Turning qualitative into quantitative evidence: a well-used method made explicit[1]

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Many historians reject quantitative methods as inappropriate to understanding past societies. This article argues that no sharp distinction between qualitative and quantitative concepts can be drawn, as almost any concept used to describe a past society is implicitly quantitative. Many recent advances in understanding have been achieved by deriving quantitative evidence from qualitative evidence, using the two dialectically, and indexing them against other quantitative findings from the same population. We show that this triangulation method can be extended to many apparently qualitative sources. Despite its successes, the potential of turning qualitative into quantitative evidence has only just begun to be exploited.

Social and economic historians live uncomfortably these days. They are under pressure from two opposed sides. On the one side are economists, who want everything formalized and have little time for any but quantitative evidence (preferably very large, homogeneous datasets). And on the other side is the historical profession, which increasingly rejects quantitative methods altogether. How to mediate between these hostile and mutually uncomprehending tribes?

We address this question here in two ways. First, we make explicit a long practised but never codified historical method that bridges this gap in a way both sides could live with. And second, we ask how historians devise their conceptual frameworks: what do they mean by the concepts they apply to past societies? By considering the two opposed sides from this perspective—as different ways of shaping concepts—we hope to show that the tension between them is largely spurious. In fact, we will argue, understanding past societies always requires both approaches, and we illustrate this by reference to historical work that has employed the well-used method we seek to make explicit.

In section I, we informally motivate and describe this 'micro-exemplary' method, as we call it, portraying it as a synthesis of the opposed methodological tendencies presently competing for the social and economic historian's allegiance. Neither of these tendencies by itself, we argue, can empirically anchor the concepts applied by historians to past societies; only a synthesis between them can do so. In section II, this method is described more systematically, but still in rather simplified and idealized terms. In section III, we proceed to show how actual historians have implemented it. Finally, in section IV, we argue that for all its fruitfulness to

date, the scope of this method can nonetheless be significantly extended. We emphasize that this method, even if specified in far greater detail than we have attempted here, could never become a formula or an algorithm, mechanically applicable to any given problem, or replace historical judgement. In history, unlike more theoretical areas of study, there will never be a way to avoid getting one's hands dirty.

I

This article will argue that qualitative and quantitative evidence are mutually indispensable. It is impossible to understand a qualitative document from the past, we maintain, without a knowledge of the basic categories or parameters (such as 'household' or 'property' or 'law') of its social context. And these basic parameters have no inherent, cross-cultural or universal meanings; their meanings have to be found out. This, in turn, cannot be done by looking at a particular society in isolation. Only by comparing it with other relevant societies or communities (such as nearby ones) can the role of such parameters in a particular context be understood. And this comparison, we argue, is inherently (if in practice not always explicitly) *statistical*: it must refer to the distribution of some variable of interest over a range of possible values.

In itself this claim is perhaps not very controversial; what inflames passions is the degree to which such statistical or quantitative comparisons should set the tone within the larger historical enterprise. Nearly all historians accept that the medium of historical discourse is and should be ordinary language, rather than a system of precise and unambiguous concepts such as those employed in chemistry or economics. On the other hand, nearly all historians also accept that, at least around the edges, the vagueness and imprecision of ordinary language must be remediated by auxiliary concepts from other, more precise systems of thought, in which a higher degree of uniformity has been imposed on usage than ordinary language affords. What has been controversial among historians since the nineteenth century has been the degree to which these 'other systems of thought' with their more precise concepts can or should be allowed to encroach on traditions of historical writing and thought previously untroubled by them.

A wide range of alternatives has been proposed, but they can be seen as falling into two broad categories, corresponding to two major priorities in any historical inquiry. On the one hand, we want to understand a society in its *own* terms. Like the anthropologist or participant observer, we want to understand the significance of the past society's events and institutions to the people who lived in it. Second, we want to understand the underlying processes in the society, the forces and constraints operating on the visible stream of interactions and events as well as the institutional evolution we observe in it. Like the economist or biologist, we want to make sense of the past society's events and institutions in *our* terms. The first priority might be called a first-person perspective, the second priority a third-person perspective. In terms of history as a discipline, the first priority might be called 'historicist', the second 'materialist'.

These two approaches have been locked in semi-permanent controversy over just how far precise concepts from 'other systems of thought' should encroach on ordinary language within the historical enterprise as a whole. Such disputes result almost inevitably from the continuing problem of definition forced on history by

its use of ordinary language as the basic medium of thought and exposition—its use of ordinary language, that is, as its basic *meta-language* for discussing the discourse of the particular society under study (the *object language*).[2] Proposed solutions have fallen into the two patterns, historicist and materialist, just discussed. The historicist proposal is essentially that we should make our concepts precise by systematically uncovering the conceptual system of the past society we are studying, and make our meta-language immanent to the society being studied. That is, our meta-language of historical discussion should be absorbed into the object language; the distinction between them should be obliterated. The materialist approach, in contrast, seeks to impose our own concepts on past societies, even if these concepts would have been quite unrecognizable to the people being studied. This approach, then, also levels the distinction between meta-language and object language. In this case the object language is absorbed into the meta-language.

Neither of these extremes exists in its pure form among real historians. But there are examples that come close. A purely historicist approach to definition is approximated by the gigantic compendium of *Geschichtliche Grundbegriffe* (*Fundamental historical concepts*) launched after 1945 by Otto Brunner and Werner Conze, and completed in the 1980s by Reinhart Koselleck and others.[3] The premise of this undertaking was spelt out by Brunner in his famous 1943 book *Land und Herrschaft* (*Land and lordship*).[4] Modern historians misunderstand past concepts, he held, and are thus incapable of grasping past documents, because society has changed so much that the use of important words has also changed, sometimes radically. To make past documents comprehensible to us, we must undertake a thorough archaeology of their basic concepts. This idea, which to our ears has a Wittgensteinian or ethnomethodological ring, in fact derives from German historicism.[5] So the *Fundamental historical concepts* project really just took to an extreme the approach to definition long practised among historians, especially legal historians in the tradition of Savigny and Maine.

The materialist approach is exemplified, to some degree, by the first and second generations of the *Annales* school, whose founders were strongly influenced by Durkheimian structuralism as well as by the geographer Vidal de la

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[2] The distinction between 'object language' and 'meta-language' was first introduced by the mathematician David Hilbert, and developed by Alfred Tarski and Kurt Gödel. It was introduced into philosophy more generally by Rudolf Carnap in his *Logische Syntax*, which has generally been taken to mark the 'linguistic turn' within analytic philosophy; see the editor's introduction to Rorty, ed., *Linguistic turn*. Note that in most scientific contexts, the *objects* of study have no discourse of their own, so there is no object language in our (above) sense. In philosophical parlance, a 'meta-language' is usually the language in which the language of a *scientific theory* is studied (its logical properties, for instance). In that sense, a meta-language of the language in which historians study their object societies would thus be a *meta*-meta-language with respect to those object languages.

[3] Brunner, Conze, and Koselleck, eds., *Geschichtliche Grundbegriffe*.

[4] Brunner, *Land und Herrschaft*. The programme was subsequently given greater conceptual sophistication by Koselleck and others; see Koselleck, ed., *Historische Semantik*, esp. Koselleck's own contribution, pp. 19–36.

[5] Brunner, *Land und Herrschaft*, esp. pp. 17–41. Brunner's ideas were thought worthy of an extended critique by Braudel ('Sur une conception'). A defence of Brunner against these and other criticisms is attempted by Oexle, 'Sozialgeschichte', who argues that Brunner was not 'historicist' in the above sense, but was well aware of the need to use modern concepts—provided their historical relativity is kept in mind. Oexle fails, though, to deflect Braudel's central objection to Brunner's 'two-dimensional' approach to social history. In Braudel's view, history must triangulate on the past from different viewpoints ('Sur une conception', p. 191)—in our terminology, it must not only retain, but actively construct, a meta-language.

Blache, to whose adaptationism they added geological, climatological, and demographic components.[6] Although cultural and social history were not outside their scope, processes at these levels were largely seen as epiphenomena, 'the foam on the surface', as exemplified in Braudel's scheme of underlying physical and geographical *structures* that shaped the economic, demographic, and institutional *conjonctures*, which in turn tightly constrained the superstructure of *évènements*. The demographic research initiated in the postwar period by Louis Henry and others, for example, was not systematically related to community-level records from which statistics were drawn. Economic and demographic results were abstracted from the local records and then analysed at a theoretical level, rather than jointly with qualitative sources from those localities.

But perhaps the most radical example of the materialist approach over the past half-century has been that of the American cliometricians. Their method has been that of 'positive economics',[7] where the subjective view of participants in the social processes being studied is not considered. The minimal unit of study in economics has generally been a particular market,[8] not the behaviour of particular communities or even of institutions. There is no requirement, in economics, that the participants in markets are even aware of the aggregate behaviour being studied, much less that they have an intuitive understanding of its underlying determinants. The cliometricians, accordingly, have shown little interest in the specific impact of larger structures or trends on the local level—in the interaction of institutions or communities with markets at the point of contact.[9]

While examples of pure historicism or materialism are rare, it is also true that nearly all historians tend more or less to one side or the other. The gap between them has not been systematically overcome. An exception is the work employing the 'well-used method' we seek to make explicit in this paper, which we shall call the 'micro-exemplary' method or approach, for reasons given below. The earliest example we have been able to find is Peter Laslett's 1963 study of household structure in Clayworth and Cogenhoe, where demographic facts are put in a qualitative, local context of particular villages.[10] Many of the subsequent preoccupations of the micro-exemplary method were anticipated in that paper, which contains extended discussions of household size and headship, migration and servanthood, the social and income inequalities among villagers, occupations, and also of other local institutions. No new techniques were involved. But Laslett's approach to the study of households, even in this early paper, was quite different from Henry's or that of the *Annales* school. Instead of merely using household data for demographic analyses, or of distilling quantitative

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[6] Friedman, *Marc Bloch*; Müller, *Lucien Febvre*, pp. 239–65.

[7] As classically formulated by Friedman, 'Positive economics'. A succinct history of cliometrics is given by Lamoreaux, 'Cliometric revolution'.

[8] 'Market' not in the ordinary-language sense of a particular location in which buying and selling takes place but in the economists' sense of a certain set of relationships among buyers and sellers of a particular good or set of goods.

