

ECONOMIC GROWTH AND GLOBAL INEQUALITY IN LONG RUN PERSPECTIVE

Review of *The World Economy, A Millennial Perspective*
by Angus Maddison (2001)

The defining issue for economic science remains, as it was for Adam Smith, how best to explain “the wealth of nations.” Although our understanding of why some peoples are rich and others poor remains highly incomplete, major advances have been made in just the last few years. Angus Maddison’s new monograph, *The World Economy* is an important benchmark in the fast-growing interpretative literature on the nature of long-term economic growth and inequality between nations. During a career that spans four decades Maddison has published individual case studies of Brazil, China, Europe, India, Indonesia, Japan, Mexico, and Russia in addition to a widely-cited series of publications that summarize most of what we know about the empirical dimensions of long-term growth (Maddison, 1982, 1983, 1989, 1991, 1995, 1998). The new book contributes on an even grander scale to our knowledge of the empirical dimensions of economic change and to the ongoing interpretation and reinterpretation of the evidence.

Maddison’s premise is that before one can explain why economic growth has differed, one must have credible evidence of such differences. Reliable evidence about the extent and timing of growth rate differentials is therefore absolutely central to the historical debate—and Maddison has compiled the necessary evidence through a critical and discriminating collection of the most useful estimates of population and national income from scholars around the world. For example, his 1995 book *Monitoring the World Economy* reported estimates of GDP as early as 1820 for 30 countries. These data permitted, for the first time, a systematic analysis of comparative growth that combined the 19th and 20th centuries on a truly global scale. Now, in *The World Economy*, Maddison extends his own compilation and interpretation to take account of studies emerging from other research groups in order to update his data and reconsider his own interpretations. The new volume is both a documentation of the level of economic activity in past economies and a prolonged reflection on both the theory and experience of economic change.

The scale of activity since 1995 dedicated to the measurement of long term growth is startling—even leaving aside studies of specific sectors, of price index/exchange rate methodology and indicators other than income (see Batista *et al.*, 1997; Eltis, 1995; Toutain, 1997; Bertola *et al.*, 1998; Maddison, 1998; Mancall and Weiss, 1999; Campbell, 2000, pp. 406–7; Schultze, 2000; Sivasubramonian, 2000; Smits, Horlings, and van Zanden, 2000; van Zanden, 2000; Maddison, 2001; Maddison, Rao, and Shepherd, 2001; van Zanden, 2002). Pre-1800 estimates of varying reliability and scope have appeared recently for Belgium, Barbados,

England, France and Italy. New 19th-century benchmarks have become available for Australia, Austria-Hungary, Indonesia, Italy, Netherlands, Portugal and Uruguay. Various teams of scholars are now intensively examining the evidence for China, Russia, Scandinavia, Spain, Russia, and Vietnam. The North American and Australasian data are being revised to take account of aboriginal population and production.

It would not be fair to give credit to Maddison for all of this activity, but there is no doubt that much of it has arisen in response to his work, and in particular to *Monitoring the World Economy*. As much as anyone since Simon Kuznets and Colin Clark, Maddison reminds us of the importance of national income estimates to the understanding of economic growth. Equally important, he has created a database of evidence with accompanying critical assessment that allows him and other scholars to revise, extend and systematically analyze the record of economic growth. Maddison's compilation and organization of historical national accounts data is likely to be used for some time to come.

The broad contours of Maddison's compilation are well known. In 1991 he reported annual GDP, population, labor, cost of living, capital and related series since 1870 for 16 OECD countries. The 1995 compilation extended the coverage from 16 to 56 countries with benchmarks for select years beginning in 1820. Maddison himself produced the estimate for only one country (Indonesia). For the remainder he relied on existing estimates (10 countries) and extrapolations (another 19 countries). Annual data began in 1870 for the so-called advanced capitalist countries, 1900 for Asia, Latin America and southern Europe and 1950 for Africa. Purchasing power parity (PPP) adjustments were made, although capital and price data did not appear in the 1995 volume, and many countries do not have a complete range of data. For the first time in 1995 Maddison attempted to compile consistent long-run data for countries which adopted and then abandoned socialist accounting.

