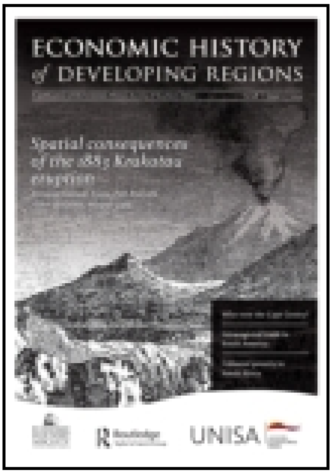

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ABSTRACT

The number of original articles published in Indian economic history shows a boom in the 1980s ending in a recession from the 1990s. The paper surveys the evolution of the field and explores the reasons behind these tendencies. It concludes that the trends reflect shifts in the popularity of archives-based research on economic issues among historians in India. Following on from a rise in the 1980s, in the last 20 years, cultural history crowded out economic history, and debates about the process of long-term economic change became rare. More recently, the link between comparative economic growth and Indian history has strengthened, leading to a modest revival.

Keywords: Economic history, British Empire, India, economic nationalism, Marxism

JEL classification: N01, B2, A2

The evolution of economics teaching and research in India "underlines the view that Economic Theory is rooted in Economic History". With these words the editor of the first-ever reader published in Indian economic history concluded the editorial introduction (Singh 1965b, 19). Economic history as a discipline started in the interwar period as a branch of economics, and the bond between them was especially close in the middle decades of the 20th century. Economists used history as a problem-solving tool, one needed to answer policy questions of the day. In the classroom, economic history usually formed one part of an all-inclusive 'Indian economics' course. Historiographical models were simplistic and derived from ideological frameworks. Few economic historians used the archives. To illustrate with reference to Singh (1965b), only two of the 32 contributors to the book, Hiroshi Fukazawa and Kshitimohan Mukerji, were to become known for

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their archive-based empirical work. The others came mainly from economics and wrote experimental pieces, if not amateurish ones.

The 1970s initiated two significant changes. In 1968 the first serious debate in the historiography of economic change in colonial India had taken place. The debate identified the major areas of disputation, such as the role of the state and the role of the 19th-century world economy in shaping growth, inequality, and welfare in India. Thereafter, almost every serious work in Indian economic history would position itself with reference to this debate (see "the first turning point" below). Second, within history, a variety of intellectual movements joined to make archival research on economic issues a mainstream pursuit. The focus of archival research was the peasant. Export of agricultural commodities greatly increased in the 19th century. Despite a vigorous "commercialization of agriculture" agricultural productivity remained low and stagnant. This was one of the key paradoxes to be explained. The result was an impressive growth of publications in the 1980s ("the second turning point"). In little over a decade, the boom ended and publications on Indian economic history started to fall (Figures 1 and 2, discussed below). The last few years have seen modest reverses led by environmental history, the divergence debate, and 18th-century studies. But overall the recession has deepened.

The present paper asks three questions. Why did the boom happen? Why was it followed by a retreat? Where is the discipline going? The answers develop over five sections – professionalization of economic history (1920–65), the historiographical turn (1963–68), growth of archival research (1970–90), the retreat (1990–), and new directions. The treatment is chronological, but the aim is to show how thematic and methodological concerns shifted between these phases.

ALLIANCE WITH ECONOMICS 1920–65

It is standard practice to begin a story of economic thinking in India with three publicists writing around 1900. They were Mahadev Govind Ranade (1842–1901), Dadabhai Naoroji (1825–1917), and Romesh Dutt (1848–1909). None of the three was a professional scholar, though Naoroji had taught in his early life. Ranade was a judge, Naoroji a merchant, and Dutt a civilian. All three of them were well educated and prolific writers in English. In addition, Ranade and Dutt wrote in their own mother tongues, Marathi and Bengali respectively. A significant part of their writings went to the advocacy of a reformed government for India, one that would be more welfare-minded (Chandra 1966; Dasgupta 1993; Adams 1971). The term 'nationalist' can be applied to these writers in that specific sense. While Ranade and Naoroji wrote on subjects of contemporary interest, Dutt ventured more fully into history.

Dutt, like the others, had seen acute famines in the last decade of the 19th century, and felt compelled to develop an analytical understanding of famines. He believed that the famines were a result of high taxation in the countryside. In his lifetime, agricultural commodity exports had grown, but the peasants did not gain from the process, he believed. In fact, by selling too much food abroad they compromised their own food security during famines. The tax system, the food export, and the decay of the handicraft industries as a result of textile imports from Britain became for him the foundations of a critique of British colonial rule in India. Naoroji's viewpoint was distinct from this, but he too held free trade responsible for Indian poverty. The main thrust of his analysis was that India paid too high a price for the services it imported from Britain, which became known as "drain". In later Marxist parlance, drain illustrated surplus extraction under colonial conditions. This was an indirect criticism of free trade because the services were paid for with a positive trade balance. Dutt for the first time used the words "Economic History of India" in the title of a book (Dutt 1902). His critique of British rule influenced popular history and textbook models of Indian history. But in the development of economic history as a specialist field, the influence was indirect. Dutt's book was a polemical piece. It was selective in its use of sources. His theory of famine causation was simplistic. The book is not likely to be read today as an example of the use of the historical method.

Still, the writings of Dutt and Naoroji did have an unintended consequence on the economics profession. The popularity of their writings alarmed the small number of university professors teaching economics. They created a forum to discuss the effects of trade on Indian economic growth. The Indian Economic Association, a body of professional economists with some representation from Indian industry, started its annual meetings from 1917. The initiative was taken by Charles Joseph Hamilton, Minto Professor of Economics at the Calcutta University. Hamilton was the first President of the Association and the author of a book on trade history published two years later (Hamilton 1919). The book, a history of the Indian Ocean trade leading up to the establishment of free trade as an official policy, was written to rebut a pessimistic view of India's trade with England. Later members and presidents of the association veered away from ideological battles. But the path set by Hamilton proved to be a lasting one. Economics research began as research in Economic History.

