In two books published in 2013 and 2015, entitled Poor Numbers and Africa: Why Economists Get It Wrong, the economic historian Morten Jerven criticizes the way some of his fellow researchers claim to account for the long-term development of Africa.1 These scholarly polemics are written in a vivid and frequently ironic style. While most of his arguments draw on rational debate, Jerven has chosen to express them in the form of an indictment, leaving his plea for a proper economic history of development to emerge between the lines. This approach can be seen as an attempt to subvert or transform a specific section of a scientific field by attacking the dominant tendencies within it.2 Part of the problem is that the particular subfield Jerven targets is very narrow: a highly specialized research area lying at the intersection of two hybrid subdisciplines—development economics and economic history—and dealing with a specific object of study—sub-Saharan Africa. Although small, this subfield is nevertheless not an isolate, and Jerven’s criticism potentially has wider relevance not only for the two subdisciplines concerned, but also for economics as a whole. However, this broader relevance is not really developed in his books, which may explain why they have so far attracted most attention within the fields...
of African Studies and Development Studies, especially among the (presumably numerous) people who remain unconvinced by the type of evaluations furnished by mainstream economic analyses. These are fields, however, which by their very nature function as meeting spaces between researchers from different disciplines and actors from the development sector rather than proper, clearly defined areas of scientific inquiry.

In this article, I will briefly outline the main thrust of Jerven’s ideas, and show why his skeptical outlook must be seen as broadly justified. At the same time, I will argue that closer attention should be paid to exactly how the true economic history that he calls for should be conducted, especially in regard to the respective methodologies of economics, history, and the social sciences. Given the renewed interest in long-term history among economists and even the general public, this seems like an opportune time to explore what directions a renaissance of African economic history might take.

**Africa: Why Economists Have It All Wrong**

Jerven’s goal in *Poor Numbers* is to show that African macroeconomic data are generally of very poor quality, and that this is especially true of gross domestic product (GDP) and the sectoral output data used to compile and construct this indicator. This is problematic, since many economists uncritically rely on these data to produce econometric analyses that seek to highlight the causal factors behind Africa’s growth rate, determinants which include geographic or ecological constraints, the impact of economic policies, and the long-term influence of historical institutions such as the protection of private property. In *Africa*, Jerven sets out to describe in more detail the inherent limitations of these economic analyses of Africa’s development, highlighting three common problems.

First of all, he observes that a disregard for questions of measurement leads to such imperfect and inexact data that any conclusions based on them are bound to be erroneous or impossible to interpret, in effect “garbage in, garbage out.” 3 Not only are the “variables to be explained” (especially GDP per capita) inadequately measured, as Jerven argues in *Poor Numbers*, but this is also true of the explanatory variables relied on by researchers, such as the quality indicators used to assess institutions or economic policies. What is more, the available data typically cover only limited time-spans, which even in the best of cases do not go back further than the 1960s. Jerven’s second grievance is that these analyses rely on cross-sectional regressions in which past characteristics are correlated with present-day growth rates or income levels, without any attention paid to their evolution over time, and therefore to the historical mechanisms behind this correlation. This approach is a good example of the “compression of history” denounced by Gareth Austin: it proceeds

4. Austin is a historian and economist specializing in Africa, and was Jerven’s supervisor during his PhD thesis. See Gareth Austin, “The ‘Reversal of Fortune’ Thesis and the
“as if” the African continent had not known any periods of economic growth, however episodic, at any stage of its history. Third, Jerven deplores that many of these analyses proceed “by subtraction,” assuming that the growth or development of an African country can be described using the same equations as for any other country, while at the same time claiming that the continent’s growth deficit can be tied to the absence or weakness of some essential fundamental at work in European, American, or Asian comparisons. If the absence in question, whether of appropriate macroeconomic policies, human capital, anti-corruption measures, democratic political institutions, or any other factor, really proved to be a factor exogenous to economic growth or income levels, then it would also call out for a more thorough analysis. Above all, the context-specific and eminently local dimension of the chains of historical causality is routinely overlooked, even though a large proportion of these supposedly interchangeable determinants (especially via the simplest specification of a linear regression) really stand in a complementary relationship to each other. The “Wikipedia with regressions” parody can be seen as a brief but striking summary of Jerven’s criticism: too many researchers rely in an uncritical and careless manner on data that they find on the internet, which they use to produce a few cross-sectional or panel regressions that they then hold up as evidence and interpret on the basis of a very summary understanding of the historical, social, and political context.

