

## BOOK REVIEWS

BRIGGS, ASA and PETER BURKE. *A Social History of the Media. From Gutenberg to the Internet*. Polity, Cambridge 2002. ix, 374 pp. Ill. £55.00 (Paper: £15.99.); DOI: 10.1017/S0020859004011435

At the end of the last century optimism about the so-called Communications Revolution – with the Internet as its mightiest symbol – knew no bounds among some observers. Typical was the message that the journalist Frances Cairncross put across in her book *The Death of Distance*:<sup>1</sup> reduced costs and the increasing speed of means of communication will change our lives radically. She viewed this as a positive development. But there were others who were not quite convinced that this was a good thing. They included the French sociologist Pierre Bourdieu, who in his short essay, *Sur la Télévision*,<sup>2</sup> denounced contemporary television as a medium that would threaten the long-standing (and vital) tradition of critical journalism. Historians did not join in the debate until more recently. Two highly respected academics – Peter Burke (author of *Culture and Society in Renaissance Italy*,<sup>3</sup> and other books on culture in early modern Europe) and Asa Briggs (who has already published a multi-volume history of broadcasting in the United Kingdom<sup>4</sup>) – have now published *A Social History of the Media*, with the subtitle “From Gutenberg to the Internet”. In their preface they reveal that part of the manuscript was written in “long-hand”, that they dislike using e-mail, but that this is “in no way incompatible with an interest in technological and social change”.

In their introduction, Burke and Briggs stress that they want to concentrate on the changes in the media (rather than on the continuities), while at the same time avoiding the trap of seeing these only as a continuous improvement or decline. They therefore dismiss both technological determinism and evolutionary progress as a useful starting point. In six chapters Burke and Briggs discuss such topics as the print revolution, the concept of “public sphere” (Jürgen Habermas’s *Öffentlichkeit*), the consequences of steam and electricity, the development of various communication media (starting with the railways), the use of the media for entertainment and educational purposes, and the concept of “convergence”.

The chapter on the “print revolution”, the changes that were taking place in Europe from 1450 onwards as a result of the invention of the printing press, offers a good example of the approach used by Burke and Briggs. Often seen as one of a trio of inventions (along with gunpowder and the compass) that triumphantly changed the world, the authors point out that there were contemporary observers too who considered printing a catastrophe. Historians also hold different ideas about the effects of the print revolution. The American historian, Elizabeth L. Eisenstein for example, claimed in her *The Printing Press as an Agent of Change*<sup>5</sup> that printing represented “the unacknowledged revolution” and that its

1. Frances Cairncross, *The Death of Distance: How the Communications Revolution Will Change Our Lives* (Boston, MA, 1997).

2. Pierre Bourdieu, *Sur la Télévision* (Paris, 1996).

3. Peter Burke, *Culture and Society in Renaissance Italy*, 1420–1540 (London, 1972).

4. Asa Briggs, *The History of Broadcasting in the United Kingdom*, 6 vols (Oxford, 1961–1979).

5. Elizabeth L. Eisenstein, *The Printing Press as an Agent of Change: Communications and Cultural Transformations in Early-Modern Europe* (Cambridge, 1979).

role as an “agent of change” had been underestimated in traditional historical accounts. For printing made possible the standardization and preservation of a body of knowledge and it encouraged the critique of authority. But other scholars have argued that printing did not fundamentally change Europe’s oral culture. Typically, Burke and Briggs refuse to take sides in this debate but instead point out that each view has its merits. In this particular case, they write that the survival of oral culture is indeed a phenomenon too easily forgotten by the defenders of the revolution thesis. Returning at the end of the chapter to this critical debate, the authors argue that it is “more rewarding to ask what insights each group of scholars have to offer”. While “contextualists deal more satisfactorily with the short-term” and with the role of individuals, the “revolutionarists [...] grapple more closely with the long-term and with the unintended consequences of change”. It is this approach that makes *A Social History of the Media* such a useful book.

Burke and Briggs point out that “old” media coexist with “new” ones. Or, as they put it: when the automobile arrived, there was still a use for the bicycle. They consider the particular uses of those media that did become obsolete – such as the Telefon Hirmondo subscription service that the Hungarian, Theodore Puskas, offered from 1893 onwards: provided with a special headset, telephone owners could listen to a number of “programmes” that we would now associate with radio, such as news bulletins, stock exchange reports, and music being played in the Budapest opera house. This forerunner of cable radio offers a good example of the observation made by the American media historian, William Uricchio, namely the urge to achieve “simultaneity” – an urge which in his eyes was so pervasive in late nineteenth-century thinking about the media.<sup>6</sup> Indeed, “live” radio and television programmes are generally seen as presenting the essence of these media. Uricchio goes so far as to argue that the cinema was an accident, a detour on the road to television. While the latter medium enables spectators to be “simultaneously” present at the event shown on the TV screen, for example the first man setting foot on the moon, the former still requires chemical processing before it can be screened. Although the newsreels, in particular, achieved heroics in presenting reports of soccer matches within a couple of hours in the cinema, inevitably there was always a time lapse. This explains the success too of the digital photo camera, which enables the user to see instantly what kind of picture he or she has taken, and, if need be, erase it (or edit it) and, when satisfied, print it. Again, this is proof of the coexistence of the “old” and the “new” media.

*A Social History of the Media* is full of illuminating details and quotations. Writing, for example, about the influence of cultural studies on thinking about the media and about the seminal work of Richard Hoggart and Raymond Williams, Burke and Briggs mention the fact that both started their careers in adult education. With regard to “convergence”, they point out that this term is defined in dozens of different ways by different authors. My personal favourite is Daniel Boorstin’s interpretation: “the tendency for everything to become more like everything else”. The authors also examine the metaphorical language (like cyberspace or virus) in which observers have tried to couch the development of the media.

