COMMENT ON *THE ECONOMIC CONSEQUENCES OF EMPIRES (1492-1989)* *

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Patrick O'Brien and Leandro Prados de la Escosura say they are discussing «the economic consequences of empire», but in fact they are discussing «the costs and benefits of European imperialism». This is clearer and more specific, but I fear less useful. It is clearer, since «costs and benefits» are easier to quantify, and less useful in that I do not agree that «economic consequences» are the same as, or can be reduced to, an assessment of «costs and benefits». Furthermore, as they themselves note, «counterfactual assumptions are... implicit in any attempt... to measure costs and benefits of macro economic strategies» (O'Brien & Prados, 1998, p. 31), and I shall want to argue that what we need to do is make «factual assumptions».

The organizers tell us that they are seeking to «respond to the recent stimulus offered by» world-systems analysis, and the way to do this, they say, is «to respecify questions that are venerable and to go over the arguments again» (1998, p. 36). Hence they have assembled a stellar group of economic historians, each to take a specific Western European country, and asked them to analyze whether the colonial empires they constructed or attempted to construct served them well economically, either over the whole period of the past five centuries, or over of the two segments into

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which the organizers have periodized colonial rule, 1415-1846 and 1846-1974. What results is a series of studies that have individually very much to add to a cost-benefit assessment for the country in question. I have no quarrel, or only an occasional one, with the analyses offered by the separate authors, which seem to me on the whole informed, comprehensive, intelligent, cogent, and balanced. They are in fact perhaps too balanced, as if balance were the central scholarly virtue.

The editors seem to set themselves comfortably in the Aristotelian mean. They suggest two extremes. On the one hand, there are the neoclassical economists, and on the other side there are the Marxists (a less clearcut category these days than neoclassical economists, it should be noted in passing). The former are accused of denigrating the relevance of colonies for the economic growth and relative economic successes of the Western European states in the modern world, while the latter are accused of explaining all of economic growth and relative economic successes by the exploitation of the colonies. The organizers seek to deflate both extremes. On the one hand, they insist that there were numerous economic benefits from empire but on the other hand they doubt that these benefits were «palpably significant for their long term development» (p. 85). Speaking of England, they ask:

> «But how far did England's famous transition to an industrial market economy emerge within a mercantile and mercantilist matrix dominated by colonisation and commerce with continents beyond Europe? To an important but not overwhelming extent is the short answer to this pertinent question» (1998, p. 51).

Although they are claiming to occupy a moderate middle position, they seem to come down more heavily against the economic significance of colonies than against their total insignificance.

The various authors throughout the volume come close to the same conclusions for their individual national studies. Engerman, for example, concludes about the first British empire:

> «The absence of any strong conclusion to the questions of the benefits and costs of the Empire should not be too surprising, given the large number of relationships that have been posited and the great difficulty of satisfactory theoretical analysis and empirical measurement» (1998, p. 225).

Bartolomé Yun, analyzing the less «successful» Spanish case sums up his analysis this way:

> «Expensive for the Crown, profitable for the ruling groups, and economically inefficient for the country, the imperial system contributed nonetheless to the maintenance of Castile's social and economic structures. For that reason the weak positive stimuli it generated could not easily revivify the economy» (1998, p. 140).

And Peter Cain's analysis of the second British empire concludes in praise of the in-between path pursued by Great Britain in the nineteenth century:

> «Neither the radical nor the constructive imperialist alternatives would necessarily have produced a more vibrant economy, a more cohesive society or one more capable of defending itself against its enemies than the actual one presided over by the gentlemanly capitalists before the 1950s... [I]n steering a compromise between these alternatives, Britain remained a stable society and survived the great economic and political upheavals of the first part of the twentieth century relativeley unscathed. Empire played its role in maintaining such a stable society and in preserving its independence. In that sense it "paid" handsomely» (1998, p. 372).

