INTRODUCTION

The Study of Wages in India 1500–1900

At first sight, the study of wage levels and the purchasing power of wages looks like a specialized and academic topic of little concern to the general public. As this book will demonstrate, however, this is not the case. On the contrary, the study of wages opens up vistas of the daily lives of the working people, their standard of living, as well as economic developments at large. For that reason, wage levels not only take an important place in social and labour history but also in economic and world history.

In social history, the way wage labourers are remunerated is an indicator of their well-being; in labour history, disparities between effort and remuneration are indicators of unequal power relations. In economic history, real wage levels (i.e., the purchasing power of wages) are among the main components of national income (Broadberry et al., 2015; Drèze & Sen, 2013; Leeuwen, 2007). In global history, the comparison between the performance of countries, which includes their national incomes, takes centre stage, particularly in the so-called Great Divergence Debate, as we will see below.

Obviously, this also goes for India. Indeed, currently, it may be more relevant to India than to any other country. Why such a bold statement? First, because the virtues and flaws of the different great eras in Indian history (the Mughal, the successor states, the colonial and the independent ones) are a hotly debated subject. Second, the same is also true for the virtues and flaws of social inequality, where the traditional Indian hierarchical society model is juxtaposed with modern egalitarian thinking along the lines of

the Declaration of Universal Human Rights. Third, India is one of the major civilizations in world history—next to especially Chinese and European culture (without disparaging others)—and the performance of Indian civilization with respect to economy, society and culture is crucial for any historical comparison on a global scale.

We cannot, and will not, address all of these big issues, but we will try to demonstrate how the knowledge of wages in the past is a crucial building block for starting to understand them. We will do so in this Introduction by addressing the following matters: the Great Divergence debate; the place of wage labour in Indian society over the last 500 years; the opportunities and pitfalls of the historical study of wages in India and the contribution of the chapters in this volume to an advanced understanding of these phenomena.

I.1. INDIA IN THE GREAT DIVERGENCE DEBATE

European political and economic domination over the Americas, Africa and major parts of Asia in recent centuries, and its consequences, which still reverberate today, raises questions about whether this arose due to a specific coincidence of circumstances, or whether there were deep-rooted inequalities between the major centres of civilization around the globe (Lucassen, 2021; Pomeranz, 2000). Of course, the victors, including the Spaniards, the English and the French, became convinced that their civilization was superior in many, if not all, respects. They defined themselves as the inheritors of the cultural achievements of the Greeks and the Romans and the morality of Christianity, culminating in the Renaissance, Enlightenment and the Industrial Revolution. No wonder that commercial entrepreneurship was then combined with a missionary zeal to convert their overseas subjects to Christianity, most of all in the Americas.[1]

This classification of civilizations as inherently successful or failing was already being questioned, albeit initially only by a few, in the heyday of colonialism and imperialism, but this critique gained force in the late 20th century. On the one hand, it was inspired by the disasters caused by the internecine world wars, which undoubtedly had to be blamed on European nations and resulted in the Universal Declaration of Human Rights (1948). On the other hand, decolonization movements became a catalyst and

also triggered academic research into the root causes of especially economic inequality between 'the West and the rest'. Is this to be blamed solely and entirely on the long-term consequences of colonial exploitation? To a certain extent it is, and many historians, including Immanuel Wallerstein (1974–1989; cf. Pomeranz, 2000, pp. 14–15), have expanded on this. However, the longer the distance between the present and the date of decolonization, the stronger the curiosity for additional and alternative explanations. Crucial for any historical assessment, of course, is the precise dating of unequal development: have the different parts of Eurasia been divergent since a time before European overseas expansion, or did this happen later? And, if so, did these two processes coincide exactly, partially, or not at all?

These questions have been raised in an explicit and very precise way by American historian Kenneth Pomeranz and his colleagues of what has been called the 'California School'. In a path-breaking study that inaugurated the second millennium of our era, Pomeranz compares the economic achievements of advanced parts of Europe and China in especially the 18th and 19th centuries by stating:

> differences in climate, soil, etc. might have given different areas different preindustrial possibilities. But it seems unlikely that Europe enjoyed a substantial edge in those possibilities over all densely settled regions, particularly since [...] it did not in fact become much better-off than East Asia until industrialization was well under way. (Pomeranz, 2000, p. 9)

Leaving Pomeranz's arguments and the extensive discussion they triggered in relation to China aside for now,[2] we instead focus on his suggestion that other 'densely settled' parts of Eurasia may also have experienced similar economic levels as Western Europe and China until the end of the 18th century. Besides Japan, he also considers India to be a candidate, particularly Gujarat and the north of the subcontinent in general—though he has some clear reservations (Pomeranz, 2000, pp. 146–148, 212–215; see also pp. 40, 131–134, 174–178, 259–260, 293–295). These include the caste system, the extension of bonded labour and the restrictions that these systems put on the mobility and hence the earning and purchasing power of the common man and woman, which are necessary for economic growth. Pomeranz is careful not to present strong

conclusions about India. In reaction to an influential study (1998) on Indian wages by the Indian historian Prasannan Parthasarathi, for example, he thinks that, for the 18th century, 'it still seems too early to speak of rising popular consumption in India as comparable to that in other places' (Pomeranz, 2000, p. 147, in response to Parthasarathi, 1998).[3]

In 2011, the same Parthasarathi published a comprehensive study on the Great Divergence, in which India took centre stage (Parthasarathi, 2011, also 1998, 2001 and 2005). He is more confident than Pomeranz about India's performance.

> the period between 1600 and 1800 was a time of great economic and political dynamism in the advanced regions of the Indian subcontinent. Vibrant production of cotton textiles for export led to sizable inflows of silver, gold, copper and cowries which fuelled a commercial revolution.

However, this would subsequently change: 'From the second decade of the nineteenth century there was a sustained economic regression in Bengal, South India, Gujarat and other regions of seventeenth- and eighteenth-century dynamism' due to the 'rise of the British colonial order' (Parthasarathi, 2011, pp. 265–266).

Between Parthasarathi's 1998 article, which compares wage levels in South India and England in the 18th century, and the publication of his monograph on India in the Great Divergence debate 13 years later, a number of economic historians, in particular Stephen Broadberry and Bishnupriya Gupta (2006), Robert Allen (2007), Allen and Studer (2009), Allen et al. (2011), Sashi Sivramkrishna (2009), Tirthankar Roy (2010) and others, had entered the debate on real wage levels (for later contributions to the discussion: Broadberry & Gupta, 2017; Broadberry et al., 2015; Ghosh, 2015; Yazdani, 2017; Zwart, 2016; Zwart & Lucassen, 2020).[4]

Whereas Sivramkrishna, in a study on Mysore 1800, broadly supported Parthasarathi's arguments, the others did not. Conceding that more research was still needed, Parthasarathi nevertheless argued that his original conclusion was right: 'Thus there does not appear to be any compelling reason to believe that the labouring populations of Britain possessed a higher standard of living [than those of India]' (Parthasarathi, 2011, p. 46).[5] In doing so, he

remained an advocate of the California School, which stressed that the bifurcation between 'the West and the rest' only really happened around 1800 against the idea of 'a European takeoff in the context of long-standing European dynamism *versus* long-standing Asian stagnation' (Studer, 2015, p. 14).

A few years later, Roman Studer gave a new twist to India's place in the Great Divergence debate in a monograph that took market integration between 1600 and 1900 as a starting point (Studer, 2008, 2015; cf. Caruana Galizia, 2015). For Studer (2015, pp. 147–149), market integration is an important, if not the pre-eminent, motor for economic growth. In his view, market integration of especially grain prices in India 'only really started in the second half of the 19th century', thus refuting the claims of Pomeranz or Parthasarathi. The value of his conclusion is weakened, however, by the fact that he does not have price data before 1700 and that his real comparisons between different grain markets in India start only in 1764 (Studer, 2015, pp. 72–98).