[9] This rough characterization, for illustrative purposes, in fact applies more universally to the first generation of cliometricians than to current work, which in some cases has taken more of an interest in institutions and particular organizations such as business firms; cf. Lamoreaux, 'Cliometric revolution', pp. 75–7.

[10] Laslett and Harrison, 'Clayworth and Cogenhoe'.

results from the *Rector's book* kept from 1676 to 1701 by William Sampson, the rector of Clayworth, such sources were used to paint a detailed picture of the functioning of a local society against the quantitative backdrop obtained from them. It was only a beginning, and it would be a few years before full-scale village or community studies were undertaken along these lines. But it was already evident that the goal and approach were quite different from those of Braudel or Fogel. The goal was the reconstruction of a historical community, in the way that an anthropologist would reconstruct the internal functioning and social relations of a village she had lived in and observed first hand. The demographic and economic facts were not simply recorded and aggregated and used as evidence for larger-scale 'structures' or 'processes' which were then explained independently at a theoretical level. On the contrary, the numbers were just a starting point, a background against which the complex social and institutional life of the village could then be reconstructed—not in its entirety, but enough so that the particular question or problem of interest could be seen in three dimensions, within the context of an interlocking set of practices, norms, and institutions.

Oddly, this highly original approach to the reconstruction of historical communities was not articulated as an explicit programme by Laslett or by anyone else in the Cambridge Group for the History of Population and Social Structure.[11] It became an established and understood sub-practice or subculture within the Group, but was consciously stated as a programme only in the later 1970s by Alan Macfarlane and his village study project—acknowledging the inspiration of Laslett and the Cambridge Group.[12] The question of how and why Laslett and some other members of the Cambridge Group adopted this approach, especially in the absence of an explicit programme, remains something of a mystery.[13] Given Laslett's staunch commitment to the use of quantitative evidence wherever possible,[14] and his partnership with Tony Wrigley (whose more geographical and quantitative-demographical perspective put him closer to the Vidalian-Durkheimian structuralism of the *Annales* school), the anthropological and community-oriented component is an original and somewhat unexpected dimension in the Cambridge Group's approach. Its provenance is unclear. The only model among contemporary historians remotely within range

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[11] The first general statement by a member of the Group, as far as we can tell, was Wrigley, *Local and the general*, published as a typewritten pamphlet.

[12] Macfarlane, Harrison, and Jardine, *Historical communities*, pp. xi–xii. Levi's *Inheriting power* does not appear to have been directly influenced by Laslett or the Cambridge Group, but was only published (in its original Italian) in 1985, too late to have inspired the work cited in section III below.

[13] Village studies of other kinds had, of course, already been undertaken; the claim here extends only to specifically 'micro-exemplary' studies in the sense defined below. Apart from W. G. Hoskins (n. 15 below), there was the 'Toronto School' of medieval historians, who produced village studies (though without the quantitative demographic backdrop employed by Laslett) as far back as 1964; see Smith, 'Corporate village community', esp. pp. 155–7. Smith himself (the author of the first full-scale 'micro-exemplary' study; see below, section III) was apparently influenced by the presence of J. A. Raftis at Peterhouse in 1968–9, but there is no evidence that Laslett was influenced by the Toronto historians. Earlier work within German historical economics (such as Troeltsch, *Zeughandlungskompagnie*) focused on a particular community and used both qualitative and quantitative evidence, but again not in the 'micro-exemplary' style described below.

[14] One of the more uncompromising, but by no means atypical, statements to this effect is the introduction to Laslett, *Family life*, pp. 1–11.

was the work of W. G. Hoskins, though this made no use of demographic or household sources.[15]

Whatever its origins, this approach to reconstructing historical village communities, especially its close integration of rigorous quantitative study with a qualitative understanding of the institutional and social framework in which villagers interacted (as a kind of subjective, participative, quasi-anthropological *Verstehen* of their motives, against the backdrop of their measurable, objective—physical, biological, institutional—constraints), has characterized a considerable body of first-rate historical research over the past three decades; examples are given in section III. This approach constitutes a distinct subcategory of what has been called 'micro-history',[16] a subcategory focused specifically on the reconstruction of a nexus of interactions within a group or community for the purpose of identifying and exemplifying the operation of larger-scale processes (such as demographic change, state formation, or the evolution of markets or of institutions such as the Church) at their point of contact with individuals and households. To distinguish it from the larger penumbra of micro-history, we call this the 'micro-exemplary' approach or method.[17]

At the core of this approach is a particular strategy for using qualitative evidence quantitatively. The idea is not to turn micro-level qualitative evidence *into* quantitative evidence, which can then be discussed or analysed only at an (aggregative or theoretical) macro-level without further reference to local peculiarities. On the contrary, the quantitative data obtained are held up to the light of the historian's qualitative picture of what is going on in that society—to influences and interactions that can be observed at the micro-level itself. That is where the interface, the point of contact, between larger external trends (demographic change, market growth) and local processes is directly observable. Those who follow this approach thus focus on much smaller units than traditional economic history (or the *Annales* school, or the cliometricians): household, manor, community, and other local

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[15] Hoskins, *Midland peasant*. In its effort to use qualitative and quantitative evidence in a mutually supportive (though hardly systematic) way, and to use a single village as a 'microcosm' of the larger society (see n. 17 below), this study of the village of Wigston Magna deserves to be called at least a forerunner, if not the first actual example, of the 'micro-exemplary' method. The main element still missing, apart from the household and demographic dimension, was the progressive ('dialectical') feedback between qualitative and quantitative sources that would later (see below, section II) be supported by record linkage. Hoskins personally was very supportive of the Cambridge Group's work in its planning phases and early years, and attended a crucial planning meeting in July 1964 along with Wrigley, Laslett, Goubert, and Henry.

[16] The distinctness is evident even in its origins; the micro-exemplary approach dates to the early 1960s, while according to Schlumbohm, 'Mikrogeschichte', p. 11, there is 'international agreement' that the beginnings of micro-history were in the late 1970s. This trend toward micro-history is generally portrayed (ibid., pp. 18–23) as part of the wider revival of cultural history in the late 1970s, and indeed, many of the paradigmatic works of this later micro-history, such as Ginzburg's *Cheese and the worms* and Levi's *Inheriting power*, were focused on cultural history, especially popular culture and its interfaces with elite literacy. The micro-exemplary method may also be *applied* to cultural history (we discuss the work of Medick below, section III; and Levi is another example), but is not restricted to it. Although work using the micro-exemplary method is usually classified as *part* of micro-history, we prefer (despite borderline cases such as Levi) to regard as open the question of whether it is indeed a part of that larger trend or incompatible with it. Certainly the recent outgrowths of purely cultural micro-history that have pushed it toward a return to the narrative form (Levi, 'On microhistory', pp. 109–10) are hardly compatible with the micro-exemplary method as we describe it here, let alone the extreme form of this narrative tendency represented by what Darnton ('It happened', p. 60) has called 'incident analysis'.

[17] It could also be called the 'microcosmic approach', after an aperçu of Postan, quoted by Wrigley (*Local and the general*, p. 1), distinguishing between the legitimate 'microcosmic' study of medieval village society, which discerns the general in the local, and the merely 'microscopic' preoccupation of the antiquarian.

institutions. At that scale, the confrontation of quantitative results with a rich panoply of qualitative evidence makes it possible for qualitative and quantitative data to illuminate each other.

The distinguishing characteristic of this research strategy is to maintain a distinction between the historian's meta-language and the object language, the discourse of the society being studied; it does not attempt to absorb either of them into the other, nor does it reduce either to the other. Its mode of concept formation, therefore, is both immanent and imposed. A good illustration is the study of families and households that grew out of Laslett's above-mentioned study of Clayworth and Cogenhoe, now a small subdiscipline with its own internal standards and characteristic expertise, its own conferences and journals.[18] Its method is on the one hand immanent or bottom-up; it starts from what we know directly (from primary documents such as household listings and censuses), not from some pre-existing conceptual apparatus brought in from outside. But it also imposes on these primary data an ad hoc classificatory framework (the Laslett-Hammel household classification)[19] designed to highlight differences among household forms that are of interest from various theoretical points of view. Much of the expertise developed in this area consists of the iterative solution to a straightforwardly empirical problem: how to make the widely scattered evidence about past household forms comparable (across different social strata, occupations, communities, regions, societies, or continents). The Laslett-Hammel classification has been criticized for privileging cross-sectional over longitudinal analysis and using categories specific to England or Europe.[20] But where longitudinal sources are available, they yield findings consistent with those of the (far less scarce) cross-sectional sources. Moreover, alternative classificatory schemes have proved much *more* culture-specific; Laslett-Hammel continues to be used by the vast majority of household studies, because it permits systematic comparisons *across* societies as well as flexible extensions and variations to suit the specificities of *particular* societies.[21] In the 40-year process of developing the techniques of household analysis, a great deal of knowledge has accumulated about what household forms to expect in various economic and social environments, about the demographic causes and consequences of household forms, and so on. But these higher-level 'theoretical' considerations did not drive the process of empirical concept formation.[22] The basic empirical concepts of this literature were developed in response to the data, and remained close to the data. Still, rather than using the concepts

[18] Thus, for instance, the analysis of census-type household listings along the lines laid down by Laslett beginning in 1963 informs a large number of articles in the *Journal of Family History* (founded in 1975), *Continuity and Change* (founded in 1985), and *The History of the Family* (founded in 1996).

[19] This classification scheme divides households into five main types (solitary, no-family, nuclear family, extended, and multiple), each involving a multiplicity of sub-types; each type can also be modified according to the presence of servants and 'inmates' (lodgers, boarders, and so on); Hammel and Laslett, 'Comparing household structures'.

[20] Berkner, 'Use and misuse'; Lee and Gjerde, 'Household morphology'; Hareven, 'History'; Kertzer, 'Living with kin'.

[21] Wall, 'Transformation', esp. pp. 219–21; Guinnane, *Vanishing Irish*, esp. pp. 139–46; and Dennison, 'Serfdom and household structure', esp. section III ('Methodological issues'), pp. 398–404.