So where does all this leave us? Well, it seems that there were some pluses and some minuses, and colonies were important but not all that important. One wonders what the fuss was all about. Why did European states seek for some five centuries to create empires, and to stop other states from doing so? Why the loud screams about «economic drain» which were already being vigorously formulated by Indian scholars in the mid-nineteenth century, which reached a crescendo in the mid-twentieth century, and which have, if anything, grown stronger since the worldwide decolonization of the past 50 years?

And how is it possible that no one perceived the limitations of imperialism at the time? Of course, many people did argue such a thesis, but the fact is that ultimately they were in a minority in just about every instance. To be sure, some countries never quite managed to have colonies, or as many as they wanted (say Denmark or Italy). But the explanation lies far more in their objective inability to succeed in this role due to the strength of other imperial powers than in their wise self-restraint.

Indeed, our editors themselves, when they are criticizing the analysis of Great Britain by neoclassical economists, say:

> «Sceptical economic historians who, with hindsight, now suggest that realistic and less costly counterfactual strategies were in fact available, might in fact lay them out for inspection and explain why a "polite but commercially

> aggressive" people failed to entertain, let alone adopt, them between 1688 and 1815?» (1998, p. 53).

This is indeed a good question. As far as I can see, the answer they offer themselves is that there were indeed some powerful beneficiaries of the policies. As for the country as a whole, however, ultimately the only answer the editors offer is:

> «[T]ime and again statesmen, generals, admirals, projectors, entrepreneurs and investors involved in the promotion of Europe's imperialistic enterprises overseas underestimated the expenses and risks involved» (1998, pp. 85-86).

If they mean they under*stated* the expenses and risks, of course. But if they really mean they under*estimated* them, we are reduced to explaining 500 years of history as a colossal mistake, a mistake that implicates not only the promoters of the policies but those they persuaded, presumably the rest of the political class. I hesitate to build historical analysis around the assumption of widespread, continuing human bad judgment. I tend to think that there are social correctives to bad judgments, correctives inspired by failures, and that even if mistakes sometimes persist for a while, five centuries is stretching credulity.

So, I come back to my fundamental question, why all the fuss? There must have been something in it for somebody, indeed for a lot of people. How might we go about thinking about such a question, and what instruments are available for coming up with plausible hypotheses. I do not speak of answers, but just sustainable tentative first approximations. The first issue to resolve is the unit of analysis. That I insist on this issue will come as no surprise, since world-systems analysis has from the outset been built around the insistence that, until one discerns a useful unit of analysis, all the measurements in the world will get you nowhere. Our editors hint at the problem themselves twice. The first time is when they lay out the problem of *outcomes.* They assert:

> «When, how, where and why did European societies (and Europe as a whole) gain or lose economically from connexions established and maintained (in stronger or looser froms) with national Empires are the difficult questions confronted by this complex and wide ranging exercise in European and comparative economic history» (1998, p. 30).

But of course «European societies» and «Europe as a whole» are not at all the same thing, and one cannot uncover the realities of the latter by «comparative economic history». It is clear that what is good for a given European «society» may or may not be good for «Europe as a whole». They themselves say so:

> «Thus in so many ways, silver turned out for the development of Spain (not for Europe!) to be a lot less valuable as a return cargo than seemed to be the case at the time» (1998, p. 42).

«Not for Europe» has an exclamation point after it. They seem to be sure of their judgment. I think they are in fact right, but do they have data to sustain the argument? And if so, in which of the articles will I find such data?

