

People, Places and Business Cultures

Essays in Honour of Francesca Carnevali

Edited by

Paolo Di Martino, Andrew Popp and Peter Scott



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Contributors

Andrea Colli has a Ph.D. in Economic and Social History (Bocconi University, Milan) and is Professor of Economic History at the Department of Policy Analysis and Public Management, Bocconi University, Milan. His research interests range from the history of family firms, to small and medium-sized enterprises, to the role played by international entrepreneurs and firms in the global economy, and to corporate governance in historical perspective. He has also devoted research activity to the study of the history of entrepreneurship in different contexts.

Paolo Di Martino is Senior Lecturer in International Business and Economic History at Birmingham Business School, University of Birmingham (UK). His research interests are financial history, business history and the history of legal institutions. He has published extensively in edited books and journals, including *Business History*, *Economic History Review* and *Enterprise and Society*. He is an active member of the Economic History Society.

Leslie Hannah lives in Japan and is a visiting professor at the London School of Economics. He previously held posts at various American, Asian and European universities and was pro-director of LSE and dean of two business schools. He was Francesca Carnevali's Ph.D. supervisor and co-authored an article with her, and also published a history of Barclays Bank, jointly with Margaret Ackrill. His writings are now focused on the comparative business history of twentieth-century Europe, America and Japan.

Matthew Hilton is Professor of Social History at Queen Mary University of London. He has published widely on the history of charities, social activism, consumption and NGOs. His most recent books are *Prosperity for All: Consumer Activism in an Era of Globalisation* (Ithaca, NY, 2009) and, with James McKay, Nicholas Crowson and Jean-François Mouhot, *The Politics of Expertise: How NGOs Shaped Modern Britain* (Oxford, 2013). He has co-edited several collections of essays, including *The Ages of Voluntarism* (Oxford, 2011) and *Transnationalism and Contemporary Global History* (Oxford, 2013). He is co-editor of *Past & Present* and is currently engaged on

a history of British approaches to humanitarianism. The article presented in this volume is the product of ongoing conversations between members of the Centre for Modern British Studies at the University of Birmingham.

Kenneth J. Lipartito is Professor of History in the Department of History at Florida International University. He has been a Newcomen Fellow (1991) and a Thomas McCraw Fellow (2009) at Harvard Business School. He is the author or editor of six books, most recently, *Corporate Responsibility: The American Experience*, published in 2012 by Cambridge University Press. His articles have appeared in the *American Historical Review*, *Journal of Economic History*, *Technology and Culture*, *Industrial and Corporate Change* and the *Business History Review*. He was editor of *Enterprise and Society: The International Journal of Business History* (2003–07) and President of the Business History Conference.

Lucy Newton is Associate Professor in Business History in the School of International Business and Strategy, Henley Business School, University of Reading. She has published her work on financial history and nineteenth-century consumer durables in a variety of business history journals. She has been an active member and Trustee of the Business History Conference (USA) and twice elected as Council member of the Association of Business Historians (UK).

Andrew Popp is Professor of Business History at the University of Liverpool Management School. He has published on a wide range of topics in British business and industrial history, principally of the nineteenth century. His most recent book is *Entrepreneurial Families: Business, Marriage and Life in the Early Nineteenth Century*, published by Pickering & Chatto in 2012. He is Editor-in-Chief of *Enterprise and Society: The International Journal of Business History*.

Alberto Rinaldi is Associate Professor of Economic History at the University of Modena and Reggio Emilia. He has published extensively on contemporary Italian economic history, focusing in particular on industrial districts, trade, economic growth and the structure of the corporate system. His works are published in leading international journals, such as *Explorations in Economic History*, *Cliometrica*, *Business History* and *Enterprise and Society*.

Peter Scott is Professor of International Business History at Henley Business School, University of Reading. He has written extensively on the history of British and American mass retailing; consumer durables sectors; household consumption; housing; and related topics – mainly for the early twentieth

century. His most recent book, *The Making of the Modern British Home: The Suburban Semi and Family Life Between the Wars*, was published by Oxford University Press in 2013.

