

## REVIEW ESSAY

Real World Globalization and Inequality Yesterday and Today: A Review Essay  
Andre Gunder Frank

*Globalization and History: The Evolution of a Nineteenth-Century Atlantic Economy.*  
By Kevin H. O'Rourke, Jeffrey G. Williamson. Cambridge, MA: The MIT Press. 328 pages, ISBN 0-262-65059-2, \$27.95 (paper). <http://mitpress.mit.edu/>

This book shows that the current “globalization” buzzword refers to a process that in fact already characterized the nineteenth century until the beginning of the First World War. From then and until the end of the Second World War, the process of globalization was severely reversed—in exemplification of one of the authors’ sub-theses that this process can and does have its own ups and downs and is not a one way ever-onward and—upward road, as latter day parlance would have it. During the half century past, the globalization process was re-initiated laboriously but haltingly until the last two and especially the last decade of the twentieth century, when globalism more than globalization recovered the reach it had already nearly a century earlier. Although this book focuses on the nineteenth century period, the authors nonetheless express important concerns for present and future praxis and policy. Not the least of them is the warning that “globalization” should not be regarded as an automatic and irreversible process; but that it must also be cultivated and protected, in particular from protectionism itself.

This wide-sweeping and far-reaching book represents a major piece, or set of pieces, in a still on-going assembly of a yet larger jigsaw puzzle, the outlines



Andre Gunder Frank  
World History Center  
Northeastern University  
270 Holmes Hall  
Boston, MA 02115 USA  
[franka@fiu.edu](mailto:franka@fiu.edu)  
<http://csf.colorado.edu/agfrank/>

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of which one of the authors has kindly provided me in the course of e-mail correspondence between us. As reviewer, I will here of course concentrate on the book. Nonetheless, I will try also to situate this book among the authors' growing concerns. These include debates of long standing among economists and historians to which the authors bring their own innovative sophisticated analysis of a rich database that in they have had to assemble themselves.

Their related works both tap into and further expand this database, which they thereby generously also make available to others as well, including those who may wish to use it also to contest their conclusions. I include myself among these, if only because in some of their work our authors take specific issue with my concerns and conclusions, which they dispute on the basis of their data and their analysis in this book and elsewhere. I therefore also permit myself to engage in what in a simple book review would normally be a no-no: to use the review of another author's book to push the reviewer's own agenda. I will do so below on pretext at least of writing a review *essay*, which not only engages a problematique that is wider than that of the book itself, but also one within which the authors themselves have already challenged my position by quotation and me by name.

The central question that the authors address is whether the Atlantic economy experienced convergence of income among its constituent parts. Their short answer is *yes*, in which some however receive more equal attention than others. Moreover, the rest of the world remains beyond their scope in this book, although not in their later work. The authors rely less on the usual per capita GNP or GDP and prefer to use the real PPP [purchasing power parity] wage rate of the majority of workers as an index of income, because it takes better account of the otherwise all too much and often disregarded important domestic distribution of income. So the second main question posed is to what extent and how openness and especially trade impact domestic distributions of income. This factor price wage rate of labor and its relation to the factor prices of land and capital are the empirical pieces and the analytical red thread that guides the authors' innovative and coherent assembly of this part of their larger puzzle still under construction.

Another part of the jigsaw puzzle the authors are assembling turns on their insistent and repeated question whether factor movements and trade substituted or complemented each other. The [neo]classical Heckscher-Ohlin Theorem had it that trade can be and is generated by equalizing factor prices and benefits from trade between two regions: A labor rich region exports labor intensive goods and imports capital and/or land intensive products from regions that are relatively rich in capital and land, thereby also tending to equalize factor prices between both regions. An additional question is whether such equalization also occurs *within* these regions, as per below. The authors' time series and other analyses

suggests that for most factors, products and times the trade of products instead complements the movement of the labor and capital factors that produce these products. That is, trade and capital flows, as well as trade and migration mostly rose and fell in tandem and re-enforced rather than replacing each other: increased capital flows and migration among regions also generated more trade among them.