Further, Studer's wage data (2015, pp. 200–202) are very limited: 79 for the 17th century; 47 for the 18th century; 65 for the first half of the 19th century and 200 for the second half of the 19th century. Regionally, this meant 131 wage quotations for eastern, 78 for western, 63 for northern and 119 for southern India. The fact that he includes data produced by his predecessors indicates how limited the quantitative basis for all this work is—a fact that all participants in the debate deplore while simultaneously drawing firm conclusions (cf. Bosma, 2014). Nevertheless, some even use the few wage data they know to reconstruct the gross national product (Broadberry et al., 2015; Roy, 2010; Studer, 2015). According to Studer (2015, pp. 170–176, 200–201), 'the overall picture that emerges closely resembles the one on integration and GDP per capita: throughout the early modern period, wages were substantially lower in India than in Europe, the ratio being again in the region of one to two in the eighteenth century.' And real wages remained at that same low level during the next century.

This lack of good and representative sources motivated Pim de Zwart and Jan Lucassen (2020) to collect substantially more data for a new attempt to establish when precisely real wage levels for unskilled labourers started to diverge between Western Europe

and North India. They collected over 7,500 observations between 1595 and the early 1870s for that part of the subcontinent, 20 times as many as their predecessors. Although aware that their data for the second half of the 17th century is fairly thin, they arrive at a conclusion midway between the pessimists, who think that, from the 16th century on, India's wage levels were already lagging behind those in Western Europe, and the optimists, who think that this dates only from *c*. 1800. To be fair, and neglecting for a moment the narrow evidence base of the optimists Parthasarathi and Sivramkrishna, it is possible to reconcile their outcome and that of de Zwart and Lucassen: after all, the latter authors base their conclusions on data from North India and Parthasarathi (to a great extent) and Sivramkrishna (completely) on data from the South (for Mysore, see also Yazdani, 2017, pp. 149–150, 165–170; for Gujarat, see Yazdani, 2017, pp. 391–401). This possibility of diverging economic developments in the North and the South in the 18th century is, however, only conceivable if we accept a very low degree of market integration. Currently, this seems unlikely, but it may be researched in the future as soon as enough data for South India become available.

In any case, for the north of the subcontinent, de Zwart and Lucassen discern a 'slightly downward trend since the late 17th century' of unskilled wages, and they would remain (like those of Beijing) around subsistence level from *c*. 1700 until far into the 19th century (see also Chapter 6 by Jan Lucassen in this volume), whereas, in the same period, those in Europe (and most of all in London) fluctuated at around twice this level or more. This gap between England (to be precise, wages in a small town like Oxford and certainly in London) and North India, which appeared in the late 17th century, widened after the 1720s and especially after 1800. This trend is the same for both unskilled labourers and skilled craftsmen. After *c*. 1700, only the contribution by waged women and children to household income allowed Indian families of wage earners to survive.[6]

If we accept that the provisional outcome of the debate about wage levels in the framework of the Great Divergence debate provides some clarity for the period after 1700, or even 1600, the question of what happened before that remains unanswered. de Zwart and Lucassen's assertion about India's favourable economic achievements before *c*. 1700, on a par with other major civilizations in

Eurasia, is in line with a number of studies by Najaf Haider. In the tradition of the Aligarh School (in particular Irfan Habib), this expert in the economic history of Mughal India has collected as many early sources as possible regarding wages and prices (including those in Persian and Arabic; Habib, 1994; Haider, 2004, 2007, 2010; cf. Manzar, 2021).

Leaving aside wage indications in the *Arthashastra* of Kautilya (1992; cf. Lucassen, 2021), which, in the extant version, may be placed in the 3rd century CE, the earliest wage data for North India are available for the decades around 1300, and published and analysed by Habib. Expressed in kg of wheat or rice per day, they were substantially lower than the wage rates given in the Ain-i-Akbari of *c.* 1595, as analysed by Haider. Recently, Luso-Indian wage data have become available for Kannur 1516–1517, which, to a certain extent, may bridge the gap of three centuries between 1300 and 1600. They suggest stable low wage levels between 1300 and 1500, as compared to much higher levels in 1595 and possibly later (de Matos & Lucassen, 2019).

A few even earlier wages are known for the South. Unfortunately, they only concern skilled and highly skilled artisans, employed by temples in three different districts, and Vijaya Ramaswamy (2004) is hesitant to draw firm conclusions.[7] If, however, we consider the masons' wages of 1011 as exceptionally high outliers among the other early evidence, we might conclude that the wage levels up to *c.* 1500 are more or less at the same low level, and certainly much lower than those *c.* 1600, which suggests a wage hike somewhere in the 16th century (see Table I.1, which summarizes what was known before the publication of our volume about the purchasing power of wages, expressed in wheat and rice up to 1600).

The first contribution to this volume (Chapter 2) attempts to be more precise as to when this hike may have taken place and points to the 1540s as a good candidate. Consequently, this may have been the start of a high wage level in the 17th century, but the authors hesitate to say anything definitive about how long these much-improved income levels, found around the mid-16th century, would subsequently endure.

There seems no doubt that at the end of the 17th century, wages started a downward trend, reaching a stagnant minimum

Table I.1 Purchasing Power of Wages (Kg of Grain per Day), India 950–1600

			Wheat Grain Wage		***Rice Grain Wage***	
			Unskilled	***Skilled***	***Unskilled***	***Skilled***
951–952	Erichchaudaiyar (Tirunelveli)	Stonemason				2.24
		Master carpenter				4.48
978	Umamahesvara (Thanjavur)	Blacksmith, carpenter				1.12
		Master carpenter				2.24
1011	Brahadisvara (Thanjavur)	Stonemason (apprentice)				6.72
		Master mason				7.00
		Brazier, master goldsmith (supervisor)				8.96
		(Master) jeweller				13.44
		Architect				26.89
1264	Nataraja (South Arcot)	Blacksmith, goldsmith				2.24
		Master carpenter				4.48
		Stonemason				5.45
1311	Delhi		1.75	3.00	3.00	4.50
1516–1517	Kannur		2.13	2.41	2.22	2.52
1595	Agra/Delhi		4.19	12.60	0.50	1.01
1610–1613	Golconda				5.70	

Source: Ramaswamy (2004, pp. 576–579); de Matos and Lucassen (2019, p. 125).

in the 19th century (Zwart & Lucassen, 2020). For the last part of this period of wage stagnation, that is, from the last quarter of the 19th century onwards, we have statistical evidence, systematically collected by colonial authorities. They were galvanized by the abyss between their lofty pretensions, on the one hand, and the harsh reality of apparently unavoidable famines and social unrest on the other. By critically analysing the wage series published for 1873–1912 and later wage data, Tirthankar Roy (2007, pp. 82–83) has demonstrated that low wage levels persisted for no less than one century: 'Comparing 1873 with 1968, we observe that the average rural labourer appears to have earned a real wage that did not change very much.'[8] Drèze and Sen (2013, p. 23, 27–33, 201–202, 291, 333) even state that 'there was virtually no reduction of poverty, especially rural poverty, in India for most of the three decades that followed the launch of the First Five Year Plan in 1951,' and, even after the growth in the 1980s, real agricultural wage levels stagnated again (Drèze & Sen, 2013, p. 23).

Combining all these data, we find a depressed state of rural agricultural wages that started around 1700 and lasted until recently. This has had a severe impact on a country like India, where, until very recently, the majority of its population had to earn a living in agriculture. At the same time, it highlights the extraordinary achievements in the mid-16th century and possibly later—certainly given the limited technical possibilities of those centuries.

I.2. THE PLACE AND ROLE OF WAGE LABOUR IN INDIAN SOCIETY OVER THE LAST 500 YEARS

Whatever the outcome of the rather technical but, at the same time, highly necessary discussions among economic historians, its implications for society at large depend on how many Indians drew their income from wages and to what extent. Here, we enter into two other debates, one regarding the importance (i.e., its proportion of total income) of wage labour in Indian society and the other on the relation between wage levels, on the one hand, and geographical and social mobility, on the other—a question related to the impact of mobility on economic performance. We will discuss them briefly here.

To what extent were Indians dependent on wage incomes?

