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Impact of Institutions on Cross-Border Price Dispersion

Author: Bc. Jiří Schwarz
Supervisor: doc. Ing. Štěpán Jurajda, Ph.D.
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Declaration

I hereby declare that I elaborated this master thesis independently, using only the listed literature and resources.

In Prague, June 29, 2009

Prohlášení

Prohlašuji, že jsem diplomovou práci vypracoval samostatně a použil pouze uvedené prameny a literaturu.

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Jiří Schwarz

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Abstract

This thesis, building on existing studies on border effect, analyzes price dispersion among cities in the European region over the last twenty years (1990-2009). An extensive overview of the literature reveals that the authors completely neglect the entrepreneurial aspect of the arbitrage process, even though arbitrage is the main power behind the law of one price. Once we understand arbitrage as productive entrepreneurial activity, institutional quality should be one of determinants of arbitrage attractiveness and should, therefore, influence the price dispersion. To test this hypothesis I express the quality of institutions as one of the factors influencing total costs of arbitrage, together with population density in cities used as a proxy for competition intensity, and distance. The regression analysis proves that all three variables explain a part of observed price dispersion – the higher is the density and the better are the institutions, the lower is the predicted dispersion. This result can also be viewed as a small contribution to the emerging literature empirically testing the theory of productive and unproductive entrepreneurship.

Keywords: border effect, price dispersion, price convergence, law of one price, institutional quality, entrepreneurship

Abstrakt

Tato práce vychází z existujících studií efektu hranic na mezinárodní obchod a analyzuje rozptyl cen mezi městy evropského regionu během posledních dvaceti let (1990-2009). Obšírný přehled existující literatury odhaluje, že autoři zcela opomíjejí podnikatelský aspekt procesu arbitráže. A to přesto, že arbitráž je právě tou silou, která stojí za zákonem jedné ceny. Jakmile chápeme arbitráž jako produktivní podnikatelskou aktivitu, měla by institucionální kvalita spoluurčovat přitažlivosti arbitráže a ovlivňovat tudíž rozsah rozptylu cen. Za účelem otestování této hypotézy vyjadřuji kvalitu institucí jako jeden z faktorů ovlivňujících celkové náklady procesu arbitráže. Dalšími jsou hustota obyvatel měst, jež funguje jako proxy pro intenzitu konkurence, a vzdálenost. Regresní analýza prokazuje, že všechny tři proměnné přispívají k vysvětlení části rozptylu cen – čím vyšší je hustota a kvalita institucí, tím nižší je modelem předpovídaný rozptyl. Na tento výsledek je možné nahlížet také jako na malý příspěvek k vznikající literatuře, která empiricky testuje teorii produktivního a neproduktivního podnikatelství.

Klíčová slova: efekt hranic, rozptyl cen, cenová konvergence, zákon jedné ceny, kvalita institucí, podnikatelství

JEL classification / JEL klasifikace: E31, F31, F41, L26

CONTENTS

I. INTRODUCTION

Why do people trade is not a tricky question, indeed. Division of labor together with trade gave rise to unprecedented growth. However, it is still not very clear how trade patterns observed in reality emerge. There is, for example, the Feldstein-Horioka puzzle which states that for some countries saving and investment rates are highly correlated, even though there is no clear theoretical explanation for this phenomenon (Feldstein and Horioka 1980). But there are two other major puzzles in international economics which are in fact closely related to each other. First, there seems to be a large home bias in trade. And second, real exchange rates seem to be much more volatile and deviations from the purchasing power parity (PPP) more persistent than justifiable by economic theory. The amount of unexplained missing trade, size and persistence of PPP deviations, and factors influencing them are the subject of this text.

Purchasing power parity is a very simple empirical proposition that says that the nominal exchange rate between two currencies of two countries should equalize the aggregate price levels between these countries. Or, in other words, one unit of currency converted at the market exchange rate to some amount of foreign currency should be able to buy the same basket of goods in both countries. If the purchasing power of a unit of currency in the domestic economy is exactly equal to the purchasing power of this unit converted to a foreign currency in the foreign economy, we speak about absolute purchasing power parity. This hypothesis is, however, not easy to test empirically because we rarely identify the exactly same basket of goods in both economies. A weaker version called relative PPP is, therefore, commonly tested. Relative PPP states that the rate of growth in the market exchange rate offsets the differential between the growth rates in chosen domestic and foreign indices over a given period.

The results of empirical tests evolved in time (Alan M. Taylor and Mark P. Taylor 2004). First, economists assumed that the PPP exchange rate should hold continuously and tested this hypothesis after the end of Bretton Woods system in the 1970s. However, it became very quickly obvious that the continuous PPP simply didn't hold as the nominal exchange rates are in reality much more volatile than the price levels (Frenkel 1981). Leaving the short-run PPP behind, the economists turned their attention to the idea that PPP is retained as a long-run equilibrium. But the lack of available data at that time produced mixed results. Only recently, empirical studies using longer time periods and large country samples were able to provide convincing evidence in favor of long-run PPP (Alan M. Taylor 2002).

Even though the long-run PPP holds, there are still a few puzzles left to solve. One of the major questions is to find out "[h]ow is it possible to reconcile the extremely high short-term volatility of real exchange rates with the glacial rate … at which deviations from PPP seem to die out?" (Rogoff 1996, 664) Answer to this question is linked to a hypothesis closely related to the PPP called the law of one price (LOOP). LOOP is a disaggregated version of PPP and can be, again, introduced in two ways. Its absolute version states that the price of any tradable good expressed in the same currency should be the same across all locations, be they intra- or international. In the relative version of LOOP, transaction costs are allowed to create a constant difference among prices at different locations. The relative price between two goods, therefore, doesn't have to be equal to one but has to be mean-reverting (Funke and Koske 2008).

The reason why LOOP, at least in its relative version, should hold is that if it were possible to buy some particular good on one place, transport it to some other place and still sell with profit, such arbitrages would tend to bring the prices in both locations to one another. In a world of no transaction costs, prices of same goods would be expected to equalize almost instantly. If the composition of each country's market basket is the same with all the goods inside having the same weights then LOOP implies that PPP exchange rate between countries should hold.

The LOOP is expected to, at least approximately, hold in a highly globalized world. It is, therefore, not surprising that it didn't in medieval times. But as geographically distinct markets got more integrated, the time needed to reduce price differentials started to decline rapidly. Volckart and Wolf (2006), for example, show how much time it took to reduce price deviations of gold bullions in different areas of the medieval Europe. They found out that it took about eight months to reduce deviations between Flanders and Lübeck by 50 percent. Deviations between Flanders and Prussia were roughly twice as persistent. It was expected that during the process of globalization and deeper market integration the price differentials would diminish to amounts accounting only for transportation costs.

However, economists empirically testing the LOOP found the opposite. Even today, the functioning of LOOP is still very slow and imperfect with long-lasting price dispersion among different states. Vast literature deals with this paradox, calling the unexplained part of price differentials a "border effect", i.e. an impact of the existence of national borders on trade. The general conclusion is that there isn't nearly as much international trade as the standard models suggest there should be while the formal barriers such as various tariffs are too low to explain the revealed missing trade (James E. Anderson 2000, 115). The first wave of studies in the second half of 1990s addressed only the size of this border effect. Only recently the authors started to explain it, i.e. look for other explanatory variables in addition to transportation costs and reduce the extent of unexplained residuum.

Formally, the researches take two distinct ways of estimating and explaining the border effect. The first stream of authors, starting with McCallum (1995), try to explore how borders affect trade by looking at the difference between intra- and international trade after controlling for distance and some other variables. The main problem of this approach is that there is often very limited data on intranational trade especially for smaller countries. A method of estimating border effects in the absence of such data was introduced by Wei (1996). However, it is very sensitive to the estimates of internal distance and can easily lead to large biases.

Another way of measuring the border effect was introduced by Engel and Rogers (1996) who showed that the standard deviation of relative prices in US and Canadian cities is systematically higher for cross-border city pairs than for city pairs within the same country. Even after controlling for potential sources of this excess price variability, such as language differences, distance, exchange-rate volatility etc., there still remains relatively large unexplained residuum credited to the mere existence of national borders. However, this approach, too, is a subject of possible bias stemming from cross-country heterogeneity in the distribution of within-country price differentials, as shown by Gorodnichenko and Tesar (2009).

Many authors estimate the size of the border effect and explain the role of various factors influencing cross-border price dispersion. The underlying idea in their studies is that arbitrage is a process which should automatically equalize the prices in different places once we remove the influence of these factors. But arbitrage is an entrepreneurial activity and should be, therefore, influenced by institutional quality. However, the role of institutions is completely neglected by the existing literature on border effects.

First, I describe in greater detail development and weaknesses of above mentioned approaches to border effect analysis and present existing results. Then I provide a brief theoretical explanation of the role of institutions in entrepreneurial activities. And finally, in the last section of this text I carry out a regression analysis to empirically assess the impact of several factors including institutional quality on the cross-border price dispersion.

II. LITERATURE ON BORDER EFFECT

II.1. Gravity equations and missing trade

II.1.a. National borders and Canada-US trade patterns

One possibility of looking at the effect of borders on international trade is to use the gravity model to examine the determinants of trade patterns. McCallum (1995) in his pioneering paper used Statistics Canada data set which included trade flows among Canadian provinces and between each Canadian province and each state of the United States. It was for the first time when both intra- and international trade flow data were used simultaneously to estimate the impact of national border. He estimated following equation:

$$x_{ij} = \alpha + \beta y_i + \gamma y_j + \delta dist_{ij} + \epsilon DUMMY_{ij} + u_{ij} \qquad (2.1)$$

where x_{ij} is the logarithm of aggregate trade flow from region i to region j, y_i and y_j are the logarithms of gross domestic product in regions i and j, $dist_{ij}$ is the logarithm of the distance from i to j, $DUMMY_{ij}$ is a dummy variable equal to one for interprovincial trade and zero for province-to-state trade, and u_{ij} is a normally distributed error term. McCallum's data set consisted of imports and exports for each of the ten Canadian provinces and imports and exports between each of the province and 50 states from the US in 1988. He, however, decided to include only 30 states, i.e. 20 with the largest population and all border states. This selection left him with 683 nonzero observations.

Using the ordinary least squares estimator, the obtained values of parameters looks as follows:

$$\underset{(0.03)}{x_{ij} = 1.21\, y_i} + \underset{(0.03)}{1.06\, y_j} - \underset{(0.06)}{1.42\, dist_{ij}} + \underset{(0.13)}{3.09\, DUMMY_{ij}} \qquad (2.2)$$

Standard errors are reported in parentheses, adjusted R^2 equals to 0.811. The estimated parameters from equation (2.2) imply (as $exp(3.09) \approx 22$) that trade between two provinces is about 22 times larger than trade between a province and a state.

Table 1 shows both the actual trade flow patterns and the trade pattern predicted by the regression if it were no borders. E.g. for the whole Canada, the trade inside the provinces amounted in 1988 for 44 percent of the whole trade. From the rest, 23 percent of the trade went to other provinces, 24 percent to the US and the remaining 9 percent to the rest of the world. Assuming that the sum of interprovincial and province-to-state remains the same, the gravity equation predicts that with no borders, the interprovincial trade should account only for 4 percent, whereas the trade to US should account for 43 percent. This analysis uses data only from 1988 which could be a problem because in that year the Free Trade Agreement (FTA) was signed. To check whether the obtained estimates keep their validity also under FTA, McCallum analyzed Canada-US trade over the period 1950-1993. The data suggest that the post-1988 increase in the mutual trade share doesn't break the previously existing trend. Therefore, FTA shouldn't have any significant impact on the validity of these results.

Table 1: Canadian trade by destinations

		Destination (percentage of total shipments)			
Origin	*Shipments ($ billion)*	*Own province*	*Other provinces*	*United States*	*Rest of world*
Canada	387	44	23 [4]	24 [43]	9
Atlantic provinces	18	37	29 [12]	19 [36]	15
Quebec	85	47	27 [6]	19 [40]	7
Ontario	179	45	21 [3]	29 [47]	5
Praire provinces	67	41	28 [9]	18 [37]	13
British Columbia	37	43	13 [2]	19 [30]	25

Source: McCallum (1995, 618)

To sum up, McCallum found that Canada-US border, despite the advanced integration of these two markets, still has a decisive effect on the trade patterns. And, as these two countries are very similar in terms of culture, language and institutions, such border effect could be expected to be at least as significant in case of other countries as well. Helliwell (1996) updated and extended the McCallum analysis by including also years 1989 and 1990. With data combined from these three years, he found the estimated border effect to be 21.1 which further confirms McCallum's findings.

II.1.b. Border estimation without intranational trade flow data

Canada and the US were straightforward candidates for this kind of analysis not only due to similarities mentioned above but also because of their division into provinces or states. This setting allowed collecting necessary data on trade within the national borders. In order to overcome this lack of data by the majority of other countries, Wei (1996) proposed a measure based on an assumption that country's imports from itself is the difference between its total production and exports to other countries. This enables him to use the gravity equation to estimate the size of home bias in the goods market among OECD countries over the period

1982-1994. His approach is also interesting because he provides a theoretical framework with extended gravity model based upon simple microfoundations where the bias comes solely from some sort of barriers to trade. He starts with a maximization of a utility function

$$\begin{gathered}\max U_j = \sum_i \beta_i c_{ij}^{\theta} \\ s.t. \sum_i p_{ij} c_{ij} = Y_j\end{gathered} \tag{2.3}$$

where c_{ij} is the consumption of good i by the representative agent in country j, p_{ij} is the price of good i in country j, Y_j is the income of the representative agent and $\theta = (\sigma - 1)/\sigma$, where $\sigma \in [1, \infty)$ is the elasticity of substitution between any two consumption goods. Assumption of the gravity model is that every country produces a different good. Solving the optimization problem, we get for any good k the optimal consumption as

$$c_{kj} = \frac{Y_j \beta_k^{\sigma}}{p_{kj}^{\sigma} \sum_i p_{ij}^{1-\sigma} \beta_i^{\sigma}} \tag{2.4}$$

After some rearrangements and modifications, Wei ended up with an equation describing export from country k to j

$$\begin{aligned}\log C_{kj} = {} & \log Y_k + \log Y_j - \sigma \log D_{kj} - \log Y_w + \sigma\left(H_{k=j} - 1\right) \log t_j \\ & + \log R_k + \log R_j\end{aligned} \tag{2.5}$$

where D is the distance, Y_w world income, $H_{k=j}$ takes the value of one when $k = j$ and zero otherwise, t is the size of barriers in a form of an *ad valorem* tariff rate and R_k (R_j) is a weighted average of exporting (importing) country distances from all of its trading partners. Wei's final regression specification is as follows:

$$\begin{aligned}\log Export_{kj} = {} & \alpha + \gamma Home_{kj} + \beta_1 \log GDP_k + \beta_2 \log GDP_j \\ & + \beta_3 \log Distance_{kj} + \beta_4 \log Remote_k \\ & + \beta_5 \log Remote_j + \beta_6 Language_{kj} \\ & + \beta_7 Adjacency_{kj} + u_{kj}\end{aligned} \tag{2.6}$$

By including the measures of remoteness of countries, Wei addressed the issue that the pattern of trade was, according to Deardorff (1995), influenced not only by the absolute distance between two countries, but also by their geographic positions relative to other countries. Intuitively, the farther are two states from other states, the larger will be volume of trade between them. $Home$ takes the value of one if $k = j$, $Language$ takes value of one if the countries share a common language, and $Adjacency$ is one if the countries share a common land border. First, Wei ran the regression only with the first four regressors to test the specification used by McCallum (1995). Then, he also included the remaining explanatory variables. Using the simplified specification, the γ coefficient was estimated to be 2.27, which means that a country's trade to itself is, other things being equal, about 10 times higher than trade with a foreign country. This is already about a half in size compared to the McCallum's results. After including the measures of remoteness and dummies for a common language and land border, the estimated home bias drops to a factor of 2.6 ($\hat{\gamma} = 0.94$). Using a different remoteness measure, Helliwell (1997) found that on average, the OECD countries had in 1990 a border effect of 13.