Nonetheless, not only is trade found to be largely derivative from factor flows, but its contribution to wage/income convergence is much smaller than that of factor flows themselves. On the other hand, trade does have important consequences for the *distribution* of income. However, these effects are not the everywhere the same. They can result in both less and more equality of domestic economies, depending on differences in their pre-existing political economic structure. Usually, the effects are to accentuate both their already more equal and more unequal domestic distributions of income. Notably for instance, in the two Atlantic economies with the already previously most *un*-equal income distributions, that is Brazil and the United States, three decades of late twentieth century 'openness' *multiplied* this inequality. In a subsequent paper, the authors pose the question whether the Heckscher-Ohlin supposition that trade also affects the distribution of income is confirmed by the experience of England before the nineteenth century. They suggest that not, first because the nature of the goods traded could not have sufficient such effects, and second because trade did not, or was not sufficient to, equalize commodity factor prices among the trading regions.

This last observation becomes the index and key to a still later work that asks "When Did Globalization Begin?" They say that contrary to Frank and also to Bentley the answer is only since 1820–28, because only then did commodity factor price equalization or even convergence begin on a global scale. In an e-mail exchange, I objected and one of the authors O'Rourke agreed that equal factor prices across regions cannot serve as an adequate index of trade globalization; since even in the today's globalized economy world—and even national—factor prices and of course especially the wage price of labor—are still unequal. But if we agree that the presence or lack of price equalization *cannot* be used as an, let alone the, index of globalization, then what remains of our authors' argument that the absence even of convergence proves that there was no world economy?

Perhaps this is a glass-half-full/half-empty difference of appreciation. For if factor prices really *were equal*, there would be no incentive or reason to trade at all! In yet another and so far last paper in this series, the authors wonder why international trade grew rapidly from 1500 to 1800, even though they find no factor price convergence, which according to them only begins in the 1820s. In this regard, it may be observed that in fact tradable commodities like grains,

sugar, coffee, tea, silk etc. and goods, especially textiles, were already competitive substitutes from one end of the world to another during this earlier period (Frank 1998, Barendse 2002). That also tended to generate factor price convergence among them. Importantly furthermore, the worldwide trade of silver responded to different prices in different parts of the world. Thereby the trade of silver generated even more price convergence, not only for silver itself, but also for all the commodities and goods that were exchanged for and through silver. That is, and contrary to our authors' claims to the contrary, the already then globalized division of labor [that is, of production], trade and investment led to some convergence but *not* equality of factor prices, which had then become equal and would have stopped rather than increased international—really global—trade.

In this book however, the authors are not yet so interested in *when* factor prices converged and with what effect, than they are with *why* these prices—and in particular why real wage rates and income—converged in the Atlantic economies. Limiting the question to the circum-Atlantic is of course their privilege. However as I will argue at the end of this review essay, it is *not* adequate or satisfactory to answer questions of why what happened *in* the Atlantic Basin by drawing on evidence and its analysis, which is also limited only *to* the Atlantic. That is because in any one region factor prices are also formed through its participation in an already pre-existing and still continuing globalized world economy. Of course, and perhaps in part for that reason as the authors aver in their above cited works, part of our dispute is precisely whether a world economy did or did not exist before the nineteenth century.

For that period, the authors proceed with a sophisticated factor analysis of what did and did not have much influence on factor price equalization and on wage/income convergence among various countries for which there are statistics within the Atlantic region. They consider and reject education, demonstrate the importance of cost reducing innovations in transportation, and discuss tariff and other policies. They conclude that far and away the most important factor was mass migration, which in their estimation accounted for 70 percent of the convergence: Migrants reduced the labor/land and capital ratio and exerted upward pressure on wages in the labor exporting regions and increased the labor/land & capital ratio and exerted downward pressure on wages in the labor importing regions. Capital flows, although large and increasing especially in the last decade of the nineteenth and first decade of the twentieth century, contributed much less than migration of labor.

Accordingly, the authors allot three of their twelve chapters to migration and migration policies. Apart from the also important intra-European migration, especially into Eastern Europe—and also eastward within Russia—the authors re-examine the familiar overseas migration of 60 million Europeans and very

much less Asians. They also re-examine the familiar penury-push and riches-pull explanations for the sources, destinations, and timing of the migrant flows. More than elsewhere however, this book analyzes the underlying and resulting—that is changing—combinations of the availabilities and absolute and relative factor prices of labor, land, and capital.