In the interwar period, a member of the Indian Civil Service strengthened the bond. William Moreland (1868–1938) published a series of books on Mughal India, with "economic history" in the title, possibly the second occasion that the phrase was used (Moreland 1923). Moreland set out a number of hypotheses and stylized facts about the state, market, and welfare in Mughal India. These became influential in subsequent scholarship on pre-colonial economic history, which progressed partly by refuting these theses. Another civilian, Malcolm Darling (1880–1969), President of the Association in 1928, and the Madras University professor Gilbert Slater (1864–1938), President in 1920, contributed indirectly to economic history scholarship. Darling wrote books on the Punjab peasant, containing details on agricultural practices, institutions, and land tenures, which economic historians of a later era have found useful (Darling 1934). His own motivation was a worry about informal credit markets and what he perceived to be a tendency of peasants to over-borrow in years of high prices. The rent and debt crisis that broke out in the wake of the Great Depression proved him prescient. Slater had diverse interests. He wrote books on English economic history well regarded in his time. He was one of the literary historians behind the notion that Shakespeare's plays were written by more than one person. In India, he is known as the pioneer of "village studies". Slater (1918) contained dense and sensitive description of material life in a group of south Indian villages. The book became a resource for economic historians by producing a sequel, Thomas and Ramakrishnan (1940). The Madras team initiated a method that was later absorbed both in historical research (Kessinger 1974) and in development economics (Bliss and Stern 1982).

Occasional contributions from government officers aside, the Hamiltonian agenda was mainly pursued by academic economists. In fact, the leading professional economists in the 1920s did not make a distinction between the two fields. In the interwar period, several university professorships in economics were held by individuals who had published historical works, or displayed a historical perspective in their writings on contemporary India. There were, however, specializations within the larger agenda. In the next 20 years, two axes of research emerged. One of these formed in a north-south direction and can be called the Lucknow-Madras axis. The main interest of this school was in the evolution of the Indian village. One of their questions was, why Indian agriculture was depressed, which it was throughout the early 20th century, and why land yield was on average so low in India. A second axis formed in an east-west direction and can be called the Bombay-Calcutta axis. The main interest of this school was in business history and monetary policy. Since monetary policy was closely linked to foreign trade, policy overlapped with the conduct of business. There were crossovers between these two axes, and by 1947, the two were well integrated.

In Madras, Slater's mantle was carried by his successor to the chair, P.J. Thomas (1895–1965). Thomas, with a young collaborator, published a widely cited article on economic depression in early 19th-century Madras Presidency in the *Economic History Review* (Thomas and Natarajan 1936). Thomas was also the architect of an official report called the *Fact-Finding Committee on Handlooms and Mills*, which remains a classic resource for textile historians today (India 1942). The report was particularly useful because it collected data on the long-term pattern of institutional-technological change in artisanal industry and consumption of cloth, and produced the first systematic time-series data of production and trade in textiles.

It was, however, Radhakamal Mukerjee (1889–1968) of Lucknow University who left the deepest impact on the profession, among other ways, by showing how history could be deployed for analysis of the rural economy. He published well over 40 books on a diverse range of subjects, from demography, to environment, planning, distribution of food, humanism, community and social psychology, comparative politics, the "institutional theory of economics", and labour history. The writings on environment were motivated by the belief that the middle

Gangetic plains, one of the world's oldest and most densely cultivated agrarian tracts, faced diminishing returns owing to demographic pressure, changes in river morphology, and over-exploitation of land with relatively static agricultural technology (Mukerjee 1926). His prescription was a revival of soil fertility by a reconfiguration of the biological inputs, in which process, he thought, Japanese historical experience would be especially useful. Thus, he almost predicted the green revolution that took shape within a few years after his death in 1968.

Some of his longer books were attempts to synthesize an economics treatise suitable for the student of the Indian economy or a student of economics in India. Of particular importance is *The Economic History of India 1600–1800* (Mukerjee 1967). The book was written to project his distinctive ideas of Indian economic history. For example, he made bold conjectures on the long-term standard of living, basing his conclusions on a series of real wage that contributors to the present-day divergence debate have made use of. The Global Prices and Incomes History website maintained by economic historians of University of California Davis reproduces the full contents of Mukerjee's wage and price series.

If the Madras-Lucknow school preoccupied itself with agriculture, economists based in centres of finance and industry, Bombay and Calcutta, wrote books on currency and banking. Jehangir Coyajee (1875–1943) of Presidency College, Calcutta, was a Bombay and Cambridge trained economist who wrote on fiscal and financial subjects. One of his early works was a history of the Indian financial system (Coyajee 1930). C.N. Vakil, a professor in Bombay, outlined in the preface to a three volume book series "the plan of an Economic History of Modern India, to be studied in three main divisions (1) Financial, (2) Industrial and Commercial and (3) Agricultural" (Vakil 1924; cited text from Vakil and Muranjan 1926, iii). His associate S.K. Muranjan of Dharwar wrote histories of Indian banking (Muranjan 1952), though the pioneering work on indigenous banking was a London University thesis by an economist who taught in Punjab and Allahabad (Jain 1929).

When India became independent in 1947, the two groups had partially converged. Economists suddenly found their services in great demand, and started travelling and circulating more than before. Some of the individuals who had been born at the turn of the century were established economists and still relatively young in 1947. Almost invariably, they moved into policy-making, advisory roles, and institution building, and moved from the regional centres to New Delhi. Leading figures in this cohort were the Madras University faculty P.S. Lokanathan (1894–1972), D.R. Gadgil (1901–71) of Pune, and V.K.R.V. Rao (1908–91). Lokanathan (1935) was a systematic account of the evolution of corporate organization in India. Rao, a Cambridge-trained economist, played a prominent role in the national income accounting systems, and produced estimates of income of British India (Rao 1962).

Gadgil, educated in Cambridge, was a policy advisor for half of his working life. Among his writings, there were several that were either historical or connected to history. Three of these were especially important, the first systematic accounts of Indian industrialization up to 1914 and of business history, and a short lecture text on women's participation in the workforce in the long run (Gadgil 1924, 1959, 1965). The business history work never appeared in print, limiting its circulation, but it still remains useful and not completely overtaken by later scholarship. One of the useful features of the work was an emphasis on trading firms, whereas much later work in business history tended to focus on the manufacturing firm. The work on women investigated the exit of women from the workforce when work shifted from the household to the workshop. In labour economics worldwide, this phenomenon was recognized as a key stylized fact. However, Gadgil's exploratory piece and the global scholarship never met.