With the new volume Maddison extends coverage to 124 countries, although as before not every series is available for every country. Only benchmarks are available until 1950 at which point annual data take over. As in previous Maddison publications, extensive appendices and tables comprise fully half of the volume. The interpretative section of *The World Economy* consists of three more or less independent essays that examine the broad contours of global growth in the long run, the impact of western development on the rest of the world, and growth in the second half of the 20th century.

The first chapter summarizes a by now conventional view that world economic and population growth gradually accelerated over a millennium in which western Europe (and its overseas offshoots) began to pull ahead of the rest of the world as early as the 14th century. The second chapter is an eclectic account of European overseas expansion and interaction with the rest of the world. Maddison estimates the burden and benefits of colonialism (as measured by the colonial export surplus share of colonial and imperial income) to be rather small (about 1%) for the Dutch East Indies in the 18th century and for British India in the late 19th and early 20th centuries. Nevertheless, the burden to Indonesia and the gain to the Dutch increased considerably from 1868 to 1930 (p. 87). By the late 1920s, Indonesia was losing about 10 percent of its income every year through

the Dutch harvesting of its trade surplus. The final chapter provides an overview of the world economy in the latter half of the 20th century, from post-war adjustments to golden age to political and economic realignments after 1980. The interpretations are most incisive for the OECD and Latin America. The discussion of Asia emphasizes the diversity of experiences and of explanatory factors (even among the countries of east Asia, which are sometimes represented as having shared a common pattern). The discussion of Africa focuses on the legacy of colonialism, the nature of transition to independence and governance.

The interpretative discussions are valuable, but the data, of course, are the most novel aspect of the book. Maddison's inclination consistently has been to push back the data a little further than others have found possible. This is done in a cautious and critical manner, but he remains committed to pushing at the margins of reliability. In practice, the limit of useful evidence for most of Africa and parts of Asia has been the middle or early 20th century. Secure evidence tends to peter out in the early 20th or late 19th centuries for much of Latin America and other Asia countries. For OECD countries the frontier tends to be the early and middle 19th century, and the 18th century for a small number of better documented cases.

How much confidence can we have in the estimates which are close to the frontier of useful sources, for example Africa in the early 20th century, Asia in the late 19th century or the OECD economies in 1850 or 1820? Clearly, estimates for some economies are more reliable than others, and reliability depends on the uses to which the data are put, but in general, the quality of the estimates deteriorates considerably before 1900 (with an earlier break point for North America and later for most of Africa).

This reviewer recommends that *none* of the pre-1900 income estimates be used without first examining the individual studies from which they derive, or at least reading Maddison's useful notes on the origin of the data. The underlying sources are few and in some cases of unknown representativeness. The estimating assumptions for many countries are eclectic and, occasionally, inconsistent with those for other countries. Only in a very few countries do estimates reflect a consensus generated by successive examinations of the evidence or a debate that tests the critical assumptions and sources. Indeed, some data originate with scholars concerned to assess or to promote a particular explanatory hypothesis. Whether or not such data are helpful for a particular research project needs to be assessed after examining the nature of the assumptions that they embody.

Some aspects of the Maddison corpus will give rise to uneasiness even among his supporters. The new tables in Maddison (2001), for example, must be used together with those in Maddison (1995 and 1998) in order to obtain full explanation or in some cases more detailed benchmarks. Although per capita estimates are affected less than the population and total income data, an obvious source of awkwardness is boundary changes which make the various generations of estimate inconsistent for some countries.

Organization of the pre-1820 data in a separate appendix facilitates a focus on the most difficult aspects of data construction but, when a single table integrates more reliable data with highly conjectural guesses and intensely disputed estimates, confidence in the more robust data is unnecessarily undermined. Some

sense of data quality may be gained from reading through the appendices and notes of the 1995, 1998 and 2001 volumes, but even this material cannot do justice to the complexity of underlying judgements and sources, and in any case is not represented in the tables.