This seems to be a rather factual question, which may be solved by an analysis of occupational censuses but, in reality, it is a very contentious one distorted by views on the very nature of Indian society. The orientalist view, which gained force in the 19th century, depicted India as a subcontinent dominated by self-sufficient villages and, by implication, devoid of a labour market worth this name. After influential English authors like James Mill, Karl Marx (1818–1883) wrote that India, organized 'in small centres by the domestic union of agricultural and manufacturing pursuits [...] had brought about, since the remotest times, a social system of particular features—the so-called *village-system*, which gave to each of these small unions their independent organization and distinct life' (Parthasarathi, 2011, pp. 7–8, 265).[9] The famous Max Weber (1864–1920) more or less followed him in this respect (Weber, 2019). As especially Marx's intellectual influence increased in the 20th century, also among a number of Indian academics, this interpretation of a stagnant Indian non-market economy, many centuries before British domination, gained influence.

Another influential scholar, anthropologist Karl Polanyi (1886–1968), also tended to play down the significance of markets before the Industrial Revolution. This 'primitivist' position spurred both debates on the nature of society in classical antiquity (with Moses I. Finley as their champion) and on 'traditional' non-Western societies, including India (Hall, 1994, p. 60; Lucassen, 2021; cf. Subrahmanyam, 1994, pp. 5–7). The impact of the primitivist school on classical studies on the historiography of China has been summarized by Anthony Barbieri-Low (2007, p. 27) as follows, and this is equally applicable to India.

> agriculture, and not commerce, was the dominant form of activity in Greece and Rome [...] that private trade was carried out only on a minimal scale, and usually only in luxuries [...] that Roman towns and cities [... were] parasitic centers of consumption and redistribution and not [...] productive industrial centers [... that] individuals were most concerned with gaining status, and [that] loans and investment were not made for economic purposes. (cf. Lucassen, 2021; Subrahmanyam, 1994)

In a clear summary of this debate, Sanjay Subrahmanyam (1994, pp. 8–11, 55) writes:

> In the context of colonial India, [the] hypothesis [of forced commercialization] was used to argue that commercialization was forced on the peasantry by a variety of means—some subtle and some more explicitly coercive in nature. An older 'moral economy'—an economy underpinned by a set of mutually reinforcing and relatively ideal customs, say *jajmani*, was thus replaced by the market.

Most authors equate this brutal change with the advent of British rule in India. An alternative dating of this so-called forced commercialization is defended by Irfan Habib and others of the 'Aligarh School', who concede that it started already in the Mughal Empire with the excessive taxation of the rural population and its consequent immiserization.[10]

As Subrahmanyam (1994, p. 7; cf. Manzar, 2021, p. 301) notes, there are some major empirical problems with this primitive view of pre-colonial Indian society in the period 1100–1700. If this were true, 'the facts of international and long-distance trade, production of both agricultural goods and manufactures for trade, extensive coinage and importation of precious metals, all have to be explained.' He argues persuasively and extensively that Indian society in the pre-colonial period was already highly monetized and that its markets and commercial and financial instruments were rather sophisticated and therefore ends with a plea for more research into these aspects, preferably region-specific.

Whereas Subrahmanyam's important volume of collected essays, now already nearly 30 years old, invites us to study pre-colonial India with an open eye for market forces and to look for new empirical evidence, there is still one more counterargument against the importance of wage labour that must be considered. That is the potentially inhibiting factor of caste and of the unfree nature of the lowest strata in this hierarchical system. Here, the question is not about the presence and importance of the market in society but whether these strata could participate in labour and commodity markets at all. To what degree did the lowest strata of society—the majority of its population—receive a wage, or rather

just maintenance in kind? We thus enter the contentious field of slavery and unfree labour more generally, which is very specific for India.

Although there is an initial debate about the extent of slavery in the classical sense, that is, productive workers who could be owned, bought or sold, none of the participants maintains that its significance in India comes near to that in the Americas, and especially in the Caribbean or the southern states of the USA (Campbell, 2005, 2007, 2011; Chakraborty, 2019a, 2019b; Chatterjee & Eaton, 2006; Fukazawa, 1991; Gommans, 2003; Rossum, 2021; Rossum et al., 2020; Vink, 2003). Nevertheless, to give one example, in 1818, the number of slaves in the conquered Maratha kingdom was estimated at 18,000, most of whom would have been women. Fukazawa (1991, p. 117, 120) reports for the governmental centre of Pune in 1763 some 200 female slaves employed by the Maratha government in the courts and various departments, plus some 10 of them in each aristocratic family.[11] As far as we know now, in the long run, a few per cent of the occupational population in India at large may have represented this type of unfree labour.[12] Most of them were women, forced to perform not only household chores but also sexual services in the dwellings of the middle classes and the well-to-do. As an outflow of the worldwide abolition movement, unfree labour was legally abolished in India in the mid-19th century (Campbell, 2005; Chakraborty, 2019b; Fukazawa, 1991, pp. 126–127; Saradamoni, 1974).

More importantly, many more working people may have been covered by forms of what has been called 'agrestic slavery' (Ludden, 2005, p. 93; Rossum et al., 2020; Saradamoni, 1973). It has been studied especially for Kerala, where it is supposed to have gained strength between the 8th century and the 11th century. Whatever its exact origins, it is well documented from the 16th century onwards and in particular detail for the 19th century. According to Saradamoni, there were many slave castes in Kerala, but the main were the Cherumas in Malabar and the Pulayas in Travancore (Thiruvithamkoor) and Cochin (Kochi), both of which groups performed the bulk of agricultural labour for the dominant Nambutiri Brahman caste or Nayar or Christian landlords and, indeed, under similar conditions.

According to Francis Buchanan (1762–1829), who toured the region in 1800–1801, these agrestic slaves performed any work that the master wanted them to do. Moreover, they could be 'sold, leased and mortgaged, like the land itself, or like any cattle or thing', as Major Walker observed in 1828 (Saradamoni, 1973, p. 375; cf. Mizushima, 1986, pp. 316–319). Not surprisingly, these agrestic slaves received no wages, merely a subsistence allowance, which, according to Buchanan, equated to two measures of paddy weekly per adult man or woman and half of that for children or the elderly. For infants, there was no allowance at all. Once a year, adults also received one cloth to wear, seven cubits (3.15 m) for a man and double that for a woman. In the neighbourhood of large towns, they took the opportunity to earn a little bit extra through wage work. Because the members of these castes were not only considered to be untouchable but also unapproachable—they had to maintain a precisely circumscribed distance from all others—domestic slavery was out of the question, and they could only toil on the land.

For this volume, it is important to know what proportion of the working population consisted of slaves in the way described here and, as such, were completely divorced from the labour market. Because this varied substantially according to regions, this question is difficult to answer in general, but for Kerala it may be regarded as tens of percentages. This caveat aside, along with Sanjay Subrahmanyam, most modern authors now agree that, at least from the 16th century onwards, India in general was characterized by a full-fledged labour market, a conclusion that is crucial for any relation between wage levels and the welfare of the common man and woman, and for the state of the economy as a whole (Lucassen, 2021; Ludden, 1988; Parthasarathi, 2011; Subrahmanyam, 1994; Roy, 2013, 2014).

The next question to be answered is the proportion of wage labourers in the occupational structure or—more precisely—which part of total income is derived from wage labour. Various studies, based on statistical data collected from the end of the 19th century onwards, suggest that between one quarter and one half of the working population depended on wages for an important or even a major part of its income. For earlier periods, this is a field that has hardly been touched, though even a limited survey of the literature reveals a number of earlier occupational and caste surveys for

several parts of India.[13] In this volume (Chapter 6), this has been worked out in great detail for the Deccan in the first quarter of the 19th century. In this case, it turns out that only a few per cent were craftsmen and service providers in a *jajmani*-type relationship with local farmers, but even these workers may have received part of their income in cash instead of in grain (cf. Commander, 1983; Fukazawa, 1991; Kulkarni, 1996).