Of course, the flows of these factors were related; since new capital investment, especially in infrastructure, was required to make labor and land productive in the regions of recent [European] settlement. [Although land and other natural resources of course remained in place, they may also be regarded functionally as flows inasmuch as they were incorporated into the economy through expansion of the frontier of—European!—settlement and resource use]. To render all of these profitable, they had to be bound together by international as well as internal trade and finance (e.g. railroads and canals).

The book also has several chapters on other aspects of political economic policy, both as an effect of/response to and as a cause of changing economic circumstances and events. Thus, there is a chapter on the movement to free[er!] trade at mid-century and the return to renewed protectionism during the last quarter of the century. The latter was a response to the “Great Depression” following 1873 and especially during the 1880s, although as it would again during the Depression of the 1930s, protectionism also re-enforced the very tendency that gave rise to it. In the earlier period however, such restrictions were imposed primarily on trade but not on factor mobility, while both trade and factor movements, again both of capital and of migration, were restricted during the second period. This difference can be attributed to or at least is correlated with more peace in the former and more war in the latter period, though it may be disputable which was more cause and which was more effect.

Another chapter examines the causes and consequences of capital flows, and the following one pursues the aforementioned issue of their substitution for or complementarity with commodity trade and migration. As per the book's title, the authors limit their empirical work and its analysis to only the circum-Atlantic regions; and they find that among these convergence did exist. However, some of these regions and convergence among them were more equal than others. Europe as a whole and the regions of recent European settlement are the beneficiaries both of more attention by the authors and of more convergence among them. Perhaps that correlation also has causative significance, although it is less clear in which direction the causes run.

The most important finding of the book and argument of the authors is that convergence among economies is a function and result of their degree of openness, also of trade but primarily to factor flows in response to underlying inter-regional differences in factor availability and relative prices. Among these

in turn, most important was the globalization of labor markets through migration and the expansion of the frontier. Indeed, the two should be regarded as largely functionally equivalent: Pushing the North America and Australasian as well as Argentinean and South African frontiers outward further globalized the labor market. The related migration obviously also served to extend the frontiers within these regions. Less often noted however, is that opening these regions and expanding their domestic frontiers through overseas out-migration from Europe also served functionally to extend the frontier of Europe itself.

Contrary to the Hecksher-Ohlin study, theorem, and predictions however and as already observed in general but also in a chapter especially devoted to thereto, the authors find that not commodity and manufacturing trade but rather factor mobility is the major contributor to wage rate and income convergence. The authors note on several occasions that received theory is rather ambiguous on a number of important policy related questions. But so is their work. To their credit, they want to speak to today's "debates" [p.3]. But how? They clearly demonstrate and stress that openness is correlated with, indeed causes [desirable?!] convergence, where the latter was observed: In much of the North Atlantic in the nineteenth century; and with the cessation of openness during the twentieth century war and inter-war period, so was convergence replaced by divergence. Ah, but not altogether, since parts of Latin America—also part of the Atlantic region—and certainly East and South Asia and significantly so the Soviet Union, experienced important measures of convergence. So how is it then that as the authors can state (p. 284) that "we believe that catching up of poor countries with rich may have as much to do with economic linkages as with any other force identified by growth theory.... Where there has been openness, there has been convergence: where there has been autarky, there has been either divergence or cessation of convergence." Ergo, the authors suggest that even still today it is important to resist temptations or forces to revert to controls and restrictions on movements of capital and migration that have sometimes been invoked during some periods in the past.

If that is the authors' conclusion and policy recommendation for the present and future, it is open to serious reservation on at least three counts, including some that they even raise themselves: [1] One is on their argument as it stands so far, [2] another is on how widely in the Atlantic economy convergence was *not* operative, and in the remainder of the world still less so, and [3] to what extent the authors' good cause and effect factor analysis is or is not adequate to account for observed, let alone unobserved, effects or consequences. We may inquire into the first two reservations here and then more extensively into the third one below.