[22] As they did in the classification schemes devised by the Toronto School; Smith ('Corporate village community', pp. 156–7, 174) suggests that at least some of the results achieved by these studies were 'solely an artefact of [the] method' (p. 174).

employed *by* those societies, this approach imposed an ad hoc set of intermediate (empirically generated) concepts to enable researchers to consider the behaviour of a past society from their own, sometimes theoretical, points of view (whether these be sociological, economic, political, anthropological, or something else altogether).

We emphasize that the micro-exemplary approach is not a compromise, a halfway station, between historicism and materialism, between immanence and objectivity. It is a genuine synthesis, and thus something fundamentally different in conception from either. It is, in a sense we will make more precise in the following section, a dialectical approach that tries to get equally beyond a slavish immersion in the historical categories of the societies under investigation (those embedded in the object language) and beyond a domineering imposition of our own theoretical (meta-language) categories on empirical evidence to which they are in the first instance quite foreign—to arrive at a progressive interplay between the voices of the sources themselves and our own analytical categories.

The micro-exemplary approach has never quite been spelt out. It has been practised without being preached. Only occasionally did its early practitioners pause to articulate what they were doing. Keith Wrightson, for instance, in a review of David Hey's study of Myddle,[23] criticizes the reliance on a single qualitative source (the history of Myddle written by one of its yeomen, Richard Gough, in the late seventeenth century) without testing its perceptions using other documents. The result, says Wrightson, is that Hey 'is rarely able to give more than an impressionistic discussion of issues which demand rigorous examination'. Wrightson is particularly sceptical of Hey's reliance on Gough regarding the 'homogeneity of attitudes, interests, and behaviour among all save the "bad families" of the parish', and the resulting picture of social and doctrinal conformity during this period. 'These arguments,' Wrightson says, giving eloquent voice to the micro-exemplary approach,

> . . . depend for support upon Gough or upon silence, upon the comments of a sternly pious yeoman or upon the silence resulting from a dearth of source materials against which to test the accuracy and completeness of his testimony or the typicality of his attitudes. The registers of the parish offer little hope of accurately assessing the incidence of illegitimacy or bridal pregnancy. The area lacks the full series of ecclesiastical court or Quarter Sessions records which might, and in other counties do, contain valuable evidence of behaviour, sources of conflict, attitudes and opinions in village society. Where such evidence is forthcoming, a disciplined attempt can be made to establish the nature of the norms governing village life and the extent to which men deviated from them. The prescriptions of contemporary moralists and the opinion of men like Gough can be tested against independent evidence.[24]

This is both a more open-minded and a more sceptical approach to sources than the historicist one. It is more open-minded in that it does not impute a single homogeneous use of language to all the actors in a past society (as Brunner's approach implicitly does), but acknowledges a polyphony of often conflicting voices. And it is more sceptical; it does not assume that the language used in a society must correspond to its norms, or its norms to the actions of individuals.

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[23] Wrightson, 'Villages', pp. 635–6, reviewing Hey, *Myddle*.
[24] Wrightson, 'Villages', p. 636.

These are matters to be investigated empirically, not to be assumed in advance. On the other hand, neither does this approach turn its back on the immanent view altogether, as the materialist approach does; it does not seek to impose some completely external theory on the actions of villagers but takes its starting point, rather, in their sayings and doings.

At the heart of the micro-exemplary approach, we said, is a characteristic way of turning qualitative into quantitative evidence, in which this transformation is not a one-way street. Quantitative and qualitative evidence are progressively and iteratively confronted with each other in the context of a particular focus of interaction, a particular local community. Quantitative evidence generated from qualitative sources is used to resolve doubts about reliability and to resolve inconsistencies among qualitative sources. It enables us to know which norms actually influenced behaviour and which did not, which laws were enforced and which were not, and how well the rhetoric of local clergy matched the realities they saw in their parishes. When we find it claimed that socialization in household and church were effective in transmitting 'traditional' values, and that sexual misdemeanours were 'negligible', for instance, we want to be sure we have parish registers to enable us, as Wrightson suggests, to assess the incidence of illegitimacy or bridal pregnancy.[25] If we were to find a high ratio of illegitimacy this would not of course make us ignore our qualitative source, but we would understand it quite differently from Hey's uncritical acceptance at face value. In fact, we only really understand it—at all—by virtue of knowledge *independent* of such a single qualitative source. It is only by virtue of such independent knowledge that we know 'how to take' what qualitative documents tell us. In the absence of such independent knowledge, we have only linguistic artefacts, without any idea of the practice in which any linguistic usage is rooted.[26]

But an illegitimacy ratio is only a beginning; it is not enough by itself. Without a standard of comparison we have no idea whether a ratio from a particular village is 'high' or 'low'. Only the quantitative comparison with ratios of other parishes, in England and Europe, across time and space, and with trends across time in these different places and epochs, tell us whether a particular ratio is high or low. We can only gauge the significance of any particular local finding against a background of relevant comparisons. So we only understand even *qualitative* sources, in the end, by reference to a background system of quantitatively defined terms.

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[25] Ibid., p. 636.

[26] Wittgenstein argued in *Philosophical investigations* that the meanings of words in an ordinary language derive from their use in the practical life of the society in which the language is embedded. Meanings of words and sentences, in this conception, are not 'in the head'; they are not private and subjective, but public artefacts whose objectivity can be established by reference to their range of possible uses. Wittgenstein argues at length against the possibility of a 'private language'—the possibility of establishing meanings of words without a surrounding context of practices in which they are embedded and to which they refer. Those who would use qualitative documents, such as Gough's account (or other literary sources), as their sole informants about a past society—without reference to other documents that give us independent access to the society in which the language of those documents is rooted—are in effect, in Wittgenstein's terms, attempting to study that object language as a 'private language'. Any justification of such a procedure would, at least, have to take on Wittgenstein's formidable battery of obstacles; cf. also Carus and Ogilvie, 'Poverty of historical idealism'.

II

The starting point of the micro-exemplary approach is straightforwardly historicist. Immersion in the documents of a past society gives the historian a first-hand acquaintance like that of an anthropologist who lives in a society as a participant observer. The historian develops an intuitive grasp of the interactions among participants in the institutions or populations of interest, from observing a sample and extrapolating an imaginative picture from this sample. There can still be surprises, of course; at any point in the investigation, the intuitive grasp is always to some degree tentative. But as the sample grows larger, acquaintance becomes closer and more confident. This, of course, is the classic process of hermeneutic *Verstehen*, intuitive understanding, relied on by Droysen and the German historicists. One becomes so well acquainted with, say, the political culture of the papacy at a certain period that one is able to predict quite well how this entity will react in given circumstances. The institutional structure of papal policy-making becomes clear; one understands how the system responds to internal upheavals and external shocks. One understands a totality in which the strategies, opportunities, and risks of particular participants become graspable.[27] It seems impossible to formulate this intuitive 'feel' for such a configuration in other than qualitative terms, by illustrating it narratively, or by qualitative, literary description in ordinary language.[28]

Such qualitative immersion in the object language is the indispensable starting point for the micro-exemplary method. But unlike historicism, this method then seeks to test that total, qualitative, hermeneutic view against quantitative data—in just the way that one could, if documents were available, test the pronouncements of people like Richard Gough about sexual conformity against illegitimacy and bridal pregnancy ratios (and these, in turn, against a broader picture of local and temporal variation among such ratios).

It is recognized to varying degrees among nearly all quantitative historians—though rarely if ever spelt out—that to know what questions to ask, and to make sure these questions are relevant, the strict dichotomy between qualitative, hermeneutic understanding and quantitative, explicit theory (a dichotomy on which both the devotees of regression models and the devotees of 'thick description' are prone to insist) must be bridged. There is a continuum between the barest intuitive inkling at one extreme and the fully specified regression model at the other; most of what we do lies *between* these extremes. In history, we cannot afford to insist on all or nothing; we must learn to negotiate the intermediate ranges, and find a way of talking about them.[29] One does, after all, use one's qualitative, hermeneutic understanding of a society as a basis and framework for constructing a meta-language in which to ask quantitative questions. One thereby puts that understanding to the test, and so in effect regards it as a kind of incipient proto-theory, a guide (though not yet a fully articulate or explicit one) to the eventual construction of a system of empirical concepts. It is not regarded as

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[27] The classic formulation of this conception is Gustav Droysen's *Historik*, which drew on the earlier traditions of hermeneutics, romantic *Volkskunde*, and local antiquarianism described in Meinecke, *Historismus*.

[28] Droysen and others had, of course, already suggested that such description, as it is not theoretical or (in later terms) 'nomothetic', must employ the indirection, the metaphorical and rhetorical devices, commonly employed in literary art; in other words, that such description is unavoidably 'thick' (in Geertz's more fashionable term; Geertz, 'Thick description') rather than simply literal.

[29] Cf. Schofield 'Through a glass darkly', esp. pp. 128–9; Black 'Noise'.

a purely subjective intimation incapable of further probing or analysis, but as progressively revisable and improvable.[30]

In the micro-exemplary approach, though (as distinct from most other quantitative approaches) such quantitative tests of the hermeneutic understanding do not then simply supersede that original understanding. The progress from qualitative to quantitative is not a one-way street. The qualitative, intuitive, holistic understanding derived from immersion in documents is not *displaced* by its quantitative explication or cross-checking in the meta-language. The quantitative cross-check is the beginning, rather, of an open-ended dialectical or mutual feedback process between the original hermeneutic understanding and quantitative data generated to test it. If the quantitative test contradicts the original understanding in some respect, then the understanding is not simply abandoned, or displaced by some more abstract and precise concept not fully definable on the basis of primary evidence alone. The holistic understanding of the society is, rather, revised to accommodate the new fact. And the new understanding, usually arrived at by re-immersion in the qualitative sources, has more empirical content than its predecessor(s). This new understanding will then suggest further quantitative tests, and so on.