The growing space of economic history within economics led to many attempts at writing textbooks. Almost all of them tried to connect contemporary India to an account of its past, in order to draw lessons on development from history. To a student of economics in 1947, five books, Wadia and Joshi (1926), Pillai (1925), Vakil (1924), Shah and Khambatta (1924), and Jathar and Beri (1930) would be familiar texts. Vakil and Wadia were Presidents of the Indian Economic Association. Wadia was a professor of history and Joshi an economist, of Wilson College, Bombay; Jathar and Beri taught at Khalsa and Sydenham Colleges, Bombay, respectively; Shah and Khambatta, like Vakil, were associated with the Bombay University. Pillai was an officer of the League of Nations and his book a London School of Economics thesis. These works, which were reprinted into several editions, established a few canons around which an understanding of the historical roots of Indian poverty then stood.

All of them advanced the idea that economics was a useful subject for India, and that knowledge of Indian history was essential to doing fruitful economics to the benefit of Indian development. All of them (except Pillai) also believed, with Dutt and Naoroji, that free trade and colonial taxation had retarded development. But another root cause of poverty was partly structural, low agricultural productivity. They advocated industrialization as a solution to low-yielding agriculture. Industrialization must be aided by tariffs, in order to overcome the obstacle of low labour productivity. Some of these works, Shah and Khambatta (1924) in particular, advocated socialism. Wadia and Joshi (1926) took a sociological line and advanced the further thesis that the Indian social order was crumbling away as an effect of economic modernization, and this process had some positive sides to it because the Indian caste system and anti-entrepreneurial attitude acted as obstacles to economic development.

British economic journals reviewed these works with interest, and the reviews were not all critical. However, an alternate text written by Vera Anstey (1889–1976), a teacher at the London School of Economics and the spouse of Percy Anstey, principal of the Sydenham College, Bombay, offered a more acceptable position to the international readers of Indian economic history. Anstey (1929) blamed Indian poverty on its backward social order, which had led, among other distortions, to high population growth and unenterprising and irrational attitudes. Anstey was acceptable in the Indian world too, for many Indian writers shared the belief that the social order posed an obstacle to growth.

By 1960, the Indian Economic Association was still the most important platform for academic economists in India. By and large, the economists took history seriously. Economic history was taught everywhere as one component in a course on "Indian Economics", and by a combination of technical research and the made-easy texts. Many of the names mentioned above served as Presidents of the Association. But more than a decade after independence, passionate ideological battles did not animate economic history any more. Increasingly, the leadership of the Association had passed on to a generation engaged too deeply in the mathematical foundations of planning to waste time on a soft subject like economic history. The historiographical models that attributed poverty to either free trade or the social order, being more ideological than informed, seemed to offer little stimulation.

On a more positive side, New Delhi had emerged as the new intellectual capital, with a relatively young team of theoretical economists and historians dominating the Delhi School of Economics. When in 1963, an American economist wrote a critical survey of Indian economic history, the article met with an unusually robust reception in New Delhi.

THE FIRST TURNING POINT

In 1963, Morris D. Morris, a faculty at the University of Washington, published an article in the *Journal of Economic History* (Morris 1963). Morris (1921–2011) was a member of that generation of post-war American social scientists who chose India as their main field of research. Some of them, including Morris, would contribute to developing the institutions that nurtured South Asian studies in the USA. Although Morris (1963) was an influential piece, it is useful to discuss his contribution with reference to the other publications that appeared between 1960 and 1983. This is so because Morris developed the key propositions on Indian economic history in several articles. There were three main propositions. First, the factors usually cited as obstacles to Indian development – Indian society and colonial policy – were overrated. Second, 19th-century trade did not have a negative effect on the Indian economy. And third, counterfactual history that asked why India did not industrialize faster than it actually did, and answered the question with reference to imagined obstacles that came in the way, asked the wrong question.

Consider the sociological argument first. In the decade before writing the economic history piece, Morris had completed a fieldwork in India on the history of the factory workers of Bombay. This work would challenge a view then popular among the American "modernization" theorists that factory workers in India were insufficiently committed to industrial work. The argument implied that the spread of industrial capitalism in India was constrained by a shortage of efficient and committed workers. It ran parallel to another popular idea originally stated by Max Weber that Indian, rather "Hindu" entrepreneurs tended to be risk-averse and against innovation. The worldview that economic growth met with cultural obstacles in India, was shared by many economists writing on India, as we have seen. Morris (1960, 1965) used two grounds to reject this view: we cannot be sure, nor can we prove, that Indian culture obstructed anything, and the historian ought to show why industrialization happened at all in India, rather than why it did not happen in a certain way or on a bigger scale. Morris noted that in Bombay, the growth of a textile labour force had been accompanied by relatively stable real wage for a considerable length of time, suggesting that labour shortage was not a problem. Many of the so-called symptoms of lack of commitment, he argued, could be understood with reference to managerial policy in response to the prevailing factor-price-ratios (for a restatement of these positions on the role of culture and the role of market, see Wolcott 1994, Broadberry and Gupta 2009; and Gupta 2011). He concluded that the challenge of recruitment, training, and disciplining an industrial labour force was relatively easily overcome in every case of early industrialization, and India did not differ from the more familiar examples in any essential way.

In two follow-up articles, Morris (1967, 1979) took on entrepreneurship, arguing that too much attention to cultural norms or the colonial state involved an inability to appreciate the constrained choices made by private businesses. It is futile to speculate what might have happened to enterprise had there been a different state or a different culture. It is more important to analyse what actually happened. By 1914, the fourth largest cotton textile mill industry in the world was "almost entirely Indian financed and managed" (Morris 1967, 598). India was unique in the tropical world in experiencing a robust industrialization. Port cities with some of Asia's best business infrastructures emerged in Bombay, Calcutta and Madras in the 19th century. The "fascinating" aspect of the industrialization was that Indian capitalists "poured capital into the very sector where it would seem that competition with Britain was the sharpest". Their decision owed to a "rational responsiveness to available technology and factor-price relationships" (Morris 1967, 599).

