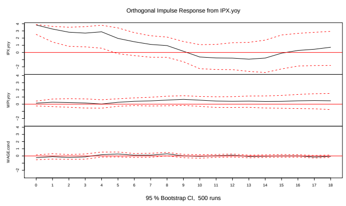
(d) FRA (IPM)



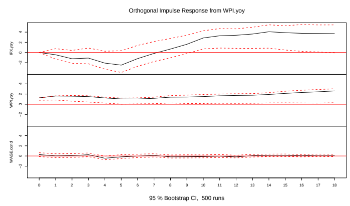
(e) FRA (WPI)



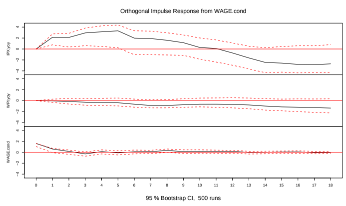
(f) FRA (WAGE)



(g) ITA (IPM)

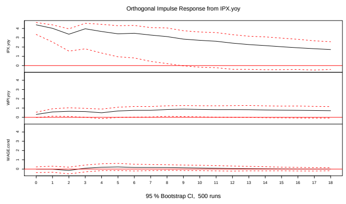


(h) ITA (WPI)

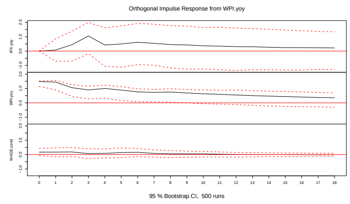


(i) ITA (WAGE)

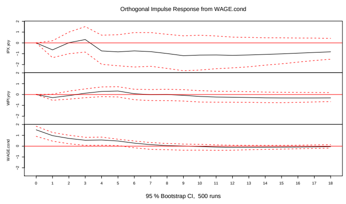
Figure 16: VAR results following shock in output (IPM), nominal wages (WAGE), and prices (WPI)



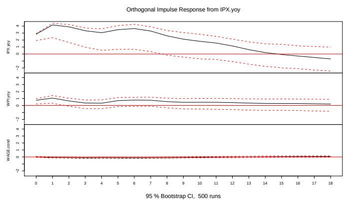
(a) PL (IPM)



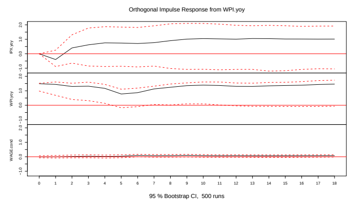
(b) PL (WPI)



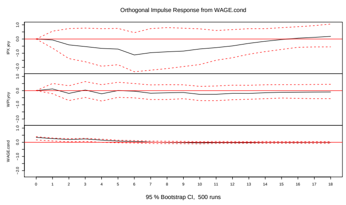
(c) PL (WAGE)



(d) CSR (IPM)



(e) CSR (WPI)



(f) CSR (WAGE)

Figure 17: VAR results following shock in output (IPM), nominal wages (WAGE), and prices (WPI)

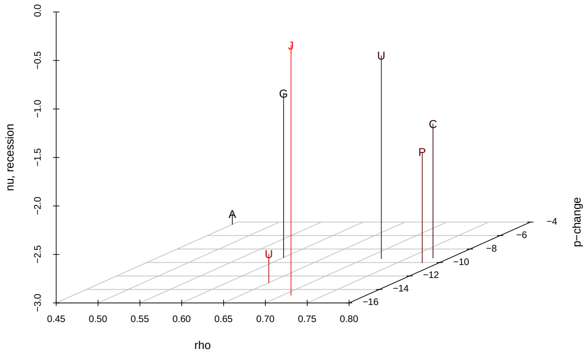


Figure 18: Interaction of wage rigidity, deflation and depression. A (AUT), C (CSR), J (JP), G (GER), P (PL), U (US, UK)