In Search of Fundamentals, or How to Avoid Doing History

Most of Jerven’s criticism is well-founded, and applies to many economic studies of Africa. In spite of what the title of his book may suggest, however, he does not hold all economists equally guilty. In a number of cases, he also distinguishes quite clearly between different works by the same author, which he does not consider equally flawed.6 That being said, given that Jerven’s overall assessment is critical, a quick bibliometric analysis of studies on African growth published in the major American and British economics journals could have helped to “quantitatively” assess the scale of the problem.7 It would also be necessary, however, to take into account the ripple effects that these studies have had within the
political sphere, for instance in institutions such as the International Monetary Fund (IMF) or the World Bank, as well as in the media through coverage in magazines such as the *Economist* or via best-selling books by the likes of Paul Collier or Daron Acemoglu and James Robinson.  

Beyond precautionary caveats, there would be a real advantage in better defining the issue at stake—the claim to identify an African exception in terms of development capacity—and in explaining how this exception is routinely justified by a range of “fundamentals” from the distant and not-so-distant past, as well as by an insistence on a path dependence that remains essentially unexplored and potentially teleological. The focus of Jerven’s criticism is a certain manner of analyzing growth or economic development from an economic perspective, or, in other words, of claiming to write economic history without actually doing so. Had he been clearer about this aspect, his indictment would have hit its target even more effectively.

Indeed, Africa as an object of study merely serves to magnify a broader tendency: since scientistic presumption becomes more pronounced in direct proportion to the lack of historical knowledge, it emerges particularly strongly in the African case. As Claire Lemercier has pointed out in a different context, a wealth of economic studies have been produced in recent years which focus on “the long historical roots of the modern development of countries,” and many of these can be seen as extreme examples of a type of “history without the social sciences.” Discussing the future of economic history in the United States, Naomi Lamoreaux likewise expresses concern about the lack of scientific rigor manifest in a certain kind of study, especially but not exclusively devoted to Africa, that limits itself to establishing a correlation between an element of the—sometimes very distant—past (the slave trade, the introduction of the plow) and a characteristic of modern economies or societies (income per capita, confidence, the social position of women), without tracing the trajectory linking the past to the present, and thus amounting to a form of “historical” economics from which history itself is entirely absent. In a recent article, Yannick Dupraz and I similarly criticized a strand of research that seeks to analyze the precolonial sources of present-day development differences between African countries based on George Murdock’s *Ethnographic Atlas*. Here, once again, we can observe an uncritical attitude to data, whether those furnished by Murdock, figures from the slave trade, or maps based on

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8. Paul Collier, *The Bottom Billion: Why the Poorest Countries are Failing and What Can Be Done about It* (Oxford: Oxford University Press, 2007); Daron Acemoglu and James A. Robinson, *Why Nations Fail: The Origins of Power, Prosperity and Poverty* (London: Profile Books, 2012). By choosing to address a nonspecialist audience, Jerven is clearly trying to counter this type of work; the book by Collier, an Oxford professor and former research director at the World Bank, is explicitly mentioned in the last paragraph of the conclusion of *Africa*: “The bottom line is that there is no bottom billion” (Jerven, *Africa*, 132).
satellite imagery. An overreliance on cross-sectional regressions and a limited grasp of historical context typically lead these authors to overestimate the importance of ethnicity. I am therefore in full agreement with Jerven when he observes that data pertaining to Africa’s economic history are often poorly understood and inadequately measured, and that the significance of many influential contributions in the field has been seriously overrated, while others still are highly contestable. Furthermore, from a statistical and econometric perspective, the methodology underlying many of these studies is essentially flawed.

How can we account for the recent surge of interest among economists in the economic development of Africa? It is always risky to draw broad conclusions about contemporary developments in a scientific field, especially in areas like economics that are strongly influenced by politics and the media, and all the more so when one has to assess an essentially American phenomenon from the vantage point of Paris. From an academic perspective it seems that neoinstitutionalism, having achieved a dominant position in part thanks to the work of Acemoglu and Robinson, now seeks to corroborate its theses as widely as possible. To that end, it has naturally sought to enter the field of economic history, where it however encounters some resistance from the likes of Robert Allen and Kenneth Pomeranz, who have written in different ways about the rise of the West, and question the preeminence of the institutionalist explanation. When it comes to studying non-Western countries, American scholarship has tended to mainly focus on Latin America, and the divergence between North and South America still attracts a lot of attention in the literature. However, the dynamics of Asian development and the specific hurdles facing Africa shed doubt on the supposedly universal dimension of the neoinstitutionalists’ theses. The overly deterministic nature of these principles has furthermore been challenged by a number of development economists such as

Abhijit Banerjee or Esther Duflo, who favor a more behaviorist approach and have focused their microeconomic studies on various parts of the developing world, including Africa and India. Outside academia, Africa’s role in a globalized world is also perceived as a pressing issue by politicians and the media, who continue to alternate between an apocalyptic and overly pessimistic view of the continent, and an equally excessive form of afro-optimism inspired by pro-globalist wishful thinking.