This profound knowledge of the subject has its disadvantages too: particularly in the last part of the book, so many different names and acronyms crop up that it is easy to get lost. While Burke and Briggs give ample consideration to the role of remarkable individuals (Edison for example) and corporations (Apple), no mention at all is made of labour and the

6. William Uricchio, *Media, Simultaneity, Convergence: Culture and Technology in an Age of Intermediality* (Utrecht, 1997).

labour movement. These are only minor criticisms however. *A Social History of the Media* is an ideal textbook for students and a lesson to professional historians that they need to take the media seriously as a subject in its own right.

Bert Hogenkamp

BEER, HANS DE. *Voeding, gezondheid en arbeid in Nederland tijdens de negentiende eeuw. Een bijdrage tot de antropometrische geschiedschrijving*. Aksant, Amsterdam 2001. 249 pp. Ill. € 22.65; DOI: 10.1017/S0020859004021431

This book, a Ph.D. thesis defended at the University of Utrecht, is a remarkable historical study by a scholar who is not a professional historian. Hans de Beer trained as a food scientist at the Wageningen Agricultural University. Becoming interested in the history of food consumption, he turned in this study his attention to three problems: the connections between food consumption and labour capacity, c.q. productivity; between nutrition and mortality (levels); and between (food) consumption and standard of living.

De Beer saw these three issues as being economic and demographic. The point of departure for his research was the discussion by scholars like Fogel, Komlos, Engerman and Crafts on the use and value of data on body length. This explains the subtitle of the study: “a contribution to anthropometric historiography”. From the outset, however, it is evident that the value of the book lies not so much in the economic and demographic issues as such, but in the fact that these historical questions connected with food consumption were looked at by someone well educated in food science and its relevant literature. Here the (general) historian is the amateur, and he the professional. Consequently the primary value of this study for the historical sciences is its contribution to and clarification of historical questions through a professional knowledge of nutritional influence on human developments in the past, derived from insights produced by current research by food scientists into actual nutritional issues.

This book is divided into seven chapters. After putting forward the central themes of the study and introducing the most important historians who have written about these in an international as well as a Dutch context, De Beer begins Chapter 2 with an explanation of the concepts he uses as a food scientist. He looks at many of the theoretical understandings and discussions on the value of anthropometric data used by historians in studies on the standard of living. It is in this chapter, for example, that the fundamental question is asked about how relevant knowledge of modern caloric food consumption is for the interpretation of historical data. Here we learn that using body length as such is a problematical yardstick for drawing trustworthy conclusions about sufficient or insufficient food consumption. Other aspects covered in this chapter include the role of protein intake in a person's diet, the possible role of genetic and ethnic factors, the determinants of body growth, the existence of critical life phases for body growth, sicknesses and their influence on growth, and even questions about body growth and emotional wellbeing. These thirteen pages give the historian who is a nonspecialist in food sciences a better understanding of the pitfalls he may meet when he is carrying out research as a layman in this field. Similar complications also appear in the other chapters.

Chapters 3 and 4 form the core of the book. Here, De Beer examines the national evolution of and regional difference in living standards during the nineteenth century from

the standpoint of nutrition and health. He discusses such things as changes in diet and nutritional value, protein and fat proportions in the diet, data on people's height, mortality rates, literacy rates, and real wages. When the standard of living rose in the second half of nineteenth century, did an improved diet play a major role in declining mortality rates and an increase in body height, or were improved hygienic conditions inside and outside the family the decisive factor? Factual historical data are abundantly presented but always penetratingly analysed with the help of knowledge derived from the modern science of nutrition. And that is exactly what makes this study so important.

The next two chapters contain case studies of occupational groups. Chapter 5 discusses the low height of young cigar-makers in the city of Utrecht around the middle of the nineteenth century. Chapter 6 examines the question of a possible increase in labour capacity among workers and artisans during the second half of the nineteenth century, a period with a rising standard of living and better nutritional conditions. De Beer looks at occupations with a relatively heavy labour demand and either none or few technological changes: harvest labourers, brickmakers, peat-diggers, masons and hod-carriers, textile workers, and metalworkers. His discussion, as well as his conclusions, are very illuminating. It is true that young cigar-makers showed a very poor body height, largely because the occupation was not physically demanding. This meant that it attracted physically weak workers and consequently offered low wages. However, other low-paid workers with physically undemanding work, such as tailors, shoemakers, and typographers showed importantly taller body heights than the nineteen-year-old cigar makers. Therefore, circumstances other than food consumption or selection at the age of about twelve years have to be taken in to consideration. De Beer argues convincingly that in the case of the cigar-makers, the harmful effect of tobacco dust and/or tobacco smoke at an age at which the growth spurt had to be performed has to be considered as a main cause of differences in body height with young workers in other physically nondemanding occupations.

The same restricted influence of the (better) general food situation on physically demanding occupations during the second half of the century is discussed in Chapter 6. De Beer's arguments and conclusions are once more intelligent and convincing, and amply sustained by knowledge derived from food sciences. Space does not allow me to state these in detail, but the result may best be summarized in sentences like "we should emphasize that the effects of better nutrition and health on labour productivity or efficiency can only be distinguished with difficulty from other effects, such as changes in the labour process linked to increasing specialisation or better motivated workers"(p.167); and "it seems likely that the evolution of the body length in the past has been more intensively influenced by illnesses, hygiene and care than by nutrition"(p.162).

It is a pity that this praise for the first six chapters of the book cannot be applied to the last one (pp. 147–162). Admittedly, the first section, an international comparison with other countries during the nineteenth century, is a sensible approach. The height of Dutch people around the middle of that century was similar to that of people in Europe as a whole. However, by the end it was among the highest. By the end of the twentieth century, the Dutch were the tallest people in Europe. De Beer's discussion of the causes of these developments and his statistical analysis are very good, and add new insights to our knowledge.