But sometimes such a clash between holistic understanding and quantitative tests cannot be so easily repaired, and the immanent vocabulary of the society itself will be insufficient to repair or revise one's original understanding of it. Instead, the conceptual repertoire has to be expanded; the construction of the meta-language has to be taken a step further. The micro-exemplary approach takes a minimalist approach to this expansion. Rather than abandon the immanent discourse of the object language completely, the micro-exemplary method prefers piecemeal substitution of ad hoc, bottom-up (data-defined), purely empirical concepts (such as the illegitimacy ratio or the Laslett-Hammel household classification scheme) into the language of the society under study. The raw data extracted directly from the sources is compiled or organized into a loose classification system, a meta-source.[31] It is the empirical concepts of this meta-source, then, that are used in conjunction with source-immanent language to describe informally, in the first instance, how the local data can be made internally consistent—can be made consistent, that is, without referring to larger, more abstract concepts or to the wider society in which the particular community is located. And this bottom-up process of establishing and testing empirical robustness becomes a foundation for any use of the quantitative data generated from these same qualitative data in higher-level, more abstract theories, or in comparative studies with other localities or within a wider society.

But in the micro-exemplary approach such higher-level uses (if any) do not *prescribe* the lower-level empirical categories of the meta-source, the first step in the construction of a meta-language. On the contrary, this approach purposely makes those lower-level categories as *independent* as possible of higher-level theo-

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[30] A 'hermeneutic understanding' is not usually regarded as corrigible in quite this way; one pervasive theme of Gadamer's *Wahrheit und Methode* is that a hermeneutic understanding cannot be made sufficiently explicit to be directly refutable or confirmable by particular facts, like a theory. This is not the place to enter into such epistemological questions.

[31] As it has appropriately been called by Medick (*Weben*, p. 28).

retical concerns.[32] The idea is to choose categories as 'close' to the data themselves as possible—the most obvious or 'natural' concepts, those that 'suggest themselves' to the researcher. (Obviousness or 'naturalness' here is not used in some ultimate or realist sense. It is not assumed by the micro-exemplary method that there might be some 'true' or 'correct' set of categories to impose on the processes observed in object-language documents. 'Natural' is to be understood here, rather, in an entirely pragmatic way, as something like 'unproblematic', or 'what we can agree on for now'—we historians with differing theoretical and ideological preoccupations—or 'what seems to jump out at us from the sources'.) Thus parish registers, for instance, lend themselves 'naturally', in this sense, to counting—to being turned into quantitative evidence—because they generally come in the form of physical lists. The discreteness of the items (demographic events) is presented to us in the form of the document itself. So the degree to which such quantitative concepts (birth rates, illegitimacy ratios, and so on) are imposed on the raw sources strikes us as minimal.[33]

Even just this first step to such very rudimentary empirical concepts can give us a very powerful external (meta-language) perspective on the object-language sources, and greatly enriches our understanding of them. The accumulated household literature of the past three decades, for instance, provides a much more empirically robust and reliable grasp of the phenomena it studies than any amount of unguided immersion in household listings, literary evidence, censuses, or other documents could possibly have afforded. The household literature provides the researcher with a set of standardized empirical categories that can be applied, with minor modifications, to local data from almost any society worldwide, past or present. But these empirical categories are concrete enough that they do not themselves impose a particular abstract, theoretical (anthropological, economic, or sociological) framework.[34] Though household investigations are often motivated by such theoretical ideas, they are able to agree on a set of

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[32] Considerable attention was given to just this question of maximizing flexibility in the early stages of the attempt to harness computers to the transformation of qualitative into quantitative evidence, at a time before relational database software was widely available; Schofield and Davies, 'Data input'; Thaller, 'Automation on Parnassus'; idem, 'Verfahren'. As Macfarlane (in Macfarlane et al., *Historical communities*, p. 81) put it, 'these processes may appear to the non-historian to be fairly trivial matters, yet upon the methods of collecting and preparing the data for subsequent analysis will depend the whole success or failure of a project concerned with studying a particular community'.

[33] The philosophical debate initiated by Quine's paper, 'Natural kinds', is of only marginal relevance to the discussion of 'naturalness' in this paragraph. Quine's (and his interlocutors') 'natural kinds' are in *nature*, or in the world, whereas the 'natural kinds' discussed above are in the *sources*. (An analogy: the blind spot in the human visual field, though a 'natural kind' of *perception*, clearly does not correspond to a 'natural kind' in whatever it is that we see!) So there is no suggestion here that the 'natural' categories arrived at as a first-order, rough-and-ready set of ad hoc meta-linguistic working tools correspond in any way to some supposed social *ontology* of simplest or most elementary processes or entities to which all social processes (in this society or more generally) could be reduced. The only assumption we make here is that two kinds of *practical* question can be distinguished: (a) what categories can we all agree on as reasonably obvious or unproblematic? and (b) what categories will be most useful or suitable from a *theoretical* perspective that we might ultimately, down the road, want to adopt?

[34] See nn. 18–22 above. Although critics of the Laslett-Hammel scheme often accuse it of imposing an 'English' framework on households of other cultures, it is striking how such critics themselves make use of it to point out weaknesses in Laslett's other ideas (about household zones within Europe, for instance). The alternative classification schemes advocated by critics are invariably specific to the culture they themselves are studying and hence fail to be widely adopted. The Laslett-Hammel classification scheme has been applied to households in practically every world culture for which census-type data are available, suggesting that it is in important ways more 'neutral' than other proposed classifications.

relatively neutral empirical categories which thus provide this sub-discipline with a framework for mutual comprehension and dialogue. These empirical concepts, because they are so close to the data, merge easily into new and revised holistic and hermeneutic intuitive understandings of the local society under study. Such a revised, holistic, intuitive grasp of a society—one that incorporates the new empirical concepts—is clearly an improvement on the original, quantitatively untested one. It contains or accommodates more information in just the sense that a revised theory in physical or biological science contains or accommodates more information than its predecessor.[35]

An improved intuitive 'feel' for the society is *not* a 'theory', of course, in the sense in which this word is used in most sciences. The islands of precision are still tiny in the vast sea of intuition. But it is a better, more informed understanding of the society in question. Some of its vague intimations have been replaced by better defined and more precise or explicit concepts. The difference is like the improvement in medical understanding brought about by replacing vague, imprecise intuitive concepts with the empirically more robust ones of biology and medical science. Though the abstract concepts of these sciences derive their meanings from their place in formalized, theoretical systems, they are learned by the aspiring medic in such a way that they are integrated into her everyday practice. In the process of medical education these precise concepts are assimilated into her intuitions, and become part of the practical reason she applies to every diagnosis and every treatment.[36]

An original holistic 'feel' for a society is confronted with quantitative data, then, and revised to accommodate those data, sometimes incorporating new empirical concepts defined directly on the basis of data. What makes the micro-exemplary method *dialectical* (in a Platonic, not a Hegelian sense)[37] is the openness of this inquiry process to continued iteration. This process is *progressive* in the sense that the amount of information accommodated by it increases with each iteration. The ad hoc empirical concepts devised in response to it become more robust each time around. They may even converge sufficiently to allow analysis at a more abstract, theoretical level. But the dialectical process also remains open-ended; there is no end point at which understanding could be demonstrably complete. We can never exclude the possibility that a historian's ingenuity can find some new way of putting the latest, most revised understanding of a certain society to a quantitative test.

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[35] It is fashionable now to dismiss the idea that one scientific theory could be 'superior' in any way to its predecessors. Theories are held, in this view, to be strictly 'incommensurable' in the sense of Kuhn (*Structure*) and thus not comparable. Kuhn himself did not, of course, believe this; indeed he offered a list of criteria for theory choice in idem, 'Objectivity'. Certainly it is—at least—possible to compare theories for practical usefulness (in medicine, for example; see n. 36); this is all we require here.

[36] It is in this practical sense—that the modern doctor's science-acculturated intuitions are more likely to result in a cure than the medieval doctor's—that one intuitive understanding can, ultimately, be called 'better' than another. The additional complication in history is that, unlike medicine, history does not have a single widely agreed-on practical use. It has many, and the criteria of usefulness might turn out to be different for these different uses. We suspect that the criteria would actually turn out to be surprisingly convergent for a wide range of uses, but this is not the forum to make that case.

[37] Hegelian (and marxian) dialectic is attributed to the world, the historical process itself, from whence reason can infer it because the process is itself in accordance with (and for Hegel, an emanation of) reason. Platonic dialectic, in contrast, is a strategy for gaining *knowledge*; it is not imputed to the *object* of knowledge. It is the inquiry concerning first principles. Platonic dialectic is iterative in that each science takes its first principles as given, until a breakdown occurs and they are no longer adequate; at that point, dialectic must supply new or revised ones that are also consistent with all other first principles.

So even if the social sciences one day defy all predictions and become a theoretical discipline, the intuitive, hermeneutic 'feel' for a society resulting from immersion in it would continue to be indispensable, just as a medical doctor's intuition continues to be indispensable, however scientifically educated this intuition becomes. However many aspects of a hermeneutic understanding may in future be capable of theoretical explication, the overall qualitative understanding incorporating these explications would still—just as in medicine—be essential for applying the theory in particular cases.

But there is a further reason why hermeneutic understanding is inexhaustible, specifically in history. Unlike medicine, history does not have a single, clearly defined or widely agreed practical purpose. So even if history—improbably—became a theoretical discipline (in place of its current reliance on ordinary language), the choice of an appropriate theoretical language could not be made once for all. It would have to be done over with each new investigation. The choice of theoretical language is a *practical* question, asked within the looser frameworks of the languages we adopt to guide practice (which are closer to ordinary language and its looser, more impressionistic concepts). In any case, the choice is undertaken against the background of our (increasingly) disciplined hermeneutic understanding of the society under study. For this reason alone, then, the overall qualitative understanding of a society is strictly inexhaustible.[38] It can never be reduced to the sum of its partial explications by theoretical concepts. There is no need, then, for historians to fear the encroachment of precise, quantitative, 'scientific' concepts on a formerly 'humanistic' territory; such concepts can only expand the dominion and the reach of history. They can never overrun its borders or threaten its territorial integrity, but are inherently peaceful, law-abiding subjects and citizens.

III

The above description of the micro-exemplary method is not motivated by abstract considerations of methodological hygiene. It is an after-the-fact reconstruction of an existing, lively, and diverse tradition of concrete historical research. A general exposition of this kind is bound to be somewhat idealized, and to stress what the historians working in this tradition have in common, rather than exhibiting their wide variety of particular interests and ideas. In this section we give examples.