Consider colonialism next. Morris (1963) acknowledged that the British Empire did not have Indian development as its aim, but he did not place much weight on the fact. The British Indian state was one of the poorest states in the contemporary world (its tax-GDP ratio was exceedingly small in the 19th century not only in relation to Britain but also in relation to most tropical colonies). It could not possibly become either an exploitative state, because there was little to be gained by exploiting the already poor Indians, or a developmental state, because it had little money left over after defence and administration to spend on development. The state, instead, saw itself as a "night-watchman", a provider of defence, law, and justice, while trying to promote market integration within the empire.

Did market integration work? Or was foreign trade damaging to the economy? Pursuing this question, Morris rejected the notion that the import of manufactures from Britain destroyed Indian handicrafts. "Deindustrialization" is an overstatement, for more than 10 million artisans continued in business in 1950.

He speculated that the positive changes introduced in British India in fact strengthened the artisan sector.

Morris' methodological point was a stand against counterfactual history, and against a tendency of the nationalist imagination to believe that if only the Europeans had not ruled, development would automatically follow. Analytical work on Indian economic history has tried to answer the wrong question. Could India develop faster than it actually did? The question led historians to a search for obstacles like colonialism and culture. We should ask instead, if colonialism or Indian culture was so powerful and obstructive, why did commercialization and industrialization happen at all? Either the Empire was not as powerful as we thought, or it followed a different logic. A plausible view is that the British Empire influenced economic development not only as a form of government but also as an agent of market integration on a world scale, and Indian merchants and mill-owners profited from that fact as long as the world economy was healthy.

Morris (1963) was a tentative and incomplete statement of these propositions. In 1968, the article was reprinted in the *Indian Economic and Social History Review*, a journal newly launched by economists and historians based in the Delhi School of Economics, together with three assessments by Bipan Chandra, Toru Matsui, and Tapan Raychaudhuri (see Morris et al 1969). For more than one generation of students in India, the symposium was mandatory reading, indeed a point of entry into the subject.

The impact of the debate was not wholly salutary. It led to a sharp polarization of opinion on colonialism. Two camps emerged. One was represented by the Marxist-cum-nationalist historians who held that the Empire was the most important agent of economic change and that it was a malign agent. The Empire impoverished India by persisting with free trade as well as by extracting and transferring surplus value. The opposite of this view, such as Morris' critique, has often been confused with a "neocolonial" or pro-Empire position. It was, in fact, something quite different. A real imperialist view, represented in the work of the administrators John and Richard Strachey (1882) for example, would claim that the scale of improvement introduced by the British in India was staggeringly large. The non-Marxist view neither made an assertion about scale, nor denied that the raj usually worked for British and not Indian interests. In Morris' words,

> British economic policy in India could not rise above the ideological and policy level at work at home. And a general economic policy appropriate for Britain at the peak of her economic power was not adequate to provide for long-run growth in India. (Morris 1963, 615)

But then the government was too small for this factor to matter very much. State power was hardly capable of directly causing either large-scale improvement or large-scale regress. And the raj mattered not only as a government but also as an agent in market integration. An almost contemporary scholarship that read the British Empire as an embodiment of private business interests strengthened the second camp (on this scholarship, Louis 1976). In short, criticizing the Marxist-nationalist paradigm. Did not amount to batting for colonialism. Rather, it signified a belief that the Empire was a many-sided thing, and therefore, signified a willingness to appraise rival interpretations of the changes that the Empire had made possible.

The *Cambridge Economic History of India* volume 2, edited by Dharma Kumar (1983), the leading representative of the non-Marxist camp, was a product of the second perspective. The volume did not carry an editorial introduction, a decision that suggested deliberate refusal to enter debates about the Empire. Predictably, it received fierce criticism from the Marxists. Morris continued to be cited for contributions to research in specific fields. But perhaps increasingly, he was cited as the representative figure of a dangerous intellectual tendency. The tone was set by the three critics of 1968, Chandra, Raychaudhuri, and Matsui. All three saw Morris to be trying to defend colonialism, sometimes quoted his words out of context, and by these means, turned an intellectual debate into a moral one. It is hard to argue with sentiment, and therefore, the charge against him stuck. Twenty years later, a Marxist historian would again accuse Morris (1983) of trying "to exonerate the colonial regime from any culpability throughout" (Habib 1985, 380). The moral nature of the discourse clouded the question Morris had raised: why did the tropical world's most impressive industrialization occur in India, notwithstanding an indifferent state, high capital cost, small fiscal resources, and low agricultural productivity?

A second effect of the debate was a more positive one. It gave urgency to fundamental statistical questions. Was there economic growth? Was there increasing inequality? Was there a decline of the handicrafts? Already ongoing statistical work on output, income, and labour force found a meaning in the context of the debate (major works in these areas are Blyn 1962; Sivasubramonian 2000, originally part-published in 1965; Maddison 1971; Heston 1983; Thorner 1962; M. Mukherjee 1965; Krishnamurty 1972). The works on agricultural output and income established what must be regarded as the one stylized fact that explains Indian poverty most directly: extremely low, stagnant, sometimes falling agricultural yield.

The 1968 symposium was not the main influence behind the cluster of new research to appear in the 1970s and the 1980s, even though nearly every new publication would cite the symposium. The surge owed to two other sources – radical politics and expansion in source-based research.

THE SECOND TURNING POINT

Global Marxism and the rise of dependency theory introduced three key changes in the relationship between history as a discipline and the theoretical understanding of Indian development. First, the attack on American modernization theory supplied an analytical support to the nationalist criticism of the British Empire. Indian nationalists like Dutt or Naoroji were no longer isolated critics of the British Empire in India, they were precursors to a global radical and revolutionary voice. Secondly, the division of the world into a centre and a periphery helped the emergence of a peripheral version of imperialism, whereas imperial history until then had been mainly written from the perspective of the centre. Thirdly, revolutions in China and Cuba strengthened a hope that socialist revolution was possible without a "bourgeois revolution", whereas the world of the industrial bourgeoisie, being an imperialist creation, carried little promise for social change (for a general discussion on the influence of dependency on history, see So 2010). A direct effect of the new radicalism, therefore, was that the focus of historical research fell on the countryside.