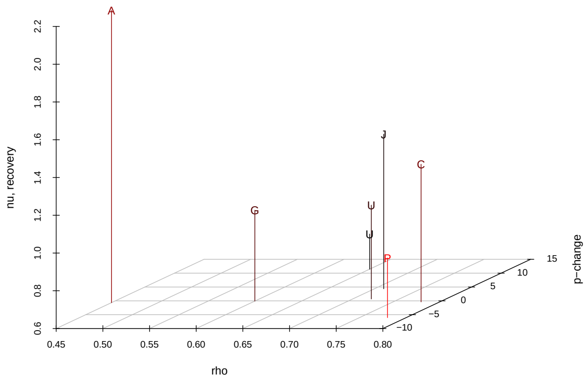


Figure 19: Interaction of wage rigidity, reflation and recovery

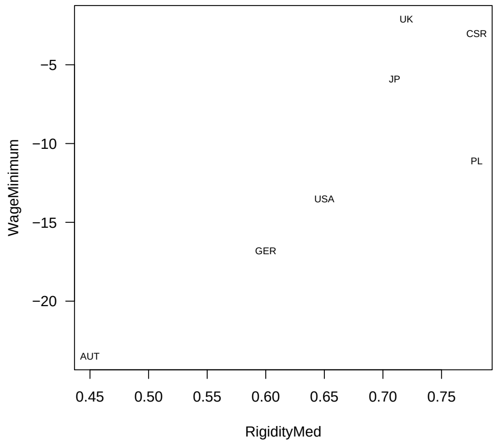


Figure 20: Extreme values in negative wage growth during the Great Depression vs $\hat{\rho}_i^*$ (cf. table 1)

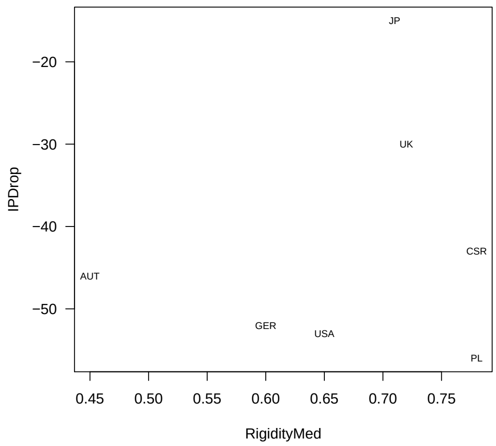


Figure 21: Severity of the crisis (as % drop in IPM) vs $\hat{\rho}_i^*$ (cf. table 1, section 2)

APPENDIX F

Zusammenfassung

1. Deutsche Zusammenfassung

In den ersten beiden Teilen dieser Arbeit bewerte ich die Auswirkung des Ersten Weltkriegs auf die wirtschaftliche Integration Zentraleuropas, insbesondere zwischen Deutschland und Polen. Als Maß für interregionale Integration verwende ich aus praktischen Gründen die Handelsintegration. Einerseits sind Handelsströme statistisch gut erfasst und ihre Integration ist gut messbar, andererseits sind sie ein brauchbarer Indikator für den Grad interregionaler Arbeitsteilung.

Als der Erste Weltkrieg zu Ende ging, nutzten in Zentraleuropa viele nationale Gruppen die Möglichkeit eigene Staaten zu erlangen. An Stelle der großen Reiche der Heiligen Allianz traten kleine staatliche Einheiten, was zu einer Vervielfachung internationaler Grenzen in dem Gebiet führte. Ihre neu gewonnene politische Unabhängigkeit suchten die jungen Nationen auch ökonomisch zu sichern, unter anderem durch ihre Handelspolitik. Nach dem ersten Weltkrieg steht der Zollkonflikt zwischen dem Deutschen Reich und der Republik Polen beispielhaft für die Instrumentalisierung der Handelspolitik für außenpolitische Zwecke weltweit und insbesondere in Zentraleuropa.

Der Weltkrieg und seine Folgen —die Ziehung neuer Grenzen, zunehmender Protektionismus und Autarkiebestrebungen— werden gemeinhin als Anzeichen für die Schwächung und teilweise Zerstörung der interregionalen und internationalen wirtschaftlichen Integration Zentraleuropas gesehen. Vor dem Krieg hatten etwa im Deutschen Reich industrielle Zentren —wie Schlesien, das Saargebiet oder auch Lothringen— in enger Arbeitsteilung gestanden mit weitgehend agrarischen Regionen wie Großpolen oder Pommern. Beide Gruppen von Regionen belieferten schnell wachsende Agglomerationsräume wie Berlin oder Hamburg. Nach dem Krieg gehörten diese Gebiete zu verschiedenen, neuen Staaten und waren durch internationale Grenzen und unterschiedliche Handelspolitiken voneinander getrennt. Ähnliches gilt auch für das Königreich Polen im Bezug auf Russland.