One of the first projects systematically employing the micro-exemplary approach was Richard Smith's 1974 study 'English peasant life-cycles and socio-economic networks', which issued in a whole series of publications on inheritance, land markets, demographic trends, and social networks.[39] Like Laslett, Smith was not interested in writing a 'total history', but had specific hypotheses to test about medieval peasants (arising mainly from the historiography of medieval England

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[38] The idea that our choice of languages for science (and, of course, our meta-language for talking *about* science) is ultimately itself a practical choice, subject to normative considerations, was central to the late writings of Rudolf Carnap; cf. Carus, *Carnap*.

[39] Smith, 'English peasant life-cycles'; idem, 'Kin and neighbours'; idem, 'Rights in land'; idem, 'Families'; idem, 'Social groupings and their relationships in a Suffolk market settlement, 1260–1320', paper delivered at the Social Science History Association Conference, Bloomington, Ind., 1982; idem, 'Coping with uncertainty'.

and from the 'peasant studies' literature centred around the ideas of Alexander Chayanov, Robert Redfield, Eric Wolf, and George Foster).[40] Beyond testing these specific hypotheses, he wanted also 'to establish quantitative variables capable of being used for objective regionalization of medieval rural society'.[41]

Smith recognized that these questions could only fruitfully be studied within the framework of a relatively small 'community'—in this case not a single settlement but the dispersed population of hamlets and farms over which the Redgrave and Rickinghall manorial courts in Suffolk had jurisdiction.[42] This was partly because these questions required an understanding of the local ecology, institutional prac-tices, and social structure. But it was also partly because generating the 'quanti-tative variables' he desired made it essential to link different qualitative sources referring to the same individual (manorial survey entries with manorial court cases, for instance), and resource constraints limited such a labour-intensive procedure to a relatively small group and time period.[43]

Smith's first step was, in effect, to create a set of empirical concepts of his own that arose directly from, and stayed close to, his sources. These were not only the traditional medievalist's sources (tax documents, manorial extents, compotus rolls, and charters),[44] but also manorial court rolls, which were more likely than other documents to record less well-off social groups, but had never 'been used in a systematic, let alone scientific fashion'.[45] He extracted all persons named in these records, noting all relationships among different persons, and linking all references to the same person across different documents.[46] Smith thus created a 'meta-source' consisting of all observed persons, their relationships with one another, and their involvement in particular transactions and conflicts. The organization of the concepts in this meta-source was sufficiently neutral that he was not only able to use them to confront the disparate hypotheses he had begun with, but could then also use the same data to ask questions ranging far beyond those for which it was originally collected, ultimately using it to analyse kinship and neighbourliness,[47] inheritance,[48] customary law,[49] landholding,[50] seigneurial strategies,[51] social strati-fication,[52] and women's economic position.[53]

Using the micro-exemplary approach, Smith was able to achieve a fundamental reappraisal of existing theories about medieval peasants. Medievalists and peasant theorists alike had assumed that peasants had strong family ties to the land, a weak sense of private property, and little market involvement. But Smith found that the sanctity of the family holding was hardly in evidence. Much land was held and sold individually, he discovered, with a substantial proportion of market sales of land to

[40] Smith, 'English peasant life-cycles', pp. 5, 9–10, 101–49.
[41] Ibid., p. 1.
[42] Ibid., pp. 2, 11–14; idem, 'Families', p. 138.
[43] Smith, 'English peasant life-cycles', p. 4; idem, 'Families', pp. 144–6.
[44] Smith, 'English peasant life-cycles', p. 2; idem, 'Families', pp. 138–46.
[45] Smith, 'English peasant life-cycles', p. 2.
[46] Smith, 'Families', pp. 144–6.
[47] Smith, 'Kin and neighbours'.
[48] Smith, 'Families'.
[49] Smith, 'Rights in land'.
[50] Smith, 'Families'.
[51] Smith, 'Rights in land', esp. pp. 114–19.
[52] Smith, 'Social groupings' (see n. 39 above).
[53] Smith, 'Coping with uncertainty'.

non-kin even when inheriting kin were available.[54] Such findings, which could only have been generated through his innovative method for transforming the qualitative data contained in court rolls and land surveys into a 'meta-source' that could be analysed quantitatively, fundamentally changed existing views of the medieval economy which had previously been based on theory or qualitative impressions alone.[55]

The same approach was used by Keith Wrightson and David Levine in their 1979 study of the Essex village of Terling.[56] Their questions differed from Smith's. They began by investigating illegitimacy, but 'the subject of our initial investigation was gradually subsumed within a larger, more comprehensive framework'.[57] They describe this dialectical interaction between data and theory as follows:

> . . . it was never our intention to write 'total history'. We came to our task with certain questions in mind. In seeking answers, however, we let the records speak to us. As our work progressed, our range of questions and of source materials grew. The emerging picture of society in Terling sometimes crystallized and confirmed, sometimes modified or challenged the ideas and prejudices of our earlier work . . .[58]

This was possible precisely because of the methodology they adopted. Their first step was, like Smith, to select a particular community with a population small enough (at 300–600 inhabitants) to make it possible to extract all individuals from surviving documents for the community over a period of nearly two centuries, reconstruct the relationships between them, record other characteristics of these individuals (wealth, social stratum, religious dissent, demographic behaviour), and then link all references to each individual across documents.[59] The 'meta-source' was compiled in a sufficiently neutral way that the researchers could eventually ask questions relating not just to demography—their initial interest—but also wealth, migration, conflict, education, and religion.

The micro-exemplary method led the two researchers to a whole array of striking and pathbreaking findings. Counter to traditional views of the importance of kinship in pre-industrial society, linking family reconstitution data with tax records and wills showed that Terling's kin links were narrow and loose compared to historic French and modern English villages.[60] Counter to traditional theories that pre-modern farmers felt a strong attachment to the family farm, Wrightson and Levine found that market sales were nearly as important as inheritance and few landholdings stayed in the family for more than two generations.[61] And counter to traditional views of closed and immobile rural communities, Wrightson and Levine's innovative mapping of transaction partners showed that in almost every aspect of life a non-trivial percentage of villagers' interactions were with outsiders.[62] They were even able to cast light on traditional views of Puritanism,

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[54] Smith, 'English peasant life-cycles', p. 131; see also idem, 'Families', pp. 149–86.
[55] Schofield, *Peasant and community*, assigns central importance (for example, pp. 2–3) to Smith's work in revealing the commercial nature of the English medieval economy, and thus in overthrowing the previously established Chayanovian view.
[56] Wrightson and Levine, *Poverty and piety*.
[57] Ibid., p. ix.
[58] Ibid., p. x.
[59] Ibid., pp. ix–x, 45.
[60] Ibid., pp. 82–94.
[61] Ibid., pp. 29–31.
[62] Ibid., pp. 48, 69, 75–82.

showing much higher levels of religious dissent among respectable householders than in the lower village strata, who manifested widespread religious indifference.[63] These findings could not have been obtained simply by immersing oneself in the impressions gained from qualitative sources: it was necessary to turn qualitative documents—parish registers, wills, land transactions, recognizance bonds, mortgage indentures, court records, and more—into a 'meta-source' that could be analysed quantitatively, in order to find out how widespread certain activities were, whether in the village at large or among particular social groups. Wrightson and Levine's findings concerning kinship and religion outraged many historians who had relied on qualitative sources alone, but have proved remarkably robust as additional data have emerged for Terling and other early modern English villages.[64]

As these two studies were proceeding, a larger-scale project was underway that spectacularly illustrates the methodological procedure of creating a 'meta-source' of empirical concepts, capable of quantitative analysis, and using it in dialectical interplay to test questions arising from the qualitative sources themselves. In 1963, Wrigley chose the Devon parish of Colyton as the first English village to be subjected to Henry's technique of 'family reconstitution'. Wrigley's initial aim was to test theories about pre-industrial demographic behaviour, arising partly out of the discipline of demography but also partly from the reading of other qualitative sources on pre-industrial English family structure by his colleague, Laslett.[65] Questions about nuptiality, fertility, fertility limitation, and mortality could only be answered through micro-level approaches that required transcribing, indexing, and linking baptisms, weddings, and burials by name and other characteristics. (The notoriously labour-intensive family reconstitution technique still, even now that it has been computerized, encounters serious resource constraints when attempted for larger populations over longer time periods.)

But Colyton was not destined to remain a simple reconstitution study. Over the years, both the original researchers and others who followed them compiled, added, and linked to the family reconstitution a whole array of additional historical documents relating to the parish—inventories, tax documents, poor relief accounts, wills, estate surveys, court records, and many more. One important aspect of this operation was to attach one of four social status designations to each individual and family in the community: this involved the systematic transformation of qualitative into quantitative data, using a hierarchy of criteria beginning with wealth indicators, but then resorting to qualitative indicators such as occupation, landownership, inventoried possessions, wages received, taxes paid, receipt of poor relief, or having children put into pauper apprenticeship.[66]

As a result, what started out as a purely demographic family reconstitution gradually turned into a multi-faceted and continually expanding database which could be used to address a broad array of issues about English society between the

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[63] Ibid., pp. 165–72.
[64] See the quiet but persuasive re-assessment of the 'Terling thesis' in Wrightson 'Postscript'.
[65] See esp. Wrigley, 'Family limitation', and idem, 'Mortality in pre-industrial England'.
[66] Sharpe, *Population and society*, pp. 318–25.

sixteenth and twentieth centuries: nuptiality,[67] marital separation,[68] migration,[69] ageing,[70] disease,[71] baptism practices,[72] childrearing,[73] religious dissent,[74] childhood,[75] poverty,[76] welfare provision,[77] occupational structure,[78] agricultural practice,[79] proto-industry,[80] apprenticeship,[81] women's work,[82] domestic service,[83] and social class.[84] Cross-linking new sources to the family reconstitution also enriched the demographic questions that could be addressed—how demographic behaviour, for instance, varied with social stratum, wealth, and occupation.[85]

This transformation of the qualitative sources for Colyton into a meta-source that could be analysed quantitatively, and the extension of the meta-source over four decades, generated striking findings that transformed many historical debates. The original family reconstitution for Colyton, for instance, generated the astonishing revelation that seventeenth-century couples were already deliberately limiting their fertility, long before the 'demographic transition' of the nineteenth century.[86] Another striking finding emerged in the 1990s when Pamela Sharpe found that, counter to traditional assumptions, marriage rates could sometimes be more strongly influenced by women's than by men's wages and that proto-industrial activity could reduce rather than encourage nuptiality by offering women attractive non-domestic livelihoods.[87] Neither finding could have been obtained without transforming qualitative sources into a community-level interlinked 'meta-source' which could be interrogated quantitatively over a series of decades by historians with very different theoretical preoccupations. No doubt the Colyton database will be used in future to address questions that historians have not yet devised.

