

Introduction

In *Globalization, growth and poverty*, the 2002 edition of its influential Policy research report, the World Bank (World Bank 2002) argues that globalization has been a powerful source of growth and reduction in poverty. It admits that benefits within each country have been unequally distributed, although cases of absolute (as opposed to relative) fall in standard of living have been very rare. The reports also admits that several countries, especially in Africa, have not benefited that much from globalization, but it blames inefficient institutions and/or misguided trade policies for this outcome. Indeed, economists have been convinced of the benefits of market integration for long-run growth since Adam Smith. The specialization and the better allocation of factors increases total output and income, the reduction in price volatility reduces risks and thus indirectly fosters investments, the greater size of the market makes it possible to exploit economies of scale in production and also helps innovation via transfer of technologies.

Economic historians, by and large, share the positive opinion on the effect of globalization. They deem the growing integration for markets for goods, capital and labour to have been one of the most powerful source of modern economic growth in the “long” 19th century, from Waterloo to World War One (O’Rourke and Williamson 1999). In contrast, they are much more cautious about the previous period. O’Rourke and Williamson (2002a and b) rules out any trend towards globalization before 1800. Their opinion, however, contrasts with the conventional wisdom among historians, inspired by Braudel (1979) and Wallerstein (1972-1989) and continued by Gunder Frank (1998) and others (cf. Flynn-Giraldez 2004 and the rejoinder by O’Rourke and Williamson 2004). They believe that globalization started earlier, as result of European imperialism, and are much more skeptical about its benefits. To some extent, this debate hinges upon the optimal measure of globalization. Historians rely on a wide range of indexes, vaguely related to trade, while economic historians focus on convergence of prices among different areas. They deem this latter the best yardstick for integration because prices are the key factor for decisions by agents about consumption and production, which ultimately determine welfare.

The proposed research aims at mapping the process of integration and at estimating its effects on welfare and growth. From this point of view, globalization is only the culmination of a much wider and more complex process, which has deeply shaped economies and societies. The first task will thus to explore the pattern and timing of integration at all its level – countries, regions, continents and the whole world. “When did globalization begin?” is just one of the possible question to be addressed. Can we speak of a domestic French market already by 1700, or should we wait until the Revolution or perhaps the construction of railways in the mid-1850s? Was the substantial trade within South Asia evidence of an integrated market? Was the creation of the European single market after World War Two the final stage of a linear trend towards more integration, started sometimes in the past, or the last of a series of fluctuations – thus potentially reversible?

A thorough analysis of what happened is a preliminary step to a more ambitious research agenda about the effects of integration in history. How big were static gains from specialization? How were they distributed among countries, social groups, households and within households (e.g. by gender, age)? How much did integration foster economic growth in the long-run, and thus how much could it compensate the short-term losses? This research agenda is still widely unexplored. Thus far, scholars have focused on the measurement and –to a lesser extent- on causes of the process. Very few scholars deal with the effects of integration. Indeed they seem to assume that the more integrated and/or

efficient is the domestic or continental market, the better for long term growth. A telling case is the recent debate about the so called Great Divergence. Pomeranz (2000) argued that markets in China in the 18th century were well developed, so that poor integration could not explain the country failure to develop. This statement implies that that similar (high) integration must have similar (positive) effects. This inference is theoretically plausible, but it is not backed by hard estimates of gains from integration (or losses from dis-integration). Actually, such estimates are scarce even for the more recent years (no such data in pro-globalization books such as Bhagwati 2004 or Wolf 2004). The existing figures refer to specific areas or process of integration, such as the creation of the Single Market (Badinger 2005). To some extent, this dearth of estimates reflects the technical difficulty of measuring the dynamic effects of integration. Many economists prefer to give up the issue altogether rather than to settle on a partial, static estimate. This project does not share this attitude. It assumes that even partial and imperfect measures are better than no measure. Correspondingly it will try to measure benefits of integration (through better allocation of resources and/or reduced price volatility) for as many countries/areas for as long period as collected data will allow it. In thus work, due attention will be paid to countries of the “Atlantic economy” (Europe and the countries of Western Settlement) during era of first globalization but we will also try to extend our analysis to other areas and periods. We will deal as extensively as possible, within the constraints set by the literature and linguistic skills, with non-European countries and also with “peripheral” countries of Eastern Europe.

The last section of the project deals with institutions. A lesson, which everyone seems to have learned from history, is that globalization is fragile. Change in relative prices can stimulate the reaction by interest groups, which in turn can cause a backlash, as in the 1930s. As it often happens, this conventional wisdom relies on very few cases. The research project plans to cover more cases, including not only these short-term reactions but also long-term effects on institutions. A particularly interesting case is the slow demise of traditional systems of regulation of cereal markets. They aimed at reducing price fluctuations and protecting the (urban) population from famines and sudden shock in prices. The 18th century Enlightenment writers criticized these regulations as disruptive of the market in “normal” times and inefficient and/or unfair towards excluded population at times of crises. To what extent was this criticism justified? Were these institutions really so harmful to the market and so inefficient? How much did their demise owe to integration itself, which reduced the perceived need for protection?