For the time being, the recent incursions of neoinstitutionalist economists into the field of African economic history have clearly failed to spark the kind of renaissance called for by Jerven. Current research seems to specialize in baroque trompe-l’œil effects rather than thorough historical, economic, and social data construction programs, which require more long-term investments. Writing about the rise of inequality in present-day developed countries, Thomas Piketty points out that economics as an academic discipline has simply abandoned the research program launched by Simon Kuznets fifty years ago. One is similarly surprised to note that attempts to quantify colonial economies ceased, with a few rare exceptions, after the studies carried out in the 1960s by Marxist writers like Samir Amin. As Austin and Stephen Broadberry observe, the economic history of Africa appears to have fallen out of fashion during the “lost decades” of development, in the 1980s and 1990s. Although they consider that a renaissance now appears to be underway, impelled by the work of researchers such as those affiliated with the African Economic History Network, founded in 2011, or by journals like the South Africa-based Economic History of Developing Regions, they caution that a lot of work remains to be done, not least in order to involve young academics based at “universities between the Zambezi and the Sahara.”

The Mismeasurement of African Development

To what extent is the analysis of Africa’s economic development hampered by the absence of reliable data? Here Jerven’s assessment comes across as partial and overly

15. As Jerven reminds us in Africa, p. 1, recalling the front pages that the Economist devoted to Africa between 2000 and 2011.
19. Gareth Austin, “African Economic History in Africa,” Economic History of Developing Regions 30, no. 1 (2015): 79–94, here p. 84. In this regard, economic history appears to be worse off than other social sciences in Africa, although as a whole they are weak and disempowered, and badly affected by the continent’s significant brain drain.
pessimistic. He is nevertheless justified in pointing out that many economists show little interest in factual data. *Poor Numbers* focuses exclusively on macroeconomic data, namely GDP and the inputs from which it is derived, such as agricultural production statistics. It should be said (all data fetishism aside) that accurately measuring a territory’s output and revenues is as useful as it is fiendishly difficult to do well. Generally speaking, few people pay enough attention to the uncertainties surrounding this elaborate construction, including in countries that produce vast amounts of statistics such as France or the United States. It is for instance not unusual for statistics in these countries to be revised, *a posteriori*, by as much as 0.5 percentage points for any given year. It goes without saying that cliometric estimations are subject to even greater variations, even if these confidence intervals are rarely mentioned in the literature. In the case of developing economies, where many activities take place outside monetary exchanges, operate on a very small scale, or show considerable variability, and where market integration and thus the spatial correlation of prices remain very limited, attempts to estimate economic phenomena are further complicated by specific hurdles that often leave statistical systems at a loss. These include uncertainty about the way production and income are recorded, how they are weighted, and how price differentials are determined across space and time—questions to which the recent Nobel Prize winner Angus Deaton has devoted several of his works. The important margins of error linked to African national accounts come as no surprise to many economists who work with them.

In his books, Jerven pays little attention to large-sample microeconomic data, including the household surveys that have increased exponentially since the 1980s, and whose importance Deaton has repeatedly highlighted. The production of these data is still heavily dependent on initiatives and funding provided by international aid agencies—this is true, for instance, of the demographic and health surveys financed by the United States Agency for International Development (USAID), the living standards measurement studies supported by the World Bank, and the “1-2-3 surveys” spearheaded by French and European development...
agencies. Others surveys are of course carried out as part of specific programs organized by national statistics institutes or in the context of particular research projects. The objectives of these surveys and the constraints they face are quite different from those of national accounts. They also produce a good number of macroscopic indicators such as monetary poverty indexes, child mortality rates, and even school enrolment rates, and from this perspective they are less susceptible to political manipulation. Certain variables, such as school enrolment or height charts, can even potentially shed retrospective light on historical developments dating back to the colonial era. What is more, these surveys also make it possible to carry out analyses other than rankings or cross-sectional regressions. Their multidimensional nature and the heterogeneity that they place center-stage at least partially compensate for the lack of relevance of correlation analyses based on macroeconomic aggregates.