The micro-exemplary style of research has not been confined to England, but has also been applied to many other kinds of society. One early example was its application to the Piedmontese village of Alagna by Paulo Viazzo.[88] As in the

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[67] Sharpe, 'Literally spinsters'; also idem, *Population and society*, esp. pp. 161–207, 274–9.

[68] Sharpe, *Population and society*, esp. pp. 290–4.

[69] Schofield, 'Age-specific mobility'; Wrigley, 'Life-time mobility'; Sharpe, *Population and society*, esp. pp. 161–207.

[70] Robin, 'Family care'; Sharpe, *Population and society*, esp. pp. 295–9.

[71] Schofield, 'Anatomy of an epidemic'.

[72] Wrigley, 'Baptism coverage'.

[73] Sharpe, *Population and society*, esp. pp. 252–6.

[74] Ibid., esp. pp. 29–64.

[75] Sharpe, 'Poor children'.

[76] Sharpe, *Population and society*, esp. pp. 208–49; idem, 'Poor children'.

[77] Robin, 'Family care'; idem, 'Poverty'; Sharpe, 'Poor children'; idem, *Population and society*, esp. pp. 208–49.

[78] Wrigley, 'Changing occupational structure'.

[79] Sharpe, *Population and society*, esp. pp. 120–59.

[80] Sharpe, 'Literally spinsters'; idem, *Population and society*, esp. pp. 67–119.

[81] Sharpe, 'Poor children'; idem, *Population and society*, esp. pp. 256–70.

[82] Sharpe, 'Literally spinsters'.

[83] Sharpe, *Population and society*, esp. pp. 271–3.

[84] Sharpe, 'Total reconstitution method'.

[85] Wrigley, 'Marital fertility'; idem, 'Baptism coverage'; Schofield, 'Anatomy of an epidemic'; Robin, 'Prenuptial pregnancy'; idem, 'Illegitimacy'; Sharpe, *Population and society*, esp. pp. 161–207, 279–89; Vann, 'Unnatural fertility'.

[86] Wrigley, 'Family limitation'. Although these findings evoked some debate in the 1970s (see esp. Morrow, 'Family limitation', and the reply by Wrigley, 'Marital fertility'), other studies strongly supported them (see esp. Crafts and Ireland, 'Family limitation'). The most recent consensus is that family limitation was present in Colyton among certain social strata, as well as in some other early modern English communities (see Sharpe, *Population and society*, pp. 189–97).

[87] Sharpe, 'Literally spinsters'.

[88] Viazzo, *Upland communities*.

English cases just mentioned, Viazzo began with questions raised by qualitative research, and then created a 'meta-source' capable of quantitative analysis. The qualitative research, in this case, was 18 months of anthropological fieldwork between 1979 and 1981 in Alagna, which was a Walser (German-speaking) community in the Italian Alps. Viazzo's initial question related to ethnic change, arising from anthropologists' interest in cultural frontiers.[89] But he soon realized that his fieldwork also shed light on the quite different issue of 'relations between the physical environment and human social organization',[90] something of equal concern to anthropologists interested in closed corporate communities and demographers (or geographers) interested in the balance between population and resources.[91] For two reasons, this question was best studied at the community level: first, environmental management is most appropriately examined at the interface where individual human beings interact with ecological constraints; and second, the community itself is generally viewed as the key institution for managing environmental resources.

As in other micro-exemplary studies, Viazzo's approach combined different types of qualitative evidence into a database that could be explored quantitatively. He began with qualitative data collected in his anthropological fieldwork in 1979–81—interviews and an informal village census—but then added parish registers, village censuses, and descriptive sources dating from the fifteenth to the twentieth century.[92] This methodological decision was guided by his realization, based on the qualitative data, that the balance between humans and the natural environment raised questions that could only be addressed by adopting a long-term historical perspective. It was also based on a realization that his informants' statements in 1979–81 about traditional household structures were at odds with the evidence emerging from the quantitative analysis of village censuses.[93] The meta-source that Viazzo compiled from qualitative data subsequently proved susceptible to quantitative analysis not just to address questions of inter-ethnic and class relations, but also family structure,[94] marriage patterns,[95] illegitimacy,[96] communal institutions,[97] and 'environmental history'.[98]

The micro-exemplary approach enabled Viazzo to find that, counter to traditional assumptions, Alpine communities did not stay in balance with their environment only through permanent emigration. He showed that ecological balance had long been managed primarily through late marriage, high lifetime celibacy, seasonal emigration, and low fertility.[99] Counter to models of Alpine communities as 'autarkic' and closed, Viazzo showed that they were often more closely integrated into the wider economy than were villages at lower altitudes.[100] His trans-

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[89] Viazzo, 'Ethnic change'.
[90] Viazzo, *Upland communities*, p. 1.
[91] Ibid.
[92] Ibid., pp. 6–7, 9–11, 127–35.
[93] Ibid., pp. 13–14, 95–7.
[94] Albera and Viazzo, 'Peasant family'.
[95] Viazzo and Albera, 'Population'.
[96] Viazzo, 'Illegitimacy'.
[97] Viazzo, *Upland communities*, pp. 276–85.
[98] Ibid., pp. 1–5, 27–8, 113, 119, 221–2, 277–8, 264.
[99] Ibid., esp. chs. 4, 8, and 10.
[100] Ibid., pp. 276–85.

mutation of qualitative into quantitative data for the village of Alagna, and his sophisticated and wide-ranging comparison of these findings with those for other villages throughout the Alps over a period of centuries, enabled Viazzo to cast serious doubt on both ecologically and culturally deterministic accounts of Alpine demography and institutions.

Another well-known application of the micro-exemplary approach outside England is Hans Medick's 1996 study of the south-west German community of Laichingen. Like Viazzo's study, it shows how this approach can be used to explore not just the demographic, social, and economic questions for which it has so often been used, but quite different fields—in this case, cultural history.[101] Medick began his Laichingen research in the later 1970s with the aim of exploring how 'proto-industrialization'—export-oriented rural industry—affected demographic and economic life.[102] This question arose mainly from the theoretical debate about proto-industrialization, to which Medick himself had contributed significantly.[103] In response to what he found in the qualitative sources for the village, however, Medick expanded his perspective to explore cultural questions such as clothing styles, responses to famine, and book-ownership, confounding cultural historians by finding levels of book-ownership in inventories of modest villagers that rivalled those observed in the capital city of Stuttgart.

Despite this different focus, Medick's community micro-study approach followed the method laid down by Smith, Wrightson and Levine, the Colyton researchers, and Viazzo: concentrating on a single community, creating a database of the individuals and relationships within it using qualitative evidence from a wide range of historical sources, and then using this database as a meta-source which could be analysed both qualitatively and quantitatively.[104] Once again, the concepts of the meta-source were sufficiently close to the data, sufficiently neutral, that they could be used to address questions radically different from those for which it was originally compiled.[105]

In all these cases, the basic demographic, household, tax, and/or property data for the community under study form the quantitative backbone of the 'meta-source' that was created. Our final example pushes this idea of creating a 'meta-source' of data-generated concepts one step further. Here the qualitative source that formed the basis for generating quantitative evidence was more 'purely' qualitative, in the sense that it did not occur (as does a parish register, a tax list, or a cataster) in the form of a list or of serial, discrete, repetitive units that—though qualitative—fall into obvious 'natural' sets ready-made for counting. In this final case there was not, on the surface, anything obvious to count; there was nothing 'naturally' countable (in the sense of section II above).

The context was a study undertaken (by one of the present authors) to gain a better understanding of women's work in pre-industrial society.[106] Impressionistic methods using literary or other qualitative evidence have long held sway in this

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[101] Medick, *Weben*.
[102] Ibid., pp. 16–19.
[103] Kriedte, Medick, and Schlumbohm, *Industrialisation before industrialisation*.
[104] Medick, *Weben*, pp. 19, 26, 28.
[105] Ibid., pp. 26–8.
[106] The results of this study are to be found in Ogilvie, *Bitter living*. The following pages draw heavily on that book.

field, and it has seemed all but impossible to penetrate the barriers of near invisibility in which the female half of the pre-industrial population has been shrouded. To overcome this problem, the study focused on a social unit that could be circumscribed reasonably easily; in this case, the small district of Wildberg in the Württemberg Black Forest, consisting of 10 communities with a combined population of about 5,000 persons in the seventeenth and eighteenth centuries. Detailed reading of one particular source for these communities—the church court minutes—revealed that they contained many references to women's economic activities. To turn this rich qualitative source into quantitative evidence that could be used to test our intuitive understanding of this society, all references to work by both sexes were extracted from over 7,000 handwritten pages of church court minutes for two communities of the district, the small town of Wildberg (with a population of 1,200–1,400) and the village of Ebhausen (with a population of 600–900). This required the adoption of certain conventions. Only references to actual work behaviour were noted, not to occupations or crafts, partly because it was thought that these would be biased toward males, who would be more likely to be formally designated as holding occupations (though not, as was known from informal acquaintance with these records, more likely actually to be doing the corresponding work).[107]

This resulted in a dataset of 2,828 observations of people working. These data did not, of course, represent a *complete* picture of time allocation in this society; they were a sample. But like any statistical sample, this one could be tested for representativeness and robustness in various ways. For one thing, the observations all came from the same society, so the social and economic environment was held constant. Certainly the church court dataset was influenced by the fact that testimony was given under conditions of stress or conflict, in the somewhat unnaturally formal context of a court meeting (normally presided over by the pastor, with three or four fellow citizens also present). However, these same conditions also guaranteed that information not under dispute and peripheral to the case (derived from references to work being done by accusers, accused, or witnesses) was unlikely to be fabricated or merely subjective, as it was validated (considered at least plausible) by all present at the proceedings. So there was no reason to believe that the observations of work might be systematically distorted in any particular direction, and thus no reason to believe that descriptive statistics based on it should be treated more suspiciously than, say, the results of a modern survey, which might also be thought to suffer from inherent collection and situational biases, and from the unintentionally loaded wording of questions.[108]

There are, however, two considerations that make a meta-source of this kind more problematic than the previous examples cited in this section. First, there is the problem already mentioned: court records are not, on the surface, 'naturally' countable. The 'units' to be counted are not clearly suggested by the physical arrangement of the source itself; therefore, their identification requires conceptual intermediation—it requires interpretation. Second, the concept whose instances are counted is a concept of our meta-language, not of the language in which the

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[107] For other conventions adopted in extracting this data, see ibid., pp. 24–6, 29.
[108] Inference from survey data is discussed in Rosenberg, *Logic of survey analysis*.

source is composed. True, this is also the case where we count instances of 'birth', 'death', and so on, but in these cases we can feel very confident, for obvious reasons, that our meta-linguistic concept overlaps closely with the corresponding concept in the object language.[109] In the case of 'work' (even though it appears to correspond quite closely with the object-language concept '*Geschäft*'), we have less reason to be confident, as once again a certain amount of interpretation unavoidably inserts itself between us and the raw evidence.