In the second section, however, he seems to have thrown care and caution to the winds. We are confronted with one of the most raw manipulations of historical data that I have

ever met in my career. The section is an attempt to put the Dutch body height of the nineteenth century into a secular perspective. The data on the Dutch population's height from the nineteenth century are compared with earlier ones (before 1800) and later ones (after 1900). The last comparison is perhaps a correct approach from a Dutch food scientist's point of view. However, it is absolutely astounding that De Beer has taken only two pages (pp. 160–162) to do this after devoting about 150 pages to the nineteenth-century situation. Surely he should have allowed a chapter of thirty pages or more to the comparison if he were to present any more than a superficial and senseless exhibition of common knowledge. Its insertion into the book is unworthy of a thesis, unnecessary, and would have been better left out.

Even more fundamentally flawed from a methodological historical viewpoint is his longitudinal comparison of nineteenth-century findings with some data from the centuries between 1200 and 1800 (pp. 155–160). First, the comparison is between data on the average Dutch body height of adults around 1850, 1900, 1965, 1980, and 1997 with data from Delft (1265–1652), Dordrecht (1275–1572) and Leyden (between about 1650 and 1800). All these data from before 1800 are based on palaeographic information obtained from very restricted numbers of skeletons, dug up where? In churches in Dordrecht, Delft, and Leyden? Or in the churchyards? Or somewhere else? The place of burial is indicative of important social-economic differences between the one group and the other. Can that have influenced the measured mean length, based on not more than some dozens of skeletons, undated but spread over a time period of some 100 years? To compare these data with overall data of the Dutch population's body height in the nineteenth century is more than risky. It is senseless as long as a lot of questions remain unanswered and the number of cases are not substantially increased. In earlier chapters De Beer maintained that the body height in the western part of the Netherlands (where data before 1800 come from) was shorter than in other parts. Why did he not compare data from before 1800 with nineteenth-century regional data from that western part? Or, better still, why did he not compare local data from before 1800 with local data from after 1800? Leyden skeletons with Leyden inhabitants and the same for Delft and Dordrecht?

But even the comparison between the earlier data from these three cities is a hazardous one. The differences between cities during the Middle Ages and early modern times may have been as great as between national regions during the nineteenth century. Dordrecht was the most important city of the northern Netherlands in the field of commerce, trade, and transport before about 1500. But the Leyden of the seventeenth and eighteenth centuries was one of the most prominent industrial cities of Europe with a relatively poor population and a different ecological situation. Is it really possible to make any meaningful comparison between these two populations based on a few dozen cases, and then to compare them with the overall Dutch population data from after 1800? It is evident that my judgement on this section can only be that, on the basis of such a methodological piece of bungling, the conclusions of De Beer may be true, but are just as likely to be false. We simply do not know. We can only guess at what caused this blunder by such a serious scholar. He should have been restrained from inserting this unnecessary appendix to an otherwise very useful and convincing study.

This leads me to my last remark. This book discusses a technical professional issue that is also of relevance for foreign researchers. Given the quality of the study and taking into account that, while few Dutch people are working in the field of the history of nutrition there are many foreign colleagues interested in the topic, I believe this book may be a

useful example and guide for them once it is translated into the lingua franca of our day – English. It should be translated. But with the deletion of chapter 7.

*Ad van der Woude*

SCHECHTER, RONALD. *Obstinate Hebrews. Representations of Jews in France, 1715–1815*. [Studies on the History of Society and Culture, vol. 49.] University of California Press, Berkeley [etc.] 2003. viii, 331 pp. \$60.00; DOI: 10.1017/S0020859004031438

Voltaire was obsessed with Jews. Variants of the French word for Jew and Jewish occur 2,360 times in the *Encyclopédie* (by comparison, the number of references to English people and English things is only slightly higher) and during the tenure of the National Assembly from 1789–1791 issues relating to Jews were discussed at no less than 32 sessions. All this in a country whose 40,000 Jewish inhabitants formed one-fifth of 1 per cent of the total population and possessed no real power whatsoever. Various studies have been written about the Jews in Enlightenment, Revolutionary and Napoleonic France, and all except one<sup>1</sup> have overlooked the basic question that Schechter tries to answer in his book: why was so much attention given to such an unimportant, and small minority?

Jews were “good to think”, as Schechter puts it, following Claude-Lévy Strauss’s explanation of totems (p. 7). In analysing the texts of the *philosophes*, writers, revolutionaries, and reformers, he convincingly argues that Jews facilitated the conceptualization and articulation of a number of ideas that were of special importance in the age of Enlightenment and Revolution, such as the merits of agriculture and commerce, fanaticism (religious and otherwise) and tolerance, the meaning of citizenship, the possibility of an indivisible “nation” containing public-spirited citizens, of pluralism and, most of all, of human perfectibility – to be achieved by regeneration.

Other groups of outsiders (“others”), including native Americans, blacks, and women, were also used for this purpose, and in his conclusion Schechter makes some interesting comparative remarks on how these groups too have been represented. Unlike the native Americans and blacks, Jews knew about Christianity but “obstinately” refused to accept it. It was precisely this obstinacy that made them a test case for the possibility of an indivisible, universal community (p. 251). Moreover, most Enlightened and Revolutionary writers generally saw the physical, moral, and political deformation of Jews not as innate (in contrast to the physical deficiencies of women, which were inescapable) but as the consequence of reversible factors, which made it possible to regenerate Jews. It was this supposed malleability that made Jews such a good group “to think”.