These large-sample microeconomic data can of course be misused, like any other real-world observations. They also suffer from specific imperfections and limitations, not least of which is the problem of sampling. Compared to population censuses, these surveys can “miss” a lot of single men and migrants because of their focus or the way they are organized. They also fail to adequately reflect the conditions of social elites and their income, and thus provide an incomplete measure of social inequalities. The format of many surveys represents another limitation, as they are often ill-adapted to family structures and African co-residency practices, or to agricultural activity and informal-sector work. However a number of researchers, including at the World Bank, are currently working to develop surveys that better reflect the social and economic reality of the countries they target. Finally, these kinds of surveys are typically under-used by researchers working on contemporary African society; instead, they are mostly used by technocrats to calculate the evolution of indicators like the poverty rate.

In short, the statistical basis available to researchers analyzing the contemporary evolution of African countries remains shaky. For instance, although household surveys make it possible to assess the (very slow) progress of electrification across the continent, and can usefully be paired with satellite imagery showing light output, it remains very difficult to retrace the construction of power stations and electrical networks. To take another example, although these surveys make it possible to at least partially observe structural changes in land usage and agricultural practices (and can, once again, be compared to satellite imagery measuring

24. In his commentary on Poor Numbers in Politique africaine 133 (2014): 177–81, Nicolas van de Walle reminds us that demand for African statistics rarely comes from African societies themselves, but rather from foreign funding bodies, albeit in a necessarily limited way. Mesplé-Somps makes a similar point in “L’Afrique et ses statistiques.”
vegetation cover), most of the countries concerned have no cadastral register and very few reliable statistics relating to agricultural production apart from export crops. And while these surveys provide useful indicators about the distribution of household spending or urban income levels, no country (except South Africa) publishes tables based on income tax, which would make it possible to fill in the gaps in the survey data when it comes to high earners. It is worth pointing out, incidentally, that a significant amount of human and financial resources have been deployed to assess the impact of development aid, often in the form of controlled experiments focused on local interventions by government agencies or private actors. These assessments rarely result in new data about social dynamics, since they tend to produce “snapshots” focusing on the specific issue in question. In cases where researchers actively seek to broaden their perspective, however, these operations can result in a better understanding of social logics in a given context.

Jerven is therefore not wrong to observe that some economists, as well as multilateral and bilateral aid agencies, show very little interest in the construction of factual data, whether about the past or the present. A few years ago, I highlighted the weakness of the economic data concerning the colonial period in Africa, notably those from former French colonies. As mentioned above, the last significant works of economic history to involve the collection of their own data appeared in the 1960s and 1970s, and were often the work of Marxist historians or economists like Amin or Jean Suret-Canale. It is as if development economists after this moment consciously chose not to focus on anything before the 1960s, or even on any period other than the immediate present.

Colonial archives cannot of course be treated as raw natural data, and raise fundamental interpretation issues. As with all secondary data, whether textual or numerical, it is essential to pay close attention to the context in which the


Much research focuses on specific regions and time-spans: André Nouschi, for instance, studies the Constantinois region in northern Algeria between 1890 and 1919, while Hélène d’Almeida-Topor focuses on Dahomey from 1890 to 1920, and Catherine Coquery-Vidrovitch on the Congo in the years 1898–1930. Vietnam and Indochina stand out as relative exceptions, though recent works deplore the fact that colonial archives have not been fully exploited: see Pierre Brocheux and Daniel Hémery, Indochine. La colonisation ambigué, 1858–1954 (Paris: La Découverte, 2001); Pierre Brocheux, Une histoire économique du Viet Nam, 1850–2007. La palanche et le camion (Paris: Les Indes savantes, 2009). Former British colonies have not been better studied: one notes for instance that an important reference work like Lance E. Davis and Robert A. Huttenback’s Mammon and the Pursuit of the Empire: The Political Economy of British Imperialism, 1860–1912 (Cambridge: Cambridge University Press, 1988), does not cover the twentieth century and therefore barely mentions Africa, focusing instead on the political economy of the metropolis.