The full extent of extrapolation and interpolation, with implied vulnerability to serious error, is understated to the extent that many of the detailed estimates already embody considerable inter/extrapolation for individual sectors. Indeed, some extrapolations on the basis of a single trend are arguably inconsistent with reasonably well-documented declines and rises in the level of economic activity. The more important implication for hypothesis testing is that data built upon the premise of gradualism will not be helpful in the examination of any hypotheses that turn on the rapidity of change in a shorter period. For example, the extent of estimated decline for some countries during the 19th century matters a great deal, but early GDP is often obtained partly or entirely by backward extrapolation from 1900. Maddison's assumption of a limited decline is part of his evidence for the contested hypothesis of a low 1800 benchmark, although this connection is never made explicit.

A related example is that data constructed on the premise of a presumed smoothness of pre-industrial growth cannot be used as evidence that the industrial revolution was uniquely transformative. There may have been episodes before 1700 of dramatic growth spanning 200 or 300 years, which were exceedingly revolutionary for their time, but for the most part we have no good evidence one way or the other about the course of incomes during these experiences (Jones, 1988). Instead of admitting that we lack evidence Maddison creates data that will be read as definitive and that appear to rule out the possibility of earlier revolutions.¹

Some readers might also prefer that Maddison integrate his discussion of income measures with other evidence—such as wages, consumption, stature, longevity and so on. The income evidence is difficult to reconcile with some of these indicators—the evidence of wages in Asia is particularly important (e.g. Allen (2001) and Parthasarathi (1998), on which see below). Some consideration of this evidence by Maddison would seem appropriate since Maddison himself introduces a discussion of English wages and, in any case, many of the studies on which he relies and indeed his own conjectures are based partly on wage evidence. More extensive discussion of wage evidence would also facilitate the integration of Maddison's work with the long-term growth studies of Jeffrey Williamson and Kevin O'Rourke, who demonstrate the value of factor price and proportions analysis (O'Rourke and Williamson, 1999, 2002; Williamson, 2002).

Maddison has promoted the use of purchasing power parity (PPP) for long-term and comparative historical research, but unfortunately, the best that can be done at the present time is to estimate PPP using late 20th-century prices and quantities. The use of a single PPP adjustment over 200 years is an obvious weakness. Hill (2000) shows the considerable sensitivity of cross national income comparisons to the choice of PPP methodology even among countries with good data at a single date. Prados de la Escosura (2000) extends the point to long-term

¹In some cases, such as China from the 10th to 13th centuries, he has explicitly considered and rejected an alternate scenario in another publication (Maddison, 1998, pp. 24–5).

growth and historical comparisons. Indeed, Hanley (1997) and van Zanden (2002) point out that consumption patterns changed enormously from the beginning of the 19th to the end of the 20th centuries and that consumption in some 19th-century societies had almost nothing in common with each other. In some societies goods consumed on a daily basis were not traded and, even if they were, prices are not known. Income inequality and not infrequent examples of labor coercion makes it even harder to obtain representative prices and quantities from pre-industrial and early industrial markets around the world (Hoffman *et al.*, 2002). In these circumstances the application of 1990 PPPs to the early industrial era may simply confuse the issue and disguise the enormity of the research challenge.²

Maddison also reports income estimates for various parts of the world before 1800 but these data are even weaker. For example, although the estimates for England in 1086, share several heroic assumptions, they still differ amongst each other by a factor of 300 percent (Campbell, 2000, pp. 406–7). Estimates for most other countries are pulled from even thinner air (Maddison, 2001, pp. 244–60). Maddison’s reading of the Chinese and Japanese evidence, for example, has failed to persuade many scholars contributing to these literatures (Hanley, 1997; Pomeranz, 2000). The contours of Italian growth appear to be particularly controversial even in the 19th century (Cohen and Federico, 2001, pp. 8–11). And so on. Even the population data are highly uncertain (Caldwell and Schindlmayer, 2002). The largest single controversy arises from the estimates of income in India and China, which together were home to roughly three-fifths of the world’s population in 1820.