Progress beyond the state of the art

The integration of financial and commodity markets is a hot topic in the current discourse among economic historians, as a quick look to the main field journals shows. Thus this is a good moment to take stock. As a starting point, one can distil the conventional wisdom among economic historians in four stylized facts:

i) until 1800, the development of long range trade did not yield any trend towards world-scale integration. The available evidence seems to rule out a long-term process of integration in Europe. The level of integration fluctuated widely both in the whole continent and in each country. The Chinese domestic market was fairly well integrated in the 18th century, while the Indian one had remained essential local.

ii) the 19th century was a period of growing integration, both within Europe and across continents, which was interrupted by World War One and the Great Depression. It took several decades of integration of the European and world economy after 1950 to return to its pre-1914 level

iii) in the long run integration was fostered mainly by technical change in transportation communication and market efficiency, while the effects of political events (wars, changes in boundaries) and trade policies have been mixed. In some periods, such as the early 19th century and most notably after World War Two, they have fostered integration, in others hampered it (e.g. in the 1930s but also throughout most of the pre-industrial period). On balance, policies have had a negative effect: without state interventions, markets would have integrated faster than they actually did.

iv) political intervention against integration has been often motivated by the interests of minorities, such as (urban) consumers until the end of the 18th century and producers in the 19th and 20th century.

Undoubtedly our knowledge is much richer than twenty or even ten years ago. Yet much remain to be done:

a) the field urgently need a rethinking of the concept of market integration. The recent literature still largely relies on the definition which the French economist Cournot put forward almost two centuries ago (Fackler-Goodwin 2001). His definition must be updated, by framing it in the theory of efficient markets (Fama 1970, Blake 2000). This task is necessary because different levels of market efficiency must be tackled with different statistical methods. So far, economic historians have used a bewildering array of techniques, from the “traditional” coefficient of correlation (e.g. Latham-Neal 1983) to the cointegration-based measures, with a simple ECM (Klovland 2005) or with a TAR -Threshold Autoregressive Models (Ernjaes-Persson 2000, Jacks 2005, Trenkler and Wolf 2005) to volatility indexes à la Engel-Rogers (1996), often without bothering too much about their theoretical assumptions and/or data requirements.

b) we are very far from having a coherent idea about what happened in commodity markets. The literature suffers from two major weaknesses. First, the use of different techniques, often on very few prices series, makes results hardly comparable in time and space. Second, the coverage by area and period is really uneven. Most historical work deals with the integration of domestic market for wheat in the 19th and early 20th century in selected European countries or in the “Atlantic economy” as a whole. Products other than cereals are barely quoted, with few exceptions - the most conspicuous being O’Rourke and Williamson (1994) and Klovland (2005). Forays into the pre-industrial era are growing in number but still limited (Bateman 2007, Özmucur and Pamuk 2007) and also the period after 1914 is, somewhat surprisingly, almost entirely neglected (Bukenya and Labys 2005, Federico-Persson 2007). The literature on domestic integration outside the “Atlantic economy” is rather thin, with the exception of China (Brandt 1985, Li 2000, Keller and Shiue 2007). There are scattered works on India (Studer forthcoming), Mexico (Dobado-Marrero 2005) and the United States (Slaughter 2001). Last but not least, there is almost no work on integration of global commodity markets in the 19th and 20th century- as opposed to the integration within the “Atlantic economy”.

c) While the causes of integration of commodity markets have attracted some attention (Epstein 2006, Jacks 2006), their effects have been largely neglected in the historical literature. There are very few estimates of benefits from integration in historical perspective. Very recently, O’Rourke (2007) has estimated that the 1807-1814 blockades reduced GDP of France, United Kingdom by 2-3% per year. This is roughly the order of magnitude of the effects of the protection on wheat in the late 19th century,

which had also a major effect on the distribution of income, increasing rents on land at the expenses of wages and profits (O'Rourke 1997, Federico-O'Rourke 2000). Some additional information can be obtained also from the literature on famines (O'Grada 2007). Economists, since Adam Smith and the Physiocrats (Persson 1999), have consistently argued that integration reduced the frequency and intensity of famines. Recent research has shown that grain markets were hardly less integrated than in non-famine years, and that they adjusted just as fast to disequilibria (Ó Gráda 2005).