28. A state of affairs that prompted me to set up, with others, a data-collection program focused on public finances, socioeconomic data, and military conscription archives, the results of which are currently being analyzed.
information was produced, and to the ideological and pragmatic demands it answered. Military conscription, budgets and tax revenues, price lists, or production and income accounts were not designed to serve today’s researchers. In some extreme but not uncommon cases in colonial contexts, the data recorded were more or less deliberately falsified or distorted in order to justify the action or inaction of local administrators. In other cases, however, for instance when different administrative services were in direct competition with each other, the incentive to be objective was sufficiently strong for actors to produce data that are useable today. Nevertheless, even the most impartial data from this period are typically partial, in the sense that they are often incomplete. Administrators only count what counts at any given moment: the size of new conscripts and their food rations, because they needed a well-fed army; the number of taxpayers eligible for poll tax or forced labor contributions; the cost of a “European” lifestyle in order to calculate the expatriation bonuses granted to metropolitan personnel; the growth of GDP following the “development” investments of the postwar years, and so on. In Poor Numbers, Jerven provides several examples of the underlying biases or blind spots that have influenced the production of data about population levels, crop yields, or income levels during various periods of African economic history up to the present day, biases that involve colonial and noncolonial administrations, international organizations, and the behavior of the actors being counted. More important still, he examines the effects of these biases—for example, the choice to interpolate crop yields from rainfall data—on the resulting analyses—for instance, when measurement errors are supposedly corrected on the basis of the same rainfall data. On the other hand, while deconstruction represents an essential stage in analysis, it is also important not to let it paralyze the exercise of collecting and analyzing useful data with the goal of establishing social facts, without yielding to naive positivism.

Although the data that can be found in colonial archives are finite in number and far less comprehensive than the corresponding information available about metropolitan conditions during the same period, many of them have yet to be unearthed, and have never been the focus of exhaustive analyses. It would be illusory to think that these archives will one day enable us to provide detailed estimates of the GDP of Côte d’Ivoire in 1920, let alone in 1850. However, they can be

29. A good example of this is the controversy surrounding “data” about the 1942–1943 Bengal famine. Several authors have pointed out flaws in Amartya Sen’s conclusion that there was no deficit in agricultural production during this period, arguing that some British officers preferred to emphasize problems with distribution and speculation rather than a bad harvest that would have justified direct food relief efforts. See Amartya Sen, Poverty and Famines: An Essay on Entitlement and Deprivation (Oxford: Oxford University Press, 1981); Mark B. Tauger, “The Indian Famine Crises of World War II,” British Scholar 1, no. 2 (2009): 166–96; and Cormac O. Gráda, Famine: A Short History (Princeton: Princeton University Press, 2009).


31. Jerven, Poor Numbers, 78–79.
used to fill the vast gaps that remain in our knowledge of African economic history, allowing us to link together the few contexts and periods that have been reasonably well studied. More often than not, these gaps correspond to unknown data waiting to be identified and analyzed, rather than to an absolute absence of information. Yet the persistence of this vast terra incognita has not prevented a proliferation of different discourses about African economies, their trajectories, the fundamental obstacles they face, the characteristics of their institutions, and so forth. As Thandika Mkandawire (whom Jerven cites) remarks, the dominant discourses tend to relish pointing out persistent failures and to bemoan a long list of African shortcomings—not least the absence of a developmental state, although this has not always been the case.

The fact that so little attention has been paid to efforts to measure and collect historical data must be ascribed to the intellectual organization of the field of economics. In a famous essay (the title of which is both an echo and an inversion of the heading of one of Albert Hirschman’s texts), Paul Krugman compares the evolution of development economics from 1950 to 1990 to Western efforts to map the African continent. The first European maps of Africa, produced in the sixteenth and seventeenth centuries, showed a degree of knowledge about inland Africa, gleaned from the oral testimonies of Arab and European travelers, even if they also contained many mentions of fabulous animals and mythological peoples. The systematic coastal exploration of Africa by European seafarers then led to a new generation of maps, which featured precise information about the coastal regions but very little about the African interior. Krugman uses this analogy to optimistically explain why the (contradictory) theses formulated by Hirschman and Paul Rosenstein-Rodan, two pioneers of development economics, temporarily had to fall into oblivion before being readapted and introduced into formal models allowing for the presence of increasing returns in production.

This parallel with African cartography is somewhat surprising, insofar as Krugman compares a supposedly accurate method of observation to a set of theoretical theses with no direct technical applications. His parable explicitly recognizes the creation of mathematical models as the prime driving force behind economic thought, but it also implicitly suggests that observation and the construction of facts play only an invisible role in this process. The “stylized facts” leading to the elaboration of these models are presented as somehow exterior to the research, either because they are “naturally” perceptible to the informed observer, or because they are drawn from obscure statistical bureaus that hardly merit any attention. The prevalence of this perspective could explain why the production of statistics

32. See above, note 24.
and economic history have both come to be considered the poor relations of the
dominant development economics. The risk here is that the resulting models
amount to little more than variations on commonly received opinions, in accord-
dance with the shifting trends of intellectual fashion or the (sometimes uncon-
scious) social fantasies of their authors; or that said models develop an obsessional
focus on the risk of unintended adverse effects, a commonplace of reactionary
thought identified by Hirschman.