[1] We must have very serious reservations about the authors' argument and policy conclusions already even on the analytic battlefield the authors selected

themselves and engaging them only with the analytic arms they use. Their insistence on openness for the future must be suspect insofar as it is based on their own factor analysis of factor movements and their consequences in the past. For the authors found that it was factor mobility of labor, primarily through inter-continental migration, that accounted for 70 percent of observed convergence. That also means that insofar as factor mobility was the crucial factor at all, the mobility of all other factors combined accounted for no more than 30 percent of observed convergence. Indeed, that percentage may also have been lower inasmuch as it is possible that some other factor mobility was *di*-vergent but compensated by labor mobility. Moreover, the authors find that merchandize trade did *not* generate convergence. That leaves capital mobility as the other most important factor. But regarding that, the authors find that capital moved as a complement of and not as a substitute for the movement of labor and the development of land and other resources. Without capital to make labor and land productive in the regions of recent settlement, their development and convergence would have been much less than it was or even nil. Moreover, it was precisely to these resource rich and labor-attracting, potentially productive regions that capital went elsewhere. So in the conclusion to their Chapter 12 on "International Capital Flows," the authors themselves observe that "late-nineteenth century world capital flows were a force for divergence, not convergence" [p. 245].

How much more so then must serious analysis of the evidence demonstrate, or even raw evidence or pure theory each taken separately suggest, that the enormous flows of speculative financial capital in the late-twentieth century had to be and have been highly *de*-stabilizing and *di*-vergent. Also still today, capital flows to the already or potentially productive regions, and not especially those with the lowest labor costs. So what does this *real world* historical experience even within the confines of the North Atlantic Region *really* teach us about factor mobility and especially capital mobility? Once openness to the global mobility of labor is closed off or even curtailed as it is today [except for the brain drain, which of course is *di*-vergent], openness to trade and to capital mobility no longer offer much of any source or generation of convergence! As the authors themselves note also still (on the same above cited page 284), "Thus, one must be cautious in applying lessons of history to the present, where mass migrations are so much more modest."

Why then would anyone wish to insist on openness [for everything except labor mobility!], unless it is for ideological reasons that mask *real world* interests, which are served by openness to capital mobility, and especially for speculative financial capital mobility, that far from accounting for convergence generates ever greater *di*-vergence.

[2] Contrary to the authors' rather wide-sweeping claims, convergence did

not operate everywhere and at all times of openness, and was even negative within Europe between its regions the North and the South of Europe, as the authors also observe. Moreover, as I observed above—and already in my 1966 article on “The Development of Underdevelopment”—relative autarky has also been associated with—and permitted?—national catch-up and thereby convergence in parts of the “Third” and “Second” worlds. However, the authors devote much less, indeed almost no, attention to what has come to be and be known as the “North” and the “South” in what also was and still is the Atlantic economy—and among them there was *no* convergence, but rather *de*-convergence and ever greater dispersion. Convergence certainly did not include the *Atlantic* economies of West and Southwest Africa or the Caribbean, nor even of west Atlantic South America—with one notable exception: Argentina and Uruguay during the period of the authors’ study, where at the end of which wages and income *exceeded* even the highest ones in Europe.

The sad decline—anything *but* convergence—of Argentina to the terrible crisis it now suffers as I write has of course been the object of unending studies. Whatever the reasons for this debacle, failure to follow our authors’ openness’ policies can *not* be said to be one of them. Even the short periods of partial economic isolation of Argentina were not due to endogenous policies but to loss of its export markets to agricultural protectionism elsewhere, by the Ottawa Commonwealth Agreement in the 1930s, the US Marshall Plan commodities exports to Europe, and then the US, Canadian, and West European Common Agricultural Policy throughout the past several decades. Ironically under these circumstances beyond Argentinean control, world and Argentinean economic policy since the 1976 military coup was to turn Argentinean “The Master Wheel” back to the pre-1930s wheat and meat export economy and to *de*-industrialize the intervening import substituting manufacturing sector, thus for the first time generating massive unemployment and declining income. Even so, Argentina in 1930 had accounted for 3 percent of all world exports; by 1990 already accounted for no more than 0.3 percent of them. The coup de grace to once proud Argentina was complete financial liberalization and the dollarization of its economy, which *de jure* incorporated and *de facto* marginalized Argentina and its by now miserably poor population.