Anna Spadavecchia is Associate Professor at Henley Business School, University of Reading, and gained her Ph.D. in the Department of Economic History at the London School of Economics. Her field of expertise includes the growth of small and medium-sized enterprises and clusters; regional and national policies for SMEs; innovation in British regions; innovation and Italian economic performance in the long run. Her publications include book chapters and articles in *Business History*, *Enterprise and Society*, the *Economic History Review* and *Oxford Economic Papers*.

James Walker is Professor at Henley Business School, University of Reading, and Head of International Business and Strategy. His overall research agenda is characterised by the application of empirical methods to solve real world problems and issues past and present. He has published in journals as diverse as *Journal of Applied Economics* and the *Journal of Economic History*, examining the spatial competition in product markets and between firms, varieties of capitalism, academic performance and pay, and attitudes to multinational enterprises.

Chris Wickham is Chichele Professor of Medieval History (Emeritus) at the University of Oxford. Among his recent books are *Framing the Early Middle Ages* (Oxford, 2005), *Medieval Rome* (Oxford, 2014) and *Medieval Europe* (New Haven, CT, 2016). He is currently working on the economic history of the eleventh century.

Economic History and Microhistory

CHRIS WICKHAM

Introduction

Microhistory was perhaps the most significant original contribution by Italian historians to historiography since the Second World War. The main microhistorical texts were written between c.1975 and c.1995, with a particularly intense period of activity in the years around 1980. Many of its practitioners carried on writing in much the same vein afterwards, and many still do, but they now see this period as belonging in the (their) past; although the key principles of microhistory still seem fresh to me, it was their movement, so I guess they are right.

This chapter provides some reflections on microhistory, on its past as well as its (possible) future, with specific emphasis on the use of microhistory as a form of economic history. The chapter is divided in the following sections: first, an account of the origins and development of this microhistory is sketched, followed by an analysis of its aims and approach and of its limitations. The chapter continues with a study of the application of microhistory to economic history and concludes with some thoughts on possible future directions, including the use of microhistory to analyse the nature and logic of medieval economic organisation.

Origins and development

The intellectual focus of the movement was the Italian historical journal *Quaderni storici*, which published a number of explicitly microhistorical monographic issues between 1976 and 1987. As a whole, it was the work of a collectivity of left-wing historians, twenty or thirty in number, mostly early modernists, led – insofar as there were leaders – by Edoardo Grendi, Carlo Ginzburg and Giovanni Levi. The movement broadened out from *Quaderni*

storici when the publishing house Einaudi (based in Turin, like many of the leading microhistorians) founded a monograph series called *Microstorie*, edited by Ginzburg and Levi and Simona Cerutti, which published longer microhistorical works, many of which became historical classics; this, too, ran until the mid-1990s. Microhistory was a highly self-conscious movement, and its activists wrote several accounts of it, including the first three historians mentioned above; of others, I would single out commentaries by Jacques Revel, and the recent backwards look by Osvaldo Raggio, another of the main participants.¹ From these self-analyses, the interested reader can reconstruct the main lines of the movement, although, unsurprisingly, the most significant demonstrations of its importance for historical understanding lie in the major empirical analyses which the microhistorians produced, such as Ginzburg's *Il formaggio e i vermi*, Levi's *L'eredità immateriale*, Raggio's *Faide e parentele. Lo stato genovese visto dalla Fontanabuona*, or Angelo Torre's *Il consumo di devozioni*.²

Microhistory: aims and approach

What actually was microhistory, though? That history writing very frequently – normally – operates on a very small scale, with great attention to detail and to narrative, is too obvious to discuss. But what the microhistorians tried to do, in their different and overlapping ways, was to *theorise* the small scale. In doing so, they opposed not simply traditional political history, but also, and perhaps above all, the grand and huge social-history syntheses, based increasingly on serial analyses, of the *Annales* school of the 1950s to 1970s. They advocated bottom-up approaches as opposed to top-down ones. They looked to anthropology for guides to how to create total analyses of small-scale societies, becoming, for example, among the first historians to use Clifford Geertz's 'thick description' as a guide to historical reconstruction. They