Other factors determined the prominence of the Jewish question in France before, during, and after the Revolution. Whereas the assimilation of blacks would compromise the slave economy, and serious discussions about the citizenship of women might lead to the conclusion that men ought to share power equally with them, “one could conduct thought experiments with the Jews at little risk to the economic or political dominance of white, male Gentiles” (p. 248). Also, Jews were “known” from the Bible, written

1. Gary Kates, “Jews into Frenchmen: Nationality and Representation in Revolutionary France”, *Social Research*, 56 (1989), pp. 213–232.

knowledge, and folklore which made their allegorical use easier and their symbolic significance comprehensible.

In analysing the texts of Voltaire, Montesquieu, and the Encyclopaedists, Schechter shows that Jews were used as a vehicle in writings about the evils of religious fanaticism and the merits of natural religion. He stresses the contradictions within these writings, showing, for example, that Jews were sometimes depicted as victims and sometimes as perpetrators of religious fanaticism. Schechter argues that these contradictory messages were part and parcel of the contradictions within Enlightenment and Revolutionary thought itself. Schechter uses well-known texts (such as the prize-winning essays submitted in response to the Metz Royal Academy of Sciences competition on the question “Is there a way of making the Jews more useful and happier in France?”), but also less familiar ones to show the reader how Jews were used to illustrate what people thought about Frenchness and citizenship. The notes of the barrister Pierre-Louis Lacratelle, who defended two Jewish merchants from Metz whose royal licence to establish shops the local *juge de la police* refused to recognize, are among the less well-known but very interesting texts that Schechter analyses.

Schechter not only analyses representations of Jews by others, he also describes Jewish self-representations, using them as “contrapuntal readings” in the same way that Edward Said read postcolonial literature in “contrapuntal” relation to European writing on non-Europeans (p. 276). For this purpose, Schechter analyses Jewish apologetic texts (such as the essay Zalkind Hourwitz wrote for the Metz competition) but also less common sources, such as Hebrew poems and the descriptions of the celebrations of royal marriages in the Jewish communities in France and the “anointing” of the King by rabbis, calling these festivities “patriotic liturgy”. In doing so he identifies strategies of Jewish self-representation.

Rather than choosing between assimilation or resistance to the powerful French culture of the Enlightenment, Jews internalized these ideals and made them their own, Schechter argues. In a *Lettre*, Isaiah Berr Bing, a prominent member of Metz’s Jewish community, stated that in Judaism reason and religion concurred to teach that all people have a common right to the eternal bounty of the Creator (p. 120). In doing so, Bing assimilated Judaism to the natural religion of the *philosophes* by characterizing it, above all, as a religion of justice. Schechter gives various other convincing examples of these strategic self-representations of Jews.

In *Obstinate Hebrews* Schechter presents us with an original view, based on a very thorough knowledge of the content and context of a wide variety of well-known and much less familiar texts on and by Jews in France. Unlike Arthur Hertzberg, author of *The French Enlightenment and the Jews*,<sup>2</sup> and other authors of studies on the Enlightenment, the Revolution, and Jewish emancipation, Schechter explicitly says that he does not want his view to be influenced by a retrospective judgement of whether these phenomena were good or bad for Jews. In his introduction he even asks his readers to forget everything they know about French-Jewish history.

This new look occasionally leads Schechter to over-interpret somewhat and to overlook a few small details. When he tries to explain the shared background of the spokesmen of the opponents of Jewish emancipation within the National Assembly for example, he looks in vain at their political convictions. The real explanation is often given and much simpler: all

2. Arthur Hertzberg, *The French Enlightenment and the Jews: The Origins of Modern Anti-Semitism* (New York [etc.], 1990).

the opponents who took the floor, whether they were conservative clerics or revolutionary members of the third estate, came from the Alsace-Lorraine region and its surroundings, where the largest, poorest, and most visible Jewish community in France lived. However, these are minor points of criticism of an impressive work of scholarship.

*Karin Hofmeester*

BOS, DENNIS. *Waarachtige volksvrienden. De vroege socialistische beweging in Amsterdam 1848–1894*. Uitgeverij Bert Bakker, Amsterdam 2001. 444 pp. Ill. € 17.92; DOI: 10.1017/S0020859004041434

One involuntary consequence of the programmatic turbulence in the field of social history in the past two decades is the fact that the history of the socialist labour movement is now out of fashion. Like all human beings, historians are very receptive to the fetishism of the new. It has been pointed out – especially in Germany – that this has resulted in “the alarming disappearance of knowledge concerning the labour movement”. A cry in the dark perhaps? Conducting new and stimulating research on the socialist labour movement will certainly be much more effective in helping to rescue the subject from its old-fashioned image. Although this was certainly not the author’s intention, his outstanding analysis of the origins and development of the socialist labour movement in the city of Amsterdam during the second half of the nineteenth century makes it, indirectly, a kind of manifesto – a clarion call to rehabilitate the history of the socialist labour movement. Leaning heavily on Clifford Geertz and Carlo Ginzburg methodologically, Bos was inspired especially by earlier local studies on the movement in Flushing by Bert Altena (1989), and in Ghent by Guy Vanschoenbeek (1995), and in terms of quality his book is very much their equal. One can only hope that the resonance of “Amsterdam” will ensure that Bos’s story does not remain as underestimated as those by Altena and Vanschoenbeek, because these “microstories” can be a great inspiration to social historians. However, because one really has to read this type of research before it can be “used”, and since Bos’s book is available only in Dutch, the chances are that it too will remain a cry in the dark.