The blend of Marxist-Leninist theory, world history, and Indian evidence is best represented by the "mode of production in Indian agriculture" debate (see essays in Patnaik 1990). Historians wanted to know how class formation had occurred in the countryside during the colonial era. They asked if it was the world market or the colonial state that shaped the conditions of the peasant more deeply during 1850–1950, and how these forces were interrelated. The drive to make "commercialization of agriculture" the central theme came also from a theoretical model of agricultural markets that foregrounded credit and property relations (the original statement by Amit Bhaduri is reprinted in Bhaduri 1999). The adoption of Marxist tools was by no means universal, and often encouraged search for alternatives. Karl Polanyi's "great transformation" cast its shadow (Guha 1985). Alexander Chayanov's theory of the peasant household and the Annales School were cited, but did not inspire major applications. The nationalist version of imperial history was challenged by the Cambridge School, broadly identified with the view that local and indigenous powers shaped how the colonial state actually worked and how effective it was. Eric Stokes, a Cambridge professor and the author of a book on the north Indian peasantry during the mutiny was influential among many historians (Stokes 1986).

It would be a mistake to believe that the radical movement alone encouraged the new research output. If anything, the quality of source-based research in some of the radical literatures was not very high, the mode of production literature being a good example. Independently of the radical movement, archival research by historians was beginning to take off. Radicalism created a demand for archival research, the supply increased thanks to the closer integration of history teaching in India with the British and continental tradition of source-based history. The radical agenda, the Morris critique, and the Cambridge School – all of these debates on historiography created an international audience for source-based history and thus indirectly helped that tradition.

In the 1960s, the history faculty of leading British universities produced a cluster of doctoral research on India, which were directly about economic history or had relevance for the field. Some of these graduates went on to teach Indian history in Britain and North America, others returned to India to teach. A few had done their graduate studies in the USA. A group of scholars emerged from Japan. And an exceptional set of pioneering researchers had been trained in India, notably Binay Chaudhuri, K. Mukerji, V.D. Divekar, and Amalendu Guha.

In turn, the Indian scholars were the products of an earlier, again exceptional, engagement of historians with economic history, illustrated by D.D. Kosambi, A. Sarada Raju, and N.K. Sinha. The 1960s cohort published on agricultural markets and agrarian relations (B.B. Chaudhuri 1964; Kumar 1965; Habib 1963; Whitcombe 1972; Fukazawa 1965); trade, trade policy and balance of payments (Harnetty 1972; Banerji 1963), government finance (Thavaraj 1962; Mukerji 1975); business history (Rungta 1970; Divekar 1978; Guha 1970), industrialization (Bagchi 1970); and the ideological foundations of economic policy (Ambirajan 1978). Several other relevant names not cited in this list touched on economic themes in publications mainly dealing with political and social history.

A particular mention must be made of Indian Ocean studies. The historiography of the Indian Ocean trade began with a series of books on the Portuguese Estado da India, and the Dutch and the English East India companies (Furber 1948; Boxer 1969; Raychaudhuri 1962). The main focus of this set was Europe-Asia trade. The works of a later cohort of scholars, especially K.N. Chaudhuri (1978), Dasgupta (1970), and S. Chaudhuri (1975), shed more light on Asian merchants and on the divisions and segments within the vast geographical area that the Indian Ocean represented. With this shift of attention from the foreign to the indigenous elements, more research began to be done on commodities, regions, and actors who were not directly linked to Europe-Asia trade. A major area of focus was the South Asian merchant or merchant group, the effect of trade on regional economies, and the European private enterprise engaged in intra-Asian trade.

Comparatively few publications appeared in quantitative economic history during this phase, with the exception of a few studies done in the USA on the railways (Hurd 1975; McAlpin 1974). Partly, the dearth reflected the scarcity of statistical source material. But there was another factor behind this tendency. The major part of fresh research was done by scholars who came from history, rarely anthropology. Their audience consisted mainly of historians. "Positivist" economic history did not interest them as much as debates about the state, markets, and institutional relationships. As a more recent scholarship illustrates, large statistical databases remained unutilized for a long time because of this bias.

In the 1980s, a much larger number of Indian students of history travelled offshore to do doctorates in economic history. Indian history was an expanding field in North America and Australia. Britain was the most preferred destination, but a significant number took their doctorate in India. A major expansion had taken place in the facility for doctoral research within India under new funding schemes that saw the establishment of a string of research institutes, scholarships, and many history jobs in colleges and universities. The Oxford University Press in India captured the top end of the monographic output coming out from Britain and India, and expanded the number of titles. The *Indian Economic and Social History Review* had established itself as an international journal. The *Economic and Political Weekly* occasionally published original research in history, and published many more interpretive pieces. In 1985, a second journal started in

Delhi, *Studies in History*. Area studies journals in Britain and North America were ready to print articles on Indian economic history. The publication of the two-volume *Cambridge Economic History of India* was recognition of the growing prestige of the field within history scholarship and in turn, a huge impetus to new work, funding, seminars, and conferences (Habib and Raychaudhuri 1982; Kumar 1983). It was easy to publish and easy to find a teaching job in India by doing economic history.

Agrarian history was by far the largest field of publication (see, for example, Guha 1985; Ludden 1985; Baker 1984; Charlesworth 1985; and essays collected in Bose 1994; Ludden 1994; Raj et al. 1985; and Stein 1992). Trade and business history produced important studies of business communities (Timberg 1978; Mahadevan 1978; Tripathi 1981) and a further crop of Indian Ocean studies (Prakash 1985; Arasaratnam 1986, and Subrahmanyam 1990). Colonial monetary policy was studied by Tomlinson (1982), and industrialization by Ray (1982). New conflicts over policy and business practices in the interwar period were subjects of Markovits (1985) and Tomlinson (1981). Population, famine, and migration formed another large area (Klein, 1984). The link between food export and famine, and in turn between trade policy and welfare, was studied by Sen (1981) and Ravallion (1987). In this phase, serious comparative history was surprisingly rare, if Marxist-Leninist history is excluded. Tomlinson (1985) was a notable exception.

In the 1990s and beyond, new work came to concentrate on a narrower set of themes. The publication of Bayly (1983) and the subsequent debates on 18th-century trends inspired a cluster of works on the 18th century (see Chatterjee 1996; Datta 2000; Hatekar 2003; Marshall 2003; Alavi 2002). Merchant diaspora were studied by Markovits (2000) and Levi (2002), and the artisan experience by Roy (1999) and Haynes (2012). The crumbling of the global economy in the interwar years provides the context for new works on the Great Depression (Rothermund 1996), monetary policy (Balachandran 1996), and business cartels (Gupta 2005). Famine demography was studied by Dyson (1991) and Maharatna (1999). Important Japanese language scholarship appeared in English (Yanagisawa 1996). In two other fields – labour and environmental history – publications grew, but much of this research was contextualized within colonial politics rather than economic history.