Es ist jedoch nicht gesichert, in welcher Weise und wie stark sich der Erste Weltkrieg tatsächlich auf die Handelsbeziehungen auswirkte. Einerseits existiert bislang keine Messung der Effekte der Zollerhöhungen, andererseits ist auch der Grad der wirtschaftlichen Integration in Mitteleuropa vor dem Weltkrieg umstritten bzw. unklar. Das Russische Reich hatte sich schon vor dem Krieg dem europäischen Freihandel nie verschrieben. Seit 1880 gab es auch in Deutschland erhebliche protektionistische Tendenzen in der Industrie und der Landwirtschaft. Für das Argument der wirtschaftlichen Desintegration ist jedoch entscheidend, dass die zentraleuropäischen Ökonomien vor dem Krieg eng miteinander verflochten waren. Andernfalls hätten der Krieg und die Grenzziehungen nur minimale Auswirkungen gehabt. Ein spezieller Fall sind daher die Gebiete, die nach dem Krieg die staatliche Zugehörigkeit wechselten oder die sich zu einem neuen Staat vereinigten, wie die polnischen Teilungsgebiete.

Die interne Integration der polnischen Gebiete vor dem Krieg in die Ökonomie der jeweiligen Teilungsmacht ist umstritten. Polnische Historiker und Wirtschaftswissenschaftler haben diese als hoch eingeschätzt —vor allem auf Grund einer gezielten Politik der Teilungsmächte. Zeitgenössische Beobachter wie Tennenbaum (1916) und Lewy (1915) beklagten die vielfältigen Instrumente, mit denen das Russische Reich das Königreich Polen ökonomisch schwächte und an sich band. Dem widerspricht jedoch bereits Rose (1917). Er argumentiert, dass das Königreich Polen nicht so eindeutig auf Russland ausgerichtet war wie unter anderem auch von Luxemburg (1898) postuliert. In einer neuen Studie zeigt Müller (2002), dass die ostdeutschen Provinzen unterdurchschnittlich in den Getreidehandel des Deutschen Reiches integriert waren und für Österreich-Ungarn finden Schulze und Wolf (2009) quantitative Evidenz für die zunehmende ökonomische Desintegration der Regionen bereits vor dem Krieg entlang ethnischer Grenzen.

Erschwert wird die Bewertung des Effekts des Weltkrieges zudem auf Grund der politischen Vereinigung bestimmter Regionen. So gelang der Zweiten Polnischen Republik die wirtschaftliche Integration in relativ kurzer Zeit. Diese Aspekte machen deutlich, dass sich die Auswirkungen politischen Zerfalls auf wirtschaftliche Verflechtung eigentlich nur abbilden lassen, indem man Handel in einzelnen Sektoren auf regionaler Ebene analysiert. Die Voraussetzung dafür sind regionale Handelsdaten, was für die wenigen entsprechenden Studien große Probleme verursachte. Meine Untersuchung basiert daher auf Daten regionaler Transporte auf Eisenbahnen. Um erstmals quantitativ den Effekt des Weltkrieges auf die wirtschaftliche Integration deutscher und polnischer Regionen untereinander und in die europäische Arbeitsteilung zu analysieren, verwende ich Handelsströme zwischen Regionen, um eine Veränderung ihrer Verflechtung zu messen. Die Untersuchung umfasst die Jahre 1910, 1913, 1926 und 1933. Der betrachtete geografische Raum umfasst beinhaltet das Deutsche Reich, das Königreich Polen sowie das Kronland Galizien vor dem Ersten Weltkrieg bzw. nach dem Kriege Deutschland, Polen, das Saargebiet sowie die französischen Départements Haut Rhin und Bas Rhin, d.h. das ehemalige Reichsland Elsaß-Lothringen.