It is, undeniably, possible to convince oneself that there exist no social facts
independent of the interrelated theoretical concepts used to construct them, that
abstract mathematical models are therefore very useful, and that it is exceedingly
difficult to quantify anything, especially since the entire process can be jeopardized
by a range of unconscious prejudices. But on the other hand, it seems risky to
seek to describe the evolutions of African economic history without attempting
to observe it in detail; unless, that is, one subscribes to the premise that the trajecto-
ries of the United States and China offer a sufficient basis on which to elaborate a
“model” of development, understood in both the normative and the positive sense
of the word.

Analyzing Intra-African Development Differences

As Jerven points out in *Africa*, many empirical analyses begin their account of
Africa’s development history in the year 1960. For a while, most studies focused
on explaining why the dummy variable “Africa” amounted to a negative coefficient
in regression analysis, suggesting the presence of a growth deficit that could not
be accounted for by the usual explanatory factors. One of the most famous arti-
cles on the subject sought to explain this mystery by pointing to the ethnic frag-
mentation characterizing many African societies: according to the *Atlas Narodov
Mira* produced by Soviet ethnographers in 1960, Africa represented the most ethni-
cally fragmented continent in the world, meaning that this variable was an inter-
esting candidate. In a way, this explanation marked the first “return” of long-term
history to the analysis of African development in the work of economists during
the 1990s, based as it was on the assumption that ethnic affiliation represented

35. The divide between economic history and analytical economy dates back to even
older controversies like the “battle of methods” (*Methodenstreit*) that opposed Carl
Menger and Gustav von Schmoller in 1883. Joseph A. Schumpeter gives a partisan
account of this debate in his *History of Economic Analysis* (Oxford: Oxford University
36. This affects even the measuring operations that seem the most mechanical, as
Stephen Jay Gould shows in *The Mismeasure of Man* (New York: W. Norton and Company,
2006).
note that according to the *Atlas Narodov Mira*, France also exhibits a certain degree of
ethno-linguistic fragmentation due to its Basque and Breton minorities.
an ancient, and possibly even primordial, structure of African society. This idea that Africa is primarily a continent of warring tribes continues to inform a large part of present-day research. But although ethnicity is undeniably an important aspect of African society, it is by no means primordial: ethnic competition was also reinforced by the political inequality introduced by colonial powers, which helped to translate preexisting economic inequalities into networks of patronage and protection.  

Furthermore, as a number of researchers have pointed out, there are other important structural differences that set sub-Saharan Africa apart from other continents, notably an abundance of land and a relative underdevelopment of intensive agriculture (unlike in Asia), as well as an absence of European settler colonialism (except in South Africa and Zimbabwe, and in contrast to most of Latin America). There is also, correlative, the question of the slave trade. As the models of international economics remind us, factor endowment (that is, the relative abundance of land, labor, and capital) can potentially influence the insertion of an economy into the international capitalist exchange system, while historical institutions can in part be seen as the outcome of a functional adaptation to the relative abundance of these factors.

Since it is difficult to distinguish between all of these intertwined variations, large-scale intercontinental comparisons have become somewhat discredited, leading researchers to focus on inter-African variations. Many potential reasons for these variations have been explored: the intensity of the slave trade, the impact of European evangelization missions, the political centralization of ethnic groups before colonialism. Influenced by the neoinstitutionalist paradigm, researchers have turned to the relatively distant past (at least in relation to 1960) to look for the “fundamental” origin of differences in development. Given the paucity of historical data, however, this means that another ethnographic atlas, the one compiled by Murdock, has suddenly acquired an enormous importance. It has been treated as if it offered a veritable map of precolonial Africa, one which could be combined with slave-trade data, maps showing the spread of Christian missions, and, later,
georeferenced surveys or satellite imagery. This has led to a revival of the ethnicity thesis, with ethnic affiliation being seen as the foundation of primordial social relations and “political cultures,” the most caricaturally straightforward example being the “trust” between different actors.

Dupraz and I have already insisted elsewhere on the problems this type of approach entails.40 Even leaving aside all the sociological and historical issues that they raise, the statistical validity of these analyses remains problematic, just like growth rate or per capita GDP regression analyses. Instead, like Jerven, we have argued for well-reasoned comparisons between actual, observed historical trajectories. Up to this moment, however, researchers have failed to determine any central historical factors that make it possible to account for the different development trajectories of particular regions, whether the presence of a precolonial centralized state, the intensity of the slave trade and its long-term institutional effects, the presence of different religions, or the nationality of the colonizing power. Instead, most research has tended to confirm the strong structural similarities between different African countries, once obvious geographical and ecological variations have been taken into account.