That would be a pity, for *Waarachtige volksvrienden* – which translates literally as “true friends of the people”, who, in all their diversity, shared an aversion to the ruling powers and preferred to address themselves to the working people – convincingly demonstrates that the Amsterdam socialist labour movement originated and acquired its specific form, size, and content as a consequence of human actions and individual options. Of course, economic parameters and the evolution of the movement in other counties and cities – in the case of Amsterdam the influences were mainly German and Flemish – had an impact, but the way they had an impact depended on the context in which they operated. By reconstructing the social networks that interconnected the *Waarachtige volksvrienden* and by pinpointing the significance of these relations, Bos offers us a well-documented description of the social, mental and cultural milieu of the Amsterdam socialists. This not only rehabilitates the crucial importance of friendships, family ties, and neighbourhoods, but clearly demonstrates too that personal honour and shame motivated early socialists at least as often as ideology and organisation or self-help did. At the same time, in his opening chapters Bos details the importance of the international network of predominantly German artisans who imported early socialist ideologies into the nascent Amsterdam workers’ movement.



Notwithstanding the attractive style in which the book is written, one might sometimes feel overwhelmed by the snow shower of details which can obscure the point the author is trying to make. This is rather common in historical literature that takes a descriptive approach. A selective reading of Bos's study might suggest an image of the early socialists as a "wild bunch", lacking discipline, organizational skills, ideological clarity, and political determination. In short, a group of individualistic dreamers, preoccupied with local problems and mostly destined to achieve nothing. After founding their own party, the SDAP, in 1894, parliamentary socialists soon became the dominant current in Dutch socialism. Although the SDAP remained small for a long time – with some 3,200 members in 1900, reaching 9,500 by 1910 and 25,000 by 1914 – and although the historical literature on the socialist movement in the Netherlands is rich and extensive, despite its size this "modern" movement blurred completely the historical image of its predecessors. However, this apparent weakness is precisely the main merit of *Waarachtige volksvrienden*, for the snow shower of details serves to help analyse the early socialists of Amsterdam in their own right and not merely as an unsuccessful prelude to later socialist successes. That is precisely what makes this book compulsory reading for all those interested in the history of Dutch socialism.

Hendrik Defoort

SILVER, BEVERLY J. *Forces of Labor. Workers' Movements and Globalization since 1870*. [Cambridge Studies in Comparative Politics] Cambridge University Press, Cambridge 2003. xv, 238 pp. £45.00. (Paper: £16.99); DOI: 10.1017/S0020859004051430

This book is an ambitious attempt to oversee global labour history over the past long century, based on an interesting database – the World Labor Group database. This dataset was put together at Binghamton University's Fernand Braudel Center in the 1980s. It records 91,947 cases of labour unrest culled from the indexes and pages of the *New York Times* and the *London Times* in the years 1870–1996. A number of tests – reported on in a 1995 issue of *Review*, the journal of the Center<sup>1</sup> – satisfied the team involved in compiling the database that these two newspapers covered labour unrest in different parts of the world well enough to create a global view. On the whole, the treatment of the methodological problems involved in using a database of this kind is convincing. For instance, to counteract the fact that each newspaper covers home affairs more thoroughly than international news, the American cases are taken from the *London Times* and the British ones from the *New York Times*. The database will therefore cover exceptional cases better than ordinary ones. Silver argues that this is not a disadvantage, since it means that turning points receive greater attention. Some unavoidable drawbacks are mentioned in the text. For instance, one type of labour unrest included in the database is resistance to

1. Beverly J. Silver, Giovanni Arrighi, and Melvyn Dubofsky (eds), "Labor Unrest in the World-Economy, 1870–1990", *Review: A Journal of the Fernand Braudel Center*, 18 (1995), esp. part 2, "Country Reliability Studies", pp. 49–151, with contributions on Italy (Giovanni Arrighi), China (Mark Selden), South Africa (Mark Beittel), Argentina (Roberto P. Korzeniewicz), Egypt (Donald Quataert), United States (Melvyn Dubofsky), and Germany (John Casparis and Giovanni Arrighi).

proletarianization. This was manifest both in Europe and its colonies in the late nineteenth century. The newspapers often referred to these actions in the colonial world as “native unrest”, a category not recorded in the database. As a result, the degree of labour unrest in the late nineteenth-century colonial world will be somewhat understated. Even so, the World Labor Group database is clearly a very useful tool, as is evident from the way it is used in Silver’s study.

For the textile industry, Silver constructs a cycle that resembles the product cycle. However, in contrast to product-cycle theory, where product innovation is the push factor, here two social variables, namely working-class formation and protest, are central to the development of production. When industrial textile production was established in England, the level of labour unrest rose immediately. The windfall profits that this innovation brought to British textile entrepreneurs also created scope for some successes by the textile unions, albeit after the crushing defeat of earlier craft-based trade unions. This defeat opened the doors to the introduction of power looms and self-acting mules. By the time this phase drew to a close, the trade unions of the minders of the new machines had become a power to reckon with in the labour market. Before too long, the new textile production technology had spread from Lancashire all over the world. This led to labour unrest wherever the textile industry was established, peaking in the interwar period. However, even if textile workers were militant, their actions usually ended in defeat. Employers were able to employ both what Silver terms a technological fix and a spatial fix. A technological fix entailed a technological innovation, such as supplanting mule spinning by ring spinning, which allowed employers to achieve the same level of production with fewer or less-skilled workers. Attempts at rationalization, which reduced the reliance of textile firms on skilled male adult workers, added to the militancy of textile workers. The spatial fix entailed moving textile mills to those parts of the world where wages were low and workers unorganized.