1 For example, Carlo Ginzburg, 'Microstoria: due o tre cose che so di lei', *Quaderni storici* 86 (1994), 511–39, translated as 'Microhistory: Two or Three Things That I Know About It', *Critical Inquiry*, 20 (1993), 10–35; Edoardo Grendi, 'Ripensare la microstoria?', *Quaderni storici* 86 (1994), 540–60; Jacques Revel, 'Microanalisi e costruzione del sociale', *Quaderni storici* 86 (1994), 561–75; Giovanni Levi, 'On Microhistory', *New Perspectives on Historical Writing*, ed. Peter Burke (Cambridge, 1991), 93–113; Osvaldo Raggio, 'Microstorie e microstorie', *Enciclopedia Treccani* (Rome, 2013), http://www.treccani.it/enciclopedia/microstoria-e-microstorie_%28altro%29/, accessed 15 December 2016; all of these cite other characterisations, by members of the movement and sympathetic observers.

2 Carlo Ginzburg, *Il formaggio e i vermi* (Turin, 1976), translated as *The Cheese and the Worms* (London, 1980); Giovanni Levi, *L'eredità immateriale* (Turin, 1985), translated as *Inheriting Power* (Chicago, 1988); Osvaldo Raggio, *Faide e parentele. Lo stato genovese visto dalla Fontanabuona* (Turin, 1990); Angelo Torre, *Il consumo di devozioni* (Venice, 1995).

stressed the variation in experience of different local social actors, and the absence of a unified context for social action on the ground, which allowed the most successful dealers to manipulate their environment, and maybe, by exploiting social contradictions, to effect real change (in this they differed from Geertz; the argument was closer to Pierre Bourdieu, but his work did not greatly influence them, as far as I can see). They stressed the problem of how to construct historical knowledge on the basis of clues in texts, which, Ginzburg convincingly argued, meant that history as a practice resembled medical diagnostics more than it did normative social science; but they rejected the deconstructive relativism for which Geertz at times argued.³ And, above all, they theorised the issue of scale: the micro as a scale of analysis meant that individual experience could properly be focused on; millers and local charismatic preachers, and local family strategies, if handled with sufficient sophistication, could become as important as – more important than – the affairs of major political leaders; details of local symbolism and interaction could be apprehended in a way that classic big-picture *Annales* histories could never manage. And, using the other end of the microscope, traditional historical grand narratives, such as the ‘rise of the modern state’, could be tested to destruction; what *did* the Genoese state look like if it was seen from the experience of families and villages up in the Apennines in the Fontanabuona valley, as in the subtitle of Raggio’s book?

Put as briefly and schematically as this, it may be that those readers who are more familiar with the Anglo-American cutting-edge social and cultural history of the last decades of the twentieth century will not find some of this approach so very surprising; bottom-up history, in particular, was very common by then and still is now (Edward Thompson was indeed a touchstone for the microhistorians), as have quasi-Geertzian analyses. What marked the microhistorians out, however, was the fact that they did all of these things at once, with very high-quality analysis; and that they articulated each monographic analysis with a constant social-scientific and literary-critical awareness, and a frequent preoccupation for how the scale of their micro-analyses articulated with the problematics of the grand narratives they were critiquing. To invoke two parallel examples from other countries: Natalie Davis’s *The Return of Martin Guerre* (1983), with its constant probing of the inconsistent and incomplete historical sources for her micro-account of a highly atypical family crisis in a small Pyrenean village, Artigat, was indeed very similar to classic microhistory, and was in fact soon translated

3 Carlo Ginzburg, *Miti, emblemi, spie* (Turin, 1986), pp. 158–209, translated as *Clues, Myths and the Historical Method* (Baltimore, 1989), pp. 96–125 (but the original article dates from 1979); for the critique of Geertz (and of the use made of him by Robert Darnton), see Giovanni Levi, ‘I pericoli del Geertzismo’, *Quaderni storici* 58 (1985), 269–77, further developed in Levi, ‘On Microhistory’, pp. 98–105.