In a word, for the also *Atlantic* regions in Latin America and Africa, openness was then in the nineteenth century and has been since in the twentieth century the road *not* to con-vergence, but to *di*-vergence. *That* must be taken as a second very significant limitation to the alleged benefits of openness and its recommendation by the authors even for the Atlantic economy, not to mention in the world economy. The same must be said also for Harold Innis’ and Mel Watkins’ related “staple” theory of growth. It holds that industrialization and convergence can be

achieved through production, early specialization and openness to the export of commodities derived from natural resources, including cattle and sheep ranching and grain farming. That was the experience in the Canada that served as their model, as well as in other land and resource abundant but labor scarce regions of recent European settlement. But it has equally assuredly *not* been the case in the labor abundant regions elsewhere in Latin America, the Caribbean and Africa, not to mention of Asia, which except in a few marginal references remain marginal and beyond the purview of this book as well. There is nothing wrong in not including this populous large part of the world in this book *per se*, all the more so as the authors increasingly do so in their above-mentioned related later work.

[3] Not so acceptable however, is the authors’ failure even to consider, much less to account for, the effects that this wider world political economy has on the Atlantic region and convergence among the northern [and non-convergence of the southern] parts thereof. For each and all of these regions is [only] part and parcel of this world economy as a whole. Nor do our authors seem to find much reason to study the complex [sub] system of trade, capital flow, and migration *relations* among the Atlantic regions and how these relations contribute to different degrees of convergence.

Instead this book focuses on and is largely limited to only inter-regional relative factor prices. However valuable their innovative study in this book unquestionably is, what this reviewer nonetheless finds missing is the examination of how *world* trade, capital movements and payments, and migration impact on the Atlantic Economy that is under study here.

All of these economic relations are and *must* be examined also as the structure and operation of the complex system of world trade and payments itself. For as the saying goes, the whole is more than the sum of its parts; and it and helps shape the parts and their relations among each other. Therefore, an adequate—or even any—analysis of *how* the causes and consequences of inter-regional [and inter-sector] flows of capital, trade and migrations and their consequences for convergence or not must also take due account of how any, e.g. Atlantic, regions were also importantly shaped by and dependent on what Ragnar Nurske called *The Network of World Trade* (League of Nations 1943). Moreover as he, Saul Condliffe and Frank (1978, 2001) analyzed and Kenwood & Lougheed apparently unsuccessfully sought to “popularize,” this network was and is characterized by a *world-wide multilateral* system of balances and imbalances of trade and payments. And arguably it is the *position within* this system, more than relative factor prices and productivity of each economic region and sector that determines their absolute and relative benefits and any convergence or not among them. Of course, if all positions were equivalent, occupying one or another would not afford any particular dis/advantage to whoever manages or is obliged to locate there.

But some positions are much more and others less beneficial than others, and even among apparently equal ones, some can in George Orwell's terminology be more equal than others. The importance of locational position in the world economy is by no means derived from or limited to only geographical location, as we will note below. But it is perhaps the easiest to visualize, e.g. in the locations over two millennia of Constantinople/Istanbul near one and Malacca/Singapore near the other end of Eurasia. The former boasted a population already of 750,000 while Paris and London were edging from 50,000 to 100,000. Both were located at natural turn-around places in Afro-Eurasian East-West and North-South trade. And what is the benefit they derived from their locations? *Monopoly Rent!* That is why I use the term *location, location, location* in the Nineteenth Century World Economy [Frank 2001] to dramatize this all too neglected problematique, also by our present authors.

Far from only [let alone perfect] competition making the system tick—that would equalize factor prices and converge incomes—it is competition to establish and hold on to *monopoly* positions from which to extract *rent* that is the real name of the game. That was an all too neglected observation already of Karl Marx, Joseph Schumpeter, Joseph Chamberlain and Joan Robinson—the latter under their titles of *imperfect* and *monopoly* competition—among economists and Fernand Braudel among economic historians. All of them alas claimed to be identifying and analyzing a structure and operation that is characteristic only or especially of “capitalism,” when the same has equally characterized political economy and the world throughout the ages. The patent illustration is the ever-present race to get patents and then by whatever ruse hold on to them and the monopoly rent that they afford, or to capture, construct, or be granted any other privileged position, not only geographical but also technological, productive, commercial, legal or just plain force/powerful in the local, national, regional and global political economy. Why else fund kings and conquerors throughout the ages, or lobby legislatures and contribute to political parties and candidates in “democracies”? Ask *Enron!*

This is not the place to elaborate thereon, other than briefly to note some of its possible consequences for our authors' analysis, conclusions, and policy recommendations. Hilgerdt and Saul analyzed and Frank (1978, 2001) further elaborated on this complex system of multilateral trade and payments imbalances in the late nineteenth and early twentieth centuries.