A related question has to do with income levels per occupational, or, for that matter, per caste category. Very rarely do we find direct information of this kind, like the list for Pondicherry town 1759, with war funds levied per caste as well as the number of heads of households taxed, which yields average amounts per household according to castes (Mizushima, 1986, p. 321). Instead, budget studies may also be useful. The earliest ones have been carried out by Francis Buchanan.[14] If complete on the income side, they also inform us about the importance of wages for total household income. This is crucial information for our topic as many, if not most, rural households combined a part of their income from wages and a part of them from tilling the land as tenants of sorts (Ludden, 2005, pp. 90–93, 158). Even if wages are only a small part of a household's income, it may mean the difference between a modest living and sheer destitution. Only live-in agricultural servants—a small and, from the 19th century, diminishing part of all wage earners—would have depended solely on what their employers wanted to give them.

Given the low levels of urbanization, domestic service offered relatively few opportunities. This seems to be in contrast with the important part it takes in the literature, which devotes more attention to the rich than to the poor. The affluent Indian and colonial households could employ impressive numbers of servants, as illustrated by Fanny Parkes (1794–1875) in her memoirs about her years in North India from 1822 to 1845. Because of its details and its implications, it is worth quoting at length.[15]

I.2.1. The Relation between Wage Levels and Geographical and Social Mobility

We may take this overview of her staff by Fanny Parkes as a starting point for another set of questions regarding mobility (Table I.2). According to standard economic theory, people may improve their

Table I.2 A List of Private Servants in the Family

No.	Occupation	Wages ₹ per Month	No.	Occupation	Wages ₹ per Month
1	A khansama, or head man; a Musalman servant who purchases the provisions, makes the confectionary and superintends the table*	12	24	A *bher-i-wala* or shepherd	5
2	The *abdar*, or water cooler; cools the water, ices the wines and attends with them at table	8	25	A *murgh-i-wala* to take care of the fowls, wild ducks, quail, rabbits, guinea fowls and pigeons	4
3	The head *khidmatgar*; he takes charge of the plate chest and waits at table	7	26	A mali or gardener	5
4	A second *khidmatgar* who waits at table	6	27	A mate.	3
5	A *bawarchi* or cook	12	28	Another mate, or a coolie	2
6	Mate *bawarchi*	4	29	A gram-grinder, generally a woman who grinds the chana for the horses	2
7	Masalchi; dishwasher and torchbearer	4	30	A coachman	10
8	Dhobee or washerman	8	38	Eight sa'ises, or grooms, at ₹5 each, for eight horses	40
9	Istree wala, washerman for ironing	8	46	Eight grasscutters, at ₹3 each, for the above	24
10	A *darzee* or tailor	8	47	A *bhishti* or water carrier	5
11	A second tailor	6	48	A mate *bhishti*	4

(Table I.2 Continued)

(Table I.2 Continued)

No.	**Occupation**	**Wages ₹ per Month**	**No.**	**Occupation**	**Wages ₹ per Month**
12	An ayah or lady's maid	10	40	A *Barha'i mistree*, a carpenter	7
13	An under woman	6	50	Another carpenter	4
14	A doriya; a sweeper who attends to the dogs	4	52	Two coolies to throw water on the *tattis*	8
15	Sirdar-bearer, a Hindu servant, the head of the bearers and the keeper of the sahib's wardrobe; the keys of which are always carried in his *kamarband*, the folds of cloth around his waist*	8	54	Two *chaukidars* or watchmen	8
16	The mate-bearer; assists as valet and attends to the lamps	6	55	A durwan or gatekeeper	4
22	Six bearers to pull the *pankhas*** and dust the furniture	24	57	Two chaprasis, or running footmen, to carry notes and be in attendance in the veranda	10
23	A *gwala* or cowherd	4	57	Total ₹ per month***	290

Notes: *If your khansama and sirdar-bearer are good and honest servants, you have little or no trouble with an Indian household; but, unless you are fortunate with your head servants, there is great trouble in keeping between 50 and 60 domestics in order.
**During the hot winds, a number of extra coolies, 12 or 14, are necessary, if you have more than one thermantidote, or if you keep it going all night as well as during the day; these men, as well as the *bihishti*, are discharged when the rains set in.
***We, as quiet people, find these servants necessary. Some gentlemen for state add an *assa burdar*, the bearer of a long silver staff; and a *sonta burdar*, or *chob-dar*, who carries a silver club with a grim head on the top of it. The business of these people is to announce the arrival of company.

income by moving to employers who pay better wages for the same sort of work, or to jobs with the same or other employers that supposedly require better skills and, therefore, command higher wages. Both types of upward income and therefore often also upward social mobility may or may not involve geographical mobility. In particular, rural–urban migration is believed to offer new opportunities. For India, we must ask ourselves what the opportunities were for these types of mobilities. In the case of Parkes' household, for instance, what were the chances of entering the ranks of the household staff, and, once in, what were the chances of improving oneself in the same profession (e.g., from carpenter at ₹4/month to master carpenter at ₹7/month), or in another (e.g., from grasscutter at ₹3/month to horse groom at ₹5/month)?

Parkes seems to be pessimistic about the latter possibility. Apart from distinguishing between religions, Parkes draws our attention to the intervening obstacle of caste sensitivities.

> The heat of the climate, added to the customs and prejudices of the natives; but you do not find them in food as in England. One man will not do the work of another, but says: 'I will lose caste', which caste, bye the bye, may be regained by the expenditure of a few rupees in a dinner to their friends and relatives. The Mohammadan servants pretend they shall lose caste; but, in fact, they have none: the term is only applicable to the Hindoos.

Does that mean that wage differentials are only a reflection of status without enhancing social mobility? Even if we forego the colonial prejudices of this author, we may observe a substantial differentiation between the earnings, ranging from ₹2 per month as a minimum for the unskilled, ₹3–₹4 for the low skilled, ₹5–₹8 for the skilled and ₹10–₹12 for the top wage earners. The important question, then, is whether these differences (the 'skill premium') work as an incentive for people to improve their position, or whether caste or other occupational restrictions make this illusionary (as Parkes thinks is the case), and thus rob the labour market of the dynamics and advantages of social and geographical mobility attributed to it by general economic theory.

This is not the place to discuss these important questions at length but let us say a few words on what we know about the

skill premium, geographical mobility as an individual attempt at improvement and social protest as a collective alternative. For the statistical period, we see an improvement in the skill premium from the late 19th to the mid-20th century in India. According to Tirthankar Roy (2007, pp. 83–84), it increased from *c*. 2 in the years 1875–1895 to *c*. 2.3 in the years 1900–1916 and *c*. 3.3 in the years 1916–1937. The low ratios for the late 19th century have also been observed for North India in the 18th century, whereas before they may have been higher (Zwart & Lucassen, 2020, pp. 659–660). Thus, also for India, there might be an inverse relation between wage levels and skill premium. What this means is not immediately apparent. Therefore, also in this field, much more research is needed to explain the nature of this relation.

Reliable geographical mobility figures are available for the 19th century as far as international and long-distance migration (e.g., to the Assam tea gardens) are concerned and, to some extent, also for urbanization, but basically they start later.[16] It is therefore difficult to say something more general about mobility in pre-statistical India, the period we are interested in. Certainly, examples of impressive numbers of unfree labourers and of soldiers are well known, but how to quantify mobility at large remains a problem (Rossum et al., 2020, p. 17). Instead, we will point to some analogies that might inspire future research. Four features of the 19th- and 20th-century Indian migrations stand out: they are male-dominated; they are, to a high degree, directed at advanced rural destinations in India and abroad; they are temporal and they provide extra income for the households involved, which, as we have seen, were confronted with stagnant wage levels at a very low level.

The male domination may be due to the prevalence of early marriage and patrilocality, and possibly also to the taboo on remarriage of widows, all of which made women more sedentary than men. As a consequence of the dramatic diminution of spinning as a source of extra female income in the 19th century, women in the countryside looked for local casual work, thereby replacing long-term farm servants (Roy, 2007, pp. 87–90).[17] Men seeking extra income looked for temporal work on plantations both at home and abroad. This is well documented for South Indians who migrated to the straits settlements and Malaya, Burma and Ceylon, and, more recently, to the Gulf and to the booming cities within India

itself. Keen not to lose whatever small plots of land at home they were entitled to, most of them did not emigrate permanently, but returned home at regular intervals and remitted the money they could save.