Zur Schätzung der Grenzeffekte verwende ich das Gravitationsmodell des internationalen Handels. Das Modell betrachtet nicht die Veränderung absoluter Größen, sondern es vergleicht den relativen, bilateralen Handel, in diesem Fall, deutscher und polnischer Regionen mit deren internem Handel sowie mit deren externem Handel z.B. mit Russland oder Frankreich. Die Untersuchung ergibt, dass die wirtschaftliche Integration über Grenzen hinweg nach dem Weltkrieg tatsächlich niedriger war als vor dem Krieg. Der Unterschied ist jedoch nur in bestimmten Sektoren von Bedeutung. Tatsächlich ergibt die Analyse einen Effekt von Grenzen auf den Handel in Mitteleuropa vor und nach dem Ersten Weltkrieg, der nur leicht negativer ist als nach einer Studie von Evans von Grenzen zwischen den Industriestaaten Anfang der 1990er Jahre. Dazu muss man mit in Betracht ziehen, dass die Zölle in den 1930er Jahren im Vergleich sehr hoch waren und der angegebene Grenzeffekt diese expliziten Kosten mit enthält.

Abgesehen vom Braunkohlen- und Kokshandel liegen die impliziten Kosten der neuen Grenzen, ausgedrückt in Zolläquivalenten, unter denen der alten Grenzen, d.h. der Grenzen, die nach dem Weltkrieg weiterbestanden. Dieses Ergebnis bedeutet, dass die nach dem Krieg neu gezogenen Grenzen den interregionalen Handel weniger stark behinderten als die alten, weiter bestehenden Grenzen. Offensichtlich überstanden wichtige Strukturen der Arbeitsteilung den Krieg und die anschließenden Grenzziehungen. Die Gebiete, die die staatliche Zugehörigkeit wechselten, hatten also in gewisser Weise eine Art wirtschaftliche Brückenfunktion zwischen neuem und altem Staat. Dies ist selbst noch Anfang der 1930er Jahre im regionalen Handel zwischen dem Deutschen Reich und den ehemals deutschen Gebieten abzulesen. Die Pfadabhängigkeit dämpfte die allgemeine Desintegration des Handels während der Weltwirtschaftskrise. Eine weitere Erklärung dafür liefert das verwendete Modell selbst. Die zu Grunde liegenden Annahmen implizieren die Interaktion sämtlicher Handelsbeziehungen und damit auch, dass die Schwäche des jeweiligen Heimatmarktes und die Abschottung des Rests der Welt die neuen Nationalstaaten und Deutschland in den intensiven Handel miteinander "zwang", obwohl ihre Wirtschaftspolitik dies teilweise mit Handelsbarrieren zu verhindern suchte.

Eine interessante Ausnahme davon ist der Kohlen- und Kokshandel zwischen Deutschland und Polen. Die starke Desintegration in diesem Fall hängt vermutlich mit mehreren Faktoren zusammen. Mit dem Auslaufen des Versailler Vertrages im Januar 1925 und des speziellen Abkommens über Oberschlesien im Juni 1925 entfielen günstige Voraussetzungen für den bilateralen Handel ersatzlos. Stattdessen erschwerte der beginnende deutsch-polnische Zollkonflikt den Handel, während der Ausbau des Gdingener Handelshafens und der Exportausfall Großbritanniens in Folge des Bergarbeiterstreik die Reorientierung des polnischen Kohlenexports begünstigten. Darüber hinaus strebte die polnische Regierung an, den Handel Polens mit Frankreich zu intensivieren, um die wirtschaftlicheSchließlich kam hinzu, dass die polnische Regierung den Handel mit Frankreich intensivieren wollten, um die Abhängigkeit vom Deutschen Reich zu reduzieren, während Frankreich Koks zur Verarbeitung des Eisenerzes aus den Lagern in Lothringen benötigte. Der geringe Effekt beim Roggenhandel ist entweder Hinweis auf eine geringe Handelsverzerrung durch die neuen Grenzen, oder aber auf eine schon vor dem Weltkrieg ausgeprägte, schwache Integration der Getreidemärkte. Dies würde jedoch nicht allein für den ehemals ostdeutschen Handel gelten.