II.1.c. Canada-US regional trade reconsidered

Datasets used by McCallum (1995) or Helliwell (1996) have an important weakness which could bias the results – they contain data combined from two different sources. While the data on total amount of interprovincial and international trade in final and intermediate goods come from Statistics Canada's Input-Output Division, the trade shares to respective states are calculated using the data provided by the International Trade Division. However, the latter are very rough. Michael A. Anderson and Smith (1999), therefore, use only the IO Division date adjusted in such way to take into account the actual source and destination of the trade flows. Moreover, in one specification of the regression model, they replaced the border dummy with two – the first takes the value of one if the shipment is a Canadian import and the second if it is a Canadian export to US.

Using the same specification of the regression model as by McCallum (1995), the estimated border effect was 12.5. This is still very large, but it is about half the value found in studies mentioned above. When estimating the border effect separately for imports and exports, they found out that the bias towards exporting to the US is 1/5.6 and towards importing from the US is 1/27.9. This suggests that Canadian provinces are more willing to export to the US than to import from there.

II.1.d. Intranational home bias

If the home bias in trade stems from explicit or implicit trade barriers, there shouldn't be found any home bias on the subnational level. However, testing this hypothesis on the 1993 Commodity Flow Survey providing data on trade flow within and across the US states, Holger C. Wolf (2000) found home bias present even for traded goods across US states. He uses the gravity equation including remoteness measures defined as

$$Remote_{ij} = \sum_{k=i,k<>j}^{48} \frac{D_{ik}}{GDP_k} \quad (2.7)$$

One difference from the previous studies of the North-American trade flows is a use of another measure of distance. Whereas other authors mostly use the great circle distance between principal cities in the respective regions, Wolf uses the minimum driving distance between the largest cities in each state. He argues that for contiguous set of countries, where the goods are mostly transported by road or rail, such measure is more appropriate. Depending on the exact specification of the model, US states are 3-4 times more likely to trade within themselves than with another state. This kind of home bias is very difficult to explain because of the constitutional protection of interstate commerce. There can be, therefore, no formal barriers among them. Common currency, language, considerable cultural and institutional homogeneity and high degree of interstate migration also suggest that most of usually mentioned informal barriers should not play a major role.

Hillberry and Hummels (2003) tried to solve the puzzle by distinguishing between shipments originated in the manufacturing and in the wholesale establishments. Their reasoning is that manufacturers distribute the goods over large distances to a number of wholesalers. Wholesalers then distribute them over short distances to retailers. Lumping these two groups together may bias the picture of trade patterns. In such situations, borders do not have to matter per se, but rather as a proxy for very short shipment lengths of wholesalers. The manufacturers also often divide the territory among a limited number of their wholesalers who are then prohibited some explicit geographic boundaries in order to allow for some kind of price discrimination. State borders could be used as such boundaries which could explain the significance of state borders for trade flows.

Using data from 1997 Commodity Flow Survey, they separated the wholesale shipments from shipments by manufacturing establishments. Furthermore, they used actual distances of shipments rather than the estimations used by Holger C. Wolf (2000) which turned out to substantially overstate the actual distances, and allowed for origin and destination fixed effects. Results of their regression suggest that the border effect, i.e. the ratio of actual to predicted trade flows inside the state, is only approximately 1.5.

II.1.e. Non-Europe: market fragmentation in the EU

Studies introduced until now were mainly focused on the patterns of US and Canadian trade, with the exception of Wei (1996) who estimated the border effect for OECD countries. However, another potentially interesting period for border effect estimation is the implementation of

Single European Act which was designed to complete Europe’s internal market by the end of 1992. Head and Mayer (2000) take this quest and estimate how nontariff barriers eliminated by the Single Market Program influenced the size of border effect.

Head and Mayer argue that once nontariff barriers (NTBs) are taken into account in the model, the remaining border effect shows solely the consumers’ preferences because trade between EU members has been practically tariffs-free since 1968. The only remaining question is which NTBs are the trade-impeding ones. Head and Mayer estimate the following equation, based on monopolistic competition model of trade:

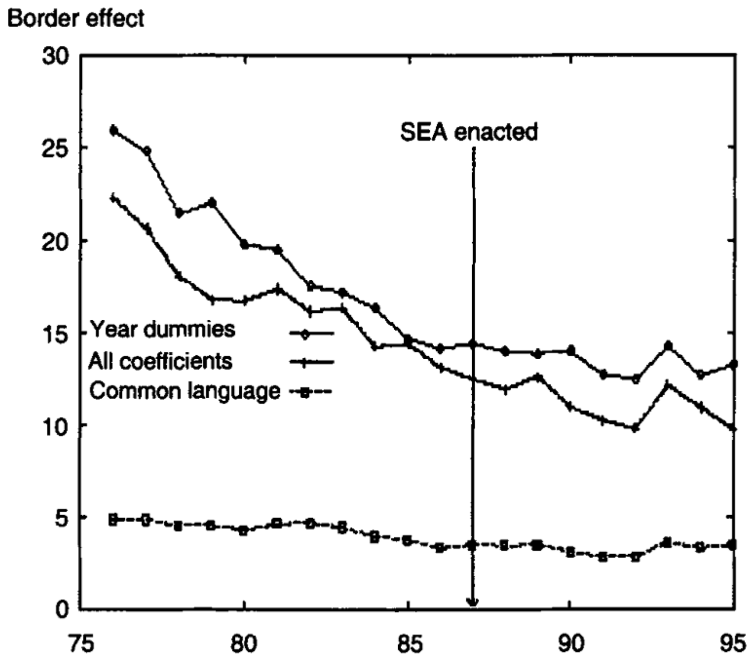
$$\log\left(\frac{m_{ij}}{m_{ii}}\right) = \log\left(\frac{v_j}{v_i}\right) - (\sigma-1)\delta\log\left(\frac{d_{ij}}{d_{ii}}\right) - \sigma\log\left(\frac{p_j}{p_i}\right) - (\sigma-1)[\beta+\log(1+\psi)] + (\sigma-1)\lambda L_{ij} + \varepsilon_{\mathrm{ij}} \tag{2.8}$$

where m_{ij} is the CIF value of imports of country i from country j, v_i is the value of production in country i, d_{ij} is distance, p_i is the mill price, β stands for the preference for home-produced goods, ψ is an ad valorem NTB (constant for all cross-border trade) and L_{ij} is a dummy taking the value of one if both countries share a common language, and zero otherwise. Trade data, as well as industry-level production data come from Eurostat. The authors estimate the constant term in (2.8) and look whether high levels of NTBs are associated with large negative values of this intercept. The NTBs, taken from a survey made by the European Commission, were actually targets of the Single European Act.

First, the border effect before the implementation of the Single European Act, estimated with pooled data from 1984, 1985, and 1986 lies between 12 and 20, depending on the exact model specification. Recalculation to an ad valorem tariff equivalent yields values from 36 percent to 45 percent. Then the authors regress obtained industry-level border coefficients on two different NTBs measures. The first set of data comes from a survey of 11,000 firms conducted by the EU Commission containing questions about how desirable for the firm would be removal of some trade barrier. The second set of indicators classifies industries into three levels of barriers.

The results show that the NTBs explain only about 10 percent in the border effects variation. Moreover, the estimated coefficients are often statistically insignificant and the moderate NTB industries are estimated to have (significantly) lower border effects than industries with low NTBs. Head and Mayer also regressed both sets of NTBs measures on changes of industry-level border effects from the period 1984-86 to the period 1993-95. Again, none of them appears to explain the changes. Also, eyeballing Figure 1 reveals no obvious change in the decreasing trend with the enactment of the Single European Act, even though the implied ratio of imports from self to imports from other European countries falls to about 10.

Figure 1: Year-by-year changes in the border effect



Source: Head and Mayer (2000, 306)

The analysis suggests that even though the border effect decreased substantially in the EU, the NTBs identified and removed by the Single European Act couldn't explain variation in the pre-SEA border effects and were, therefore, not likely to contribute to this decrease. Head and Mayer in light of these results conclude that the border effect in EU is probably caused by consumer bias.

II.1.f. Border effect in the EU: role of technical barriers to trade

Another possible source of home-biased trade patterns can be technical barriers to trade (TBTs). However, similarly to the case of NTBs, the data availability on TBTs is very poor making it very difficult to test. Chen (2004a) used the study by European Commission to link specific industries with the level of effectiveness of measures designed to remove TBTs. Moreover, she tests the impact of NTBs, geographic concentration of the firms and product-specific information costs. First, Chen estimated only the size of home bias using data from 1996. The resulting border effect, interpretable as in previous studies, varies from 6 to 25 depending on internal distance measures. This indicates that the border effect generally is very sensitive to the choice of distance measure.

TBTs seem to have substantial impact on the border effect because industries where all significant TBTs are eliminated have a home bias of 1.5, whereas industries where no solution was adopted are more than 20 times more likely to import from within the country. Effect of NTBs is insignificant as in Head and Mayer (2000). If the firms are not attached to any specific location and are, therefore, free to choose their location of production so as to minimize trade costs, then the border effect could be increased endogenously. The data show such relationship to be statistically significant, indeed. Larger border effect correlates with lower geographic concentration index of industries. The information costs are also relevant in explaining border effect suggesting that higher degree of product differentiation correlates with higher border effect.

II.1.g. Impact of distance measures on the size of border effect

Chen (2004a) showed that the border effect is highly sensitive to the used measure of distance. Most of the literature followed Wei (1996) by estimating the border effect in the absence of data on within-nation trade flows, i.e. by using some kind of point-to-point measures for both internal and external distances. However, wrongly chosen distance measures can seriously bias the estimated border effect. Head and Mayer (2002) therefore review methods used to estimate within and between-unit distances and provide their own measure – effective distance. Then they analyze how the scope of home bias changes by switching from the originally-used to their distance measure.

As was already mentioned, the between country distances are in almost all cases calculated as great circle distance between country centers. These centers can be capitals, largest cities or centrally located large cities. Selection of the city is not really important in cases where countries are far away from each other or when the economic activity is located mostly near the chosen city. The proportion between total distance and possible distance mismeasurement due to wrong choice of center is in such cases minor. When countries are close together and economic activity geographically dispersed within them, portraying the whole country into one particular point is likely to cause significant problems.

The situation is due to large variety of used measurements much more complicated by within-country distances. Head and Mayer argue that most measures overestimate internal distances with respect to international distances, and, therefore, statistically inflate the estimated border effect because the intranational trade seems to be too large for such overestimated distances between producers and consumers. Following Head and Mayer (2002, 10-12), let's outline used methods.

First employed measures were based on distances to the centers of neighbor countries. Wei (1996) proposed $d_{ii} = 0.25 \min_j d_{ij}$, i.e. one quarter of the great circle distance to the nearest neighbor country center. Holger C. Wolf (2000) used the same formula but takes one half instead of one quarter. Second way of measuring the intranational distances is by use of area-based measures. This method was e.g. used by Head and Mayer (2000) who assume that production in sub-national regions is concentrated at the center of the disk and that consumers are uniformly distributed throughout the rest of the area: $d_{ii} = 0.67\sqrt{\mathrm{area}/\pi}$. Third method is to use actual data on spatial distribution of economic activity within the nation. Holger C. Wolf (2000) also uses the distance between two largest cities ($d_{i,12}$) weighted by the share of population of the smaller city on the sum of population of these two cities ($w_{i,2}$), where

$$w_{i,2} = 1 - \frac{P_{i,1}}{P_{i,1} + P_{i,2}} \tag{2.9}$$

The intrastate distance is then defined as

$$d_{ii} = 2w_{i,2}d_{i,12} \tag{2.10}$$

For country internal distance, Head and Mayer (2000) use a weighted arithmetic average over all region-to-region distances inside a country with GDP shares of the regions as the weights. Similar average, only in more general form, is derived as the effective distance. The authors then use this new measure with datasets used by Holger C. Wolf (2000), and Head and Mayer (2000). In each case they compare border effect obtained using the original distance measure with estimates obtained using measure from equation (2.10) and the effective distance measure.

Estimating the border effect among the US states using Wolf's distance measures, only with data from 1997, they obtained border effect of 13.7, with the weighted average distance measure 9.3, and when using the effective distance, border effect drops to 6.3. In the case of EU border effect, the original area-based distance measure implies border effect 28.5, weighted average distance measure 14, and finally effective distance measure 4.2. These results indicate how important it is to use a proper distance measure when interested in absolute values of the border effect because overestimation of internal distances can lead to its substantial overestimation.

II.1.h. Contribution of other factors to the border effect

Recently, many authors turned their attention to other possible explanations of existing border effects. Paper by de Sousa and Lochard (2005) estimate the impact of currency barriers using data on two monetary unions of the CFA Franc Zone in Western and Central Africa. Their dataset includes 11 CFA and 14 European countries over the period 1980-99. They find a significant effect of currency barriers explaining about 21 percent of the overall border effect.

Another approach is taken by Rose (2005) who estimates the effect of World Trade Organization, the International Monetary Fund, and the Organisation for Economic Cooperation and Development on international trade. Rose does not estimate the border effect but he deals with a similar problem. One would expect the WTO to have the highest impact on trade volume as it is mostly concerned with trade liberalization. However, analysis of a dataset of 178 countries over the period 1948-99 reveals almost no relationship between GATT/WTO membership and higher trade. On the other hand, joining GATT/WTO is correlated with an increase in international trade.

II.2. Excessive cross-border price variability

II.2.a. US-Canada border and price volatility

As mentioned in the introduction, there is a second method of analyzing the impact of national borders on trade. Contrary to the first approach, it does not focus on the trade flows but rather on actual prices, price aggregates, or price indexes and their development over time. Existence of sizable border effect is consistent with the above mentioned literature on the speed of convergence to the purchasing power parity or law of one price. Survey of PPP studies conducted by Alan M. Taylor (2002) implies that the half-life of deviations from PPP exchange rates appears to be between four and five years. On the other hand, Parsley and Wei (1996) using a panel of 51 goods and services prices from 48 cities in the United States estimated that the half-life of deviations from the LOOP is only approximately four to five quarters for tradable goods and fifteen quarters for services.

The first paper addressing the effect of national border using prices focused on excessive price dispersion arising from crossing a border between US and Canada (Engel and Rogers 1996). The authors use consumer price indexes disaggregated into fourteen categories of goods from nine Canadian and fourteen US cities. Their hypothesis is that the volatility of prices of the same or similar goods between cities should increase with the distance between those cities and, other things being equal, prices in two cities separated by a national border should be more volatile.