Overall, however, publication activity had started falling in the 1990s.

THE FALL

The fact of a decline can be established easily. The two figures below chart articles published in economic history in four major "area studies" journals with a significant interest in Indian economic history (*Modern Asian Studies* from Cambridge University Press, *Journal of the Economic and Social History of the Orient* from Brill, *Indian Economic and Social History Review* and *Studies in*

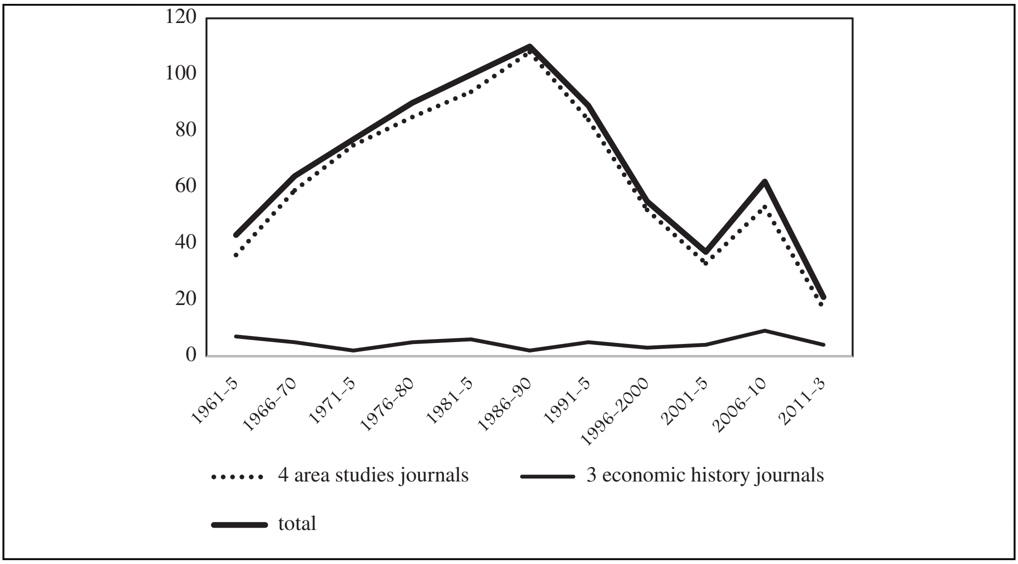

Figure 1: Publications on Indian economic history in leading journals 1961–2013

History, both from Sage), and three major economic history journals (*Journal of Economic History*, *Economic History Review*, and *Explorations in Economic History*). I track two indices – articles published on Indian economic history (Figure 1), and articles on Indian economic history published by scholars based in India (Figure 2).

In choosing an economic history piece we need to exercise judgment. The very best articles in an area studies journal would not be ordinarily refereed in an

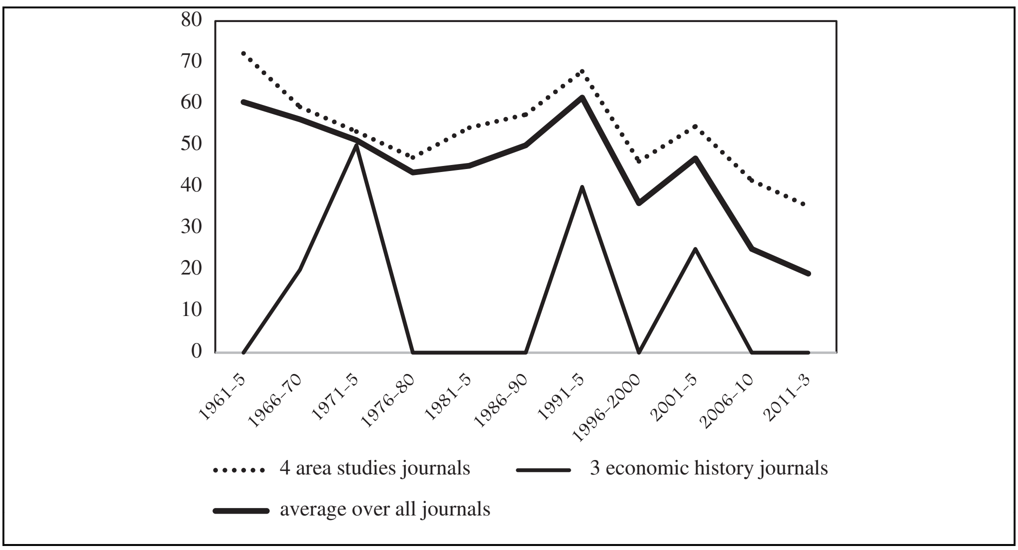

Figure 2: Publications on Indian economic history originating in India (%) 1961–2013

economic history journal, and vice versa. In choosing an article I do not ask, "would this article be publishable in an economic history journal?" Instead, the benchmark is, "is this article likely to be cited in an economic history journal piece on the same broad subject?" A few other specific rules followed are: (a) the coverage is South Asia if the article is mainly about the colonial times, and the Indian Union if the article stretches into the postcolonial times, (b) book reviews and obituaries are ignored, but review articles and long commentaries are included, (c) affiliation of authors only at the time of publication taken, and perhaps most important, (d) subjects indirectly related to economic history are included (for example, agrarian revolts, social history of business communities, labour history, railway history, and history of economic thought). In short, I use a flexible definition of economic history, in acknowledgment of the contributions made by both historians and economists to the development of the field from 1965 to 1995.

The result shows how bleak the post-1990 years were. Number of publications suffered a sharp fall, slightly relieved by a cluster of works in environmental history in the mid-2000s. The fall characterized mainly research published in the area journals. On the other hand, research published in the leading economic history journals did not see a fall. But then these papers never formed the main body of Indian economic history. There was a parallel fall in research emerging from India. The proportion went from 60% in 1961–65 to 19 in 2011–13.