d) Economic historians strongly believe in a simple theory of short-run institutional effects of globalization (Williamson and O'Rourke 1999). Losers from integration or liberalization would fight to keep their rents. If successful, as it has been often the case in history, they might hamper integration. In extreme cases, as in the 1930s, the collective action of many lobbies engendered a disastrous backlash. This approach has been widely used to interpret the return to protection in Continental Europe in the 1880s (Gerschenkron 1989, O'Rourke 1997). However, it must be tested with other case-studies

Scientific methodology

The previous discussion suggests that the existing literature needs improvements in three main respects:

a) theory and methodology: what exactly does "market integration" mean and how should we measure it?

b) measurement: what has happened? Did markets become more or less integrated in the long run? Did price become more or less volatile?

c) effects: how much has integration (dis-integration) augmented (reduced) welfare? Has it fostered long-run growth, especially in peripheral areas? Did freedom of domestic and international trade reduce or increase vulnerability to short-run crises relative to traditional institutions for managing trade? How much did integration create its own demise?

The research will address these issues following by three broad principles:

i) consider the very long run, from as far back in time as possible, using the same methodology

ii) cover as many goods as feasible

iii) cover as many countries as possible within and outside Europe

Clearly, the research group cannot promise to answer all these questions exhaustively and definitively, while abiding by these three principles. Theory is always evolving and thus the state-of-the art theory of 2007 might become outdated. The research team will aim at being as comprehensive as possible in building its data-base, but it is plainly impossible to be sure to have unearthed all possible sources for all countries. Effects are widespread and entangled with those of other long-run developments, such as technical progress, institutional change etc. Yet, it will be possible to provide a solid theoretical framework, a general overview of long terms trends and some case-studies which can be an inspiration for future research.

The first question needs an interdisciplinary approach. It uses concepts from economic theory to find (a set of) suitable econometric techniques to address a specific historical problem, given the characteristics (frequency, reliability etc.) the available data. Just for illustration, one can consider the case of the Band-TAR models, currently the most fashionable state-of the art test for market integration. Allegedly, it measures the transaction costs for bilateral arbitrage (the “commodity points”) and the speed of adjustment of prices to their equilibrium level after a shock has pushed them beyond the commodity points. But the TAR models needs a minimum number of observations and implies, inter alia, that transaction costs are constant over the period of estimation and that there is no arbitrage with a third market. It would be rash to use it with yearly data or if there is evidence of changes in transaction costs (e.g. in transportation costs, or in barrier to trade) and/or of indirect arbitrage. In these cases, other techniques, such as the factor dynamic analysis, seems much more suitable. Indeed, there is not such as thing as the “ideal” technique, suitable for all historical conditions and all data-bases.

The bulk of the research effort will be devoted to the construction of a data-base of commodity prices. It will cover the longest possible period, from as early as possible (i.e. the 14th century) to present. The research will focus on Europe, but it will gather also non-European prices whenever possible. The aim is to collect as many price series as possible – covering the greater range of tradable commodities (including manufactures) and the greater area. Historians have already dug out a huge amount of long-run price series, from a range of different sources - mercuriales (official quotations), records of private institutions (hospitals, courts and so on), newspapers and other periodical publication and, since the mid-19th century, official publications (statistical yearbooks etc.). The interest in price history started to develop in the late 19th century among antiquarians and local historians and peaked in the 1930s, with the establishment of the international Committee for History of prices. It arranged the publication of country-wide studies books covering the period to 1800, including well known classics as Posthumus (1943 and 1964) for the Netherlands or Beveridge (1965) for England. The Committee did not survive the War, but historians went on collecting prices, often as part of and many series can be found also in general purpose books (e.g. quite a few French theses of the 1960s and 1970s report price series). The historical sources are quite abundant for China as well (Wang Yeh-chien 1992). Data for India as far back as the 17th century can be obtained from Persian court chronicles and (later) from European factory records (Haider 2005). A preliminary work has showed that data on commodity prices are available also for some Latin American countries such as Mexico and Brazil. Clearly, the onset of the statistical age has greatly augmented the quality and quantity of price data. In the 19th and 20th century, statistical offices of almost all countries in the “Atlantic economy” and of some Western colonies (most notably India) publish price series in their yearbooks or in other statistical publications. Interestingly, the number of series of prices raw commodities available in these open access sources seems to decline quite sharply after World War Two. Some of these price series, especially for wheat and rye, are already easily available from historical statistics collections or websites, most notably by Allen and Unger (http://www2.history.ubc.ca/unger/htm_files/wheat.htm) and by Jacks (www.sfu.ca/~djacks/). But many more still lie un-earthed. The main task is to retrieve as much of this material as possible. Special attention will be paid to fill the gaps in our knowledge. Thus the research will target, as far as possible, areas (such as the Eastern Europe) and commodities (almost everything except cereals) not yet covered in the existing data-bases. Whenever possible, the group will resort to archival research for selected cities/commodities

Almost all price data are expressed in local weights and currencies and thus have to be harmonized. Local weights are comparatively easy to deal with, while the conversion of different currencies raises some conceptual issues. The already quoted Committee for the History of Prices decided to use silver prices throughout. This solution is still the most widely adopted for historical markets, but whenever

possible this research will resort to actual exchange rates. Historical data are available in reference collections such as McCusker (1978) and Schneider et al (1991).