For calculating the volatility, Engel and Rogers (1996) construct $P^i_{j,k,t}$ as a log of the price of good i in location j relative to location k in time t, converted to US dollars. Then they take differences in the logs of relative prices between time t and $t-2$ and calculate the volatility $V(P^i_{j,k})$ as the standard deviation. In this way the authors get a cross section of 228 volatility measures for each of the fourteen goods category. Afterwards, they estimate

$$V(P^i_{j,k}) = \beta^i_1 r_{j,k} + \beta^i_2 B_{j,k} + \sum_{m=1}^{n} \gamma^i_m D_m + u_{j,k} \tag{2.11}$$

where $r_{j,k}$ is the log of the distance, and $B_{j,k}$ is a dummy of value 1 when the two cities are separated by a border. D_m is a dummy variable for each city in the sample allowing for city-related fixed effects such as higher average volatility of US prices compared to those in Canadian cities. The estimated coefficients for a regression using all data pooled together reveal that crossing the border adds 0.0119 to the average standard deviation of prices between cities. Such increase of volatility would be, with respect to the estimated coefficient on the distance, comparable to additional 75,000 miles of distance between the cities.

The authors also provide and test two possible explanations for such sizable border effect. First, they suggest that the extra volatility could be caused by homogeneity of labor markets within respective countries so that the relative real wage is less variable for city pairs inside one country than for cross-border pairs. However, after including the standard deviation of two-month difference in the log of relative real wages, the estimated border effect coefficient stays significant and almost unchanged in its size. This means the labor market homogeneity is not the source of border effect. Second possible explanation is a possibility that prices are sticky and do not react to the changes in nominal exchange rate. The relative prices for cross-border city pairs would in such situation pick up the fluctuations of exchange rate. Recalculating the relative prices as relative real prices $(P_f/P)/(P^*_f/P^*)$, where P_f is the price of good f and P is an aggregate price index for the respective city, Engel and Rogers (1996) find that even though the coefficient on the border dummy are still positive and significant, its size decreases. Therefore, it seems that sticky nominal prices explain part of the border effect.

A significant shortcoming of Engel and Rogers (1996) is their use of price indexes which makes it impossible to evaluate the long-run deviations from the LOOP. They can only measure the short-run deviations as they compare the inflation rate of a category of goods in a US city to inflation rate in a Canadian city and rate of depreciation of the US dollar relative to the Canadian dollar. This issue has been addressed by Engel, Rogers, and Wang (2003) who use actual prices of consumer goods in thirteen US and four Canadian cities to investigate both short- and long-run price differences. The distinction may be important for policy implications. If the deviations from the LOOP are short-run, some kind of fixing of nominal exchange rates may minimize the distortion and implied welfare losses. On the other hand, significant deviations in the long run imply low integration of analyzed markets which makes them poor candidates for common monetary policy.

Engel, Rogers, and Wang (2003) use a data source which is very popular nowadays – Economist Intelligence Unit (EIU) CityData price panel which records local prices for over 160 different goods and services in 140 cities worldwide from 1990 to present. The authors use prices of 100 consumer goods in 13 US and 4 Canadian cities over the period 1990-2002, including both tradable and non-tradable. Expressed in US dollars, as a dependent variable they construct either the log price difference of good i between locations j and k $|p_{i,j,t} - p_{i,k,t}|$ for testing the long-run deviations or $|\pi_{i,j,t} - \pi_{i,k,t}|$, where $\pi_{i,j,t} \equiv p_{i,j,t} - p_{i,j,t-1}$, for estimating impact on short-run development. As explanatory variables they use log of distance between locations, difference in the log of population, difference in sales tax rates, dummy for national border and dummy for each city.

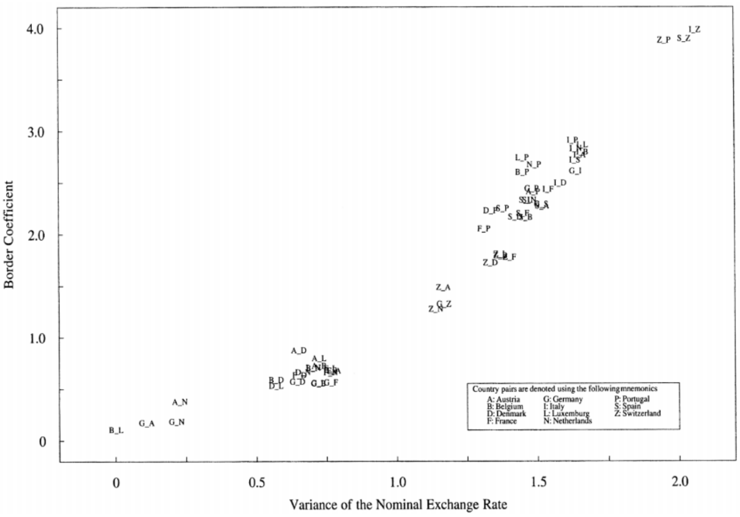
The sales tax variable turned out to be insignificant in both regressions. In the levels regression, all three remaining variables are significant and positive as expected with the border coefficient of 7.3 percent. It can be interpreted as an absolute difference in prices, other things being equal, in the US vs. Canada. In the first differences regression, distance is statistically insignificant and all coefficients are of smaller magnitude. Again, ceteris paribus, border increases the absolute value of the difference in price changes in US relative to Canadian cities by 1.4 percentage points.

II.2.b. European PPP deviations and its welfare consequences

It is also possible to directly examine the role of nominal exchange rate volatility combined with sticky prices if there is a set of cities or countries separated by national border but, at the same time, with common currency or fixed exchange rates. Engel and Rogers (2001) analyze deviations from short-run relative PPP among European cities. They use aggregate consumer price data from 55 European cities in 11 countries over the period 1981-1997. Belgium and Luxembourg share a common currency. Germany, France, Austria, Belgium, Denmark, Luxembourg, and the Netherlands had fixed exchange rate margins over the whole observed period being part of the Exchange Rate Mechanism. Italy was a member of ERM until 1992, Spain and Portugal joined ERM in 1994.

The authors measure the volatility as variance of $\Delta p_t^j - \Delta p_t^k$, where j and k are different locations. As explanatory variables they use, as usual, dummies for each location, and dummy for border. Instead of logarithmic form of distance, they use quadratic. That is, they include both distance and squared distance and expect the relationship to be concave. They also included dummies for common language and common border. However, their impact on estimated coefficients of other explanatory variables was negligible. And last but not least, the authors include variance of nominal exchange rate changes between locations under examination.

Figure 2: Estimated border coefficient vs. exchange rate variability



Source: Engel and Rogers (2001, 44)

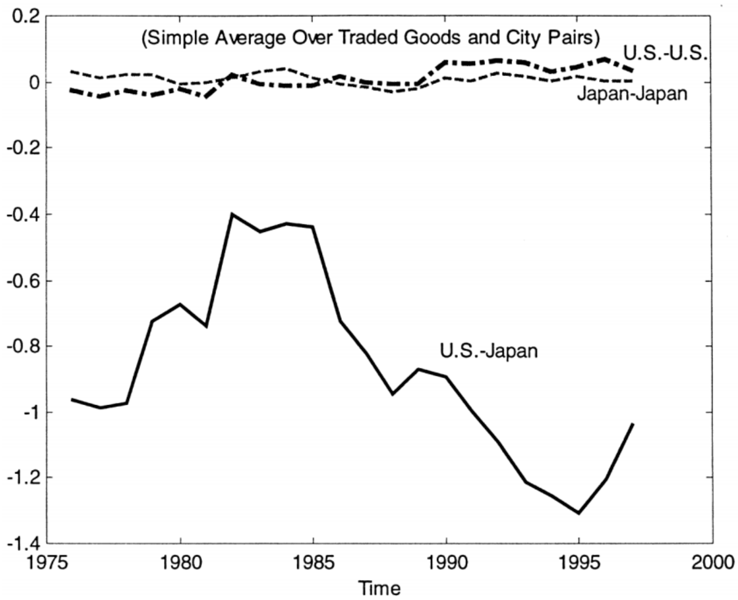
The results reveal a strong link between variability of prices and nominal exchange rate suggesting that a large part of real exchange rate variance is due to sticky prices caused by nominal exchange rate fluctuations. Without the nominal exchange rate among explanatory variables, the effect is captured by the border dummy. After inclusion, the estimate of border coefficient stays highly significant but falls from 2.85 to 0.21. Figure 2 also confirms the tight relationship between border coefficient estimates from specification without the nominal exchange rate and its variance.

The evidence shows that the failures of the LOOP in Europe are to a large degree stemming from local-currency sticky prices and variable nominal exchange rates. As was already mentioned above, paying different prices for identical goods implies a deadweight loss to the economy. However, Engel and Rogers (2001) show that it does not automatically follow from this finding that a currency union or other type of exchange rate fixing would increase the overall welfare. It is true that the evidence suggests that fixed exchange rate would substantially decrease the economic inefficiency. However, as each monetary regime has its own advantages and costs, such monetary policy would decrease the idiosyncratic risk at the expense of increasing the aggregate risk by tying monetary policies at home and abroad together. The overall impact on welfare is in this situation in advance unknown.

II.2.c. US-Japan border, its width over time and economic explanations

One drawback of method used by Engel and Rogers (1996) is that it destroys the time dimension of data and, therefore, makes it impossible to observe the evolution of border effect. This issue is addressed by Parsley and Wei (2001) using a panel data set of 27 commodity-level prices in 96 cities in Japan and the United States in the period 1976:1-1997:4. First, the authors construct good-level log real exchange rates for intra- and international city-pairs and plot the averages over all goods and all city-pairs to observe how the deviations change over time. Figure 3 shows that until 1985 deviations from the LOOP decreased but since then started to increase again. It seems as if the average violation of the LOOP doesn't have a downward trend which is strange supposing the international economic integration deepens over time.

Figure 3: Average of good-level log real exchange rates



Source: Parsley and Wei (2001, 93)

However, analyzing the price differentials per se may not be the most proper way because we test for relative LOOP allowing for a variety of arbitrage costs. There is, therefore, a zone of no-arbitrage inside the boundaries set by these costs, and the price differentials can take any values inside this interval and do not necessarily have to have a decreasing trend. We should rather ask if this zone of no-arbitrage narrows over time. This could be analyzed by looking at some kind of variability measure over many realizations of the price differentials, e.g. a standard deviation.

Following Engel and Rogers (1996), the authors then regress the standard deviation of the change in the real exchange rate on distance, border dummy, and city and good dummies. Whereas Engel and Rogers calculate the border width as $\exp(\text{border coeff}/\text{distance coeff})$, Parsley and Wei criticize it as being unaffected by the change in units of distance measurement. They propose a different border width formula, where the distance equivalent of the border is the value of Z which solves equation

$$\begin{aligned} &\text{distance coeff} \times \ln\left(\overline{\text{distance}} + Z\right) \\ &\qquad = \text{border coeff} + \text{distance coeff} \times \ln\left(\overline{\text{distance}}\right) \end{aligned} \tag{2.12}$$

where $\overline{\text{distance}}$ is the average distance between the US-Japan city pairs. Recalculating the US-Canadian border using the new formula gives a width of 101 million miles, and the border between US and Japan becomes nearly 43 000 trillion miles. The border effect seems to be really huge. However, potential economic explanations for this effect are more important than its absolute value.

Method used to measure variation in price differentials is unsuitable for analyzing the development of border effect. Another method is needed to assess the role of time. Parsley and Wei (2001) first remove the good-specific fixed effects by regressing the changes in the price differentials (real exchange rate) on individual good dummies and compute the standard deviation of the residuals across goods for each quarter in a given year. That is, each year's variability is computed using 4 quarters × 27 goods. The pooled standard deviations are then regressed again on distance, border, nominal exchange rate variability, and unit shipping costs. Inclusion of the two new explanatory variables leads to a decrease of unexplained border estimate from 0.0717 (15 billion miles) to 0.0154 (147 000 miles). Their economic explanation is straightforward – role of exchange rate variability was already described, and the unit shipping (and insurance) cost form together with the already included distance the total shipping costs. Both variables have significant and positive impact on the price dispersion.

To explore the time trends, the authors add another three explanatory variables: a linear trend, trend × log distance, and trend × border. A significant trend decline in relative price variability for both intra- and international is found. Moreover, the trend-border interaction term reveals a 0.4% decline of border effect per year suggesting a decrease in international market segmentation over time.

II.2.d. Effect of euro, EIU vs. CPI, and aggregation bias

Analysis of nontariff barriers removal by the European Single Market Program in Head and Mayer (2000) implicated no impact on trade flows within and among European countries. Engel and Rogers (2004) analyze another aspect of European integration – introduction of euro. The reason why a common currency could increase economic integration stems from several sources (Engel and Rogers 2004, 350-352). First, it reduces exchange rate volatility. However, this channel seems to be very weak. According to Rose (2000), a currency union has a large impact on trade volumes. But the effect of reduction of exchange rate volatility to zero would be, on the other hand, very minor. Whereas countries with the same currency trade over three times as much with each other as countries with different currencies, countries with zero exchange rate volatility would trade only about 13% more with each other than those with average volatility (Rose 2000, 17). Second source of economic integration could be a signal sent out by committing to a currency union that the country representatives are willing to commit to an even deeper integration including nontariff and technical barriers and policy issues. Higher confidence that the markets will stay open and the environment will be trade-friendly in future as well might motivate the producers and traders to invest more into international infrastructure.

Third, forces of arbitrage which tend to decrease price dispersion could be tricked by a sort of money illusion as prices expressed in different currencies seem to be too complex and discourage businesses to trade. Single currency reduces this complexity and gives a boost to travel and trade. And last, stickiness of nominal prices could, due to nominal exchange rate movements in situation of more currencies, lead to real price misalignments.

Engel and Rogers use price data from EIU CityData covering period 1990-2003 to estimate whether the introduction of euro in 1999 really had the effect expected in theory. First analysis of the data, however, indicates that even though there was a significant decrease of price dispersion over the period, it should be attributed to changes in the first half of 1990s. That is, before the introduction of euro. Even after controlling for other factors which could affect the dispersion, the authors found no link between euro introduction and prices dispersion decrease. On the contrary, the mean squared error of relative prices across all city pairs in different countries reveals an upward trend after 1998.

This development, however, could still be caused by some factor uncontrolled for. To be able to identify the euro effect with higher accuracy, some control group is needed. Wolszczak-Derlacz (2008a) uses the same data source but utilizes the difference-in-differences approach and forms two groups of countries. The treatment group consists of countries that became members of EMU in 1999 or 2002. Sweden, Denmark and England constitute the control group. Neither this approach reveals a decrease in dispersion after 1999. The majority of goods categories show an increase in price dispersion with a more declining trend in treatment group compared to the control group.

There is also a possibility that the use of actual prices causes measurement errors high enough to bias the results (see e.g. Engel and Rogers 2004; Wolszczak-Derlacz 2008a). The measurement problems could mainly stem from following:

1. The price data are collected from a small number of stores compared to the number of outlets surveyed by national statistical agencies when constructing various indexes.
2. The price data come only from large cities which do not have to be fully representative of the whole countries.
3. There are packaging and quality differences across countries. The EIU methodology probably doesn't account for them.
4. The list of tracked items does not represent the whole consumption basket.