There are two justifications for attributing a precise number to what in some cases is no more than an informed guess. (i) Some social scientists comprehend quantitative expression more readily than other metaphors. (ii) If the data are incorrect, other scholars may be encouraged to undertake the basic research and to get it right. The disadvantage, however, is the risk of misleading readers with a spurious appearance of precision. This danger is considerable because the underlying evidence is often extremely thin and in some cases controversial. It needs to be made clear that most (although not all) of Maddison’s pre-industrial data simply restate in quantitative terms particular hypotheses that are not easily confirmed or rejected. For the most part these data do not constitute evidence of the sort that might be used to resolve specific issues and controversies. Nevertheless, they are useful as long as users understand that most are uncertain and that some are highly contentious.

But why do these data matter?

The quest for a satisfactory interpretation of the “big picture” has a long and distinguished history. From Smith, Malthus and Marx through Weber and Hobson and on to Schumpeter, Prebisch and others, scholars have pondered the

²van Zanden (2002) has begun the construction of more appropriate PPPs for the 19th century. This attempt to extend PPP methodology turns out to have little impact on the Netherlands–Java comparison for 1820, although we do not yet know if a larger impact would be seen if the estimate were extended to goods that traded locally if at all, or if prices could be obtained for the interior of Java as well as the port city. More research along these lines is clearly needed. Until it is available, the conceptually less ambitious but empirically more tractable construction of cost of living or even food cost indicators will continue to be useful (Allen, 2001; Ozmuur and Pamuk, 2002).

determinants of the “wealth of nations.” As T. S. Ashton put it in *The Industrial Revolution* (1948):

There are today on the plains of India and China men and women, plague-ridden and hungry, living lives little better than those of the cattle that toil with them by day and share their places of sleep at night; such Asiatic standards, and such unmechanized horrors, are the lot of those who increase their number without passing through an industrial revolution.

The wording adopted by Ashton reflects the certainty of the view that the industrial revolution was a fundamental discontinuity that created for the first time the possibility of fast modern economic growth. Indeed, by 1950 analysts of all ideological persuasions agreed that industrialization provided a means of escape—perhaps the only means of escape—from poverty in a growing population. Evidence in support of this belief came from casual observation that the fast-growing economies of the 19th century also industrialized quickly (Britain, U.S., Germany) and from the identification of a systematic correlation between income growth and structural change by Simon Kuznets (1966).

Consensus on the importance of industrialization to income growth was so thorough-going that debate largely revolved around the question of how to achieve the desired industrialization. Lewis (1954) observed that inexpensive labor from the countryside had the potential to fuel industrial profits and national income growth. Rostow (1960) synthesized a number of historical experiences in support of his theory of several stages leading to a takeoff into self-sustaining growth. Increased investment and a leading sector were keys to the Rostovian takeoff. Gerschenkron (1962) also emphasized the importance of capital in a nuanced and sophisticated interpretation of late industrialization necessarily unfolding differently than the experience of the first industrial nations. By the middle and latter 19th century, he argued, a successful experience of industrialization relied on the interventions of the state and a financial market structure that would channel savings to heavy industry. Landes further entrenched the emphasis on industrialization in a celebrated work that did much to persuade social scientists that technical change is the key to economic growth and that the British industrial revolution was a fundamental watershed in human history (1965).

By the mid-1960s, there was a considerable degree of agreement that the industrial revolution was the single most important turning point in world economic history. On this view, the prospects for growth changed fundamentally following the industrialization of first Britain, then western Europe and later much of the rest of the world. Most scholars who have held these propositions have also believed that global inequality in the 20th century has its roots in the industrial revolution and its causes. On this view, the uneven spread of industrialization and consequent access to fast and sustained growth created much although not all of today’s global inequality. Very large differences in income per capita could not have existed before the industrial revolution because pre-industrial technology lacked the potential for achieving very high income. Further, backward extrapolation from 1900 under the assumption of monotonic change necessarily implies that people in most parts of the world had near-subsistence

levels of income several hundred years ago and that pre-industrial income grew rather slowly everywhere (Kuznets, 1966, pp. 34–85; Kuznets, 1973, pp. 136–43).