in the *Microstorie* series. But Emmanuel Le Roy Ladurie's *Montaillou*, which discusses another Pyrenean village in extreme detail, using parallels from anthropology, and which in 1975 marked the beginning in France of the break from *Annaliste* high-level synthesis, in just the years in which Grendi and the others were working microhistory out, is, however classic, not itself a microhistorical work. It is trusting of its inquisitorial evidence in a way that Ginzburg, who also used inquisition registers, was not, and it does not problematise its approaches, unlike every good piece of microhistory. Indeed, as a result, it has dated, in a way that *Il formaggio e i vermi*, and the other major microhistorical works, have not.⁴

Issues and limitations

My only problem with the microhistory school, to which I am otherwise intellectually close, lies in the question of comparison. If you know one village really well, and can use its specific experience to rethink the top-down assumptions of rise-of-the-state theory, then it might be worthwhile – I would argue that it is very important – to look at several other villages to see if they worked in the same way, and to use their collective, although of course different, experiences to replace the top-down models altogether. This was not an unknown *démarche* for microhistory, but it was not common. It did not fit well with the deep-rooted hostility of the leading microhistorical theorists to the big-history accounts they were reacting against. Davis's Artigat is only 50 kilometres away from Montaillou, but she only mentions it three times, and only once makes a comparative point. That was not, in her case, a result of polemic; her book is, in fact, too different for comparison to have been useful – she was, precisely, not trying to analyse a whole village, as Le Roy Ladurie was. But this is also part of the point about the microhistorical classics: they were all making different, often incommensurable, types of argument, linked together only by their wider theoretical engagement. That may also explain why the movement only lasted twenty years in its classic format: because the charisma of the major texts, so different as they are, resisted routinisation into a definable school of followers. And also because only a high-quality theoretical engagement, in the end, really distinguished these analyses from the kind of solid and dull localised history most of us write the rest of the time – one off day, that is to say, and one finds oneself simply adding 'una nuova tessera nel mosaico' ('a new piece in the jigsaw'), the stock phrase of lukewarm book reviews in Italian.

4 Natalie Zemon Davis, *The Return of Martin Guerre* (Cambridge, MA, 1983); Emmanuel Le Roy Ladurie, *Montaillou* (Paris, 1975), abridged and translated as *Montaillou* (London, 1978); see the sympathetic but critical review by Natalie Zemon Davis, 'Les conteurs de Montaillou', *Annales* 34 (1979), 61–73.

Microhistory and economic history

What can be done with microhistory now, then? Has its relevance faded, given that the *Annaliste* tradition it opposed has effectively (sadly) disappeared, and that the methods it pioneered are now much more common, indeed often standard – partly, indeed, because of the force of its example? That observation in itself shows that its individual approaches are indeed still relevant, because we follow them – it is non-microhistorians who have routinised them, that is to say. But I would argue that there is one area in which the subversive role of microhistory maintains, and maybe has increased, its potential: in economic history. In the last generation, the most systematic overarching theories have most often been in economic history. These do not replace the great *Annaliste thèses* in their contribution to empirical knowledge, for they are seldom based directly on empirical work by the authors (indeed, often enough, what their secondary or tertiary source-base actually is, is pretty obscure) – they tend to owe more to model-building in economics or social theory in general than they do to empirical engagement. They are also far too frequently focused on different versions of a hyper-modernist account of the ‘triumph of the West’ and of its inevitability. But their scale and ambition are notable; and they are also even more exposed than were the *Annalistes* to the balloon-pricking which a successful, and theoretically aware, microhistorical analysis can provide. Again, small-scale studies without that theoretical awareness are legion, and do not work to counter the overarching models if, indeed, they are trying to do so. But if one is aware of what one is doing, one can undermine these economic grand narratives quite effectively, and this seems to me an eminently positive, indeed necessary, step.