Allington, Kattuman, and Waldmann (2005) review empirical evidence on price convergence before and after euro introduction and find mixed results that do not correlate with used dataset. Isgut (2004), for example, uses EIU data and finds a positive link between same currency (and EMU in particular) reduces price dispersion even when controlled for EU. Allington et al. themselves use comparative price level indexes provided by Eurostat for fifteen EU countries over the period 1995-2002. They also utilize the difference-in-differences approach and control for possible nonlinearity in dispersion dynamics. Allington et al. find the shift effect of euro introduction negative but statistically insignificant. On the other hand, a significant downward break in trend after 1999 among EMU countries is found.

Even though aggregated price indexes do not face above mentioned problems of EIU dataset, they, as well, are not without shortcomings causing possible biases. Allington, Kattuman, and Waldmann (2005) point out that by aggregation, valuable information may be lost. For example, price deviations with opposite signs can cancel each other out. Broda and Weinstein (2008) formally show the source of an aggregation bias. They decompose the change in the relative price as $\Delta q_{ugc\,c'\,t} = \delta_{cc'\,t} + \delta_{et} + \varepsilon_{ugc\,c'\,t}$, where q is relative log price of good u from product group g in city c relative to city $c^{'}$, δ's are city pair and exchange rate shocks and ε is an idiosyncratic shock to a product. If both cities are in the same country, $\delta_{et} = 0$. Assuming all terms are independent, the variance of relative price can be written as

$$Var\left(\Delta q_{ugc\,c'\,t}\right) = \sigma_{cc'}^2 + \sigma_e^2 + \sigma_\varepsilon^2 \qquad (2.13)$$

Again, if both cities are in the same country, $\sigma_e^2 = 0$. Border effect expressed in terms of ratios of variances would be

$$\frac{Var_{international}\ \left(\Delta q_{ugc\,c'\,t}\right)}{Var_{domestic}\ \left(\Delta q_{ugc\,c'\,t}\right)} = \frac{\sigma_{cc'}^2 + \sigma_e^2 + \sigma_\varepsilon^2}{\sigma_{cc'}^2 + \sigma_\varepsilon^2} \tag{2.14}$$

But if we first aggregate the data by averaging n products in a product group before computing the variances, the ratio changes to

$$\frac{Var_{international}\ \left(\Delta q_{gc\,c'\,t}\right)}{Var_{domestic}\ \left(\Delta q_{gc\,c'\,t}\right)} = \frac{\sigma_{cc'}^2 + \sigma_e^2 + \frac{\sigma_\varepsilon^2}{n}}{\sigma_{cc'}^2 + \frac{\sigma_\varepsilon^2}{n}} \tag{2.15}$$

which is strictly larger than (2.14) for any $n > 1$. Broda and Weinstein illustrate the aggregation bias using their price data. Depending on the specification, after averaging a border effect increases from 3 to 1000-100,000 miles. Similar bias is reported by Imbs et al. (2005) who find that the persistence in real exchange rates constructed from aggregate CPI data is higher (4-6 years) than in exchange rates constructed from disaggregated sub-indexes (1-2 years) for eleven European countries and the US over the period 1981-1995.

However, several studies find a much smaller role of aggregation. Crucini and Shintani (2008) aggregate their EIU price data with weights used by national statistical agencies and report decrease of half-life by 3 months in the case of OECD countries, and increase by 4 and 8 months for non-OECD countries and the US respectively. Moreover, comparing the half-lives of aggregated EIU data with official CPI statistics, the authors find that both datasets are practically identical.

Similarly, Rogers (2002) tests the reliability of EIU data and finds out that a) price indexes constructed from the EIU data share important characteristics with the Penn World Tables and OECD intersectoral data sets, and b) the correlation between EIU price changes and the annual official CPI inflation rate is positive and large. Moreover, PPP rates resulting from EIU prices are comparable to the PPP rates reported by the OECD. In another version of his paper, Rogers (2007) finds a significant decrease in price dispersion of traded goods over the period 1990-2004, but all of it took place before the introduction of euro in 1998.

II.2.e. Tradability of final goods and inputs

Several explanations are proposed to understand why the price dispersion and deviation persistence is larger than implied by economic theory. Usage of actual price data allows relating dispersion in relative prices between two locations to various characteristics of the particular good. Following literature on trade theory, Crucini, Telmer, and Zachariadis (2005b) focus on two characteristics: tradability of the good itself and the amount of non-traded inputs used to produce it. They construct relative prices as log deviations from the average European price and suppose all retail goods are produced by combining traded with non-traded input. Under the presumption of perfect competition, constant returns to scale and Cobb-Douglas production function, $P_{ij} = W_j^{\alpha_i} T_{ij}^{1-\alpha_i}$, where W_j is country-specific cost of non-traded input, α is the share of non-traded input used to produce good i, and T_{ij} is the cost of traded input for good i in country j.

Taking logs and subtracting the average gives

$$q_{ij} = \alpha_i w_j + (1 - \alpha_i) t_{ij} \tag{2.16}$$

This equation says that the deviations from LOOP should depend on cross-country differences in production share and input costs.

The authors use data from Eurostat, covering retail prices in the capital cities of EU countries for years 1975, 1980, 1985, and 1990. Looking at the means of distribution of relative prices for different tradability levels, we see that the average price dispersion across traded goods is 27 percent versus 40 percent for non-traded goods. The average for goods with above-average share of services is 32 percent versus 25 percent for goods with below-average share.

Regressing the variance of q_{ij} on variances of $\alpha_i w_j$ and $(1 - \alpha_i) t_{ij}$, Crucini et al. estimate the exact influence of tradability and production share on dispersion. As a proxy for the share α, the authors use industry-level data on the share of non-traded inputs, and as a proxy for variance of the traded costs, they use industry-level data on the tradability of the final good. To illustrate their results, consider the good with the smallest share of non-traded inputs and the highest tradability. The predicted dispersion measured by standard deviation is 0.12. On the contrary, the good with highest alpha and lowest tradability has predicted dispersion of 0.43. Considering the average dispersion is 0.28 and the values range from 0.02 to 0.82, the difference of 0.31 is very substantial.

Crucini and Shintani (2008) address the issue of tradability by distinguishing between two types of biases: categorization and compositional. Categorization bias emerges when using product aggregates that mix tradable and non-tradable goods. For example, the food category, considered to be tradable, includes both traded and non-traded items. On the other hand, household services, presumed to be non-tradable, include also both types of items. Estimating the impact of tradability on the dispersion using such aggregates could, therefore, lead to biased results. However, the categorization bias turns out to be a minor issue, at least when using the EIU data. After sorting individual goods into correct categories, the estimated median half-life of OECD cities' LOOP deviations for non-traded goods is lowered from 24 to 22 months.

The existence of traded and non-traded inputs is in this paper called the compositional bias. To proxy for the share of non-traded inputs price α, unlike in Crucini, Telmer, and Zachariadis (2005b), the authors use the gap between retail and producer prices. They regress persistence estimates on the distribution shares and find out that while moving from automobiles (lowest distribution share) to baby-sitting services (highest share), the predicted persistence increases from 13 to 21 months.

Crucini, Telmer, and Zachariadis (2005a) show that the empirical facts are consistent with a simple model of retail trade, used also in their other papers. They provide evidence for the existence of the Balassa-Samuelson effect both for the mean and the variance of prices from EIU dataset.

Parsley and Wei (2007) confirm the existence of compositional bias by analyzing price dispersion and persistence of Big Mac ingredients. They find that while the median half-life for tradable inputs is 1.2 years, the median half-life for non-tradable inputs is three times bigger (3.6 years). The analysis of inputs tradability is, therefore, very helpful in understanding cross-sectional differences in persistence of LOOP deviations. Parsley and Wei also summarize the other explanations suggested for the persistence puzzle. First, the CPI baskets of different countries do not have to be identical and can change over time. As the goods included in the CPI baskets vary, there is no reason why their prices have to converge because the arbitrage forces do not work in such situation.

Second, as argued e.g. by Alan M. Taylor (2001), if the arbitrage costs are high enough to produce a large band of no-arbitrage, within which the process behaves as a random walk, then a linear model will fail to support convergence. Threshold autoregression model should, therefore, be used to avoid biased results. According to Parsley and Wei, once the nonlinearity is taken into account, the half-life estimates usually decrease to 1-2 years. Last two explanations are connected to aggregation issues. The first one is the already mentioned aggregation bias that leads to higher persistence than observed by individual products.

For the last explanation suppose that prices are set daily and arbitrage happens each day after the prices are known. However, we observe only weekly averages of the market prices. Alan M. Taylor (2001) shows that in such situation the estimates of convergence speed of the averaged process will be always slower than for the actual daily price gaps process. The bias, furthermore, increases as the period over which averaging takes place increases. This situation is called time-aggregation bias.

Parsley and Wei also use the prices of Big Mac inputs to decompose movements in Big Mac real exchange rate to shares attributable to movements of traded and non-traded parts. To address the impact of different market characteristics, they regress the share in variance of traded parts on exchange rate volatility, existence of dollar peg and euro in the respective country, distance, level of tariffs, common language, and membership in a trade bloc. The results show that higher exchange rate volatility is associated with a larger share of traded inputs volatility, i.e. it magnifies the importance of their deviations. Both US dollar peg and euro lower the contribution of traded inputs to the real exchange rate, even though the impact of euro is more than three times lower. In the same direction is the effect of tariffs, and distance has positive significant impact as expected. However, the influence of trade blocs is very inconclusive. Depending on exact specification, some trade blocs have positive and some negative impact.

II.2.f. Sigma and beta convergence in the EU

Generally, it is possible to observe two types of convergence – sigma and beta – when analyzing price data. Sigma convergence occurs when dispersion in price differentials decrease over time. Wolszczak-Derlacz (2008b) tests for sigma convergence both at aggregate level, using comparative price level (CPL) indexes, and at disaggregated level with EIU prices dataset. At the aggregate level, the standard deviation of log CPLs across all economies decreased during the period 1991-2005 by 45 percent from 0.20 to 0.13. However, most of the convergence occurred at the beginning of 1990s, with a slight increase in dispersion between 1993 and 95. After 1995, there can be observed a gradual decrease in dispersion.

Turning to the disaggregated data, the author finds that price dispersion of tradable products fell over the period 1990-2005 by 14.5 percent, and by 17.2 percent when looking at non-tradables. However, none of the declines is statistically significant.

Beta convergence stands for mean reversion of cross-country price differences either to zero or to some non-zero mean, depending whether we test absolute or relative version of LOOP. Using a system GMM estimator, Wolszczak-Derlacz finds a half-life of CPL deviations of 5.39 years. Pooling all individual products together, half-life obtained for the mean absolute value of log-price differences using the same estimator is 2.4 years. Distinguishing between traded and non-traded goods, the half-lives are 2 and 4.3 years respectively.

Funke and Koske (2008) use monthly data for ninety consumer price indexes covering period 1995-2005. The main finding is that the number of converging product groups is lower for ten countries that joined EU in 2004. On the other hand, the new members show lower half-lives than the EU-15 countries. Whereas the median half-lives for the EU-15 countries ranges from 1.5 to 3.2 years, for the new members they lay between 1.0 and 1.8 years. Averaging across all product groups and countries gives half-life of 2.0 years.

One possible method of estimating the impact of cross-country or cross-industry characteristics on the relative prices is to use mean reversion coefficients instead of volatility of the prices as dependent variable. Chen (2004b) uses domestic output price indices for different manufacturing industries in six EU countries between 1981 and 1997. First, Chen constructs relative prices as $q_{ij,k,t} = \ln(p_{i,k,t}) - \ln(p_{j,k,t})$, where both price indices are expressed in ecus. Then she tests for unit roots and finds that for many industrial sectors no mean reversion is found. However, impossibility to reject the unit root, i.e. rejection of the relative PPP hypothesis, could be caused by an absolute PPP convergence caused by some structural changes. Used method doesn't allow us to distinguish between no-relative-nor-absolute PPP convergence and no-relative-because-of-absolute PPP convergence.

Taking into account only those cases, where the unit root hypothesis can be rejected, Chen obtains half-life of deviation from PPP between 4 and 25 months. In the second step, she uses estimated measures of deviation persistence as a variable to be explained by a number of different country- and sector-related variables. The results are, therefore, giving us an answer to a question different from questions in other studies, where the volatility of relative prices or relative prices themselves are used as a dependent variable. Persistence is regressed on following country-related variables: distance; adjacency; nominal exchange rate volatility; absolute change (between 1981 and 1997) in the difference of the GDP per capita between countries which should capture a possible structural change. And also on sector-related variables: existence of non-tariff barriers in the sector in question; concentration ratio; share of R&D expenditure in production; intensity of advertising; tradability for each sector and country pair. The results are as expected. Chen (2004b) finds that all explanatory variables are statistically significant and of expected sign.

II.2.g. The role of transportation costs in international price dispersion

Bergin and Glick (2007) focus on how price dispersion evolves over time. They observe that convergence from 1990 to 1997 is reversed and up to the end of their EIU dataset in 2005 the dispersion of prices increases again. This U-shaped pattern is not only observed in different subsets of their data but is found also by other authors (Engel and Rogers 2004; Wolszczak-Derlacz 2008b). Existence of such pattern is in contradiction with a strong belief that markets become more integrated over time. One explanation of the pattern is that the markets indeed are more integrated than before but some of cross-country characteristics related to the price dispersion may be variable in time and reverse the converging trend.

To investigate this possibility, Bergin and Glick examine the explanatory variables usually used in regressions in previous papers analyzing effect of national borders on dispersion. However, only few of them are time-varying and none of them varies in a way useful to explain the U-shaped pattern. But it is possible that the response of price dispersion to some variables changes over time even though the variables themselves do not vary. To address this possibility, the authors run the usual regression explaining mean square errors of relative prices with a number of trade friction determinants, only include terms that interact the suspected variable with a set of individual year dummies. Even though they find a statistically significant variation in the sensitivity to nominal exchange rate volatility, it does not correspond with the U-shape found in data.

Similar exercise for variation in the sensitivity to distance proves more fruitful. Moreover, the observed pattern coincides roughly with oil prices variation. Adding log real price of oil into the regression captures the U-shape variation for most country pairs. Regressing the residuals on time trends for 1990-1997 and 1997-2005 produces much smaller coefficients than before. However, they are still significant which means that the oil prices variation doesn't explain all of the recent increase in price dispersion.

II.2.h. Cross-country heterogeneity and border effect

Evidence of a border effect is not surprising. According to Gorodnichenko and Tesar (2009), what is surprising is the magnitude of estimated border effects. Significant border effects between regions within a country are, as well, very difficult to believe and understand. These phenomena suggest that there may be serious problems with validity of used empirical methodology. Following an example provided by the authors, suppose we have price data for four cities – two in the US (Seattle, Chicago) and two in Canada (Vancouver, Calgary). Assume as well that, after controlling for distance and exchange rate, the price of some good in Vancouver and Calgary is identical, but the price of that good is different in Seattle and even more different in Chicago. If we would test whether the pair-wise price differentials of intra-country prices were statistically significantly different from the cross-border prices, we would find that cross-border pairs show larger deviations from parity than the within-country pairs. Given the country heterogeneity, a border regression based on within- and cross-country price dispersion will produce a spurious border effect driven by higher variance of prices in one country. Border effect estimated in the above described situation would have nothing to do with actual frictions of crossing a border.