These may be simplified and schematically illustrated in two alternate or complementary ways. One is a set of triangles, beginning with that—or rather those—of the triangular Atlantic trade already before the nineteenth century. The second is the in/famous opium unbalanced trade and payments triangle among India, China, and Britain. Another is the complementary US, China and

Britain triangle of trade and payments imbalances. More and more triangles were added and interwoven as the nineteenth century progressed until these triangles merged into a more complex multilateral system of trade, its underlying division of labor, that is also of the expenditure of labor power here and there—and their consequences for the convergence or not of factor prices and incomes, which is the focal point of our authors' inquiry.

All these triangles had in common that their apex was in Britain, which thereby occupied the most privileged position in the world. Visually most obvious again is the geographic location and nexus that joined all the triangles in Britain. But this nexus of triangles also operationalizes and represents its position in the global productive and commercial system of multilateral trade and payments that Britain derived its maximum benefit *in monopoly rent*, arguably more than from its alleged workshop of the world productive prowess. For that was not sufficient even to avoid or remedy a structural Britain with merchandize trade *deficit* in every year of the century, which grew from 10 million to 160 million pound sterling from 1816 to 1913. How then, was Britain able to increase its consumption and income—and be the world's largest investor besides? Neither by its own efforts nor by taking advantage of factor price differences alone.

And how would convergence come about around the North Atlantic (and with Argentina and Australasia as well), while the rest of the world *de-converged*? Further to the factor prices so well analyzed in this book, this process can also be schematically illustrated in another way. Picture a world economic circle around the perimeter of which we locate the various world regions (however roughly or finely one wishes to cut them up) in locational correspondence to how each region is advantaged or disadvantaged by its triangular and then ever more multilateral relations with all the others. Then Britain was the top dog, which benefited first from its relations with Continental Europe, and both from their relations with the Regions of Recent settlement (and among these the United States with the British Dominions), and *all* of these from their economic and in some cases also political/military relations with the rest of the later so-called Third World, which instead of converging, *de-converged* and suffered “development of underdevelopment.”

Conversely, the location on the perimeter of the world economic circle permitted each of these regions also to benefit from its relations with those behind and below it, *and* to use part of the benefits it derived from others to export commodities and payments to the other regions to whose benefit it in turn contributed and who benefited from their location and relations with the regions behind and below it. As in the schematic illustration by triangles, in this circular world system of trade and payments im/balances, it was Britain who was top dog. Bye and bye it had to cede its place of privilege on the charmed circle to the United

States. At the bottom of the pile, stack or deck were and largely still remain the now underdeveloped Third World that made up the pedestal on top of and from which all the others literally made their fortune.

Within the now mis-called Third World however, there was one region—outside the Atlantic Economy!—that carried the brunt of these relations that were beneficial to others and disastrously *dis*-beneficial to itself: *India*. In effect, it was India and its direct relation to Britain that was the pedestal on which rested the structure and operation of the entire global system of multilateral Imbalances of trade and payments *and* of direct and portfolio investment and repayment. Each of B[ritain], E[urope], U[SA], and D[ominions] was able to settle all or part of its unfavorable balances with some by drawing on its favorable balance with others. That is, each of these regions was able “to settle its accounts” with the others by drawing on the productive inputs into the system as a whole of labor, land and other resource and capital in regions other than its own. Only the T[hird World] and within it particularly I[ndia]—except for the latter partly also with China—had no one else to benefit from and instead had to allow all other B, E, U, D regions to benefit from it [conversely, each of these regions was—and still is—able multilaterally to dissipate its own entropy to the others and lastly to T].

That is, not only were—and still are!—some able to profit and consume at home from the production of others abroad. The beneficiaries were—and ever more are—able to pass much of the other costs of their “American way of living” lifestyle at home off onto the backs of those who already produced the products for that life-style in the first place. No wonder that US Presidents Bush, father and son, have explicitly rejected sacrificing even a tiny bit of the American way of life just to keep from destroying the global environment elsewhere. Analogously, when President George W. Bush says that we can and will not let terrorists change our style of life, because if we did they would have achieved what they wanted to, the President means it—and backs his words up with military power and blackmail to preserve and extend the work that after the end of the Cold War his father began and called “The New World Order.” The question comes, what else is new?