In the reports of the Royal Commission on Agriculture in India (1926–1928), we find three important reasons for the dominance of temporal migrations due to inhibitions of permanent emigration of peasants.

> In many cases, the hirer of land is subject to conditions, which make his status approach more closely to that of a labourer than an independent cultivator. Very frequently the holding is so small that the cultivator must supplement his income by working as a labourer. Nevertheless, with all these drawbacks, the average man will not give up a certain livelihood for the risks of pioneering in Assam or even of transferring his family from the Deccan to unoccupied tracts in Kanara or Khandesh in the same presidency [...] secondly, there is the problem of indebtedness. Most cultivators are tied to their village by their relations with the village money lender and trader, who for obvious reasons puts every obstacle he can in the way of their emigrating. Lastly, there is the important factor of ill-health. A population which suffers from such enfeebling diseases as chronic malaria and hookworm cannot be expected to display that energy which would accept, and triumph over, the risks incidental to the pioneer.[18]

Apart from income strategies at the household level, under certain conditions, we also see collective actions to prevent deterioration of existing wages, or sometimes also attempts at improvement. Several examples have been studied, but a good overview is still lacking.[19] Sometimes, they were successful, but as long-term wage stability has shown, not to the same degree that migration was. Nevertheless, we should not make the mistake of neglecting this type of agency in Indian social and economic history, not only out of considerations of justice but also in order not to forget how difficult it is to change existing social structures.

On the other hand, the authorities were well aware of the possibilities of social unrest as a consequence of sudden price hikes for grains and other basic products. From the Delhi Sultans, the

Mughals and their successor states, we know the nerick system in which prices were kept track of locally on a daily basis. This way, governments could take pre-emptive measures. Initially, the British maintained the nerick system in the bazars next to their barracks, but at the beginning of the 19th century they became the zealots of free market politics and consequently responsible for the drama of India's malnutrition and famines (Bajekal, 1988; Blake, 1987; Habib, 1994).

I.3. OPPORTUNITIES AND PITFALLS OF THE HISTORICAL STUDY OF WAGES IN INDIA

The opportunities and pitfalls of the historical study of wages in India deserve a separate place in this Introduction because, in this respect, the several contributions are breaking new ground, in particular regarding what is generally called the 'pre-statistical period', that is, the period preceding the all-India decadal censuses, which started between 1867 and 1872, and was executed synchronously from 1881 onwards. It was only by the actual practice of taking censuses that the enumerators and analysts discovered the manifold problems associated with it (discussed extensively in the introduction to each new census report). Before the last quarter of the 19th century, 'economic data becomes scanty and is scattered around in numerous sources and regional studies, and virtually no systematic efforts have yet been made to amass the economic data available.' Roman Studer (2015, p. 15), who made this remark a few years ago, adds: 'Undoubtedly, the prime reason for this shortage of quantitative studies is the paucity of historical economic data, which is much more pronounced for India compared with European and even some other Asian countries.' Even worse, in India, 'there is a pronounced scarcity of all economic data prior to the 19th century,' more in particular 'a general shortage of non-European sources on India'.

Luckily, the situation is not that bad (for population figures, see Dyson, 2018). First, the British took several regional censuses before they attempted to cover their entire Indian empire. But Indian polities also appear to have produced much more useful data than imagined by most historians. Two major issues must be discussed here at some more length:

How to find new quantitative evidence for the pre-1870s (with an excursion on Maratha sources by way of example) and the problems of interpretation.

How to find new quantitative evidence for the pre-1870s?

In the preceding pages, we have already seen many examples of un- or understudied quantitative sources, important for the reconstruction of wages and their modalities in the pre-statistical period, even in English, the language predominantly used by professional historians of India. This will not be repeated here, but, in particular, price history and demographic history using English published and unpublished sources for especially the period *c.* 1760–1860 is still extremely promising.[20] This is even more true for three other European languages, namely Portuguese, Dutch and French, and, to a lesser degree, also Danish, German and Swedish.[21]

It might be argued—for reasons that cannot be discussed here, and in contrast to the preceding and the following centuries—that Indian society from the 5th century until the 10th–12th centuries was never really dependent on markets, nor was it deeply monetized. Consequently, historians will search to no avail for wage data in this era (Lucassen, 2021). It should be noticed that this produces a striking parallel with most of contemporaneous Europe. This is not to say, of course, that, in both cases, those centuries are not interesting for economic historians,[22] but for this volume on the history of wages we must start later: from the deeply monetized Chola Empire in the South, and from the times of the Delhi Sultanate in the North. The available quantitative evidence has been presented in the first section of this Introduction and we hope that more will become available in the future. Apart from hard wage data for the period between *c.* 1000 and 1500, of course, qualitative information is also important. A good example is provided by the *Lekhapaddhati*, which, while technically concerned with epistolary style, is very important in terms of the information that it provides on the commercial world of the Indian Ocean before 1500 and the involvement of Gujaratis in it, as well as on other aspects of market systems. The 2007 translation of the text by Pushpa Prasad (2007) is of great value in understanding the range of, in particular, economic and administrative activities of the time (cf. Palat, 2015).

While such information is undoubtedly important for fleshing out our knowledge about commercial transactions in pre-colonial India, there is also no denying that the *Lekhapaddhati* does not easily lend itself to statistical analysis. Information from one region or one set of sources can often not be compared with that of another, with the result that there is frequently a dynastic/regional focus rather than a broader one. In addition, such sources usually do not have information that can be used to build an understanding of the actual wages, even while they provide considerable information on workers, work and areas of work, as well as on the social aspects, particularly that of caste.

When we move to the 16th and 17th centuries, information becomes much more widely available. Some of this is undoubtedly due to the increasing number of European language sources on South Asia; but what is often neglected is the Indian language sources, including those in Arabic and Persian.[23] The Mughals in particular kept detailed records, and for the late 17th and 18th centuries, we have a wealth of documentation from especially Rajasthan and Maharashtra. Given the limited space in this Introduction, we will work out one example more fully.

I.3.1. By Way of Example: Marathi Sources

The Marathi sources are of tremendous importance. Written in Marathi in the *Modi* script[24] (a script that was used through the late 17th century to the early 20th century), they contain a wealth of information. Many of the documents to be found in the Peshwa Daftar in Pune, in the Bharat Itihas Sanshodhak Mandal (BISM) in Pune, in the Sitamau Archives in Madhya Pradesh and the Saraswathi Mahal Library in Thanjavur, all contain collections of *Modi* records. The Peshwa Daftar alone has an immense collection of over 1 crore documents, many very cursorily catalogued, most unpublished and many hardly noticed, let alone studied, by researchers. In the main, these are records of state revenue. Known as *taleband*, they are account papers, detailing income collected under different headings, and the expenditure of the state under various categories, including salaries. A set of documents from the province of Vijaydurg, one of the key sea forts of the Marathas, has account papers known as *zadati*, which give details of naval

administration (Apte, 1973, p. 245). Some of the *Modi* documents have been studied in the papers by Rekha Ranade, Surendra Arjun Shirsat and Anjali Soitkar Vekhande (Chapters 3, 4, 5 in this volume). In his work on agrarian conditions, T. T. Mahajan (1991, p. 57) gives some information on wages from both the account from 1672–1674 by the French traveller Abbe Carré and from Marathi sources.[25] This information may be compared with that given in the papers on Shirsat and Soitkar to compute the change in wages (if any) over a century.