The web site www.globalfinancialdata.com reports series of exchange rates for a large number of countries in the 19th and 20th century.

The data-base will be used to address the second question- what happened to integration and price volatility.

i) the analysis of integration must be as thorough and comprehensive as possible. In principle one would aim to measure integration by “country” (i.e. any political entity with constant boundaries), by region (Western, Southern and Eastern Europe, South East Asia etc.), across Continents (Europe, Asia, Latin America) and also between Europe and World, for as many products as possible, both in the long and in the short run. Particular attention should be given to periods of political turmoil and/or of major changes in policies, which were likely to affect integration. In principle, it would be advisable to use the same measures of integration all over the periods/areas in order to insure comparability, subject to the already quoted concerns about the data quality. As a whole, this is an ambitious goal. How much will be achieved depends on the number and the quality of price series collected.

ii) Price volatility can be estimated from residuals from a linear trend regression or from standard filtering techniques (e.g. Hodrick-Prescott) after taking into account seasonality. These techniques need high-frequency data (at least monthly) – and thus the issue will be limited to a subset of series.

The data-base can be used to address three further issues. First, it is possible to explore causes of integration/disintegration, either with simple variance analysis (Federico-Persson 2007) or with regressions. Second, it is possible to contrast price data can be contra with the available anecdotal information about famines in order to date more precisely them and to gauge their severity. Second, it is possible to construct price indexes for the whole Europe or for some countries or areas. All these tasks, although not indispensable for the main design of research, would yield important insights in the economic history of Europe. The decision whether to pursue them within this project or leave it aside for further work will be taken at a later stage

The results of this analysis of integration will be valuable not only by themselves but also to focus the work on the welfare effects on periods of integration (disintegration). The effect of price convergence (divergence) towards the “European” or “world” average can be modelled as a fall (increase) in tariffs. In empirical economics, the issue is often tackled with CGE modeling. The standard models, however, needs data, such as input-output tables, which are available only for very few countries and only since the late 19th and 20th century. One can overcome the lack of data in two different ways – either by simplifying the CGE model (O’Rourke (2007) or by using partial equilibrium framework (Hufbauer-Wada-Warren 2002). Both approaches need only data on prices and on few key parameters, such as demand and supply elasticities, which can be plausibly guessed. Then, one can compare results of different methods

Market integration could increase welfare also by reducing price volatility. *Ceteris paribus*, a decline in volatility, and thus in risk would reduce need for savings for inter-temporal smoothing of consumption and would cut risks for investments, with unquestionably beneficial effects. The prevailing wisdom, assumes that integration reduces price volatility because local shocks outweigh global ones. If this is the case, integration would indeed reduce the exposure of individual markets to idiosyncratic risk.

The evidence for this assumption derives however from the literature on international business fluctuations. It will be important to test with actual price data. Furthermore, more price data will allow to investigate the behaviour of markets during famines along the lines of O'Grada (2005).

The last part of the research deals with the political economy of reactions to market integration. The issue is huge and difficult, as it is possible to deal with it only with case-studies. Here we will discuss two examples.

The first is the failed liberalization of the French market for wheat in the 1760s. The legislation was repealed few years after its introduction, in spite of the support by provincial authorities because of the opposition of Paris consumers, who did not share the Physiocrat's belief in self-regulating markets. The political events are well-known (Kaplan 1975-1976. Miller 1999) and anyway the members of the research team have little competence in 18th century French political history. However, the data-base of prices will allow to test whether and how much the public concerns about the effects of liberalization were founded. On a more general vein, the research team hopes to bring a fresh perspective to the debate on pre-industrial (urban) market-regulating institutions. The literature on the issue is vast, although widely scattered, and provides a wealth of detail about the often mind-numbingly complex detail of legislation. However, it is short in economic analysis. An informed reading of the literature with simple analytical tools and the support of statistical evidence on prices will yield important insights.

The second example is trade policy in the first half of the 19th century. The great increase in imports of grains after the Napoleonic wars did trigger a backlash in many European countries, as in the 1880s. Yet, the backlash did not last, and since the late 1820s grain markets were being liberalized (the repeal of British Corn Laws being only the most famous of many moves). The difference with the long-lasting protectionist reaction of the 1880s is striking, especially as a crude political economy approach would suggest otherwise: landowners were more powerful in the 1830s than in the 1880s. There are several possible hypotheses, from the effect of falling prices of manufactures to the appeal of the free-trade ideology, which need to be discussed.