To show the problem formally, Gorodnichenko and Tesar decompose the original Engel and Rogers (1996) specification into a city and country contributions. Omitting for simplicity the error term and controls for distance and commodities, the volatility of real exchange rate is

$$\sigma_{ij} = \beta UC_{ij} + \gamma_U UU_{ij} + \gamma_C CC_{ij} + \sum_{s=1}^{N} \alpha_s D_s \tag{2.17}$$

where UC, UU, and CC are dummy variables for US-Canada, US-US, and Canada-Canada city pairs, and D are city dummies equal to one if $s = i$ or $s = j$. Even though equation (2.17) provides perfect decomposition of variance in theory, it cannot be estimated because the UU and CC dummies are collinear with the UC dummy and the set of city dummies. The coefficient, as a consequence, cannot be identified. Equation (2.17), in order to be identified, can be rewritten by substituting for dummies UU and CC

$$\begin{aligned}
\sigma_{ij} &= \beta UC_{ij} + \gamma_U UU_{ij} + \gamma_C CC_{ij} + \sum_{s=1}^{N} \alpha_s D_s \\
&= \beta UC_{ij} + \gamma_U \left(-\frac{1}{2} UC_{ij} + \frac{1}{2} \sum_{s=k+1}^{N} D_s\right) \\
&\qquad + \gamma_C \left(-\frac{1}{2} UC_{ij} + \frac{1}{2} \sum_{s=1}^{k} D_s\right) + \sum_{s=1}^{N} \alpha_s D_s \\
&= \left(\beta - \frac{1}{2}(\gamma_u + \gamma_C)\right) UC_{ij} + \sum_{s=1}^{k} \left(\frac{1}{2}\gamma_C + \alpha_s\right) D_s \\
&\qquad + \sum_{s=k+1}^{N} \left(\frac{1}{2}\gamma_U + \alpha_s\right) D_s
\end{aligned} \tag{2.18}$$

This is in fact the specification used by Engel and Rogers. However, the border coefficient now measures the increase in volatility relative to the average volatility of intra-country pairs. This may be problematic exactly in situation when there is substantive difference in prices distribution between countries. We need both countries' price variation and economic theory to assign them weights to be able to correctly identify the border. Moreover, as within- and between-country price dispersion is determined simultaneously, within-country variation itself may be also affected by the border existence, thus biasing the estimate even more.

Gorodnichenko and Tesar show that if we augment the usual specification by adding CC dummy, we are able to compute the border coefficient relative to each of the countries' dispersion.

It does not solve the identification problem but, at least, allows us to expose the issue. Distance equivalence of border coefficient obtained with the proposed specification, estimated using Engel and Rogers (1996) data from the US perspective, falls from 71,438 km to 47 km. From the perspective of Canada, the border is, due to much higher price variability in the US, 108 million km. All we know is that the estimate of distance equivalent of the "true" border coefficient is somewhere in between.

Big differences in within-country price dispersion are not found in all country pairs. Horváth, Rátfai, and Döme (2008) analyze impact of border between Hungary and Slovakia, and check for differences in within-country variability to make sure it doesn't bias their results. Following Gorodnichenko and Tesar, the authors add a dummy for city pairs where both cities are in Slovakia. They find the dummy coefficient significantly different from zero, although not large, for a number of individual products but insignificant when pooling all products together. Border effect found between these two countries is, therefore, not driven by cross-country heterogeneity in price dispersion.

Another way to overcome possible estimation bias stemming from dispersion heterogeneity is to use regression discontinuity approach. Gopinath et al. (2009) use geographic location of stores to estimate deviations from LOOP between stores located right across the US-Canada border. They find a significant discontinuity clearly revealing the treatment effect of border. Moreover, contrary to the missing trade research, no discontinuity is correctly found at the intra-national Washington-Oregon border using this approach. Its utilization is, however, very limited due to high data requirements.

II.2.i. Border effect or endogeneity problem?

It is tempting to conclude, following the existing literature on border effects, that national borders shape trade and cause the LOOP to fail. However, it can be the other way round, as well. Regressions using the "with or without border" framework can subscribe the LOOP failure to the existence of national borders even though there can be some other aspect causing both border and LOOP failure. Only a "before and after border" approach with a control group of city pairs where no border is created would really isolate the effect of borders.

This is a motivation of Morshed (2007) who uses data on retail prices in five cities in East Pakistan and five cities in West Pakistan before and after their 1971 split into two separate countries – Bangladesh and Pakistan. Using the "with or without" technique for the post-1971 period, the author finds a significant border effect. However, the data imply an existence of the border effect before the split as well. It is, therefore, likely that the lack of trade between two places inside one nation that become later on separated by newly created national border is caused by some third factor. To analyze the impact of new border, Morshed uses only cross-border city pairs for both pre- and post-1971 periods and finds no clear trend in price variability change. He concludes that creation of national border does not add to variability of prices.

In a similar way, Heinemeyer, Schulze, and Nikolaus Wolf (2008) use data on Central European trade flows before and after 1919. They find that although new borders do create new barriers to trade, the real treatment effects of borders tend to be much smaller than just cross-sectional effects. The reason is that newly created borders in 1919 followed the already visible pattern of trade relations among individual regions. Schulze and Nikolaus Wolf (2009) confirm this finding and show that there is a border effect present already from 1880s. The authors also find that ethno-linguistic heterogeneity across regions and cities explains most of the estimated effects of "borders before borders".

II.3. Concluding remarks on existing literature

First studies linking missing trade to the impact of national border found that trade inside countries is, controlling for distance, more than twenty times larger than trade with a foreign country (McCallum 1995; Helliwell 1996). However, datasets used by the authors contained data combined from multiple sources. With more consistent data the border effect on trade is lowered to about one half using the same model specification (Michael A. Anderson and Smith 1999), controlling for remoteness of trading partners further lowers the unexplained portion of missing trade (Wei 1996; Helliwell 1997). A serious problem with the used gravity model specification is that it estimates a significant border effects also at subnational level between individual US states (Holger C. Wolf 2000).

One solution to this puzzle is to distinguish between manufacturing trade and wholesale, as wholesalers usually distribute the goods in small areas which often correspond with US state borders. More serious source of biases is the measure of internal distances. When using more theoretically grounded measure of effective internal distance, the border effect among US states drops significantly to about a half of its former value (Head and Mayer 2002). Separation of wholesale and manufacturing shipments together with use of actual distances of shipments lower the border effect, i.e. the ratio of actual to predicted trade flows inside the state, to 1.5 (Hillberry and Hummels 2003). Border effect in the EU decreases with the use of effective distance measure from about 20 to 4.2 (Head and Mayer 2002). Technical barriers to trade also have sizable impact on the magnitude of border effect (Chen 2004a).

To sum up, the missing trade stemming from the gravity model is to a large degree caused by wrong internal distance data. Correct internal distance measure and suitable data on trade flows lead to a reasonably small border effect which has its source mainly in different and sometimes hardly quantifiable trade barriers.

On the other hand, the analysis of border effect as excessive unexplained cross-border price variability does not suffer from problems with distance measures. Geographical distance between two cities is clear, even though some measure of effective distances would be more appropriate. According to the first study using indexes of price aggregates adds US-Canada border variability equivalent of 75,000 miles of distance (Engel and Rogers 1996). Short-run deviations from the purchasing power parity are strongly linked to nominal exchange rate variability (Engel and Rogers 2001), but the explanatory power is considerably reduced when looking at long-run deviations (Parsley and Wei 2001; Bergin and Glick 2007; Wolszczak-Derlacz 2008b). Even though fixation of exchange rates through the introduction of a common currency should evidently reduce price dispersion, empirical investigations of euro introduction lead only to mixed results (Allington, Kattuman, and Waldmann 2005; Engel and Rogers 2004; Wolszczak-Derlacz 2008a).

Aggregation of prices tends to bias the estimated border effect upwards (Imbs et al. 2005; Broda and Weinstein 2008) but when comparing the results obtained using disaggregated price data and official price indexes, the differences are relatively minor (Crucini and Shintani 2008). Another source of obtained border effect is the incorrect categorization of goods because even the most traded goods have a share of non-traded inputs. However, we cannot expect non-traded goods to be subject to arbitrage. Controlling for the share of non-traded inputs decreases significantly the border effect (Crucini, Telmer, and Zachariadis 2005b; Crucini and Shintani 2008).

Estimating the size of border may be very problematic due to an important problem of used models – they do not correctly identify the border effect. Countries can have different price dispersion within their borders and the effect of cross-country heterogeneity partly enters the border coefficient (Gorodnichenko and Tesar 2009). However, significant border effect between US and Canada is found also using regression discontinuity approach which is immune to this identification problem (Gopinath et al. 2009), as well as by countries that have very similar within-country price dispersion patterns (Horváth, Rátfai, and Döme 2008).

Most of the unexplained part of cross-border price dispersion which is in the early literature credited solely to the existence of border can be in fact explained by various biases and arbitrage costs such as distance, tariffs (Bergin and Glick 2007; Parsley and Wei 2007), transportation costs per unit of distance (Parsley and Wei 2001; Bergin and Glick 2007), or different languages, tax and income levels in respective countries (Wolszczak-Derlacz 2008b). The list of factors influencing cross-border price dispersion is by no means exhausted, one of currently analyzed possibilities is the use of model with sticky prices and information which is able to explain larger part of excessive price dispersion and provides predictions more consistent with reality (Crucini, Shintani, and Tsuruga 2008). Moreover, if there is really some effect of national borders that cannot be attributed to any economically meaningful factor, it can still be caused by something shaping the borders themselves, such as cultural and ethno-linguistic patterns (Schulze and Nikolaus Wolf 2009).

Even though we may not be able to correctly estimate the size of border effect, it is still worthwhile to address the impact of different variables influencing size and persistence of deviations from the law of one price. Knowing what factors cause the law of one price fail can have important policy implications.

III. EUROPEAN PRICE DISPERSION AND ITS SOURCES

III.1. The role of institutional quality

Studies introduced in previous chapters use a number of factors to explain observed dispersion of prices between cross-border city pairs. The underlying idea is that after we control for the amount of non-traded inputs, aggregation and other biases, nominal exchange rate volatility, impact of distance, tariffs, etc. then the process of arbitrage should equalize the prices in different places. However, this is a wrong view of the whole mechanism. Arbitrage is not an automatic equilibrating process, it is an entrepreneurial activity. As Kirzner (1997, 70) points out,

> […] each market is characterized by opportunities for pure entrepreneurial profit. These opportunities are created by earlier entrepreneurial errors which have resulted in shortages, surplus, misallocated resources. The daring, alert entrepreneur discovers these earlier errors, buys where prices are “too low” and sells where prices are “too high.” In this way low prices are nudged higher, high prices are nudged lower; price discrepancies are narrowed in the equilibrative direction.

In a similar manner, Baumol, Litan, and Schramm (2007, 3) understand entrepreneur as “any entity, new or existing, that provides a new product or service or that develops and uses new methods to produce or deliver existing goods and services at lower cost.” The goods do not travel from where they are cheaper to where they are more expensive by themselves, the prices do not automatically equalize. It is a process run by entrepreneurs who have to discover profit opportunity. The profitability of arbitrages is then influenced by a number of different costs such as tariffs or transportation costs. However, there is one important aspect of entrepreneurship that is mostly ignored by authors analyzing the sources of border effect. It is not the lack of entrepreneurship that leads to deviations from the LOOP. Building on Schumpeter and Kirzner, Baumol (1990, 894) notes that

> […] entrepreneurs are always with us and always play *some* substantial role. But there are a variety of roles among which the entrepreneur's efforts can be reallocated, and some of those roles do not follow the constructive and innovative script that is conventionally attributed to that person. Indeed, at times the entrepreneur may even lead a parasitical existence that is actually damaging to the economy. How the entrepreneur acts at a given time and place depends heavily on the rules of the game – the reward structure in the economy – that happen to prevail.

In other words, if the institutional framework induces prohibitive costs to engage in innovative or arbitrage activities, the entrepreneurs will direct their effort to other activities, often unproductive, such as rent seeking. Baumol (1990) himself provides several examples of various historical periods and their attitude towards productive entrepreneurship. He starts in ancient Rome, where the main sources of income of honorable class was landholding, usury and political payments, and commerce and industry were run by former slaves. The pursuit of wealth was encouraged, but not through productive entrepreneurship. Similarly in medieval China – young people studied for many years to become state officials and be able to extract rents via very widespread corruption.

Baumol continues with the description of changing attitude towards productive and unproductive entrepreneurship during earlier and later Middle Ages, fourteenth century, and before the industrial revolution. He finds that the institutional frameworks affecting the allocation of entrepreneurship between productive and unproductive activities had a notable impact on innovativeness and spread of technological discoveries.

Aidis and Estrin (2006) address the relationship of institutions and productive entrepreneurship in today's Russia. Even though they do so very informally, they emphasize several interesting differences between Russian and Chinese self-help institutions based on social networks. While in China this system evolved into a tool used to overcome the absence of well-defined property rights and contract enforcement, it was not the case in Russia, where the network is primarily used as a means to corruption (Hsu 2005; Wu and Huang 2006). Aidis and Estrin find that the entry rates of new firms in Russia are deep below rates commonly observed both in developed and developing countries.

More formal test of the link between institutional quality and the productivity of entrepreneurship is provided by Sobel (2008). He uses Economic Freedom of North America index as a measure of institutional quality and several proxies for productive and unproductive entrepreneurship. As proxies for productive, Sobel uses venture capital investments per capita, patents per capita, the growth rate of self-employment activity, the establishment birth rate, and large firm establishment birth rate. To proxy for unproductive entrepreneurship, he uses three different measures of the number of political and lobbying organizations in each state's capital and an index measuring judicial quality, where states scoring poorly have generally significant legal fraud and abuse. As expected, institutional quality is positively correlated with measures of productive entrepreneurship, and negatively with measures of unproductive entrepreneurship, no matter what measure is used.

The results of both studies are consistent with theoretical prediction. In order to promote productive entrepreneurship,

> institutions must reward socially useful entrepreneurial activity once started; otherwise individuals cannot be expected to take the risks of losing their money and their time in ill-fated ventures. Here, the rule of law—property and contract rights in particular—is especially important. (Baumol, Litan, and Schramm 2007, 7)

On the other hand, opportunity costs are also included when deciding between productive and unproductive activities. Potential gains from unproductive use of resources have to be also as low as possible, i.e.

> government institutions must discourage activity that aims to divide up the economic pie rather than increase its size. Such socially unproductive (though, in a sense, entrepreneurial) activities include criminal behavior (selling of illegal drugs, for example) as well lawful "rent-seeking" behavior (i.e., political lobbying or the filing of frivolous lawsuits designed to transfer wealth from one pocket to another). (Baumol, Litan, and Schramm 2007, 7-8)

Arbitrage is, according to definitions of entrepreneurship, a productive entrepreneurial activity. As suggested also by above introduced empirical studies, institutional quality should be one of determinants of arbitrage attractiveness. In order to test this hypothesis, I will express the quality of institutions as one of the factors influencing total costs of arbitrage. If the institutions endorsing productive entrepreneurship are not good enough, the costs of arbitrage are higher. And, moreover, if the institutions do not penalize unproductive entrepreneurship, then the opportunity costs of unproductive activities increase the costs of arbitrage as well.

III.2. Data

In the analysis I use data on actual retail prices, not price indexes. The information on prices comes from the Worldwide Cost of Living surveys conducted twice a year by the Economist Intelligence Unit (EIU). The main target market for the data source is managers who use it to compare costs of living in different world cities and estimate compensations for relocating employees. Even though goods included in the survey to some degree reflect this target, the sample overlaps sufficiently with a typical urban consumption basket. As described in section II.2.d, the EIU data share important characteristics with official CPI statistics and OECD data sets (Crucini and Shintani 2008; Rogers 2002).