I firmly believe that a responsible reviewer (responsible both to the author and to the readers) should review the book that *was* written and *not* a book that the reviewer may have liked to be, but was *not* written, and hence also *not* up for review. So why do I insist on even summarizing all this about the rest of the world, in a review of a book that is not about that? I do so for the simple reason that the economic processes of convergence and some factor price equalization within the Atlantic economy that the authors analyze so well, could *not* have taken place as it did and as they examine it in the absence of the relations

between the Atlantic Economy and its regional members and sectors with this remainder of the world.

The North Atlantic—but not most South Atlantic—regions benefited from their relations with other and especially Asian ones, but not only in some general way. The circum-Atlantic factor prices were directly related to those elsewhere in the world economy, as the same authors themselves show in their subsequent work thereon. So were therefore also the very factor movements and in particular the migration of labor within the Atlantic economy to which the authors attribute 70 percent of the convergence that they find there—but not elsewhere! But to make that labor and the new land it occupied productive and capable of generating commodities for export also required a complementary transfer of working and investment capital to provide the required infrastructure. But Britain was the principal exporter of capital all the while that its own exports were insufficient to pay even for its home consumption and investment, as also the United States today. Moreover, Britain then—and again also the US now—was unable to raise enough capital from its own savings at home to finance its investments. Its own productivity and savings were probably wanting even for its investment at home, and certainly altogether insufficient to cover its investment on the other side of the Atlantic and the world. What Britain then and the US now have been able to do however is in the name of “free” enterprise and trade of goods and services—to set up, run and manipulate a world embracing—more accurately choking!—financial system to their own monopoly advantage.

So where and how then did Britain raise this investable capital? True, some was derived from invisibles like interest and profits from previous investments and shipping and insurance fees. But that was all but sufficient for Britain to cover its structural merchandize import surplus. Moreover, to generate invisible earnings from its investments, Britain, Continental Europe and then also the United States had to place some foreign investment capital abroad in the first place. And the real source of most of that capital for British was its colony in *India*. Not only was India the linchpin or centerpiece of the arch of Britain’s—and through it the world’s and the Atlantic Economy’s—entire economic prowess in general. India was also the principal source in particular of the investment capital that Britain used to help construct and make tick the Atlantic Economy and the convergence among its northern regions.

All these regions to some extent and Britain very substantially therefore owed their growing prosperity and the convergence among them probably more to their position in the international division of labor and their ability to manipulate the world financial system in their favor than from their own labor or combination and use of productive factors in response to relative factor prices.

The same is again, or rather still, true today. The 1990s boom time in the United States, contrary to all the Clintonesque self-congratulatory backslapping, was in no way derived from any exceptional American productiveness or productivity, which the latter rose in electronics only, soon to bust there as in the economy as a whole. American consumption—despite the huge and ever growing trade deficit—and what little investment was largely derived from its *privileged position* in the world, which in turn rests on two main pillars: the dollar as the world reserve currency and the Pentagon as the keeper of the new world order. Each pillar also supports the other, and both have served American prosperity at the devastating cost of the vast majority of the population elsewhere in the world. During the 1990s, that was most spectacularly so in the former Soviet Union and Eastern Europe since 1990 and in parts of East Asia since the financial crisis of 1997, both of which were first generated and then deliberately deepened by US policy.

In conclusion, we must observe again that our authors' very laudable but sole (or main) object of inquiry has been factor price equalization and income convergence among otherwise separate productive, sectoral and geographic units. We already observed earlier on that [1] even their own evidence does not support their argument for openness even on their own turf and that [2] the evidence they do not examine beyond their own turf disconfirms their argument altogether. [3] Thirdly and most importantly however, their factor analysis of what factors and factor prices intervene in the process of con- or di-vergence are not the only factors of major significance for the economic and social outcomes that the authors are keen to observe and explain. The structure, organization, functioning, and transformation of the global world economy itself and the location within it of any particular unit also accounts for as much or very probably more, as per the titles of Adam Smith and David Landes, of "the wealth and poverty of nations," their inhabitants and of con- or di-vergence of income among them. By confining their analysis almost entirely to the former in neglect of the latter, our authors therefore also able to convey at best only half or even less of the truth. I leave it to the reader to judge whether a half-truth or less is better or worse than none.

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