If we focus, for example, on the parallel histories of two neighboring countries, Ghana and Côte d’Ivoire, the number of potential differentiating factors is striking. As a result, predictions about the good fortune of one or the other are largely based on a belief in the relative importance of different fundamentals. With the rise of the Ashanti Empire in the eighteenth and nineteenth centuries, structured around its capital Kumasi, a large part of what is now modern Ghana experienced strong political centralization, unlike neighboring regions in what would become Côte d’Ivoire. Correlatively, the slave trade was much more intense on the Ghanaian side. During the colonial era and until the end of World War II, Ghana (the “Gold Coast”) was the richest British colony in tropical Africa, while Côte d’Ivoire ranked below Senegal, the center of French West Africa, and Madagascar. Ghana became the world’s top producer of cocoa beans in the 1920s, while cocoa production only began to take off in Côte d’Ivoire from the 1950s onward, leading the country to eventually overtake its neighbor in the late 1970s. The two countries’ independence leaders, Kwame Nkrumah and Félix Houphouët-Boigny, had very different views: one was an apostle of Pan-Africanism keen to break all ties with the former colonial power, the other was a key architect of Françafrique and heavily benefited from French foreign aid.41 The next twenty years saw Ghana sink into a morass of macroeconomic

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40. Cogneau and Dupraz, “Institutions historiques et développement économique en Afrique.”
41. For a comparative analysis by a political scientist, see Catherine Boone, Political Topographies of the African State: Territorial Authority and Institutional Choice (Cambridge: Cambridge University Press, 2003), especially chap. 4, “Taxing Rich Peasants: Regime Ideology as Strategy.” Differing attitudes to the central state are explored in Lauren M. MacLean, Informal Institutions and Citizenship in Rural Africa: Risk and Reciprocity in Ghana and Côte d’Ivoire (Cambridge: Cambridge University Press, 2010), an anthropological investigation conducted at the border between the two countries.
instability and military coups, while Côte d’Ivoire entered a period of great prosperity, which lasted until the sudden drop in cocoa bean prices during the 1980s. Ghana’s adoption of structural adjustments under the leadership of Jerry Rawlings, president from 1981 to 2001, opened up access to international funding and launched a slow recovery, while Côte d’Ivoire underwent its own economic crisis, followed by a civil war from which it has yet to fully recover. Historical data reveal these intersecting trajectories, for example through Ghana’s superior primary schooling and literacy rates up to the 1950s, or through the higher wealth levels and rural electrification rates on the Ivorian side of the border during the late 1980s. However these differences were clearly the outcome of reversible political choices, rather than the predetermined and unavoidable result of institutional fundamentals. The different arbitrations between salary and employment levels in the public sector, or in the spatial distribution of work between an elitist Côte d’Ivoire and a decentralized Ghana appear to be more enduring characteristics, though they could well become less pronounced with time.

As Jerven reminds us in *Africa*, some African regions have experienced relatively durable periods of economic expansion. Little is known about precolonial periods of economic growth, which mostly seem tied to the emergence of new kingdoms or to phases of political conquest. The periods we know most about are those where growth depended on long-distance trade with Europe—the slave trade, but also “legitimate” exchanges involving mining or agricultural products. Examples mentioned by Jerven include the Kingdom of Dahomey (already highlighted by Karl Polanyi and studied by Patrick Manning), the neighboring Ashanti Empire, and the British Gold Coast. The 1860–1885 commodity price boom presumably

acted as a stimulus for the colonial annexation of Africa, ratified by the Berlin conference of 1884–1885, before prices fell again in the decades before 1940.\textsuperscript{46} Since the 1960s, periods of growth have been closely aligned with worldwide fluctuations in commodity prices, given the strong dependence of African economies on mining and agricultural exports.\textsuperscript{47} From the early 2000s until very recently, the growth performances of many African countries markedly improved, coinciding with a significant spike in worldwide commodity prices; household surveys pointed to a simultaneous reduction in poverty rates, albeit of a relatively modest scope.\textsuperscript{48} The current slowdown of the world’s economy, especially in China, and the sharp fall in global commodity prices have put many African economies under pressure, tempering some of the overly optimistic views that had been aired in recent years. Indeed, there is still little evidence of the structural change or diversification of economies,\textsuperscript{49} while the continent’s fertility transition appears to be proceeding at a slower pace than previously expected.\textsuperscript{50}