One example of this is Francesca Carnevali’s work on jewellers, sadly incomplete as we know, but including, at least, one major article in which she took on precisely this task. Her article on ‘Social Capital and Trade Associations in America, c.1860–1914’ was explicitly in the subtitle ‘a microhistory approach’, and was headed with a quote from Jacques Revel.⁵ It discussed the New England Manufacturing Jewelers and Silversmiths’ Association in the late nineteenth century in Providence, which, it becomes clear across the article, functioned much like a medieval trade guild in its creation of internal mechanisms of trust and co-operation, including by inventing its own rituals, so as to combat what it saw as its external opponents, wholesalers (‘jobbers’) and trade unions. (Francesca intended this to develop into a comparative study, but never came back to the Birmingham end of her project from this standpoint.) The whole point of the article was to get at as much detail as possible about

5 Francesca Carnevali, ‘Social Capital and Trade Associations in America, c.1860–1914: A Microhistory Approach’, *Economic History Review* 64 (2011), 905–28. All the quotations in the next two pages come from this article.

the actual processes of trust creation, at least as expressed through a single source, the pages of the *Manufacturing Jeweler*, the Association's trade journal – a problematic source, on the level of those used by some of the microhistory classics, and one used with much the same deftness.

This work on trust of course fits in with specific versions of new institutional economics – including the more wide-ranging work of Robert Putnam and others on the establishment of social capital, whose insights Francesca already understood from the inside, given her work on Italy – and she duly cites them in her article. These versions already, by re-embedding economic activity in the social, undermine some of the more abstract overarching theories in the sector. But not all; as she notes, how trust and co-operation actually derive from associational activity – and, indeed, what the initial impetus for associational activity actually was, and whom this activity excludes – are not obvious, but are too often taken for granted. The development of social capital theory is a clear advance on some of the more schematic institutional and rational-choice analyses which preceded and frame it, but it is schematic in its own way; although there are certainly exceptions, it, too, often works by model-building rather than real empirical study, creating, once again, external impositions on the material.

Francesca's microstudy was therefore aimed at getting behind these abstract models and at giving 'agency back to agents', by focusing on the processual networks of relations 'in which actors define each other', through their 'specific cultural context': precisely, a microhistorical approach. The detailed history of the Association which she provides goes a long way to creating just that sense of agency. What did the *Manufacturing Jeweler* really care about, and how did that change? Who ran it, and how did their social origins and economic status change? How did the community embodied in the Association actually manage to combat the tendency of wholesalers to go bankrupt, or the influence of unions? What did jewellers think their dinners and outings actually achieved, in terms of community building? This last was perhaps the key issue, for it is very clear from the trade journal that jewellers really did believe they were creating a community, with mutual loyalty and shared values. It did not work 100% of the time to stop its individual members dealing with recognised dishonest wholesalers, for example, but it imposed informal sanctions on such members sufficiently often that the community was and remained protected. This was a highly competitive world, and these jewellers competed with each other too, but they did so in the framework of a 'larger moral order that they codified and expressed', through the creation of social bonds and a common cultural language. This microstudy was thus not, by any means, a simple piece of empirical research; it was a conscious intervention in the historiography of social capital which was designed to show how it was actually created on the ground, rather than in the heads of modern social scientists, just as were the works of Grendi or Levi in the 1980s. Francesca's microhistorical approach was a re-creation of real social relationships, which,

if properly followed up, ideally in a comparative perspective, will enable social capital theory to be refigured from the ground up, and will further undermine the overarching certainties of the paradigms which frame it. That will be work for others to do, now, alas, but here is a starting point.