Other kinds of information can also be gleaned from the documents. For example, one set of documents relating to the city of Pune, dated to 1765, gives information about the cost of repairing houses in one of the key parts of the city, including the names of the owners of the houses and their occupations. Late 18th-century documents give us fascinating glimpses of inter-village conflict over boundaries—something that apparently became much more important in the context of extending the land under cultivation. At one level, this challenges the notion that the 18th century saw stagnation and decline in the agricultural sector all across the subcontinent; at another, it is a window into issues of rights and ownership. B. K. Apte (1973) used the documents available in the Peshwa Daftar to discuss aspects of the Maratha Navy; what is important for us is the information that he provides on prices of goods required for the ships, as well as some information on cost of some products. Some information on wages is also provided. For example, one document, which talks of a projected expedition against the Portuguese in 1771–1772, 100 soldiers were recruited at the rate of ₹5,736 per soldier, and 100 sailors at the rate of ₹6,480 per soldier (Apte, 1973, p. 249). A sum of ₹10,000 was asked from the state to pay for replacement of cordage on some of the older ships, and as new ships were being built, the total expenditure on these new ships (for iron, planks, cotton and labour) was ₹17,000. One document talks of the replacing of docks and of hiring four carpenters for the work; unfortunately, the hiring charges have not been given. In one instance, mention is made of divers appointed to maintain the depth at the docks during the monsoon, and 'to enable them to have enough heat in the body a total allowance of ₹3,150 per month was sanctioned' (Apte, 1973, p. 261). Some payment was made to widows and children of sailors who had lost their lives

at sea, or in the service of the state. Apte (1973, p. 251) mentions specifically those who died in an engagement against the British, those who died as prisoners of the Siddis and those who drowned in shipwrecks. As we said, many of these documents have been un- or underutilized, and these can be examined afresh for a better understanding of wages and prices in the 18th century.

While we have talked at fair length about the documents in the Peshwa Daftar, we should also state that these are far from being the only collections. The BISM has a large collection of mostly family papers, including those of some of the major banking families of the 18th century. These, too, have been utilized more from the perspective of a better understanding of Maratha history, but could be useful in acquiring more knowledge about the economy of the period, and of the role of such banking families in the fiscal transactions of the time. There are also family papers in the Sitamau Archives; some, such as the *Hingne Daftar* and the *Gulgule Daftar*, have been fairly extensively used to trace the families' histories and their role in the political movements of the 18th century, but, as with the BISM collection, these too remain a largely untapped source for the study of economic aspects. The Saraswathi Mahal Library in Thanjavur is a collection that records the activities of the Maratha kingdom of Thanjavur; the collection has been a rich cultural resource, containing, as it does, material on music, dance, the collections of the Maratha king Serfoji II, known for his interest in subjects as diverse as geography, astronomy and medicine; but the *Modi* documents are only now beginning to be scrutinized, and, as yet, the range of information that they may yield remains uncertain.

Archival sources in Rajasthan can also provide different kinds of information. As with the Marathi sources, these too consist of a variety of documents, including letters, petitions, tax orders and Mughal *farmans*, in Rajasthani, Urdu and Persian. The Bikaner State Archives has published a guide to their archives in 1992, which includes a list of family and business records. Rajasthani and Persian records of the 18th and 19th centuries that are to be found here provide information on revenue collection but would be of value for future research.[26] Madhavi Bajekal (1988, p. 451) has used the *Daftar Diwan Hazuri* from the Jaipur Records Section of the Bikaner archives to study grain prices in eastern Rajasthan and has used these records extensively in her thesis on agricultural

production (Bajekal, 1990, Ch. VII). However, as with the Marathi sources, there is much more that can be done. The continued importance of trade, and role of traders in the economy, is one aspect that can be taken further, as too a fresh look at the *arzdashts*, the petitions that were made to the courts for different issues.[27]

At Jodhpur, the Rajasthan Oriental Research Institute and the Maharaja Man Singh Pustak Prakash Research Centre contain documents in Rajasthani and Sanskrit, including some *dastur-ul-amals*—manuals of revenue collection, giving details about the collections from various sources (Thelen, 2019). Such *dastur-ul-amals* are also to be found in Maulana Abdul Kalam Azad Arabic Persian Research Institute in Tonk, yet another collection that would be worth exploring. As in many of these cases, these documents have so far been more for social and political history, rather than for economic history. Finally, let us not forget Marathi documents in *Modi* script in the 'colonial archives', as attested by the large collection of village community records in the Goa State Archives, only partially written in Portuguese.

I.3.2. Problems of Interpretation

There are numerous interpretation problems in the study of wages in India before the end of the 19th century, of which we want to concisely discuss the following three: occupation and caste; gender, age and the household economy and modalities of payment.

Wages are specific for different occupations and therefore economic historians are always wrestling with how to classify them, in particular if used for international comparisons.[28] Peculiar to India is that the sources use occupations such as 'carpenter' or 'coolie' as well as caste names. In theory, the relation between the two is obvious, but in practice it is not, as stressed by many authors in this volume.

Moreover, regional names and variations need to be constantly kept in mind. For example, 17th-century English and Dutch records for the Coromandel Coast mention Chettis, while those dealing with Gujarat (particularly Surat) talk of Banias and 'Moors'. Nevertheless, it is likely that the same occupational category is meant.[29] Weaving castes are often not named, but there is mention in the late 17th century of the 'Janrawar' caste of weavers, a

name that we do not find earlier than this period. Other occupations mentioned in the European records include 'muckwaes'—a corruption of *machua*, meaning fishermen, but the local variants of this word (*koli*, for example, which is used on the Konkan coast) are not to be found. A term used in the Coromandel region for a messenger was *pattamar*, again, something that is not found for other regions (Seshan, 2012).

More in particular—and this goes not only for India—it is not always easy to envisage by which types of work and under what conditions the majority of the population, that is, those working in agriculture, made a living. For Mughal times, Siddiqi makes a useful distinction between those actually working and tilling the land and those who owned the land and paid revenues to government. The former, in the Persian sources called *mazara*, *asami* or *riaya* (*ryot* in English sources), 'regardless of his having enjoyed or not enjoyed occupancy rights, did not have the right either to sell or mortgage the land tilled by him'. Nevertheless, they had to pay anything from one-third to one-half of the produce to the *muqadams* or *zamindars*, who, in turn, paid part of it, by way of land revenue, to the government.[30] However, for the student of wages, the work now only begins, because within this broad layer of peasants or ryots there are numerous variations. The most important are related to the basic truth that the majority of peasant households lacked sufficient land to make a proper living and, consequently, had to look for supplementary sources of income, mainly by way of cottage industries or wage work locally, or (for men) elsewhere in seasonal agricultural work or as soldiers. In the 19th century, big infrastructural works opened new opportunities and finally also industrial work.[31]

Here, we enter the realm of the household economy, the basic income-pooling and consumption unit, in which all members used their mutual entitlements. Like all other issues discussed here, this is a global phenomenon for which budget studies are crucial, as we have seen, but, at the same time, with many Indian specificities, of which seasonality and restrictions posed by caste, gender and age are the most important ones.

There is a substantial difference in the seasonality of agricultural and other kinds of work between North India and South India.

Whereas in the South regular rains permit nearly continuous cultivation, in the North, it is generally estimated that only half a year is needed for such activities. This leaves a lot of time, but for the (small) peasants also the necessity to look out for other activities.[32] Not all household members, however, were able to participate fully in all kinds of activities.

The most visible were restrictions for women. Because of their early age at marriage (generally between 10 and 12) and the virtual impossibility of remarriage, almost all female labour was performed within the husband's household, that is, that of her father-in-law, and, in a later phase of the life cycle, that of her husband and, finally, her son. But there were more restrictions, especially for the middling and higher castes, who did not permit outdoor work for their womenfolk in the fields or elsewhere. According to the reports of the Royal Commission on Agriculture in India (1926–1928), this taboo was especially detrimental for the income of the middling castes, as the lower ones did not maintain such restrictions.[33] In particular, home spinning lost much of its income-generating potential due to, as we have seen, the massive importation of machine thread from England in the 19th century.

Apart from restrictions on female labour as such, women also received lower wages than men. This is a global phenomenon, but, surprisingly, the so-called gender wage gap in India may have been less extreme than in Europe, as suggested for Bengal in the 18th and early 19th centuries (Zwart & Lucassen, 2020, pp. 662–663). By implication, women's earning capacities might be more important for the household budget, especially of the poorest families in India. All contributions to this volume add evidence to this question, but it is too early to corroborate or deny this assertion. At the end of the 19th century, civil servants and academics engaged in famine relief took a special interest in the work and remunerations of women, trying to find 'objective' criteria in order to determine gendered food requirements and performance. One of the problems they encountered was the close cooperation of husband and wife, which made it virtually impossible to distinguish each other's contribution to the task wages paid to them (see Chapter 8 by Amal Shahid; Jha, 2020).