The survey covers 140 cities in 93 countries and consists of local prices for more than 160 individual goods. Among the goods are products such as “white bread (1 kg)”, “paperback novel (at bookstore)”, and “women’s cardigan sweater” or services like “man’s haircut (tips included)”. Prices of many goods in the survey are collected from two types of outlets: supermarkets and mid-priced stores. In this paper, only prices from the supermarket or lower-price outlets are used, as they are likely to be more comparable across different regions. The data are annual from 1990 to 2009 with the newest prices collected in March. All prices are expressed in euros.

Forty cities from 31 countries in European region were chosen, together with 134 goods (listed in Appendix Tables Table A1, Table A2, and Table A3). Products and services were grouped into eight different categories to allow more detailed overview of prices development. Distinction between traded and non-traded goods is a common sense one and follows classification used by other authors (Engel and Rogers 2004; Bergin and Glick 2007). I do not take into account the composition of inputs, as described in great detail above in section II.2.e. Some bias is, therefore, due to existence of non-traded inputs by traded goods likely to be present in the estimates.

Two sources of institutional indicators are used. The first one is the Worldwide Governance Indicators project (WGI) which covers six dimensions of governance over the period 1996-2008. I use three of available indicators: regulatory quality, rule of law, and control of corruption. WGI gets the data from 35 different sources produced by 33 organizations. Among these sources are both surveys of firms and individuals, as well as expert assessments by commercial risk rating agencies, non-governmental organizations, multilateral aid agencies, etc. Altogether, 441 variables are used to compute the indicators. This should lead to greater precision of data compared to any individual data source.

Second source is the EBRD-World Bank Business Environment and Enterprise Performance Survey (BEEPS) which provides detailed information on how managers perceive various aspects of business environment in respective countries. The survey was conducted first in 1999 and is repeated every three years since. Even though it focuses on the European region, only developing countries are included. However, in 2004 there was a special round of the survey conducted in five developed European countries. As a consequence, I use only the results of 2005 survey and combine it with the 2004 special round to build a wider cross-section dataset with most of the countries I have price data for.

III.3. Measuring price dispersion

My goal is to measure the scope of deviations from LOOP across cities in different markets and its development in time. Moreover, I want to estimate the influence of various city- and country-specific factors on the size of deviations. In order to do so, relative log prices among all available city pairs are formed. To be more specific, let $P_{i,t}^{k}$ be the price of good k in city i at time t expressed in euros. For a given pair of cities (i,j), the relative price for a given good and time is

$$q_{ij,t}^{k} = p_{i,t}^{k} - p_{j,t}^{k} \tag{3.1}$$

where lower case denotes logs. Some studies use directly the relative log price as a measure of price dispersion between the two cities. However, this approach ignores the theory behind LOOP. Deviations from an equilibrium price level of a good exist because for some reason the forces of arbitrage are not functioning. Whatever the sources of arbitrage failure may be, it is possible to represent them by a band of no-arbitrage, within which the differences in price of one good between two places are too small to enable arbitrage with profit.

As a simple example, we can imagine a world where there are only two barriers to trade: transportation costs and tariffs. These two costs give rise to a band, or interval, where the prices of some particular product in two distinct cities are too close to each other, i.e. the relative price is too close to zero, to allow for a profit-making arbitrage. Arbitrage would start to work only after the relative price leaves this band. In a world of no other obstacles to trade, the correction of excess price difference would be instantaneously bringing the relative price back into the band where no further profitable arbitrage would be possible. The situation is illustrated on Figure 4. However, the band width does not have to be constant all the time. Suppose, for example, that in 1994 the tariff is lowered. As a consequence, arbitrage would be profitable with lower absolute value of relative price, i.e. with smaller price difference. The band of no-arbitrage can also widen, for example due to higher oil prices which increase transportation costs.

Figure 4: Relative prices in the band of no-arbitrage

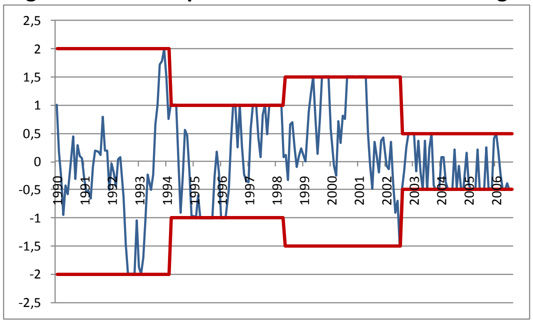
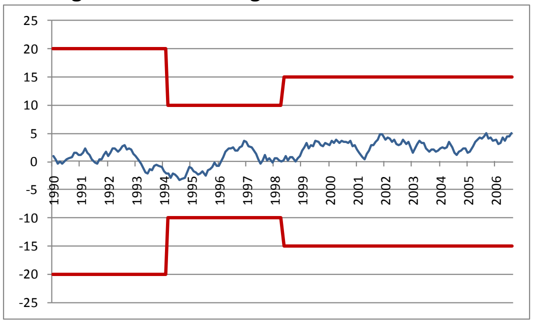


Figure 5: No-arbitrage band unidentifiable



Relative price inside the band of no-arbitrage follows random walk process. As long as it is inside the band, the price difference can move in any direction regardless of the arbitrage constraints symbolized by the band width. Using the absolute value of relative price itself as a measure of dispersion is, therefore, not proper because we are not able to observe the width of no-arbitrage band and distinguish between a situation when the relative price moves inside the band, and situation when the width of band changes as well. Moreover, only the second case is a phenomenon that could be explained by changes in external factors.

But it is possible to measure the approximate width of no-arbitrage border by observing many realizations of relative prices between the same two cities and calculating some kind of average dispersion statistic. There is, of course, an implicit assumption that the relative prices fluctuations use the whole band width. In other words, if the prices due to any reasons move in a band narrower than the no-arbitrage band both before and after the change of external factors influencing the costs of arbitrage, as shown on Figure 5, then even this method fails. Such situation is, however, very improbable given the level of world markets integration as it would happen only in case of immense trade barriers.

Existing papers use generally two types of average dispersion measure. I decided to use both and find out if the choice of dispersion measure affects the final results. The first measure is standard deviation of $q_{ij,t}^{k}$ across all products k:

$$SD_{ij,t} = \left(\sum_{k \in K} \left(q_{ij,t}^{k} - q_{ij,t}^{\bar{k}} \right)^2 / K_N \right)^{1/2} \tag{3.2}$$

where K is the set of products, K_N is the number of products, and $q_{ij,t}^{\bar{k}}$ is the average relative price over all products from the set for city pair ij.

The second measure is a mean square error of $q_{ij,t}^{k}$ across all products k:

$$MSE_{ij,t} = \sum_{k \in K} \left(q_{ij,t}^{k} \right)^2 / K_N \tag{3.3}$$

where K is, again, the set of products and K_N is the number of products. The only difference between SD and MSE is that SD removes the city-pair fixed effects. That is, MSE measures not only dispersion but also the average distance of relative prices from zero. Potentially, there are 820 city pairs, each with up to 20 yearly observations. This gives us a sample of a maximum of 16,400 observations. After exclusion of missing observation, 13,004 observations of price dispersion are left. Table 2 provides brief statistical summary of both measures – there is, obviously, enough variance in the sample.

Table 2: Descriptive statistics

	Observations	Mean	Std. Dev.	Min	Max
SD	13004	0.5287522	0.1208104	0.0522261	1.080089
MSE	13004	0.4211066	0.2977070	0.0029500	3.361958

Both measures are formed for ten different product sets: 1) perishable food and non-alcoholic beverages, 2) non-perishable food and non-alcoholic beverages, 3) clothing and footwear, 4) alcoholic beverages, 5) recreation products, 6) personal care products, and 7) household supplies form together with few other items the group of 8) traded goods which together with 9) non-traded goods and one other item form 10) all items.

Figure 6 and Figure 7 present both dispersion measures averaged over all city pairs, i.e.

$$\begin{aligned} SD_{\overline{ij},t} &= \sum_{ij \in C} SD_{ij,t} / C_N \\ MSE_{\overline{ij},t} &= \sum_{ij \in C} MSE_{ij,t} / C_N \end{aligned} \tag{3.4}$$

where C is the set of city pairs, and C_N is the number of available city pairs in time t. A U-pattern is evident for both dispersion measures during the period 1990-2002 which corresponds with findings of other authors using micro-data (Engel and Rogers 2004; Bergin and

Glick 2007; Wolszczak-Derlacz 2008b). Bergin and Glick (2007) find it especially surprising given the rise of Internet usage and continuous integration of markets leading to higher price transparency. However, as I have explained above, known existence of a non-zero relative price is only a necessary, but not sufficient condition for arbitrage to take place. The evidence merely suggests that after period of arbitrage costs decrease, since 1997 the zone of no-arbitrage widened again.

Figure 8 and Figure 9 show that the rough pattern is present also when disaggregating to individual product groups. Not surprisingly, the highest dispersion over the whole observed period shows the group of non-traded goods. On the other hand, lowest variation in relative prices has the group of recreation products which is also expected given the items included (Time magazine, paperback novel, or color television).

Figure 6: SD

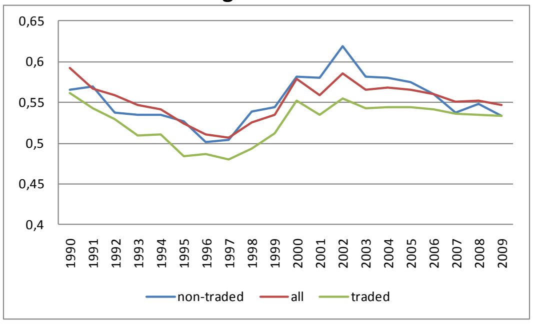


Figure 7: MSE

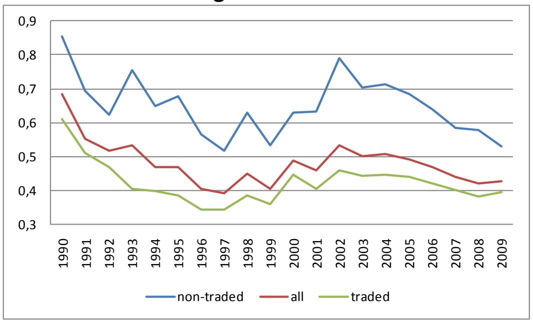


Figure 8: SD for different categories

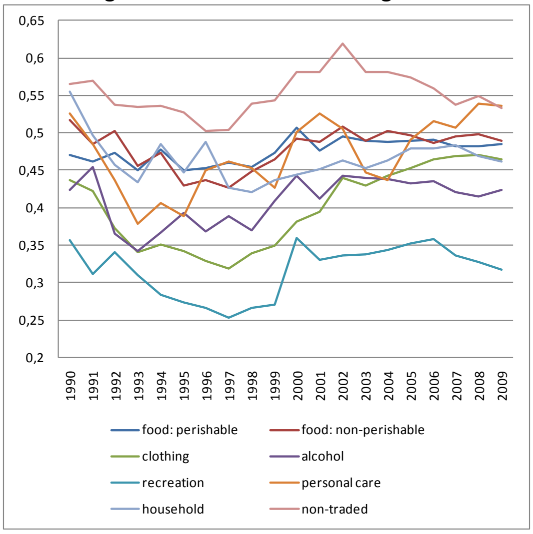
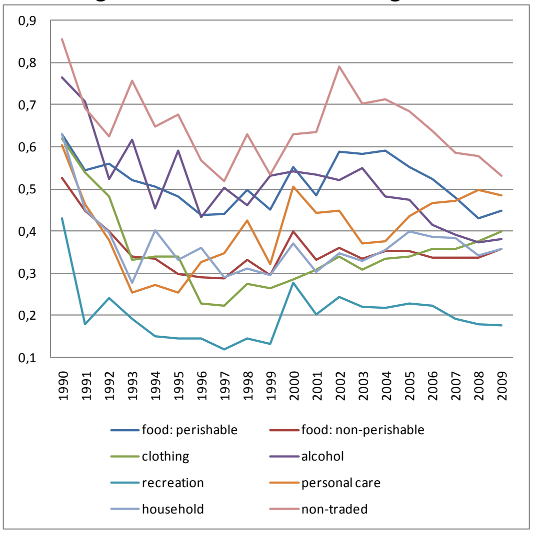


Figure 9: MSE for different categories



There is one interesting difference between the two used dispersion measures. At the beginning of 1990s, a sharp decline in MSE is documented which is not mirrored in the SD. The rest of the series development is very similar for both measures. As I mentioned before, the only difference between MSE and SD is that SD ignores the mean, i.e. the city-pair fixed effect. Because we averaged across all city pairs, a decline in MSE which is not accompanied by decline in SD signals that, on average, the no-arbitrage band moved closer to zero without changing its width.

Given the time period, one hypothesis suggests itself: Price levels in countries that opened their markets by the fall of socialism at the end of 1980s should converge with higher pace to price levels in other economies. Suppose, for example, that in 1990, all prices in West Germany were higher than in the Czech Republic. If between 1990 and 1991 all prices in the Czech Republic (expressed in ECU) increase by approx. the same proportion, standard deviation of the relative prices would remain intact but mean square error would decrease substantially. And, indeed, the data seem to provide support for this hypothesis. Table 3 shows city pairs with highest differences between 1993 and 1990 dispersion measured as mean square error. First 61 positions are occupied by pairs where one of the cities is Warsaw, Prague, or Budapest. No such pattern is observable using standard deviation.

Table 3: Top MSE differences between 1993 and 1990

#	City 1	City 2	1993-1990	#	City 1	City 2	1993-1990
1.	Helsinki	Warsaw	2.534066	11.	Barcelona	Warsaw	1.678679
2.	Helsinki	Prague	2.464104	12.	Paris	Warsaw	1.622458
3.	Prague	Stockholm	2.256737		…	…	…
4.	Stockholm	Warsaw	2.236346	40.	Budapest	Helsinki	1.254125
5.	Oslo	Warsaw	2.007358		…	…	…
6.	Warsaw	Zurich	1.997984	48.	Budapest	Stockholm	1.072657
7.	Oslo	Prague	1.979630		…	…	…
8.	London	Warsaw	1.835583	62.	Moscow	Zurich	0.654976
9.	Dublin	Warsaw	1.729069		…	…	…
10.	Prague	Zurich	1.711344	66.	Helsinki	Lisbon	0.602652

III.4. Explaining price dispersion

Authors of studies introduced in previous chapters came up with a large number of different variables to explain excessive cross-border price dispersion. Distance as a proxy for transportation costs is included in all of them. Nominal exchange rate volatility also proved to be positively correlated with price dispersion (Engel and Rogers 2001; Parsley and Wei 2001; Bergin and Glick 2007; Parsley and Wei 2007; Wolszczak-Derlacz 2008b). Other factors explaining some part of the border effect are common language in cities, tax and income levels in respective countries, and trade intensity between them (Wolszczak-Derlacz 2008b).