The solution, in the Wildberg case, was to index the meta-source to such a set of more 'naturally' quantitative sources available for the same locality; that is, to sources whose object concepts coincide more obviously with concepts of our meta-language. This was undertaken so as to anchor the reliability of the more problematic meta-source in those less problematic ones. A wide variety of such more obviously countable documents for this same district—such as parish registers, censuses, tax registers, and guild account books—had, fortunately, been previously compiled into a very extensive meta-source for an earlier project testing hypotheses about proto-industry, demography, and local institutions.[110] If certain basic parameters from the work database matched up to what was known from the 'naturally' quantitative sources whose concepts require less interpretation, it could then 'borrow' its robustness and representativeness from those sources; its reliability would be 'guaranteed' by them.[111]

In itself, this kind of internal cross-check among different sources is nothing new;[112] we find it highly developed even in the earliest of the studies discussed above. In compiling his meta-source, Smith reflected critically on the processes that generated the data, a particularly acute question in the case of court records, which are less standardized than other sources such as parish registers, censuses, or land surveys.[113] For instance, in assessing shifts between post-mortem and inter-vivos land transfers, Smith carefully considered whether one sort of transaction was more likely to be recorded than the other, whether the frequency of court sessions was changing over time, and whether incomplete survival of court records was likely to bias the results.[114] Given the inherent indeterminacy of some qualitative data—'since some kin relationships will have left no traces in the record and a certain instability in surnames is still present at this date'—Smith stated his results as lying within a range of values: thus 'the minimum proportion of customary land so held [by the laterally extended family] was 24 per cent and the maximum 40 per cent. Some intermediate value might be a not unreasonable estimate'.[115] Smith also cross-checked his quantitative findings against other

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[109] Note, however, that changing definitions of stillbirth in baptismal registers, for instance, can affect calculated infant mortality rates.

[110] Ogilvie, *State corporatism* and associated publications cited there.

[111] Once again, such 'naturalness' is entirely practical and ad hoc, with no implications whatever about some supposed ultimate 'ontology' of the society under study. The comparative 'naturalness' with which parish registers and church court minutes lend themselves to counting is a matter of degree, and is in any case a practical question of how easy it is to arrive at agreement (among historians who work with such documents) about what one is counting and what conventions to apply. There is no *ultimate* sense in which the 'naturally countable' data could be regarded as 'hard' and the church court data as 'soft'.

[112] Indeed, it was already a well-established component of the hermeneutic method of German historicism.

[113] Smith, 'English peasant life-cycles', pp. 2–4, 14–55.

[114] Smith, 'Families', pp. 151–3.

[115] Ibid., pp. 145–6.

sources, both qualitative cases from his own court rolls, and quantitative analyses of land transfers in other medieval communities.[116]

In Wrightson and Levine's study of Terling, the linking of data from many different sources to individual names also made it possible to check different sources against one another—for instance, to establish that all those receiving poor relief in the poor rate accounts were also included in tax lists among those excused tax on grounds of poverty.[117] In the Colyton reconstitution, the addition of new data to the demographic backbone made it possible to examine much more thoroughly whether the data resulted from a uniform generating process over the period: quantitative analyses and qualitative sources could be compared to find out whether there was change over time in people's decision to register their weddings or baptisms in the local community or in the Anglican church, and whether different social groups were equally represented in the data.[118]

So the indexing of the Wildberg work database to the basic demographic data merely extended a practice long established within the micro-exemplary approach. The results were reassuring. The 2,828 work observations could be sorted into two categories: work prosecuted as Sabbath-breaking, and work observed 'descriptively', in passing, within other sorts of case. The 'descriptive' observations recorded women nearly in proportion to their share in the population, and while the Sabbath-breaking observations under-represented women, they also revealed sectoral and spatial patterns of observed work that were strikingly similar to those in the descriptive observations. The proportion of women's work to total observations of work was tested for changes over time and remained relatively constant over the entire 150-year period of observation. The proportions of men and women in various marital and household statuses, in the work observation data, were checked against local censuses, and were found to correspond roughly to their proportions in the population.[119]

The nature of what was being observed was also tested against other sources of evidence. The court's regulatory concerns were inescapable, but there were several ways of checking on the extent of this bias. The immanent concept of 'work' (or 'business') was built into the definition of the church court's remit; the Sabbath regulations explicitly prohibited 'all business [*Geschäft*] in house and field, inside and outside the village'.[120] Evidence from elsewhere in the church court minutes (and other local records) showed that the church courts possessed the means to enforce this prohibition and did so in practice. Outdoor work was probably more observable, but the church court was very strongly focused on familial and sexual conflicts and hence likely to be biased toward indoor activities; the two biases to some extent cancelled out. In fact, both indoor and outdoor work were strongly represented in the database. The types of working activity manifested in the church court work database were compared against an outside source—a 'soul-table'

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[116] See, for example, ibid., pp. 159–73.

[117] Wrightson and Levine, eds., *Poverty and piety*, p. 34.

[118] Wrigley, 'Problems'; Schofield, 'Representativeness'; Sharpe, 'Total reconstitution method'; idem, *Population and society*, esp. pp. 1–28, 305–6, 313–17.

[119] This is detailed in Ogilvie, *Bitter living*, tab. 1.3, p. 27, and the accompanying discussion on pp. 25–9. The data for the underlying population are derived from censuses of Wildberg in 1717 and 1722.

[120] Only in Oct. 1799 did the central government in Stuttgart relent, and allow the pursuit of 'occupations, business, and trades' on Sundays and holidays, as long as they were not carried out during or before actual church services; ibid., p. 29.

listing the 'trade and livelihood' of all independent earning units in the same district in 1736—and this comparison showed that the church court observations covered all the different sectors and individual activities listed in the soul-table. Finally, the fact that the church court work observations extended across all sectors of the economy, registered indoor as well as outdoor work, and recorded such intimate activities as washing the parlour floor, soaking laundry at home, rolling out biscuit-dough, serving a meal, making a fire, cleaning up drunken vomit, washing lettuce, cooking gruel for a newborn infant, boiling milk, and mending a husband's trousers, suggest that it penetrated further into the realm of time allocation than has usually been thought possible for the invisible half of pre-industrial society.

The study of women's work based on this dataset in conjunction with a large body of other qualitative and quantitative evidence[121] came to different conclusions in many respects from those based on more impressionistic use of literary and other qualitative sources. We will not attempt to summarize those conclusions here.[122] However, an example will illustrate the gains from this extension of the micro-exemplary method. It has generally been thought, on the basis of (post-industrial) common sense and impressionistic use of qualitative evidence, especially from literary sources, that women's work options in pre-industrial society were constrained (a) by their physical strength, especially upper-body strength, and (b) by the need to be in or near the dwelling, to care for small children and do housework, and by the limits on mobility imposed by pregnancy.[123] These hypotheses lead to the prediction that we would find women much more heavily engaged in light, domestic activities (such as housework, child care, or weaving) than heavy, non-domestic activities such as day-labouring or agriculture. But the reverse was the case. Less than half (42 per cent) of all female work observed was indoors, a proportion not so different from that of male work (37 per cent). The sexual division of labour was more flexible than has often been thought, or than contemporary published opinion approved. In agriculture, especially, women made up an unexpectedly high percentage (nearly a third) of the observed labour force. In guilded crafts, in contrast, women were almost completely absent. This finding directed attention to gender discrimination by social institutions, rather than gender-specific biological characteristics, as the fundamental variables in explaining variations in the sexual division of labour.

This is an example of a simple, factual finding that would be practically impossible to discern by simple immersion in the church court records—as impossible as finding a mortality rate by immersion in parish registers. The improvement in understanding is of the same nature as that expounded by Wrightson above for the use of quantitative illegitimacy and bridal pregnancy ratios as a control on qualitative statements about sexual conformity. Of course the real power of such controls becomes fully evident only when one advances beyond simple factual findings of this kind to the study of *why* these facts obtained, and what their larger

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[121] As well as against the background of a series of previous studies of the institutional structure of this same society and its larger social and economic context: Ogilvie, *State corporatism* and further publications cited there.

[122] An overview can be found in Ogilvie, *Bitter living*, ch. 7; also eadem, 'Social capital'; eadem, 'Women and labour markets'.

[123] See the literature reviewed (and criticized) in Ogilvie, *Bitter living*, pp. 7–9, and eadem, 'Women and labour markets'.

consequences might have been. Here the availability of solid ground under one's feet—a backdrop of well-defined, often measurable ad hoc empirical categories—enables us to extend the rigorous analysis hitherto reserved to demographic questions about households, migration, labour markets, and servanthood to questions about the sexual division of labour and restrictive local institutions.