In this argument, western Europe must have had some advantage over other parts of the world before the 18th century since, without some prior advantage, Europe was unlikely to have industrialized first. The scientific revolution, Enlightenment rationality, and associated cultural and intellectual developments have been suggested as increasing the likelihood of a European economic advance in the pre-industrial era. The income differentials between various pre-industrial populations would have been moderate, perhaps 1:2 or 1:3, but not the enormous inequality of 1:10 and more that emerged as industrializing Europe and its overseas settlements pulled far ahead of the rest of the world economy during the 19th and 20th centuries.

Maddison's pre-industrial data reflect this presumption that Europe slowly gained on the rest of the world. He reports real GDP per capita increasing at 0.14 percent per year from 1000 to 1820 in western Europe, against 0.06 percent annually in eastern Europe, Latin America and Japan, 0.03 percent in the rest of Asia and no growth in Africa (Maddison, 2001, Table 1–2). These numbers graphically illustrate the traditional view, although it must be remembered that they have almost no empirical foundation. Maddison reports no evidence of the sort used to construct national income in support of his contention that per capita income in Brazil and the United States were identical in 1500, that Brazil income expanded at an annual rate roughly half that of the United States from 1500 to 1820, and that annual growth in 19th century-Brazil was exactly double that of the 16th to 18th centuries (p. 74). Rather, such data reflect an informed judgement based on many factors including Maddison's belief in the centrality of industrialization and technical change to economic growth.

One obvious implication of this view, as Ashton suggested, is that "late-developing" societies share in the new growth dynamic only if they achieve appropriate patterns of investment, technological change and industrialization, if necessary with the support of state regulation and subsidy. Without industrialization, the developing countries would fall further behind and, given population growth even at pre-industrial levels, would sink into a vicious circle of immiserization. Some might now dismiss this perspective as a self-congratulatory and misleading peculiarity of Anglo-American intellectual culture. It should be remembered, though, that for much of the 20th century the centrality of industrialization to economic growth dominated thinking around the world, in socialist and capitalist economies, in government and in the market place no less than in western universities.

Influential as the thesis undoubtedly was, revisionism emerged from several directions. One line of argument emphasizes the implications for industrial progress of prior changes and characteristics of agriculture. Two papers more or less simultaneously presented the argument that agriculture preceded industry and that agricultural change preceded and shaped the prospects for industrialization (Jones, 1968; Timmer, 1968; see also Reynolds, 1977). The early evidence in this agriculture-oriented literature was British and also American (North, 1961; Thirsk, 1961), but an increasingly international perspective soon developed. Bos-erup (1965) presented evidence from a wide variety of contexts in support of her

view that demographic pressures shaped agricultural productivity through choice of technology and the level of intensification. Reynolds (1985) synthesized hundreds of individual country studies, and argued that the pattern is one of acceleration (not quite a take-off) into faster intensive growth after a period of quickening extensive growth, which itself was rooted in the export of primary and most often agricultural products.

With this contribution Reynolds added to another line of argument emphasizing the importance of international trade for the domestic economy. Widely-read syntheses by Davis (1973) and Wallerstein (1974) pointed out that the west European and American economies between 1500 and 1900 were exceptional from a global perspective because complex and high-volume trading patterns emerged very early in the Atlantic Ocean. The hypothesized causal mechanisms differ but economists from Lewis (1957, 1978, 1980) to O'Rourke and Williamson (1999) agree on the historical importance of these trade and factor flows. An emphasis on the importance of trade and openness to the international economy was not easily compatible with some 1950s and 1960s attempts to encourage industrialization through government subsidy and the protection of industrial activity. Not surprisingly, by the late 1960s the tide of enthusiasm for a simple state-led industrialization strategy began to recede in the face of mounting evidence of its negative implications for growth (Hirschman, 1968; Little, Scitovsky, and Scott, 1970; Diaz-Alejandro, 1975; Taylor, 1998).