Ein weiteres Ergebnis ist, dass dort, wo nach 1919 Grenzen gezogen wurden, schon 1913 die wirtschaftliche Integration in das Deutsche Reich relativ niedrig war. Dieses Ergebnis hat bestimmte sektorale Ausnahmen, z.B. die Eisen und Stahlindustrie Oberschlesiens, die systematische Abweichung ist jedoch signifikant. In gewissem Sinne bedeutet es, dass die politische Grenzziehung der Pariser Vorortverträge in der ökonomischen Integration vor 1914 teilweise bereits abzulesen ist. Im Gegensatz dazu weisen die polnischen Regionen Deutschlands, Russlands und Österreich-Ungarns bereits vor dem Krieg untereinander eine vergleichsweise hohe, wirtschaftliche Integration auf.

Zusammengefasst heißt dies, dass sich die negativen Auswirkungen der Grenzziehungen in Zentraleuropa tatsächlich quantitativ nachweisen lassen. Der Vergleich verschiedener Studien zeigt einerseits, dass auf Grund der häufigen Grenzverschiebungen eine Untersuchung auf regionaler Ebene notwendig ist und ergibt andererseits, dass die negativen Auswirkungen überschätzt werden, solange nicht die Integration auf regionaler Ebene vor dem Krieg als Maßstab genommen wird.

Vor dem Hintergrund der äußerst ungünstigen handelspolitischen Rahmenbedingungen ist das Ergebnis bemerkenswert, dass die polnischen Westgebiete noch 1933 wirtschaftlich relativ gut mit dem Deutschen Reich integriert waren.

Im dritten Kapitel untersuche ich die wirtschaftliche Integration anhand der Konjunkturverläufe in Europa, den USA und Japan. Trotz intensiver Forschung über die Wirtschaftskrise der 1930er Jahre besteht wenig wissenschaftlicher Konsens über ihre Ursachen. Eine Ausnahme davon ist der Konsens über eine hohe internationale Abhängigkeit als Grund der Krise. Aus diesem Grund verwundert es, dass bislang nicht einmal ein Konsens über den Verlauf der Großen Depression, ihren Anfang, ihr Ende und ihr Ausmaß hergestellt wurde.

Das dritte Kapitel entwickelt daher eine Chronologie der Konjunkturzyklen während der Großen Depression basierend auf einer vergleichenden Datierung konjunktureller Wendepunkte in 10 europäischen Ländern, den USA und Japan zwischen 1925 und 1936. Ein Sample mit monatlichen Daten zur industriellen Produktion wird dazu mit Markov-regime switching Modellen analysiert. Auf Grundlage der daraus ermittelten Datierung wird dann die wirtschaftliche Integration der Konjunkturzyklen während der Zwischenkriegszeit geschätzt. Drei Ergebnisse erscheinen besonders relevant: Erstens, der Grad des Synchronizität ist hoch. Das Integrationsmaß der Konjunkturzyklen während der gesamten Zwischenkriegszeit ist etwa so hoch wie jenes für die Jahrzehnte nach dem 2. Weltkrieg. Zweitens, die Depression entwickelte sich in zwei 'Stufen' auf globaler Ebene, nicht nur in den USA. Die erste Stufe wies dabei Eigenschaften eines 'gewöhnlichen' Konjunkturzyklus auf. Sie begann einige Monate vor dem Großen Krach an der Wall Street im Oktober 1929. Die zweite Stufe war eine andere Art von Konjunkturzyklus, mit einem ungewöhnlich schweren Verlauf. Sie begann Mitte 1931 und entwickelte sich negativ, der Auflösung des Goldstandards zum Trotz.

Aus der Datierung ergibt sich, dass die globale Depression bereits 1932 abrupt endete, also deutlich bevor die Regierungen begannen, keynesianische Maßnahmen umzusetzen. Dies wiederum impliziert, dass die Weltwirtschaftskrise früher begann und eher endete als gemeinhin angenommen —wahrscheinlich auf Grund eines 'US bias' in der Forschung. Dabei ist es mit den meisten gängigen Hypothesen zur Depression nur schwerlich zu vereinbaren, dass die Wirtschaft der USA mit mehrmonatiger Verzögerung auf den globalen Zyklus entwickelte. So waren die USA unter den letzten drei Ländern im Sample, die in die Depression rutschten und auch unter den letzten drei Ländern im Sample, in denen die wirtschaftliche Erholung begann.