Finally, modes of payment also matter for the actual spending of wages earned. Here, we must distinguish between wages in cash

and in kind, between time, piece and task wages, between individual and collective payment and between different frequencies of payment, linked to questions of advances and indebtedness.[34] The distinction between payment in kind or in cash matters greatly in times of highly fluctuating grain prices. If paid out in kind, the agricultural labourer is saved from market vicissitudes, but we see that often in times of high grain prices employers shift to cash payments and vice versa (see Chapter 6 in this volume; Keatinge, 1912, p. 67, 71–72; and the Royal Commission on Agriculture in India, 1926–1928). The choice between time and piece or task wages matters not only for the relation between remuneration and performance but also for the need and intensity of supervision—it is more important in the case of time than that of piece or task work, where, in the end, only the amount and quality have to be assessed (Lucassen, 2007; cf. Keatinge, 1912, p. 77).

All 19th- and 20th-century observers of Indian labour agree on the relation between frequencies of payment and the problems caused by advances and indebtedness.[35] Advances have been well documented from the 17th century in the case of weavers. As long as the expected production was not lower than the sum of the advanced money, nothing was wrong. To the contrary, the capability of weavers to command an advance, or even to play off merchants against each other, may be seen as proof of labour power (Parthasarathi, 1998, 2001). When, however, workers accepted advances from employers or moneylenders, they could not be expected to pay back through their work, they lost their freedom, as we have already seen in the discussion on mobility. The eminent agricultural economist and sociologist Radhakamal Mukherjee (1889–1968) was very explicit in his report and oral testimony in 1927 for the Royal Commission on Agriculture.[36]

> Such is the custom of the country in many parts of India that the zamindars, malguzars or ordinary cultivator nearly always contributes to get his servant into debt, and thus obtain a powerful hold over him which extends even to his posterity. Agrarian serfdom is more discernible in those parts of India where the number of the lower and depressed orders is the largest. Bombay, Madras, Malabar, Cochin, the Central provinces, Berar and Chota Nagpur show the largest aboriginal

population, and it is in these areas that the status of the agricultural labourer verges on slavery. The ethnic composition of the village, which governs the social stratification, is thus responsible for the survival of slavery.

The less frequent the wage payments, the more the need to bridge the period without income, and the more likely that a worker had to borrow money. Employers who wanted to be sure of the permanent availability of cheap labour could thus seduce workers to accept money, especially in times of dearness or famine, or on the occasion of weddings or other festivities, which required more money than a worker or small peasant could have saved. This problem was aggravated by the exceedingly high interest rates that could be enforced. We lack a clear picture of the incidence of advances outside the textile industry for the period this volume is concentrating on, as we do for the frequency of disbursement of wages, but it is clear that such information is also crucial for workers' net income and the possibilities to improve it.

A final and perhaps unexpected problem of interpretation arises from all the misery and abuse that we have detailed in this methodological paragraph. Without romanticizing the life of the working woman, man or child, we are therefore at risk of missing part of the full picture—the warmth of human relations that also is part and parcel of work. Rabindranath Tagore, no doubt himself a member of the upper class and not totally free of its prejudices, has nevertheless attempted to catch another aspect of the life of the labouring poor in a poem about the seasonal brickmaker families in Bengal, hailing from somewhere in the United Provinces ('Big Sister', written in 1896).

I.4. INTRODUCTION TO THE SEPARATE CHAPTERS

The chapters collected in this volume are of immense importance in the ground that they break, both in the sources and the regions selected. They have been organized in chronological order, but most of them deal with Western India and the Deccan. This is a pathbreaking exercise, for the sources used by the authors are absolutely new and unutilized by earlier scholarship.

Hélder Carvalhal, Paulo Teodoro de Matos and Jan Lucassen (Chapter 1) examine newly discovered Portuguese sources to draw

a picture of wages and living conditions in the Portuguese territories of Western India in the 16th century. As they point out, this is a crucial century in India's history, seeing, as it does, the consolidation of the Mughal Empire under Akbar in the North, and the establishment of direct maritime contacts between India and Europe. Divided into three broad sections, the chapter first analyses the data, arguing for a substantial real wage hike in the 1540s, and then moving on to a broader analysis, to locate India within the global framework.

Continuing the theme of Europeans in pre-colonial India, Radhika Seshan (Chapter 2) looks at the English in Fort St. George/ Madras in the late 17th century. Concentrating on a smaller time span in comparison to Chapter 1, she examines the wages paid to various officials, both European and Indian, who were employed in and around the English settlement. However, the chapter brings out some previously opaque aspects in terms of the connections between prices and wages, so that, in addition to information about the settlement itself, the special rates for travel out of the city are also included.

With Rekha Ranade's paper (Chapter 3), we move into the first of three chapters dealing with Maharashtra in the 18th and 19th centuries. Based on previously unpublished sources, this chapter examines some of the *Modi* documents that point to the existence of a well-organized fiscal system and of the circulation of money within the Maratha kingdom and beyond through bills of exchange (*hundis* and *varats*). Wages in both cash and kind are also highlighted in the course of the chapter, with details about the timing of payments (monthly/daily) and their form (cash or kind, or a combination of the two).

Surendra Arjun Shirsat (Chapter 4) continues the focus on 18th-century Maharashtra but takes a smaller unit—the Pune *prant* or province. Again, through previously unpublished material, he studies the expenditure and income of the state and argues that the major expenditure of the state was on wages to various officials. Details of the wages, as well as the changes in these wages at the same level of the bureaucracy, are tracked. Particularly interesting are the details about the female slaves/servants employed at the forts and the payments they received.

Anjali Soitkar (Chapter 5) takes up another, smaller region within 18th-century Maharashtra, the area known as Sinnar *pargana* (a sub-unit of a *prant*), near the larger city of Nashik. Again, details of the revenue collected in the region have been highlighted, along with some details of the expenditure, particularly on salaries. All three of these chapters give both an indication of the range of material available in Marathi and the centrality of the state in the economy of the time.

Jan Lucassen (Chapter 6) goes into the 19th century and the beginnings of the colonial rule. Studying the period immediately after the defeat of the Peshwas in 1818, at the time of the existence of what was known as the 'Deccan Commissionerate', he specifically examines one of those appointed to study the region—William Henry Sykes. Sykes' report provides a wealth of information on population, village-level data, sex ratios, castes, urban and rural areas and their population, occupations, and taxation and income generated from that. Most important are his reconstructions of the wage-earning population. An immensely detailed report, the chapter emphasizes the range of information that is available from such early reports, and it would be helpful if, in future, a comparison were made of the kind of information available in the reports of the other commissioners who were appointed at the same time, particularly Pringle (Hatekar, 1996; cf. Kulkarni, 1989).

Dhiraj Kumar Nite (Chapter 7) takes us a little later into colonial times by looking at the wages of construction workers in the Deccan in the 1860s. He demonstrates that while construction workers saw improvements in real income in this period, the increase still fell far short of their requirements. The gap between need and income was sought to be filled by increasing the total family labour offered at the market; clear from his data is that women and children were paid substantially less for the same work. Here, perhaps, we can draw a comparison with something that Vijaya Ramaswamy (2000) pointed out, when she collected the songs of women working in the fields—one of the songs clearly talks about the difference in pay.

The last chapter in the volume by Amal Shahid (Chapter 8) takes us to a different geographical region—the United Provinces. Focusing on the issue of famines and famine labour, Amal Shahid

highlights both the prevalence of famines and the ways in which the colonial state brought in famine-related 'relief measures' in the form of public works. The analysis of wage payments and the work undertaken clearly foregrounds the idea that famine relief wages were predicated on aims to regulate productivity under the colonial state, as well as notions of 'efficiency' and 'waste' of labour.