The increased attention to agriculture and trade represents an important broadening of focus but it did not erode confidence in the primacy of industrialization for long-run growth and inequality since agriculture and trade could be combined in a narrative of interdependence with industry. In the past 25 years, however, other aspects of social and economic change with potentially more far-reaching implications have attracted discussion. Arguments have been made for the importance of institutions that range from particular organizational forms for government and business to the clarification of property rights in a way that enhances efficiency in all sectors (Chandler 1977, 1990; Jones, 1981; North and Thomas, 1973; Olson, 1982; Reynolds, 1985; Rosenberg and Birdzell, 1986). A renewed emphasis on the importance of "culture" to economic growth is a recent development, although the interpretative implications may be less fundamental than appears at first glance if the significance of an economically powerful culture is simply to facilitate industrialization (Goody, 1996; Lal, 1998; Landes, 1997). Demographic and biological factors on their own or in an interaction with ecological context present a different challenge since these forces are less easily accommodated within the industrialization paradigm (Bloom and Williamson, 1998; Crosby, 1986; Diamond, 1997; Easterlin, 1996; Jones, 1981; Fogel, 1991).

Undoubtedly the strongest challenge to the traditional view of the industrial revolution arises from an enhanced appreciation of Chinese economic development in the long run. The considerable sophistication of the early Chinese economy is increasingly recognized as a challenge to received thinking about the long run patterns (Chao, 1986; Elvin, 1973; Jones, 1981, 1988; Lin, 1995; McNeil, 1982; Mokyr, 1990; Pomeranz, 2000). Much of what was technologically significant about the European industrial revolution appears to have been present in China several hundred years earlier. Other evidence admittedly of an indirect

nature suggests the possibility of fast growth, especially in the Sung era spanning roughly the 10th to 12th centuries in the western calendar. And yet, by the 20th century, the Chinese were extremely poor by western standards. China appears to provide an example of an economy that came close to industrializing, and expanded very quickly for a time, but failed to cross the industrial threshold and subsequently receded into relative and absolute poverty. Depending on its interpretation, the example of China has the potential to undermine most understandings of the path to industrialization, although not necessarily the importance of industrialization itself.

The long-run decline or at least deceleration of the Chinese economy remains poorly understood. Much attention focuses on the 15th to 18th centuries in part because of the contrast with European acceleration during this period, but also because of evidence of intense commercialization and a market-responsive economy in China during this period. The most recent salvo in the debate is an argument that prior to 1800 China did not decline or at any rate did not fall behind Europe. Ken Pomeranz argues that, as late as 1800, parts of China equal in size to northwest Europe maintained a European-like standard of living (Pomeranz, 2000). Hanley (1997) argues likewise for Japan, while Parthasarathi (1998) appears to have evidence for India. Allen (2001) and van Zanden (2002) are only two of the many scholars now dedicated to a resolution of this issue. In a related argument Pomeranz suggests that China was no less likely than Britain to have crossed the threshold of industrial revolution. Britain did industrialize while China did not, according to Pomeranz, because of the geographical accident of British coal and iron being in close proximity to each other and the domestic repercussions of the extraordinary opportunities afforded by the Atlantic economy. On this view, the industrial revolution was no more likely (or no less improbable) for Britain than for China, if not for these serendipitous factors.

These arguments from Pomeranz revive a position formerly championed by Bairoch (1981) that as late as 1800 the gap between early and late industrializers was small or non-existent. Bairoch relied on a number of indicators, some of them rather indirect, to argue for relatively high labor productivity in developing countries and hence an income gap between first and third world on the eve of industrialization that was small to non-existent. The contrast, of course, is with the more familiar view of Europe slowly pulling ahead of the rest of the pre-industrial world between 1400 and 1800 and then leaping forward as a consequence of crossing the industrial threshold.