This last finding suggests that the cause of the attrition can be located *only* within the Indian academia. The decline cannot be attributed to an unpopularity of economic history worldwide. In fact, the economic history journals today publish more articles per issue than before. Some of them have increased their frequency. Along with the three listed in the table, the time covered by this article saw the start of *Cliometrica*, the *European Review of Economic History*, the *Australian Economic History Revie*w, and the *Economic History of Developing Regions*. The trend cannot be attributed to declining popularity of economic history in area studies journals. Economic history has indeed retreated as a branch of the area studies, but the lesson from Figures 1 and 2 is a different one. The lesson is that India, as a subject and as origin, retreated *within economic history published by the area studies journals*. Nor can we say that the fall in the share of Indian-origin publications reflects a globalization of economic history research. This would be the case if the total number of publication was rising, but it is not. The mantle of research has not passed on to other centres. The decline of Indian participation has caused a decline of the whole field.

Other than one essay that attracted a few responses (Roy 2004; Frank 2004; Sugihara 2004; Bin Wong 2004; Pomeranz 2004; Hatekar 2004; Rammohan 2005), this issue has not been discussed in print. Later surveys of the field by A. Mukherjee (2008) and Bhattacharya (2011) do not take notice of the decline. Banerji (2005) doubts it. Parthasarathi (2012) uses the word "decline" several times, but in a different sense. A few conference panels – one of them titled "Does the economic history of South Asia have a future?" (Dartmouth College 2004) – discuss the issue, but in a half-hearted way.

The decline owed to two factors. First, historians lost interest in the kind of source-based research that they had been doing on the peasants. The retreat reflected the fact that the link between global Marxism and economic history was becoming weaker than before (Parthasarathi 2012). But there was more to the tendency. Agrarian history scholarship moved away from commercialization towards protest and resistance, best embodied in the "subaltern studies" school. Initially a small club started to revise the narrative of nationalist movement in India, the school received international attention in the 1980s with the emergence of resistance studies from elsewhere in the world, one of the more popular being the work of James Scott of Yale University. Like the Marxists, the subalterns explored struggle and displayed a keen interest in the prospect of a revolution or the absence thereof, but unlike the Marxists, they avoided explicit reference to the problematical category of class. By 1990, the best years of the movement were over, but it reinvented itself by allying with the postcolonial cultural studies. With the rise of postmodernism and its offspring, postcolonialism, history of the colonial territories turned into an investigation into the manner in which power penetrated the lives of people. Power detached itself from property relations and made knowledge its new habitat. Historians and anthropologists repositioned their worldviews from political economy to the cultural domination of colonial subjects by the colonial government. The new faith was historical. But there was no place in it for markets and institutions. Archival records were not usually trusted because they were produced by state officers. The American anthropologist Bernard Cohn (1996) led the attack on the colonial sources on which the earlier efflorescence of economic history had depended so much.

The second problem afflicted India in particular. Even as the world moved away from Marxist and patriotic historiographies, in India these intellectual traditions grew deeper roots. Today, the accepted position on colonialism in India is the one set out in interpretive works published by Marxist scholars in the 1970s and the 1980s (Habib 1975; Bagchi 1976, 1983). The mainstream position draws on Indian nationalist texts, global Marxism of the 1970s, and the World Systems School. All of these views believe in some form of the idea that free trade harmed India, that British colonialism extracted surplus value from India and transferred it to Britain, and that in order to carry out this activity colonialism needed collaborators, thus increasing inequality. In the last 10 years, several scholars have reiterated this position (Banerji 2005; A. Mukherjee 2008; Bagchi 2010; Bhattacharya 2011). At the same time, they also criticize revisionism by the "Cambridge School" and by Roy (2004). The criticism is unusually fierce in tone, calling revisionism "bad economic history and even worse political history", "return of the colonial" (A. Mukherjee 2008, 18), "far-fetched" (Banerji 2005, 2976), and "neocolonial" (Bagchi 2010, i–iii). None of these scholars states clearly what the revisionists actually say and why they say it. The tone is dismissive, not only of the revision, but also of the responses that one revisionist statement received from four leading global historians ("four foreign economists", as Banerji [2005, 2975] called Frank, Bin Wong, Sugihara and Pomeranz).

I hope to clear the air by contrasting the two positions in a balanced way. The intention is to show why big debates on Indian economic history are possible at all. The thesis that colonialism caused underdevelopment is proven with reference to five propositions that are not controversial: (a) GDP growth rate was small in the 19th century and near-zero in the early 20th century; (b) agricultural productivity was low and stagnant; (c) famines destroyed lives on a large scale between 1876 and 1898; (d) the colonial state denied Indian industry protection until 1925; and (e) the state made too little developmental expenditure. It is concluded on the basis of these facts that the British Empire in India was both indifferent and exploitative in relation to India, and that is why India failed to develop. Post-independence development policy was inspired by this reading of the past. GDP growth rate was raised sharply by a large increase in government investment. Protection was not only ensured, but strengthened to an unusual degree by nontariff barriers. Commodity export was discouraged and import-substitution encouraged. Food security became a goal, and led to the "green revolution" and near-total state control over grain trade.

The revisionist position is that the above reading of the facts overstates as well as misreads colonial agency. Consider Morris' question: if colonialism was so powerful and so negative a force, why did the tropical world's most impressive industrialization take shape in colonial India? The answer must lie in the cosmopolitan capitalism that the first global economy spawned in the Indian port cities. Roy (2002, 2004, 2007, 2012b) joins revisionism in a quite different way. Morris was not particularly convincing on the peasant. But low agricultural productivity was the fundamental proximate cause of underdevelopment in India. In the past, low land yield was not only the root of poverty, stagnation, and small tax receipts, but also of famines. Low-yield agricultural regimes did not occur only in India. They appeared throughout the tropical world, because of poor soil, and even more, because of precarious water supply. Marxists and nationalists assume rather than establish that the syndrome was created by the colonial state. By explaining these features as faults of the colonial state historical analysis glosses over one of the most difficult and enduring challenges of development in the tropical world and misses the chance of telling a fruitful environmental history.