The point is simple. Suppose we hypothesize that the capitalist world-economy was constructed around and axial division of labor which involved extensive transfer of surplus value from periphery to core. And suppose we hypothesize that there was created as a consequence of political structure of relatively strong states in the core which, however, were in constant competition with each other, as were individual capitalists. It might follow that the structure benefited «Europe as a whole» without necessarily benefiting the relative losers among the competing states in the core. To see if this were true, it is of limited use to analyze each country separately, *especially* in the terms of economic variables. It might be the political variables that accounted primarily for distribution of the surplus among the states in the core, and therefore accounted for the economic performance. Indeed, I would suggest that Yun's splendid analysis of Spain ultimately demonstrates precisely that. Spain, he argues, started out with more economic pluses than it is usually credited with, and still «lost out» in the race. Essentially, the argument boils down to saying that aristocrats, bureaucrats, and soldiers could make more money out of taking advantage of imperial rule than out for transforming some structures in Castile (or Spain). This paid off for them in the short run (of several centuries) but not for Spanish national accounts. Did they care? If not, why should we? Economic historians should not be playing the role of advisors to the prince in retrospect, what we in the United States would call «Monday morning quarterbacks». Economic historians (as all social scientists) are called upon to explain why people acted the way they did in fact act.

Even if we accept that Europe as a whole is the unit whose outcomes we are measuring, can we limit ourselves to «costs and benefits»? Costs and benefits are someone's costs and benefits. Are they the costs and benefits of a conglomerate of states, of their leaders, of their populations, of their entrepreneurs? Surely, the answers will be different to the degree that we specify of whose consequences we are speaking. And in considering outcome, can we even restrict ourselves to «*economic* consequences»? Perhaps as the dependent variable, but surely not as the explanation. Historical social structures are imbricated wholes and one cannot pull out certain manifestations of the phenomenon and measure it autonomously. That's like the proverbial story of the blind men measuring (assessing the nature of) the elephant, and one holding the trunk, another the tail, and a third a leg.

The fact is that the modern world-system has, from the very beginning, involved a strong linkage between Western Europe and «overseas» areas, usually (but not always or forever) in the form of formal colonies. The European states encouraged «discovery» and colonization. They held on to their colonies for as long as they could. They announced great profitability in the exercise. As late as thirty years ago, they were resisting vigorously the decolonization of all sorts of colonies that seemed to be offering medium economic return at best. Can we not start by assuming minimal rationality in this historical activity? I am constantly amazed by the economists who promote vigorously the assumptions of «rational choice» in all arenas except one, that of the choices made by persons holding political power. They alone are accused of operating irrationally.

Most analyses of costs and benefits emphasize trade figures. But surely the fact that real labor costs (that is, labor costs holding productivity constant) can be kept lower in colonies than at home, for all sorts of political reasons, increases the long-run profitability of all economic transactions. It not only reduces the price of inputs whose origins are in the colonies (not such a small list), but that of all other inputs, since the wider the spread of real wages in different parts of the world, the easier it is to resist the political pressures for raising wages in the relatively high-wage areas. The fact that the colonies were the best way for adventurers and lesser aristocrats to make a fortune and therefore to have upward mobility (see Yun, 1998, p. 139, for an example) served as a major mechanism of reinforcing the needed political and social stability in the states of Western Europe. The fact that collective White racism, in the context of a system that normalized colonies, served to fortify a sentiment of attachment of the working classes to their European states is surely part of the explanation of why, over the centuries, the degree of revolutionary turmoil was so much lower than many analysts thought likely. One could go on.

The point is that an attempt to measure consequences of colonialism cannot be made simply or primarily by additional quantified economic data. Indeed, once again, our editors seem to say this themselves:

> «The macro economic significance of these concrete manifestations of gains from the first age of European imperialism is, however, more difficult to elaborate and impossible to quantify» (1998, p. 55).

But if it's impossible to quantify, then why send out our noble investigators on an exercise in quantification? I have nothing against the utmost quantification possible. But quantification is the end of a very long road. The kind of data we have at our disposition today is a very early first approximation of seriously useful data. We should assemble them, but we should not take them too seriously. The data are largely useful for creating more complex analyses in qualitative form and then inventing the quantitative data that might test the plausibility of these more complex assumptions. And slowly, very slowly, we may work ourselves up to serious assessments of «consequences». But we must be doing this about the appropriate unit of analysis, which in this case is the capitalist world-economy as an historical whole. And to do this, we all have to get away from the lure of bureaucratically-created state-level data, which on the whole lead us astray from our central analytic tasks.

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