The significance of this debate is that if the West did not pull ahead in the pre-industrial era, then many accounts of the industrial revolution and the advent of modern economic growth will require revision.³ We also would need to rethink the origins of demographic transition and of global inequality. The traditional notion of a world-wide demographic transition following upon urbanization and income gains from industrialization is already at risk as we learn more about the diversity of experience in Europe, the importance of non-industrial technologies

³Ironically, one point in the traditional view that would remain unaffected is the significance of the shift to mineral-based energy sources for the industrial revolution and also the long-term acceleration of growth (Wrigley, 1988).

and social organization, and strikingly different Asian strategies to avoid Malthusian impasse (Easterlin, 1996; Lee and Wang, 1999; Livi-Bacci, 2001). The origin of global inequality is even more hotly contested. As Maddison points out, elimination of a western lead in 1800 opens the door to a return of explanations for the gap between rich and poor based on colonial exploitation. Whether or not scholars would return to the once-popular theories of dependency and imperialism is unclear (O'Brien 1982, 1988). Nevertheless, we might expect significant political implications if it becomes accepted that significant between-country inequalities were created, rather than simply being increased, during the period of European political/military hegemony and large international factor and commodity flows of the 19th century (O'Rourke, 2002).

The importance of these issues prompted a response to Bairoch 20 years ago (Maddison, 1983). This early attempt by Maddison to identify the income gap between early and late industrializers circa 1800 was weakly grounded and did not speak to the full range of evidence, but it did provide a basis for rejecting the Bairoch view of the world. It did not, however, persuade Bairoch (1993). Maddison's most recent publications (1995, 1998, 2001) provide revised evidence that is both more secure and more comprehensive. But the opposition also has regrouped and now provides fresh arguments and evidence. Contributions by Pomeranz (2000), Hanley (1997), Parthasarathi (1998) and others give the appearance of successive lightning strikes in a tinder-dry forest that threatens to erupt into an uncontrolled fire with the potential to change fundamentally the way in which we view global inequality.

At this point it is not possible to predict the outcome of the debate. In the next few years we will likely see considerable new evidence about pre-industrial income, growth, standard of living, consumption, life expectancy and stature in many parts of the world. Maddison will share in the credit for this avalanche of helpful evidence, whether or not it supports his arguments. Most of the estimates emerge in the shadow of his work, either because of attempting to prove him wrong or because Maddison's systematization of the evidence provide a historical framework in which individual country estimates have greater meaning and value.

These developments nevertheless challenge the Maddison research agenda in a new way. At this point in his career Maddison finds himself arguing hard for a particular perspective in a hotly contested debate. This is a very different enterprise than the dispassionate and critical compilation of income evidence. Attempting to combine both projects in a single publication runs the risk of confusing highly contestable evidence with more or less accepted income estimates, and diverts energy from the already large challenge of revising the 19th and 20th-century data. If there is any weakness in *The World Economy*, it arises from the "overstretch" of paying increased attention to the complicated debate about pre-industrial income just as the many new studies of modern incomes require his attention. The impressive scope of the "millennial" project is both its strength and its principal source of weakness.

If some may be tempted to dwell on the limitations of Maddison's work, it is worth reminding ourselves of its importance. A sense of long-term trajectory shapes most of our understanding of appropriate methods to improve standards of living and facilitate income growth. History matters, in the small and in the

large. Maddison's work is highly relevant to current debates surrounding globalization and the negotiations within the World Trade Organization. Most individual country negotiators, like the constituencies to which they respond, continue to believe that international inequality deteriorated but did not begin during the 19th century, that industrialization and structural change have been central to economic growth, that flourishing international activity and unfettered access to international opportunities have facilitated growth and that the evidence needed to assess policy impacts may be drawn from national accounts. Maddison's work is crucial to all these beliefs. The debate about globalization and the next round of WTO discussions may unfold very differently if Maddison loses his argument on any of these points.

KRIS INWOOD
Department of Economics
University of Guelph
 kinwood@uoguelph.ca

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