Das vierte Kapitel beschäftigt sich mit der Frage, ob Lohnrigiditäten tatsächlich zentral für das Verständnis der Großen Depression sind. Eine kürzlich geführte Debatte über die Rolle von Lohnrigiditäten während der Zwischenkriegszeit lässt vermuten, dass dies so ist. In ihrem Kern steht eine offene Frage, die sich aus Eichengreens (1992) Goldstandard Theorie der Großen Depression ergibt. Die Theorie postuliert, dass Lohnrigiditäten zu einer persistenten Nicht-Neutralität des Geldes führten. Ein hohes Rigiditätsniveau scheint jedoch implausibel angesichts von Massenarbeitslosigkeit und schwachen Gewrkschaften. Die Frage ist folglich, ob US Präsident Herbert Hoover dafür verantwortlich gemacht werden sollte, dass er wie Ohanian (2009) ausführt, Industrievorstände davon abhielt, Löhne zu kürzen als die Depression 1929 begann.

Die anhaltende Kontroverse über die Rolle der Löhne scheint verursacht durch unterschiedliche Annahmen über die Lohnrigidität zu jener Zeit, da es an empirischen Fakten dazu mangelt. Meine Analyse untersucht, basierend auf Zeitreihenmodellen, zum ersten Mal Nominallohnrigiditäten während der Zwischenkriegszeit. Sie berücksichtigt etwa 20 Mal mehr Beobachtungen pro Land als frühere Studien, was es erlaubt länderspezifische Rigiditätsparameter zu schätzen. Meine Ergebnisse stützen Eichengreens (1992) Synthese nur zum Teil. Die Ergebnisse lassen vermuten, dass die Verringerung des Lohnniveaus eine massive interne Abwertung darstellte —und das etwa 2 Jahre vor der externen Abwertung in Folge der Auflösung des Goldstandards. Dabei liefert die Zeitreihenanalyse Belege für eine (keynesianische) inverse Beziehung zwischen Reallöhnen und Industrieproduktion. Mein zentrales Ergebnis ist jedoch, dass während der Großen Depression zwei Aspekte der Lohnrigidität gesondert betrachtet werden müssen: Einerseits Lohninflation, d.h. das Trendwachstum der Nominallöhne, und andererseits die Anpassungsgeschwindigkeit, d.h. die Persistenz von Veränderungen in den Nominallöhnen. Die Analyse deckt dabei Strukturbrüche im Trendwachstum auf, die wegen niedrig frequenter Daten bislang unentdeckt geblieben waren. Wenn Strukturbrüche in Betracht gezogen werden, ergibt sich, dass die Persistenz in allen Ländern ähnlich hoch ist und in etwa so moderat wie in der Zwischenkriegszeit. Falls Strukturbrüche nicht in Betracht gezogen würden, würden die Persistenzschätzer beinahe perfekte Lohnrigidität ergeben. Da die Anpassungsgeschwindigkeit der Nominallöhne in der Großen Depression weitgehend unverändert unverändert blieb, baute sich durch die außergewöhnliche Preisdeflation zwischen 1930 und 1931 ein hoher Druck auf die Löhne auf. Die Strukturbrüche in der Lohninflation —in einem technischen Sinne— entspannten den Druck, allerdings nur schrittweise und ungenügend. Das ist der Grund, warum die interne Abwertung Raum für ausreichend Nominallohnrigidität ließ, um die Nicht-Neutralität des Geldes zu erklären. Ein weiteres Ergebnis ist, dass Levelverschiebungen im Trendwachstum der Nominallöhne eng auf Konjunkturzyklen folgten. Nur in den USA fielen die Levelverschiebungen eher mit politischen Maßnahmen zusammen wie z.B. dem NIRA.

2. Verwendete Hilfsmittel

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