Tariff rates significantly correlate with price dispersion (Bergin and Glick 2007; Parsley and Wei 2007). And consideration of inputs tradability allows more precise classification of products and reveals that a part of dispersion attributed to the existence of border may be explained by the existence of non-traded inputs even to highly tradable goods (Crucini, Telmer, and Zachariadis 2005b; Crucini and Shintani 2008). Last but not least, distance is not an ideal proxy for transportation costs because the real costs per unit of distance do not have to be constant in time. Inclusion of a measure of unit transportation costs also explains part of the cross-border price dispersion (Parsley and Wei 2001; Bergin and Glick 2007).

It is not my plan to replicate all the above mentioned results. Instead, I will focus on the neglected role of institutions influencing business environment, as explained in section III.1, and use following explanatory variables:

1. distance between cities
2. population density of cities
3. institutional quality in countries

Distance between cities is used as a proxy variable for transportation costs which are expected to influence the width of no-arbitrage zone. I calculate the distance between cities using the great circle formula. Latitude and longitude of cities is obtained from Wikipedia. Problem of distance as a proxy for transportation costs is not only the absence of unit costs, but also the fact that types of transport, as well as quality of infrastructure can drastically vary case to case. Some kind of effective distance measure would be more appropriate. However, due to data limitation, simple geographical distance is used.

Population density in cities, again obtained from Wikipedia, is meant as a proxy for competition intensity. The more is the city area populated, the bigger should be the competition among retailers. Fierce competition should then lead to lower profit margins and motivate to more intensively look for profit-making arbitrage opportunities. Population density is, therefore, expected to be negatively correlated with price dispersion. In the regression I use average density of each city pair.

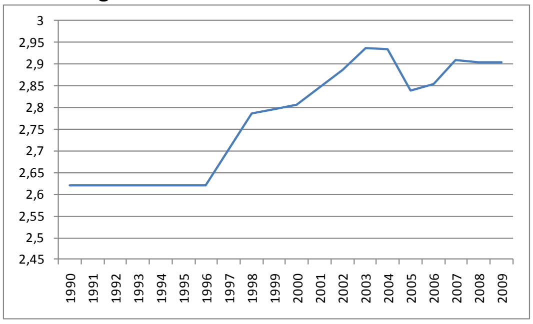
For the baseline regression I use three indicators from the WGI project. I construct the institutional quality variable as a sum of regulatory quality, rule of law, and control of corruption indicators for all countries. Table 4 and Figure 10 provide a brief overview of the constructed measure. As WGI are available only since 1996, I use the same levels of indicators for the years before. Also, until 2002 the indexes were calculated only every other year. Values in 1997, 1999, and 2001 are, therefore, always averages of value in the previous and the following year. For each city pair, the institutional measure is constructed as a plain sum of levels attributed to the countries the cities are located in. Higher value of institutional measure indicates better institutional quality. Better quality of institutions is expected to be correlated with lower dispersion because good regulations, rule of law, and low corruption lower the expected costs of entrepreneurial activity, making the costs of arbitrage smaller.

Table 4: Descriptive statistics of institutional measure

	Mean	Std. dev.
Institutions	2.850304	2.941028

	Min	Max
Institutions	-4.276190	6.366345

Figure 10: WGI institutional measure



Standard deviation and mean square error of traded goods are used as dependent variables:

$$X_{ij,t} = \alpha_0 + \alpha_1 \ln(distance)_{ij} + \alpha_2 border_{ij} + \alpha_3 \ln(density)_{ij} + \alpha_4 institutions_{ij,t} + \sum_{t=1991}^{2009} \beta_t Y_t + \varepsilon_{ij,t} \tag{3.5}$$

where $X_{ij,t}$ is either $SD_{ij,t}$, or $MSE_{ij,t}$ and Y_t are year fixed effects to capture time-varying factors influencing all city pairs. Variance is clustered on the country pair level to allow for intra-group correlation. All estimates are done using OLS estimator.

Table 5: Regression results for specifications (1)-(4)

	(1)	(2)	(3)	(4)
Log distance	0.050 (0.006)***	0.070 (0.014)***	0.052 (0.006)***	0.050 (0.006)***
Border	0.108 (0.008)***	0.144 (0.021)***	0.107 (0.008)***	0.108 (0.008)***
Log density	-0.032 (0.006)***	-0.097 (0.015)***	-0.034 (0.006)***	-0.032 (0.006)***
Sum of institutions	-0.015 (0.001)***	-0.029 (0.002)***	-0.015 (0.001)***	-0.015 (0.001)***
Trend 1990-1997			-0.007 (0.001)***	
Trend 1997-2002			0.005 (0.001)***	0.006 (0.001)***
Trend 2002-2009				-0.002 (0.001)***
Disp. measure	SD	MSE	SD	SD
Year fixed effects	Yes	Yes	No	No
Observations	13004	13004	13004	13004
Adjusted R^2	0.49	0.26	0.48	0.47

Note: **Robust standard errors in parentheses. Significance at 1%, 5%, 10% indicated by ***, **, *, respectively. Constant is not reported. Year effects are plotted in Figure 11 and Figure 12.**

Table 5 presents regressions results. Column 1 shows a regression of SD as price dispersion measure, column 2 shows results when using MSE. The coefficients are in both cases highly significant and have expected signs. Cities further apart and those separated by a national border have higher price dispersion. On the other hand, more dense cities and cities with better institutions tend to have lower price dispersion. This corresponds to prediction based on economic theory. I plot the coefficients for year dummies to see whether the used explanatory variables are sufficient to model the U-shaped pattern observed in price dispersion measures. Figure 11 and Figure 12 show that the pattern is still observable. To formally test it, I follow the procedure used by Bergin and Glick (2007) and replace the year dummies with two trend variables – one running from 1990 to 1997, and the second from 1997 to 2002, where we saw the peak in averaged SD and MSE series.

Column 3 of Table 5 reports that both time trends are statistically significant, the first one is negative and the second positive. The same exercise is done with periods 1997-2002 and 2002-2009 where we observed decreasing trend in dispersion. Again, as column 4 shows, both coefficients are significantly different from zero and of expected signs. Qualitatively same results are obtained when using MSE as dispersion measure, and are, therefore, not reported.

Figure 11: Year fixed effects (incl. 95% conf. intervals), specification (1)

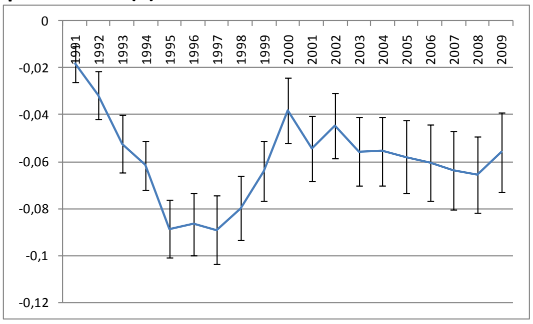
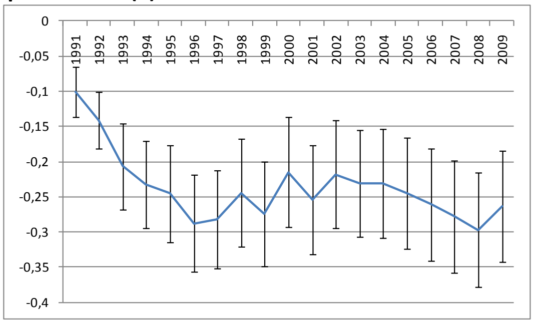


Figure 12: Year fixed effects (incl. 95% conf. intervals), specification (2)



Theoretically, the role of institutions should be more important in case of cross-border city pairs. No special permission is in most cases needed to trade among cities inside one country. It is, therefore, not necessary to communicate with regulatory authorities in such case. Moreover, already existing retailers already buy their goods from some wholesalers in their countries. Switch to a different wholesaler or arbitrage from another retailer in case of lower prices and, as a consequence, higher profit margins shouldn't be a complicated process dependent on institutional quality.

On the other hand, trade across borders is connected with significantly larger risks. The arbitrageurs have to deal with people they don't know, often with completely dissimilar cultural backgrounds. They cannot use the social networks they use in their domestic country. They have to familiarize with unknown regulations and deal with customs and tax officers. To put it in a nutshell, when trading across national borders, regulatory quality, quality of judiciary system and corruption should become much more important. To test this hypothesis, I add an interaction term border*institutions among explanatory variables. Columns 5 and 6 in Table 6 present results of this amended regression. The interaction term is significant and negative for both specifications, which is consistent with the theoretical expectation.

However, when using SD as the dispersion measure, coefficient by the original institutions variable turns positive. The problem is that when using interactions, the original terms included in the interaction cannot be so clearly interpreted. In this case, the original institutions term is an average effect of both cross-border and within-country city pairs. And indeed, when running the original baseline regression using only within-country city pairs, the institutions variable is still negative, even though very close to zero (see Appendix Table A5, column 1).

Table 6: Regression results for specifications (5)-(8)

	(5)	(6)	(7)	(8)
Log distance	0.050 (0.006)***	0.070 (0.014)***	0.067 (0.006)***	0.115 (0.014)***
Border	0.263 (0.009)***	0.355 (0.023)***	0.093 (0.011)***	0.049 (0.018)***
Log density	-0.032 (0.006)***	-0.096 (0.015)***	-0.021 (0.017)	-0.034 (0.041)
Sum of institutions	0.003 (0.000)***	-0.005 (0.001)***	-0.017 (0.003)***	-0.002 (0.010)
Border*institutions	-0.018 (0.001)***	-0.025 (0.002)***		
Disp. measure	SD	MSE	SD	MSE
Year fixed effects	Yes	Yes	Yes	Yes
City fixed effects	No	No	Yes	Yes
Observations	13004	13004	13004	13004
Adjusted R^2	0.50	0.26	0.67	0.49

Note: **Robust standard errors in parentheses. Significance at 1%, 5%, 10% indicated by ***, **, *, respectively. Constant is not reported.**

Appendix Tables A5-A7 report in columns 2-10 the results for individual product categories. All of them show significant impact of border and institutional quality on price dispersion. Distance and population density are also significant in all but two cases. However, certain categories have very low R-squared which indicates that the model used to explain the extent of price dispersion is not very suitable for these categories. Low goodness of fit can have two sources: First, the width of no-arbitrage band may be wrongly identified, as the number of included items is very limited in categories other than perishable and non-perishable food, clothing, and non-traded goods. And second, some categories of products may have specific process of price setting which is not captured by the model. This could be the case of alcoholic beverages which have, together with personal care products, larger estimated border effect than non-traded goods.

Many authors include city fixed effects to their regressions. Results with city fixed effects are in columns 7 and 8 in Table 6. As an expected consequence, density variable turns in both specifications insignificant. However, more interesting is that with MSE dispersion measure, inclusion of city fixed effects completely removed the explanatory power of institutions.

We see that the institutional quality measured by governance indicators is correlated with price dispersion in an expected way – city pairs with better institutions tend to deviate less from LOOP. But we might be also interested in a more detailed measure of institutional quality to see what impact have different aspects of institutional framework. To answer this question I use BEEPS results, specifically the answers to question "Can you tell me how problematic are these different factors for the operation and growth of your business?" From the proposed factors I choose the following: tax rates, tax administration, customs and trade regulations, business licensing and permits, labor regulations, uncertainty about regulatory policies, macroeconomic instability (inflation, exchange rate), functioning of judiciary, and corruption.

To test the roles of individual factors, I replace the $institutions_{ij,t}$ variable with set of time-invariable variables listed above. Unfortunately, the intersection of countries included in the survey and cities I have price data for is much more limited – 23 (listed in Appendix Table A4). Higher value means the factor is a bigger obstacle. The values vary between 1 (no obstacle) and 5 (major obstacle). Problem with the individual measures is that some of them are highly correlated (see Appendix Table A8). Results of a regression including all of the variables suggest that there may, indeed, be a problem with multicollinearity of the institutional measures because many of them are insignificant or have different signs than expected. Running separate regressions always with only one institutional measure shows that with the exception of business licensing and permits, macroeconomic instability, and functioning of judiciary are all measures statistically significant.[1] However, the labor regulations impact is statistically significant but negative. This means that the more managers see labor regulations as problem, the less price dispersion there is between the two cities in question.

[1] Impact of business licensing and permits is practically zero, with 95% confidence interval of -0.055–0.068; explanatory power of macroeconomic instability is significantly higher: coefficient has confidence interval of -0.013–0.062 (p-value 0.204); coefficient of judiciary quality measure has confidence interval -0.017–0.063 (p-value 0.254).

The best specification seems to be without tax administration variable which is highly correlated with tax rates and insignificant when both are included, and always with only one of the following measures: seriousness of obstacles stemming from uncertainty about regulatory policies, macroeconomic instability, functioning of the judiciary, and corruption. Only macroeconomic instability and judiciary quality yield significant impact and are, therefore, reported in columns 11 and 12 of Table 7.

The more managers felt tax rates as an obstacle for business in their country, the higher is the predicted price dispersion. The same is true for customs and trade regulations, macroeconomic instability, and judiciary quality. These four effects are significant and have expected direction. Most important, judging by the size of its coefficient, are for entrepreneurs doing arbitrages trade regulations.

The most striking are estimates of perceived labor regulations and licenses impact which are both statistically significant but have negative sign. This means that the more managers see labor regulations and licensing and permits as problems, the less price dispersion there is between the two cities in question. I have no clear explanation for this result.

Table 7: Regression results for specifications (9)-(12)

	(9)	(10)	(11)	(12)
Log distance	0.043 (0.006)***	0.050 (0.015)***	0.058 (0.014)***	0.061 (0.013)***
Border	0.080 (0.008)***	0.087 (0.020)***	0.121 (0.026)***	0.113 (0.025)***
Log density	-0.018 (0.006)***	-0.068 (0.014)***	-0.025 (0.012)**	-0.020 (0.013)[2]
Diff. in institutions	0.023 (0.002)***	0.048 (0.005)***		
Tax rates			0.036 (0.020)*	0.050 (0.018)***
Customs & trade reg.			0.136 (0.044)***	0.106 (0.042)**
Licenses and permits			-0.081 (0.045)*	-0.093 (0.044)**
Labor regulations			-0.115 (0.020)***	-0.121 (0.020)***
Macro. instability			0.026 (0.016)*	
Judiciary quality				0.045 (0.021)**
Disp. measure	SD	MSE	SD	SD
Year fixed effects	Yes	Yes	Yes	Yes
City fixed effects	No	No	No	No
Observations	13004	13004	3971	3971
Adjusted R^2	0.49	0.28	0.48	0.48

Note: **Robust standard errors in parentheses. Significance at 1%, 5%, 10% indicated by ***, **, *, respectively. Constant is not reported.**

As described above, the WGI institutional measure for each city pair was constructed as a sum of both cities' indicators. The reason for this is that we expect city pairs with better institutions, conditional on their distance, to be more arbitrage-friendly, thereby with lower differ-

[2] In the last specification, density is significant only marginally with the p-value of 0.126.

ences in prices of traded products. One other approach of estimating the role of institutions is, however, also possible: we can, instead of sums, look at differences in institutional quality. The larger is the difference, the more dispersed should the prices be.

To illustrate the idea on an example, suppose we choose two cities from, e.g. Dublin and Moscow. If there are better quality institutions in Dublin, then the deviations from LOOP should be smaller there than in Moscow, where the institutional quality is lower, because Dublin is easily accessible for arbitrage activities. As a consequence, relative prices should be more dispersed between these two cities, conditional on distance and other factors, than between e.g. Dublin and Berlin.