There are not that many examples of full-blown economic microhistories. There were some in the 1980s, it is true, like Franco Ramella's *Terra e telai*, about how family-based industrialisation worked in practice in a village in an Alpine valley in the nineteenth century, which undermines plenty of standard narratives, including ones written later. Francesca Trivellato's more recent work on early modern Livorno, *The Familiarity of Strangers*, is also in many ways a classic microhistorical text; she has related her conceptualisation of microhistory to global history very interestingly as well. In my medieval field, I would cite two recent examples: Jessica Goldberg's 2012 account of the personal relationships of Jewish merchants from Fustat (now part of Cairo) in the eleventh century, seen in particular through their letters; and Chris Dyer's 2012 reconstruction of the socio-economic world of a single Cotswold wool-merchant, John Heritage, around 1500, seen in particular through his account-book. Neither of these namecheck the microhistorians, but they are doing exactly the same sort of work: they are reassessing standard grand-narrative accounts by focusing on the micro scale, in as sophisticated a way as possible. Goldberg shows without difficulty how schematic – and incorrect – Avner Greif and other new institutional economic historians were to say that medieval Islamic trade networks, unlike those of Italy, were enforced by informal, not legal, means, which ultimately, because they could not easily be extended past a single community, undermined their force. Dyer does not critique current models to the same degree, not surprisingly given that they derive in large part from his own, less micro, work; but he shows how multifaceted, even contradictory, Heritage's economic activity was across his career, and how badly he fits into any of the standard categorisations of dealers in the commercial environment of his time.⁶

These two show how medieval economic history should be done: on the basis of complex, often inconsistent, sets of day-to-day documents, which indeed need to be treated statistically in part, but which are to a substantial degree irreducible to serial analysis – or, at least, which lose much of their usefulness if that is the only way they are treated. The micro is itself not the

6 Franco Ramella, *Terra e telai* (Turin, 1984); Francesca Trivellato, *The Familiarity of Strangers: The Sephardic Diaspora, Livorno, and Cross-Cultural Trade in the Early Modern Period* (New Haven, CT, 2009); Francesca Trivellato, 'Is There a Future for Italian Microhistory in the Age of Global History?', *Californian Italian Studies* 2 (2011), <http://escholarship.org/uc/item/0z94n9hq>, accessed 16 December 2016; Jessica Goldberg, *Trade and Institutions in the Medieval Mediterranean* (Cambridge, 2012), esp. pp. 12–17 and 148–56 for critiques of Greif, which make up only a small part of a substantial and much richer book; Christopher Dyer, *A Country Merchant, 1495–1520* (Oxford, 2012).

only way of dealing with the economy, and loses its critical force if this is the only field that a historian studies; but both these historians do indeed approach the macro as well. It is just that they do so on the interpretative basis which has been created by their microhistory and, by doing so, they offer us ways into the nuanced analysis of the way economies actually work which standard narratives often deny us.

Future perspectives: microhistory and the economic logic of medieval economic systems

There is another, final, point which needs to be made here. There has been very little work done on what the specific economic logics of medieval economic systems were. Most studies, indeed, actively deny such specificity. Apart from a pioneering and controversial monograph by Guy Bois on fourteenth-century Normandy (which was not, incidentally, any form of microstudy),⁷ historians, when they theorise, tend to assume that the economic drivers of the medieval period were the same as those of the industrial world – just less efficient, and/or blocked beyond a certain point of development. The grand overarching economic history narratives of recent years take the indivisibility of all economic systems for granted, indeed. I doubt this, very greatly, myself. If, however, we want to test it, to see whether it is possible to construct a model, or indeed more than one, for how medieval economies worked which is different from that of the industrial or industrialising world, we are going to need to look beyond serial data, which are so ‘thin’ as description. We are going, rather, to need to look at the motivations of highly localised economic actors, and at the detailed directions and framing of their economic choices; and we are going to need to look at how they dealt with their social and economic environments, how they thought they could achieve their aims, and how these aims could be thwarted. We are going to need, in short, to do microhistory – indeed, many microhistories, comparatively. Even though our aim might be to create as wide and overarching a model as those it replaces, we must do it on the basis of a detailed attention to the choices and values of individual actors; for otherwise we will be making assumptions as arbitrary as those currently on offer in the discipline. This is a direction to which studies like those of Goldberg and Dyer point us, and it is a crucial one. As, in the very different problematic of the nineteenth century, studies like Francesca’s on Providence jewellers point us. Microhistory, in this respect, has not gone away, and will not go away; it may be historiographically located in the recent past, but it marks the future of the discipline too.

⁷ Guy Bois, *Crise du féodalisme* (Paris, 1976), translated as *The Crisis of Feudalism* (Cambridge, 1984).