The automobile industry is the other sector Silver analyses at some length. Automobile workers had more workplace bargaining power than those in textiles: their jobs demanded more training and a strike in one section of the plant was more damaging to the firm. Sit-down strikes and plant occupations were weapons automobile workers could use with much effect. Here, too, the profits generated by the first round of innovations made it possible to remunerate workers relatively well in US car factories. Firms tried to become less dependent on organized workers by cooperating with the unions, but also by automating, and by moving firms to areas where wages were lower. In this way the industry moved from the US, via north-west Europe, southern Europe, Argentine, Brazil, South Africa, and Mexico to South Korea. Labour militancy followed each of these moves, but not the move to Japan, where firms employed a complex subcontracting system and pampered a core labour force with guaranteed employment. Automobile companies elsewhere tried to employ these Japanese strategies without creating a privileged core labour force.

In this way Silver offers coherent accounts of global labour relations. After its first round of innovations, the leading sector of the economy establishes a compact with workers and their organizations, under which the workers share, to some degree, in the profits generated by the industry. To counter trade-union power, firms adopt further innovations that demand less-skilled workers (the product fix) and move to areas where the labour movement is weak and wages are lower (the spatial fix). Silver can illustrate these moves because labour unrest followed these relocations.

But the database can illustrate more. An aggregate analysis of all labour unrest shows the impact of global politics on labour relations, and vice versa. On a global scale, both world wars showed the same relationship in terms of labour unrest. Labour unrest was on the rise in the years preceding the war, contributing to the unstable situation that led to the wars. The initial years of the war showed a steep decline in labour unrest as the early war effort drove class struggle from the agenda. Soon, however, labour unrest was on the rise again; this rise was not halted by the end of the war, but continued for several years after the war, resulting in twentieth-century peaks of unrest in both postwar periods. In terms of global labour unrest, the peak in 1920 was about as high as the 1948 peak, but Silver also offers a breakdown between metropolitan countries and the colonial and semicolonial world. Whereas in the metropolitan countries, which accounted for about three-quarters of all labour unrest in peak years, the postwar peaks were of about equal size, labour unrest in the colonial and decolonizing world was both much greater and lasted longer after World War II than after World War I, fuelled by and in turn fuelling nationalist movements in the Third World. This combined agitation led employers to conclude that they had to accept some reforms to *laissez-faire* capitalism. These reforms, combined with US hegemony and the restructuring of capitalism, explain why labour unrest that had been rising and explosive in the first half of the twentieth century became nonexplosive and declined in the second half of the century.

Finally, Silver questions whether the decline in labour unrest will be final or whether another sector will take the “lead”, as textile or automobile workers did in the past. The mutual stimulation of nationalist and anticolonial struggles and labour unrest after World War II points to an important argument in this context. If workers have weak bargaining power on the shopfloor, they must develop strong associational power, either through their own trade unions or through the labour movement more generally. Hotel workers, for instance, are usually weak and acquiescent on the shopfloor, but they become more active in times of local labour unrest. This is relevant because the service sector may well be the sector most likely to take the lead in terms of unrest. Within manufacturing, Silver sees the semiconductor industry as the only possible candidate, but this industry emerged almost simultaneously in rich countries and in low- and middle-income countries alike, where older industries such as textiles and automobiles have also become concentrated. This limited the potential for successful labour actions in high-wage countries.

The service sector, including transportation and communication, accounts for more than 50 per cent of world labour unrest since the 1940s, and is also the main locus of working-class formation. Transport workers have strong workplace bargaining power. Within the transport industry, unrest among railway workers has remained important. Shipping and docking is in decline, because the container acted as a technological fix. Labour unrest in aviation has risen, especially since the 1970s. In transport, the role of governments in regulating labour relations has been relatively great. The same holds true for the education sector, where waves of labour unrest are recorded from the 1940s, both in high-income countries and elsewhere. Education is a sector where workers have strong bargaining power, since they are immune to both technological and spatial fixes.

Silver’s analysis is supported by tables and graphs derived from the database. These are very interesting, but they also leave questions unanswered. It is not always clear whether an argument in the text is derived from the database or elsewhere. The author’s claim that protests among textile workers were usually unsuccessful is not substantiated. As far as one can tell from the description given of the database, information on the outcome of labour

unrest was not collected from the newspapers. Were textile workers, with their weak shopfloor bargaining position, globally less successful than dockers, who had a strong bargaining position? Did aviation labour unrest really grow to account for almost 70 per cent of transport labour unrest in the 1970s, or did this just reflect the fact that newspapers are more partial to anything relating to air transport? Silver's book is a strong argument in favour of a global analysis of labour relations which takes questions of technology and political power seriously. Even if some methodological questions remain, it also argues convincingly for the usefulness of the World Labor Group database as a tool in answering questions about global labour unrest. It would be very useful if the Braudel Center could make this tool available to the scholarly community at some stage.

*Lex Heerma van Voss*

NEWMAN, MICHAEL. *Ralph Miliband and the Politics of the New Left*. The Merlin Press, London 2002; Monthly Review Press (USA); Fernwood Publishing (Can.). xiv, 368 pp. Ill. £15.95; DOI: 10.1017/S0020859004061437

The British political thinker Ralph Miliband (1924–1994) became an international figure largely as a result of his two main achievements: his editorship, along with John Saville, of *The Socialist Register* from 1964 onwards; and his book, *The State in Capitalist Society* (1969) – and the ensuing debate with the Franco-Greek Marxist, Nicos Poulantzas. Through his publications, speeches, and educational activities in Britain, the United States, and Canada, Miliband had a considerable impact on left-wing political thinking in the English-speaking world, particularly in the 1960s and 1970s. It is not altogether surprising then that Michael Newman should have wanted to write a biography of Miliband.

Newman admires Miliband's variant of socialism – “a socialism which was Marxist but not Stalinist or Trotskyist, democratic without being Social Democratic, realistic without succumbing to ‘realism’, and which contained a vision without being ‘visionary’” – and wants to contribute to it in order to sustain it into the twenty-first century, “for it is certainly as necessary as ever” (pp. 2–3). However, Newman's excessive deference has resulted in a fairly enervating and sometimes misleading book.