The utilization of the revenue from natural resources, and of the capital from foreign development aid, thus remains a central question. What proportion of this income is diverted away from the domestic economy to the benefit of foreign agents (particularly multinationals), or for other reasons (such as capital flight)? What proportion is spent or saved and reinvested, locally, into productive capital, infrastructure, education, or public health? Finally, who benefits from the distribution of this income and the investments that it generates, or from its redistribution via state institutions or non-state networks like local communities, brotherhoods, family networks, and so forth? There are clearly significant differences between the income derived from the labor and investments of small-scale growers, such as Ghanaian or Ivorian cocoa farmers, and the significant revenue derived from the mining sector, which is shared between multinationals and the state. The political economy of this distribution, which includes corruption practices and has an important influence on the construction and legitimacy of state organizations, must be studied from a historical perspective and not simply dismissed as inherent “bad governance” caused by intangible fundamentals, irrespective of their date and origin. The history of this political economy is by nature “global,” since it implies not only the \textit{longue durée} of interactions with neighboring Europe and the Arabo-Muslim world, but also more recent ties to the entirety of the global economy, particularly the United States and China. Frederick Cooper

even encourages us to leave the old opposition between internal and external causes behind, and to think about Africa, Senegal, Ghana, and also Europe in a relational manner, as coproductions, albeit of an asymmetrical nature.51

“The choice of a subject or of a work strategy does not represent a hypothesis, as epistemology manuals claim, but rather an allocation of rare resources: given that I have a finite amount of working time, that I can only work an average of three days a week, with this or that constraint, how do I plan my investments to obtain a maximal return?”52 The “maximal return” evoked by Pierre Bourdieu must be understood in terms of an added value of knowledge, and does not always correspond to a “high-impact” publication in a prestigious journal. In terms of constraints, it is important to remain cautious about our current knowledge of African societies, subject as they are to very different historical parameters from those governing European or even Asian societies, as Jack Goody’s work has repeatedly shown.53 It is therefore essential to proceed with humility and prudence, qualities that do not always characterize an economic literature that on occasion comes dangerously close to promoting a “govern like us” message, and that remains heavily informed by a neoconservative obsession with external reforms imposed through military or economic pressure, the disastrous consequences of which should really be clear to us today.54 Jerven is right to call for more comparative studies in order to avoid these pitfalls; he holds up the more or less unavoidable but perhaps too imposing example of Pomeranz’s book on Europe and China in a bid to encourage similar projects comparing African countries to South American or Asian ones.55 Such broad comparisons can be very fruitful, not least because they open up a debate dominated by a small group of Africa specialists with a tendency to overestimate the continent’s idiosyncrasies. They also come with challenges, however, for they require extensive knowledge (or excellent collaboration skills), and can at times yield disappointing results, since they constantly have to deal with the issue of structural differences discussed above.56

When it comes to allocating rare resources and ensuring a maximal return on investment from a research perspective, well-controlled intra-African comparisons

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have the advantage of highlighting intriguing scientific mysteries that call out to be resolved, even as they challenge grand narratives based on hasty generalizations. Comparisons between neighboring countries—Ghana and Côte d’Ivoire, Kenya and Tanzania, Malawi and Zambia—are particularly useful, since they make it possible to control for some of the geographic and anthropological parameters otherwise liable to complicate the analysis. Nevertheless, other interesting parallels can also be drawn, for instance by analyzing the different destinies of territories belonging to the same colonial empire (Côte d’Ivoire, Guinea, and Senegal, for example, or Ghana and Kenya), or even focusing on less evident interimperial comparisons such as South Africa and Algeria’s experience as settler colonies with apartheid systems. The questions raised by these comparative studies can arise from the domain of short-term history, especially when the focus is on “key moments” (decolonization, structural adjustments, etc.), or on present-day economic or sociological issues, but they can also—though not always, which is interesting in itself—bring us back to long-term history. From an economist’s point of view, questions about the distribution of resources and the construction of the state are central, especially in terms of their interrelation with powerful demographic and ecological changes. As I have sought to show, even if a lot of relevant data still needs to be constructed, a renaissance of African economic history is definitely within reach.

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57. Using these country names does not, of course, mean ignoring the fact that these states and their borders are recent constructs. The literature on these borders has highlighted the differentiation processes they entailed and the constraints they imposed, but also the new political and commercial opportunities they opened up for certain actors. See Anthony Ijaola Asiwaju, Western Yorubaland under European Rule, 1889–1945 (Atlantic Highlands: Humanities Press, 1976); William F. S. Miles, Hausaland Divided: Colonialism and Independence in Nigeria and Niger (Ithaca: Cornell University Press, 1994); Paul Nugent, Smugglers, Secessionists, and Loyal Citizens on the Ghana-Togo Frontier: The Life of the Borderlands since 1914 (Athens: Ohio University Press, 2002). See also Cogneau, Mesplé-Somps, and Spielvogel, “Development at the Border.”