All these chapters have brought together a variety of sources that help us to flesh out the arguments, expounded at the beginning of this Introduction, of India's lack of representation in the Great Divergence debate, which spans the pre-colonial and the colonial period. We are beginning to identify not just the areas where some of the existing theories can be challenged but also the sources that can be used in order to continue the debate meaningfully. It is with these ideas in mind that we chose to add references to each chapter, with details of both primary sources and additional readings, to help scholars who wish to research more into these aspects. For the same reason, we have also included glossaries in specific chapters.

NOTES

1. The emphasis varies according to culture and periods. For Spaniards in the Philippines and the Portuguese in Asia, for example, the spread of Christianity was a priority; for the Dutch in Asia, it was far less important.
2. For the continuing debate on China's Great Divergence, see Deng and O'Brien (2016) and O'Brien (2020).
3. It should be underlined that wage developments in this debate are conceived as long-term trends, leaving aside short-term hikes, including famines and concomitant high prices of bread grains.
4. Remarkably, Manzar (2021, Chapter 3 for food, clothing and housing prices, Chapter 4 for wages, appendices A–D), while discussing wages in English and Persian sources, does not refer to the Great Divergence Debate.
5. It is not clear to us to what extent Mizushima's urban wage data (1986, pp. 310–319) for Madras 1733–1759 and Pondicherry 1743–1760 support Parthasarathi's conclusions, as he (1998, p. 158, fn 20) quotes only Mizushima's prices.
6. These trends run parallel to the latest population estimates for India, which suggest a slowing down of population growth in the 18th century as compared to the 17th century (our interpretation of Dyson, 2018, Chapter 5).

7. Not used in de Matos and Lucassen (2021). An extensive study about the price history and market regulations of the Chola Empire in especially the 11th century does mention shops and bazaars, as well as 'those who work for hire', 'plowmen' and 'employees' of market authorities (such as clerks, accountants, overseers, sweepers, policemen and market officials), but unfortunately does not provide a single wage notation (Hall, 1994, esp. 70).
8. For newly discovered industrial wages in the 1920s, see also Linden et al. (2020).
9. He quotes from Marx's article 'The British Rule in India' (1853).
10. For earlier persuasive arguments, see Blake (1987) and Ludden (1988).
11. Governmental slaves were paid annual wages, which could eventually turn into savings that enabled them to buy their freedom. Clearly, female slaves employed inside the courts could not belong to an untouchable caste but, beyond this, they could hail from any other caste, except the Brahmin (cf. Shirgaonkar, 2010, Ch. 5, esp. 110; Roy, 2006, p. 65).
12. This is a very rough estimate, waiting for many regional quantitative studies. Furthermore, this is not to deny the large numbers of Indian slaves, exported overseas and across the Hindu Kush (Levi, 2002; Van Rossum, 2021; Van Rossum et al., 2020; Vink, 2003).
13. A few examples: Mizushima (1986, p. 321) for Pondicherry town 1759; Ludden (1988) for Tirunelveli 1823 and Boileau (1837, pp. 223–262) for many towns in Rajasthan and western adjacent regions 1835 (see also Roy, 2006, pp. 221–226 for castes and occupations of Jaipur), examples to be multiplied as the proto-statistical period progresses.
14. For him, see, for example, Van Schendel (1992) and Sivramkrishna (2009). We do not know of a comprehensive overview of Buchanan's or other early household budgets in India. Later, they become an important tool for advocates of improvement. Lots of information may be found in Keatinge (1912), the Royal Commission on Agriculture in India (1926–1928, 1928 [abridged report]) and Maharashtra Provincial Congress Committee (1936); for industrial labourers, see Linden et al. (2020, pp. 105–111, 155–156, 131, 147–150).
15. Parkes (1850, Vol. I, pp. 209–211), not used by Zwart and Lucassen (2020); cf. Chakraborty (2019b).
16. Here, we will only refer to a number of modern studies as they cover much of the earlier literature: Haynes and Roy (1999), Denault (2009), Amrit (2011, 2013, 2014), Ramaswamy (2014) and Tumbe (2012, 2018).
17. Of course, we have to realize that patrilocality involves exogamy and one-time, short-distance, rural-to-rural migration by women (Tumbe, 2018, pp. 34–35).

18. Royal Commission on Agriculture Vol. IV, Part II, pp. 581–583; cf. Vol II., Part II, 187; Vol. VII, 397–398.
19. Some early examples: Parthasarathi (1998, 2016) for South Indian weavers in the 18th century; Sengupta (2016) for construction workers in Calcutta 1758–1761; Lucassen (2012) for gunpowder makers at Ichapur *c*. 1790–1810; Lucassen (2007) for brickmakers at the Ganges Canal 1848–1849.
20. Apart from some of the contributions to this volume, which testify to this, we would like to point to Misra (2014) and Yazdani (2017).
21. A few examples: for Portuguese, see the first contribution to this volume, and de Matos and Lucassen (2019, 2020); for Dutch, see Zwart and Lucassen (2020) and Van Rossum et al. (2020); for French, see the works of, for example, Mizushima (1986) and Koboyashi (2020); for French and German, see Yazdani (2017); for Swedish, see the contribution by Dhiraj Kumar Nite to this volume and af Geijerstam (2004).
22. For the Chalukyas, see, for example, Padigar (2010); for the Palas and the Senas, see Pal (2019) and Ghosh (2005).
23. Here, we will simply refer to the important work of the so-called Aligarh School, see, for example, wage data discussed above in Habib (1994) and Haider (2004, 2007, 2010).
24. *Modi* is a script, not a language, as the language is usually Marathi; it has sometimes been compared to cursive writing in English. However, there are also documents that are in Gujarati language and *Modi* script, and it has been suggested that this form of writing spread in all the areas of the Maratha kingdom in the 18th century. Used extensively between the 17th century and the 19th century for documenting administrative affairs, it was officially given up in 1950, in favour of the Devanagari font. For a survey, see Kulkarni et al. (2014).
25. His reference, though, is not clear. As to the abbé, he paid his own coolies (eight of them) 'three rupees each, which is one and a half ecus, to take me to Bijapur, without being obliged to give them any food' (M. E. Fawcett & C. Fawcett, 1947, p. 226).
26. These archives have been used by Singh (1990) for his study of agricultural production of Rajasthan in the second half of the 18th century.
27. For the Mughal period of Jaipur, important so-far untapped sources are also available in the Marathi/*Modi* collections at Sitamau.
28. For this purpose, the taxonomy of HISCLASS has been developed, see Zwart and Lucassen (2020).
29. Records of Fort St. George for various years; also the Foster edited series, *The English Factories in India.*

30. Siddiqi (1970, pp. 10–20; cf. Ludden 2005). In his testimony before the Royal Commission on Agriculture in India (1926–1928, Vol III, p. 322), George Paddison, the Commissioner of Labour at Madras, warns us against misinterpreting the number of landowners, tenants or labourers in the census:

    The figures are there, but they must be taken with a grain of salt as socially the position of a tenant or a landowner is so much higher that people are inclined to put themselves down as such when for practical purposes they are agricultural labourers.
31. See Chapter 6 by Lucassen and Chapter 7 by Nite in this volume; for soldiering: Kolff (1990) and Yazdani (2017).
32. Extensively documented in the reports of the Royal Commission on Agriculture in India (1926–1928). See also Parthasarathi (1998, p. 89).
33. For example, Royal Commission on Agriculture in India (1926–1928, Vol. II, Part II, p. 186; Vol. IV, p. 341). Religious restrictions occurred, especially against poultry and certain kinds of silkworm breeding (Vol. II, Part I, p. 377, 565; Vol. II, Part II, p. 185).
34. Many examples of time, piece and task wages around 1600, as well of advances to textile workers in the 17th century in Manzar (2021, Ch. 4).
35. The frequency of wage payments is a neglected topic of research. For the emerging industry in India, it was mostly fortnightly or monthly (Linden et al., 2020, p. 56, 121, 137), but what the prevalent frequency in agriculture was is unclear.
36. Royal Commission on Agriculture in India (1926–1928, Vol. VII, pp. 368–422, esp. p. 389, 393–396; quotation from pp. 393–394; cf. Vol. III, p. 314 for Madras; Ludden, 2005).

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