Based on Miliband's personal papers and a great many interviews with those involved, Newman writes informatively of the various stages in Miliband's life. The narrative structure is straightforward. The first two chapters describe Miliband's youth up to the mid-1950s. The rest of the book is constructed around the development of Miliband's political ideas and his most important published works, including *Parliamentary Socialism* (1961), *The State in Capitalist Society* (1969), *Marxism and Politics* (1977), *Divided Societies* (1989), and *Socialism for a Sceptical Age* (1994).

Newman's description of Miliband's youth is by far the liveliest part of the book, since it offers a degree of insight into certain experiences that would continue to play a role in Miliband's life. We learn that Ralph Miliband was born Adolphe Miliband to Polish Jewish parents in Brussels in 1924, that he and his father fled to Britain in 1940, and that Ralph rapidly felt at home there, soon diving into left-wing politics. We also learn several things about how he viewed himself, and how he developed his arguments: “[H]e would begin with an apparently ‘dogmatic’ assertion, anticipate an attack on it, and modify the original statement, thereby both pre-empting the counter-argument and making his case more persuasive” (p. 33).

After this fairly interesting opening, the narrative takes on a cyclical nature. Miliband works on a manuscript – discusses it with others – publishes the manuscript – eliciting reactions from others – and then begins work on his next manuscript. In the meantime, he becomes involved in various aspects of the broader political debate, supports various committees, and is in regular contact with other men of learning and political activists. The average life of a left-wing intellectual you might say, and not really something warranting 300 pages.

Newman's book might have been more interesting if the author had not described his main character so one-dimensionally and if he had been more receptive to the contradictions and inconsistencies in Miliband's life. But this would have required Newman to take a more critical view of Miliband. Take, for instance, the issue of communism. In autobiographical notes dating from the 1980s Miliband wrote: "[T]he position I have always occupied is that of an independent Marxist, unattached to any party save for a few years in the fifties when [...] I was a member of the Labour Party" (p. 61, n. 40). The facts presented by Newman give quite a different impression. In 1941, as a young student in Cambridge, Miliband approached the Secretary of the Communist Party branch, "telling him that he had been in the Belgian Young Communist League and was seeking a close association with the British party" (p. 20). In 1943, Miliband spoke of the Soviet Union as "the country of the workers and peasants, of the common people" (p. 32). In 1947, under a pseudonym, he published an article in *Le Drapeau Rouge*, the Belgian communist daily newspaper (p. 49). In 1956–1957 "he believed that, despite its weaknesses, the Soviet bloc still represented a genuine path to socialism" (p. 79; see too p. 83). During the 1960s Miliband "had remained moderately optimistic about the possibility of a gradual evolution in the [Soviet] system" (p. 139). It was only the crushing of the Prague Spring that led to a "definite shift in his position" (p. 142).

Miliband initially seems to have been a communist but without being a party member, and he had to struggle for many years before adopting an independent position. This idea is anathema to Newman though, since it cannot be reconciled with Miliband's own autobiographical notes. Newman therefore claims that, despite appearances, Miliband was "an independent thinker" as early as 1941, and that "probably" he would have "found the [communist] organization and discipline unacceptable" (pp. 20–21). And when Miliband made an extensive visit to the Soviet Union in 1961 at the invitation of the Central Committee of the CPSU, Newman offers the following apology: "he seems to have let his normal guard of scepticism drop a little" (p. 81).

Newman is just as apologetic when it comes to gender relations. As a twenty-one-year-old, Miliband regarded the marriage between Karl and Jenny Marx as a perfect example of a husband-and-wife relationship: he was an academic and revolutionary; she "never complained" (p. 34). Newman offers some critical remarks on this passage, but immediately starts by trying to find excuses: "[H]e was living in a different era, was young and unexperienced, and these were his private thoughts" (p. 34). This is all true of course, but the interesting question is how this left-wing thinker responded to the women's liberation movement later on. Were his "private thoughts" fixed, regardless of what he might have said in public? A few quotes suggest that, even at the end of his life, Miliband had scarcely any appreciation of the feminist struggle (p. 290), and that, in this respect at least, his thinking represents a continuum. Newman fails to explore this issue at any length. Miliband's variant of socialism certainly deserves serious attention, but by giving insufficient consideration to the complexity of such learning processes and the limits

of these processes Newman's book – which is dedicated incidentally to Miliband's widow Marion Kozak – borders on the hagiographic.

*Marcel van der Linden*

PARREÑAS, RHACEL SALAZAR. *Servants of Globalization. Women, Migration and Domestic Work*. Stanford University Press, Stanford (Cal.) 2001. xi, 309 pp. \$55.00; £40.00. (Paper: \$18.95; £12.95.); DOI: 10.1017/S0020859004071433

This book provides an important contribution to the literature on diasporas, transnational identities, and citizenship. It is innovative in scope and breadth and breaks away from traditional single-site ethnographies characteristic of traditional anthropological studies. Following the more recent spate of studies working beyond bounded entities, Parreñas uses a multi-locale ethnography, involving two different sites, Rome and Los Angeles. This comparison, which forms the core of her study, becomes the methodological instrument through which Parreñas views the structure–agency relationship and in which she highlights the experiences of Philippine domestic migrant workers in divergent locations and structures. Although Parreñas underlines the importance of “agency”, where individuals are able to create their own space and room for manoeuvre, she stresses that “the subject” (which she distinguishes from “the individual”) is nevertheless strongly influenced and constrained by larger forces. This ambiguous and often contradictory relationship between structure and agency, a situation which confronts all researchers, is tackled by Parreñas through using a multi-tiered analysis, involving three levels which she refers to as the macro-, intermediate- and subject-levels.