Columns 9 and 10 in Table 7 report the results for regression where for each city pair the sum of institutional indicators is replaced with their absolute-value difference. Regardless of used dispersion measure, the impact of difference in institutional quality is significant and of expected direction. The same was applied to BEEPS indicators, however with mixed results. When using SD, most of the institutional explanatory variables turn insignificant or have coefficients with negative signs. On the other hand, with MSE as price dispersion measure, the results are as expected. I do not report the results because the reasons stay very unclear to me.

IV. CONCLUSION

There is large literature on two puzzles present in international economics. First, there seems to be sizable home bias in trade. And second, the size and persistence of deviations from law of one price and, in aggregate version, from purchasing power parity is much larger than justifiable by economic theory. These two topics gave rise to analysis of size and sources of an effect of national borders.

This paper investigated the price dispersion among countries in European region between 1990 and 2009. First of all, in accordance with existing literature, sizable border effect is found during the whole period. Moreover, price dispersion in my data follows an already documented U-shaped pattern with price dispersion declining until 1997, increasing between 1997 and 2002, and then slowly diminishing again. In contrast to other studies I used two measures of price dispersion – standard deviation and mean square error – to find out whether the choice of dispersion measure influences the outcome of the whole analysis. Regression analysis results suggest that even though both measures tell qualitatively the same story, inclusion of city fixed effects leads to differences in explanatory variables significance.

Previous studies use several variables to explain excessive cross-border price dispersion. Distance as a proxy for transportation costs, nominal exchange rate volatility, tariff rates, taxes and income levels in respective countries, language differences, etc. Their underlying idea is that after we control for these and other sources of price dispersion between cross-border city pairs then the process of arbitrage should equalize the prices in different places. However, arbitrage is not an automatic equilibrating process, it is an entrepreneurial activity. I argue that once we understand arbitrage as productive entrepreneurial activity, institutional quality should be one of determinants of arbitrage attractiveness and should, therefore, influence the price dispersion.

To test this hypothesis, I expressed the quality of institutions as one of the factors influencing total costs of arbitrage, together with population density in cities used as a proxy for competition intensity, and distance. The regression analysis proves that all three variables explain a part of observed price dispersion. I find that the higher is the density and the better are the institutions, the lower is the predicted dispersion.

We can interpret the result from two viewpoints. First, it shows that institutional quality does explain another part of dispersion previously attributed to the sole existence of borders. It also confirms the hypothesis that arbitrage, as an entrepreneurial activity, should be affected by the existing institutional framework. However, we can also see the result from the opposite perspective as a small contribution to the literature empirically testing the theory of productive and unproductive entrepreneurship.

Further work remains. It would be, for example, very useful to obtain time series data on different disaggregated institutional measures and estimate the impact of individual aspects of business environment on the costs of arbitrage. Such work could have serious policy implications especially for developing countries.

V. APPENDIX

Table A1: Traded items in sample

Food and non-alcoholic beverages: perishable White bread (1 kg) Butter (500 g) Margarine (500 g) Spaghetti (1 kg) Flour, white (1 kg) Sugar, white (1 kg) Cheese, imported (500 g) Cornflakes (375 g) Milk, pasteurised (1 l) Potatoes (2 kg) Onions (1 kg) Tomatoes (1 kg) Carrots (1 kg) Oranges (1 kg) Apples (1 kg) Lemons (1 kg) Bananas (1 kg) Lettuce (one) Eggs (12) Beef: filet mignon (1 kg) Beef: steak, entrecote (1 kg) Beef: stewing, shoulder (1 kg) Beef: roast (1 kg) Beef: ground or minced (1 kg) Veal: chops (1 kg) Veal: fillet (1 kg) Veal: roast (1 kg) Lamb: leg (1 kg) Lamb: chops (1 kg) Lamb: stewing (1 kg) Pork: chops (1 kg) Pork: loin (1 kg) Ham: whole (1 kg) Bacon (1 kg) Chicken: fresh (1 kg) Fresh fish (1 kg) Orange juice (1 l) ***Food and non-alcoholic beverages: non-perishable*** White rice (1 kg)	Olive oil (1 l) Peanut or corn oil (1 l) Peas, canned (250 g) Tomatoes, canned (250 g) Peaches, canned (500 g) Sliced pineapples, can (500 g) Chicken: frozen (1 kg) Frozen fish fingers (1 kg) Instant coffee (125 g) Ground coffee (500 g) Tea bags (25 bags) Cocoa (250 g) Drinking chocolate (500 g) Coca-Cola (1 l) Tonic water (200 ml) Mineral water (1 l) ***Clothing and footwear*** Business suit, two piece, medium weight Business shirt, white Men's shoes, business wear Men's raincoat, Burberry type Socks, wool mixture Dress, ready to wear, daytime Women's shoes, town Women's cardigan sweater Women's raincoat, Burberry type Tights, panty hose Child's jeans Child's shoes, dresswear Child's shoes, sportswear Girl's dress Boy's jacket, smart Boy's dress trousers ***Alcoholic beverages*** Wine, common table (1 l) Wine, superior quality (700 ml) Wine, fine quality (700 ml) Beer, local brand (1 l) Beer, top quality (330 ml)	Scotch whisky, 6 y old (700 ml) Gin, Gilbey's or equiv. (700 ml) Vermouth, Martini & Rossi (1 l) Cognac, French VSOP (700 ml) Liqueur, Cointreau (700 ml) ***Recreation*** Compact disc album Television, colour (66 cm) Kodak colour film (36 expos) International foreign daily newspaper International weekly news magazine (Time) Paperback novel (at bookstore) ***Personal care*** Aspirins (100 tablets) Razor blades (five pieces) Toothpaste with fluor. (120 g) Facial tissues (box of 100) Hand lotion (125 ml) Lipstick (deluxe type) ***Household supplies*** Soap (100 g) Laundry detergent (3 l) Toilet tissue (two rolls) Dishwashing liquid (750 ml) Insect-killer spray (330 g) Light bulbs (two, 60 watts) Batteries (two, size D/LR20) Frying pan (Teflon or good equivalent) Electric toaster (for two slices) ***Not included in any category*** Yoghurt, natural (150 g) Mushrooms (1 kg) Shampoo & conditioner in one (400 ml)

Table A2: Non-traded items in sample

Non-traded Laundry (one shirt) Dry cleaning, man's suit Dry cleaning, woman's dress Dry cleaning, trousers Man's haircut (tips included) Woman's cut & blow dry (tips included) Telephone and line, monthly rental Hourly rate for domestic cleaning help Maid's monthly wages (full time) Business trip, typical daily cost Hilton-type hotel, single room, one night including breakfast	Moderate hotel, single room, one night including breakfast Babysitter's rate per hour Cost of developing 36 colour pictures Daily local newspaper Three-course dinner for four people Four best seats at theatre or concert Four best seats at cinema Cost of a tune-up (but no major repairs) (low) Cost of a tune-up (but no major repairs) (high) Regular unleaded petrol (1 l)	Taxi: initial meter charge Taxi rate per additional kilometre One drink at bar of first class hotel Two-course meal for two people Simple meal for one person Fast food snack: hamburger, fries and drink Hire car, weekly rate for lowest price classification Hire car, weekly rate for moderate price classification ***Not included in the category*** Telephone, charge per local call from home (3 mins)

Table A3: Cities in sample

Almaty	*Kazakhstan*	Geneva	*Switzerland*	Moscow	*Russia*
Amsterdam	*Netherlands*	Hamburg	*Germany*	Oslo	*Norway*
Athens	*Greece*	Helsinki	*Finland*	Prague	*Czech Rep.*
Baku	*Azerbaijan*	Istanbul	*Turkey*	Paris	*France*
Barcelona	*Spain*	Copenhagen	*Denmark*	Rome	*Italy*
Berlin	*Germany*	Kiev	*Ukraine*	Reykjavik	*Iceland*
Belgrade	*Serbia*	London	*UK*	Sofia	*Bulgaria*
Bratislava	*Slovakia*	Lisbon	*Portugal*	St. Petersbrg	*Russia*
Brussels	*Belgium*	Luxembourg	*Luxembourg*	Stockholm	*Sweden*
Bucharest	*Romania*	Lyon	*France*	Tashkent	*Uzbekistan*
Budapest	*Hungary*	Madrid	*Spain*	Vienna	*Austria*
Dublin	*Ireland*	Manchester	*UK*	Warsaw	*Poland*
Düsseldorf	*Germany*	Milan	*Italy*	Zurich	*Switzerland*
Frankfurt	*Germany*	Munich	*Germany*		

Table A4: Countries included in BEEPS04/05

Almaty	*Kazakhstan*	Frankfurt Dublin	*Germany Ireland*	Munich Moscow	*Germany Russia*
Athens	*Greece*	Düsseldorf	*Germany*	Prague	*Czech Rep.*
Baku	*Azerbaijan*	Hamburg	*Germany*	Sofia	*Bulgaria*
Berlin	*Germany*	Istanbul	*Turkey*	St. Petersburg	*Russia*
Belgrade	*Serbia*	Kiev	*Ukraine*	Tashkent	*Uzbekistan*
Bratislava	*Slovakia*	Lisbon	*Portugal*	Warsaw	*Poland*
Bucharest	*Romania*	Madrid	*Spain*		
Budapest	*Hungary*				

Table A5: Additional regressions

	(1)	(2)	(3)	(4)
Log distance	0.005 (0.014)	0.055 (0.005)***	0.043 (0.006)***	0.001 (0.005)
Border		0.228 (0.009)***	0.248 (0.010)***	0.082 (0.007)***
Log density	-0.096 (0.009)***	-0.026 (0.005)***	-0.024 (0.006)***	-0.015 (0.005)***
Sum of institutions	-0.003 (0.001)*	0.006 (0.000)***	0.003 (0.000)***	-0.009 (0.000)***
Border*institutions		-0.019 (0.001)***	-0.014 (0.001)***	-0.002 (0.001)***
Category		Food: perishable	Food: non-per.	Clothing
Disp. measure	SD	SD	SD	SD
Year fixed effects	Yes	Yes	Yes	Yes
City fixed effects	No	No	No	No
Observations	308	13004	13004	12886
Adjusted R^2	0.32	0.40	0.32	0.31

Note: Robust standard errors in parentheses. Significance at 1%, 5%, 10% indicated by ***, **, *, respectively. Constant is not reported.

Table A6: Additional regressions

	(5)	(6)	(7)	(8)
Log distance	0.020 (0.007)***	0.042 (0.007)***	0.044 (0.013)***	0.029 (0.006)***
Border	0.316 (0.013)***	0.247 (0.012)***	0.328 (0.019)***	0.235 (0.010)***
Log density	-0.027 (0.007)***	-0.019 (0.007)***	-0.011 (0.011)	-0.030 (0.007)***
Sum of institutions	0.003 (0.001)***	-0.001 (0.001)	0.009 (0.001)***	0.010 (0.000)***
Border*institutions	-0.022 (0.001)***	-0.022 (0.001)***	-0.025 (0.002)***	-0.015 (0.001)***
Category	Alcohol	Recreation	Personal	Household
Disp. measure	SD	SD	SD	SD
Year fixed effects	Yes	Yes	Yes	Yes
City fixed effects	No	No	No	No
Observations	13004	13004	13004	13004
Adjusted R^2	0.28	0.37	0.16	0.11

Note: Robust standard errors in parentheses. Significance at 1%, 5%, 10% indicated by ***, **, *, respectively. Constant is not reported.

Table A7: Additional regressions

	(9)	(10)	(11)	(12)
Log distance	0.071 (0.007)***	0.056 (0.006)***		
Border	0.312 (0.014)***	0.296 (0.011)***		
Log density	-0.057 (0.008)***	-0.039 (0.006)***		
Sum of institutions	-0.001 (0.001)**	0.002 (0.000)***		
Border*institutions	-0.021 (0.001)***	-0.021 (0.001)***		
Category	Non-traded	All		
Disp. measure	SD	SD		
Year fixed effects	Yes	Yes		
City fixed effects	No	No		
Observations	13004	13004		
Adjusted R^2	0.42	0.52		

Note: Robust standard errors in parentheses. Significance at 1%, 5%, 10% indicated by ***, **, *, respectively. Constant is not reported.

Table A8: Correlation matrix of BEEPS institutional measures

	1	2	3	4	5	6	7	8	9
1	1.0000								
2	0.7750	1.0000							
3	0.6585	0.7805	1.0000						
4	0.2199	0.4580	0.7236	1.0000					
5	0.5046	0.4165	0.4552	0.3795	1.0000				
6	0.4758	0.3750	0.6219	0.5516	0.3039	1.0000			
7	0.5442	0.3846	0.5918	0.5631	0.4945	0.9395	1.0000		
8	0.4682	0.4853	0.7622	0.7315	0.5422	0.8951	0.8856	1.0000	
9	0.5018	0.5922	0.8109	0.7174	0.1840	0.8199	0.7255	0.8376	1.0000

1 – Tax rates, 2 – Tax administration, 3 – Customs and trade regulations, 4 – Business licensing and permits, 5 – Labor regulations, 6 – Uncertainty about regulatory policies, 7 – Macroeconomic instability, 8 – Functioning of judiciary, 9 – Corruption

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Project of Master Thesis

Author: Bc. Jiří Schwarz
Supervisor: doc. Ing. Štěpán Jurajda, Ph.D.

Preliminary title: **Impact of Institutions on Cross-Border Price Dispersion**

Objective of the thesis:

There is already large literature on the magnitude of the so called "border effect", i.e. an impact of the existence of national borders on trade. The researches take two distinct ways of measuring such effect. The first stream of authors, starting with McCallum (1995), try to explore how borders affect trade by looking at the difference between intra- and international trade after controlling for distance and some other variables. One problem with this approach is that there is often very limited data on intranational trade especially for smaller countries. A method of estimating border effects in the absence of such data was introduced by Wei (1996). However, it is very sensitive to the estimates of internal distance and can, therefore, easily lead to large biases.

Another way of measuring the border effect was introduced by Engel and Rogers (1996) who showed that the standard deviation of relative prices in U.S. and Canadian cities is systematically higher for cross-border city pairs than for city pairs within the same country. Even after controlling for potential sources of this excess price variability, such as language differences, distance, exchange-rate volatility etc., there still remains relatively large unexplained residuum credit to the mere existence of national borders. However, this approach, too, is a subject of possible bias stemming from cross-country heterogeneity in the distribution of within-country price differentials, as shown by Gorodnichenko and Tesar (2008).

In this thesis an attempt will be made to explain another part of the border effect by addressing the impact of business environment on the variability of cross-border prices. As the data source for price information, the EIU City Data price panel, containing absolute prices for more than 160 different products and services in 140 cities worldwide in the period 1990-2008, will be used. This dataset was already used in some of the recent papers, such as Engel and Rogers (2004), Wolszczak-Derlacz (2008), Crucini and Shintani (2008). Institutional framework affecting the business environment should have a significant impact on the size of border effect as it strongly influences the trade possibilities and potential profitability. Different proxies for the institutional environment will be used, including the EBRD BEEPS data, used e.g. by Commander and Svejnar (2007).

Hypotheses:

1. The activity of businesses is influenced by the institutional environment
2. Institutional environment has a significant impact on the cross-border price dispersion

Outline:

1. The law of one price in theory and reality
2. Survey of existing empirical literature on border effect
3. The role of entrepreneur/arbitrageur in the law of one price
4. The impact of institutions on the border effect

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