The revisionism can lead to a critique of the post-independence development strategy as well. It is true that GDP growth rate was much higher after 1950 than before. But then the post-independence development strategy needlessly destroyed the most important positive legacy from the colonial era, a vibrant and international capitalism functioning in its ports. Furthermore, whereas import-substituting industrialization and the green revolution were the two most important factors behind the higher GDP growth, the growth delivered by both these factors was unequal and costly. It was unequal because the particular recipe for the success of green revolution could not work in the dry-lands and uplands. And it was costly because both green revolution and import-substituting industrialization were sustained by the tax-payer's money. Economic growth in 1950–85 involved a redistribution of taxes from public goods to private goods (subsidies to farmers, state trading of food, and state enterprises). The policy involved distorting financial markets, crowding out, and a continued neglect of infrastructure. Without money flowing in from a resurgent private enterprise after the economic reforms (around 1990), this policy would very likely have led the government to bankruptcy and the economy to macroeconomic collapse. In a manner, post-colonial India saved itself by returning to the colonial model of open economy and cosmopolitan capitalism.

Although serious debates on these alternative historiographical models have more or less ceased, a few new questions have emerged in the last 10 years.

NEW DIRECTIONS

The motivation to seek new research questions in the recent years comes from two sources. First, as long as Indian economists advocated a closed economy (c. 1950–90), the Marxist-nationalist critique of the colonial-era open economy served as an ideological guide to economic policy; after the liberalization of the 1990s and opening up of the economy to world capitalism, it lost that role. The opposition to openness, especially to foreign investment, is still powerful, and economic nationalism has not lost its appeal. But the willingness to acknowledge the gains from 19th-century globalization is growing in an era when India has cautiously re-embraced globalization. The second factor is that among economists working outside India, there is a desire to integrate India with current discourses on comparative economic growth. Themes such as institutions, public goods, worker efficiency, and divergence achieve the integration in different ways.

There is growing interest among economists in the foundations of the British Empire in South Asia, which was traditionally a subject in which narrative history ruled. Hejeebu (2005) rethinks the incentive structure involved in the operations of the British East India Company in India. Others have explored the process of transformation of maritime merchants into sovereigns in India in the late 18th century. Roy (2013) draws attention to the fiscal foundations of the military enterprise. Oak and Swamy (2012) analyse the strategic mix between conflict and collaboration, and attribute the Company's battlefield successes partly to the credibility of its commitments to military alliances.

One of the stylized facts that never received serious attention earlier was the persistently low productivity in all forms of work in India. Wolcott and Clark (1999) foreground efficiency and work ethic in a discussion on comparative economic growth. In an earlier application of the idea, Wolcott (1994) suggests that the Indian cotton mill workers had a cultural norm of low effort. This is reflected, for instance, in high worker-to-machine ratios in textiles. The evidence is viewed differently by Gupta (2011), who sees high worker/machine ratios as the profit maximizing response to the low cost of Indian labour.

That the British Indian state chose to play a limited role in economic development is well known. But the public goods and infrastructure that it did produce were also distributed unequally between regions. A set of recent papers explore the reasons, and emphasize institutions, local autonomy, and fiscal resources (Banerjee and Iyer 2005, 2013; Kapur and Kim 2006; Iyer 2010; Banerjee, Iyer, and Somanathan 2011; Iversen, Palmer-Jones, and Sen 2013; Chaudhary 2010; see also an innovative exercise on measurement of regional inequality, Caruana-Galizia 2013). The major field of public-private investment in British India – railways – is the subject of fresh research. Donaldson (2010) revisits welfare effects, and Bogart and Chaudhary (2012) reassess management after state takeover (see Hurd 1983, for an earlier estimate of welfare effects). A broader notion of efficiency as tradability is explored in papers on market integration, with reference to agricultural labour (Collins 1999), grain markets (Studer 2008), and post-railway markets (Andrabi and Kuehlwein 2010). The need to develop a nuanced understanding of regulation, by the state and by economic actors, leads to exploration of contracts and law (Kranton and Swamy 1998, 2008; Roy 2011).

By asking the question, *when* India or China started falling behind Western Europe, the divergence debate popularized the measurement of comparative living standard and pushed the date of meaningful comparison further back in time than conventional economic history has been used to. The major contribution, Pomeranz (2000), studies early modern China and shows that around 1800 China was like Western Europe in many respects. A similar point was earlier advanced by Parthasarathi (1999) with Indian data. These works, as well as independent advances in the use of anthropometric data, encouraged research on the measurement of living standards in the long run. The material using Indian data can be divided into four clusters – real wage (Allen 2007; Broadberry and Gupta 2006), heights (Guntupalli and Baten 2006; the use of Indian anthropometric data was initiated by Brennan, McDonald, and Shlomowitz 1997), interpretation of trends in prices (Clingingsmith and Williamson 2008), and income and consumption (Roy 2011, 2012a).

Among older subjects, the one that continues to flourish is the study of Indian Ocean trade. Dynamism in this area is a result of a shift of attention away from trade networks to merchant networks and the regional economy, discussed earlier (for example, Parthasarathi 2001). Along with that shift, historians have also moved towards new geographical frontiers. One of these is the Afro-Asian commercial world that predated and remained relatively autonomous of Eurasian trade until the 18th century. The Afro-Asian sphere is an important finding in itself, and has spawned attempts to question and contextualize the significance of the entry of European traders in the Indian Ocean c. 1500 (see essays in Prakash 2012).

CONCLUSION

This brief survey of the field paints a contradictory picture. A pessimist would note how source-based research has fallen since the mid-1990s, and how serious debate within India on the long-term process of economic change ended. I have suggested that on three large questions, debates ended without conclusion, and can be restarted. The questions are the following ones. What was the Empire in relation to economic history, a state or an agent of globalization? How powerful was the state in relation to economic forces? Was low productivity in India cultural, political, or environmental in origin?

On an optimistic note, quantitative economic history of India has maintained a steady, if small, presence in the long run. More promisingly, economists are making a serious effort to include India in the discourse on comparative economic growth. This is a positive trend, not only for the new theoretical and statistical tools that these writings have introduced, but also because comparisons have been rare in the field so far. Some strands of Marxism aside, economic history of India has tended to be self-absorbed and insular. The emerging discipline of global history is as good as invisible in history research or teaching in India. On fundamental issues, such as the historical roots of persistently low agricultural productivity, South Asia can be fruitfully compared with other world regions. There is increasing awareness of that possibility in the international scholarship on India, though the new turn does not yet resemble a coherent movement.

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