She identifies, first of all, the global processes influential in determining who migrates, where they migrate to, and how they are received in the host countries. This involves looking at the overarching world-system that organizes nations into unequal relations which create corridors through which sending and receiving countries, intentionally and unintentionally, develop structural linkages. At the intermediate level, she addresses the questions which macro-level discussions cannot answer, namely: why migration flows are concentrated in specific communities; why they persist after the initial causal factors of migration have eroded, and why there is a specific gender and class constitution to migration. At the subject level, she examines the specific position of subjects in sets of relationships and intersecting axes of domination. Underlining again the strength of structural constraints, she emphasizes that, although each subject is involved in everyday and “immediate struggles”, they are at the same time recreating structural inequalities.

Although Parreñas gives ample space to the changing policies of the receiving countries (and also of the Philippines itself) regarding international migrant workers and to the institutions involved in channelling migrants across national boundaries, her main arguments are based on the level of the migrant subjects. It is particularly in dealing with the women's experiences with their employers, in their emotions and imaginings regarding their children and family in the Philippines, and in their relationships with fellow workers that Parreñas puts her ethnographic material to full use. The agency of these Philippine domestic workers is tested, constrained, and shaped by the structures of unequal relations between the different nation-states (the Philippines versus Italy and the

US), by the different racial and gender composition of the receiving countries, and by the different social networks and work situations evolving from these different contexts.

The choice of the two different sites adds a highly significant and interesting dimension to her argument. The crucial element distinguishing Los Angeles and Rome is the timing of the migration flows and the different conjunctures under which this occurs. Migration to Los Angeles had already started in the early twentieth century, whereas the flow of women to Rome only started since the 1970s. This led to different racial and class compositions of the migrant communities at the place of destination. In Rome there is a smaller number of Philippine migrants than in Los Angeles. Their legal position is weaker since they cannot obtain citizenship (following the *ius sanguinis* prescription) and there is a larger degree of xenophobia. The majority of Filipina migrants are domestic workers, and compared to the domestic workers in Los Angeles, there is a larger number of part-timers. Despite the racial segregation between immigrants and the “host” community, Filipina women are higher on the scale than other immigrants. They are paid more than the Latinas, Africans, and even the Polish women. These high wages, however, do not allow for better integration into the community so that they still live in segregated pockets in the municipality. The meeting spaces they create in the urban public sphere are still distinctly separated from the public spaces of the dominant community. Contacts among them are based in remittance centres and public transport centres, but most of all in the bazaar and the church. These contacts are cemented by forms of mutual assistance arrangements and micro-enterprise, such as the rotating credit association, money lending, and the sale of various consumer goods. Such activities nurture not only collective bonds but also what she terms as “anomie”, a condition where competition and tension among community members prevail.

In Los Angeles, the older tradition of migration into the country has resulted in a larger Philippine community, with more diverse occupations and therefore a higher degree of differentiation within the Philippine community itself. This has shaped the relations among Filipinas who work as domestics. Because spatiality does not distinguish membership in the community, as the Philippine migrants in Los Angeles are residentially more spread out and more integrated into the dominant community, there is also less attempt to maintain contact among domestics in the different parts of the city. Feelings of inferiority and hierarchy are caused, not by their social gap with the host community but by the gulf between themselves and the middle-class Philippine families for whom they work.

Parreñas also examines the immediate struggles which migrant workers face in their social and work relations. She identifies three different positions in which migrant workers find themselves: as part-time workers, as live-in workers, and those specializing in care of the elderly. The part-time worker is paid by the hour and has more control over her work. This means also that the employer has less control over her life and self-perception, because the worker is less isolated from her own social networks. Live-in housekeepers are those whose work includes house-cleaning and child care. Their more extended presence in the employer’s household allows a less rushed routine in the implementation of their tasks but at the same time they lead a more isolated existence. Those who are involved in care of the elderly are considered more respectable and have more autonomy but experience a large degree of loneliness and isolation. This distinction between the different types of paid domestic work is a good reminder of the differentiation existing among domestic workers themselves. However, this illustration would have been analytically richer had the author placed these diverse relations of reproduction within the different structural (and cultural) contexts of Rome and Los Angeles.

Despite the clear differences between the two research sites, in the analysis of work and family relations, and in her conclusion Parreñas gives more priority to the general rather than the specific, leaning more on the similarities and the parallel lives of these domestic workers than the differences. They are particularly characterized by their status as partial citizens leading dislocated lives, and the diverse struggles in the different cultural and political spaces are placed in the background. Because the structural dimension weighs heavily on the subjectivities and the spaces that domestics create for themselves, Parreñas is consistent with her earlier statement about agency, in that “the subject” is not free to do as she wants, although she has some space to manoeuvre. One wonders how this argument would look, had the author integrated her comparative findings more thoroughly into her analysis.

Also, the way Parreñas handles the concept of the household, which she refers to as “postindustrial household structure with preindustrial values”, deserves some comment. She argues that migrants are only able to form transnational households because of cultural resources that instil collectivization in the family, embodied in the term *utang na loob* (“debt of the soul” or life-long debt). Much of the existing literature of the last decade on family and household relations has stressed the tensions and conflicts which coexist with common interests. However, this does not seem to be reflected in this study. Indeed, although Parreñas underlines the ambiguous phenomena of solidarity and conflict among workers themselves, she does not push this further to look at similar ambivalencies between employer and employee, between workers and their family members. Would the ethnography look different if the author had searched more for difference rather than commonality? This is a question that can be raised not only for this otherwise important and refreshing book, but also for most of us who have to deal with the articulations between the global and local, the general and specific in a balanced fashion.

*Ratna Saptari*