IV

Pared to essentials, the micro-exemplary method consists in upholding a distinction between a historical meta-language and the object language of the society under study, and in maintaining *both* these perspectives. It amounts to an anti-reductionism: the refusal to sacrifice *either* the sober, science-informed third-person perspective of our objectivity-aspiring meta-language *or* the qualitative, holistic, first-person understanding of the object language we are studying. Nearly all the features of the method are simple consequences of this dual commitment. Since neither meta-language nor object language is regarded as reducible to the other, they must co-exist in dialectical interchange. And since the purposes of history are many, it is best to constitute our basic meta-language, at the outset, with concepts that are as close to the data—as neutral and 'natural'—as possible. This maximizes the usability of these concepts for the wide variety of different theoretical perspectives that different users of history will want to apply.

The only other assumption from which the micro-exemplary method proceeds is a practical constraint: the recognition that the historian is in the peculiar situation of utter dependence on data. More than any other kind of intellectual worker, the historian has to make the data her starting point. Of course when she immerses herself in her data, she brings all her theoretical interests, her ideological prejudices, her first-hand knowledge of her own and other societies, all her reading and her ignorance, to the table. These are her privilege: they give her questions to ask of the data—the more the better, as she will soon find, upon immersion, that the data are highly uncooperative. There is almost never a smooth fit between a question or hypothesis one entertains at the outset and the documents one encounters. Immersion in data is like any confrontation with a new and foreign culture; it requires adjustment, reorganization of one's categories, recognition that there are ways of thinking and feeling that are recognizably human but differ from one's own. So to insist on the need to *begin* with an explicit theory—as Karl Popper and his followers sometimes did—is simply a failure to acknowledge the peculiar constraints of historical research.

There is nonetheless a germ of truth in Popper's rejection of mere fact-gathering empiricism.[124] The slogan of 'total history', as propagated by the second-generation *Annales* school, was briefly fashionable, but the danger of achieving nothing more than a shapeless mound of meaningless data bits, a kind of archival core dump, soon became apparent.[125] History cannot be neutral, comprehensive description, but must always answer some directed question. And the most natural context for such questions to arise is that of immersion in documents—not that the

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[124] Popper, *Logic of scientific discovery*, p. 280; also idem, 'Logik der Sozialwissenschaften'.
[125] Wrightson, 'Villages', p. 634, refers to remarks by Skinner ('Role of history') to indicate the wider disillusionment with the idea. See also Medick, *Weben*, ch. 1.

documents themselves could somehow suggest the questions, or that a mechanical pasting together of facts could even so much as guide the historian to a sense of what was worth asking. The historian's questions come, rather, from whatever she brings to the process to begin with. Her own questions and preoccupations will always at least partly determine what appears to her as an internal discrepancy, or what seems to distinguish this society from others she knows. Only the dialectical process of quantitatively testing the view that results from qualitative immersion in documents, revising that view, re-testing it, and revising it again (and so on) anchors the intuitive 'feel' for the society in a more intersubjective realm of public discourse.

The historian's starting point, then, is neither within the historical meta-language she ultimately wants to construct nor in the object language (which at this early stage is still largely foreign). The formation of her meta-language results, rather, from a dialectical process of inquiry, whose first step is the culture clash between the historian's own socialization, her own categories, and those of the documents she confronts. From this first encounter she begins to learn some of the vocabulary and grammar of the object language. She is not very fluent yet; there are many gaps. But it is enough to reveal discrepancies, to raise questions. And so she can begin to try to test some of her hunches by systematizing the qualitative data into a rough and ready meta-source.

By means of this systematization, the historian begins to construct the first, rudimentary components of an appropriate meta-language. But in doing so she must maintain a certain distance; she must step back from her particular, imme-diate guesses and questions; she must restrain herself from catering to them exclusively. She must keep in mind that she will also have future questions—that she is working, ultimately, toward a standpoint, expressed in the meta-language, that integrates her future, more fluent knowledge of the object language with some version of the impersonal, scientific standpoint that represents her aspiration toward objectivity.[126] The extraction of quantitative from qualitative data is an early step in this dialectic, and the aspiration to objectivity is reflected in the striving to keep the categories of the resulting 'meta-source' as neutral, as immanent, and as close to the data as possible, to leave open the possibility of asking things in future that were not thought of at first.

This neutrality desideratum is in obvious tension with the question-drivenness of the original immersion in documents. But it is equally indispensable, because the particular aspect of the society focused on at first has to be seen ultimately in the wider context of the entire society in which that aspect is embedded. This does not mean reconstructing the entire society (in 'total history' fashion), but it means that the data gathered in response to the original questions should not solely be

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[126] We regard the dialectical process described here to be one whose outcome is the *replacement* of the vague intimations one began with, in one's initial immersion in the object language, by a more comprehensive and informed view in the meta-language that does justice to—is informed by—everything learned through that immersion. That is the sense in which a narrower 'subjectivity' is replaced by a wider perspective of 'objectivity', but there is no assumption that the viewpoint attained is a 'true' or 'correct' one, from some imagined *ultimate* viewpoint of objectivity; there is only more and less. We assume that although one's initial immersion is 'theory'-dependent (guided by one's prejudices, prior knowledge, and so on), there is often no way of *knowing* what that 'theory' is other than by *using* it, trying it out in the process of inquiry.

usable for answering only those particular questions.[127] Evidence can never, of course, be completely theory-neutral, but the neutrality desideratum drives us to make it as neutral as possible under the given conditions.[128] We aim for empirical robustness, as in the example of the household literature. Otherwise we cannot hope to recover the wider social, institutional, and economic context of the particular phenomenon we are interested in studying. We cannot see it in three dimensions.

The micro-exemplary requirement that the investigation focus on a relatively small community is an immediate consequence of the need to balance these first two desiderata of the method (question-drivenness of the initial immersion and neutrality in compiling the meta-source), in the context of dialectical iteration between qualitative immersion and quantitative testing. If the extraction of quantitative from qualitative data is—on the one hand—to be driven by questions, but also—on the other hand—in a way that makes it possible to study the wider social and institutional context of this question, then an upper bound is set on the size of the community it is practically possible for a single researcher (or even a small group) to study in a limited number of years. It is not clear exactly where this limit lies, and perhaps it varies with different kinds of study and with the technology available to the historian. But to judge from existing community studies, it seems to be about 5,000. For the kind of reasonably well-documented community needed for such research even to be feasible, a larger community would have too many documents for even a reasonable cross-section to be sampled within a few years to yield a coherent 'feel' for the workings of the community as a basis for quantitative cross-checking.[129]

How 'close to the data' a concept is, and how 'naturally' it lends itself to counting, are practical matters, and matters of degree, as we saw, not of kind (let alone of ontological taxonomy). The concept of an 'illegitimacy ratio' obviously has no meaning in a society that lacks a stably defined institution of marriage. And as we saw above, there are differences among societies in their definition of 'household', which can make even the most straightforward household classification scheme appear problematic. So the creation of a meta-source for studying

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[127] This, again, is why the early efforts at record-linking software sought to be as flexible as possible in the way they stored data and enabled it to be accessed; see above, n. 32.

[128] In the long-running debate about the 'theory-dependence' of observational evidence, following N. R. Hanson's revival of Duhem's famous point that 'it is impossible to leave outside the laboratory door the theory we wish to test' (*Aim*, p. 182), most participants agree that theory-neutral evidence is impossible. But this literature usually regards 'theory-dependence' as either present or absent, not as a matter of degree. And yet in most practical contexts it *is* regarded as a matter of degree—and as improvable by appropriate interventions. Law courts, for instance, accept that evidence is inevitably somewhat tainted by the particular viewpoint of the witness, and corroborating evidence is required in most cases. There is no standard of complete or final objectivity in establishing questions of fact, only better and worse. This, it seems, is also the attitude implicitly adopted in practice (though less consistently in their theoretical pronouncements) by most historians.

[129] Our figure for the maximum feasible community size assumes a highly multi-stranded society of the kind one would expect to find in early modern continental (especially central, southern, or eastern) Europe, or in many parts of present-day less-developed countries. In modern European or more developed Asian countries, relations are more single-stranded, so the number of links among individuals is normally much lower. This would then mean that, depending on the kind of group or 'community' studied, the maximum population in a micro-exemplary study of a modern society could be much larger than 5,000. Cohen and Morgan, for instance, applied something very like the micro-exemplary method in their study of the Communist Party of Great Britain, with a total population of around 20,000 ('Stalin's sausage machine'). The relevant upper constraint, then, is not the population size but the number of links.

a particular community will always be somewhat ad hoc. There is no way of knowing in advance which concepts will endure in the course of a research project and which will be ultimately winnowed out. It is only by proposing concepts (classification systems, categories) that seem 'natural' for some particular, given set of documents from a particular community, and then using them, that progress can be made. It is only against some defined set of existing classifications that researchers studying other communities in other parts of the world can discover that some tentative classification does not work in their case. So the true ultimate potential of the micro-exemplary method lies in the number and variety of studies that use it. However interesting and valuable any particular study might be, it is in their cumulative contribution that the real value of micro-exemplary studies lies.[130]

Meanwhile, though, each individual micro-exemplary study has no choice but to do its own laundry; it has to go as far as it can toward ensuring that its data are internally consistent. Studies that employ a single source over a long period, for instance, can usually determine whether the generating process underlying that source is uniform, in relevant respects, over the period studied. Qualitative immersion in the documents should make adequately clear whether the institution or social group generating those documents changed in any way over the period studied, and whether such changes would have been likely to affect the consistency and comparability of the information produced over that period. If there were such changes, then either the period for which the quantitative data is extracted is changed to fit, or further cross-checks can be undertaken to be sure that the changes introduce no systematic biases of relevance to the questions being asked. (Obviously this needs to be reconsidered each time these same data are to be used to answer a new kind of question). One kind of change that may affect the consistency over time of the information from a data source is volume. A court that hears 10 cases per month may well change its procedure (and thus the information to be gleaned from its records) if this increases to 100 cases per month. Cultural changes may affect the emphasis of courts whose main interest is social control (such as church courts). Even the quality of information from parish registers can be affected if, for instance, there is a gradual change (or a change from one parish scribe to the next) in the recording of illegitimate births, emergency baptisms, or circumnatal deaths. Often such changes can be controlled for, and quantitative data from the series of sources can still be used; they can still act as an external control on the subjective impression the historian gets from qualitative immersion. But it is important to be aware of such changes, so that if the data are to be applied to a question where the shift is of relevance, suitable cross-checks are applied, and an appropriate degree of scepticism attaches to the results.

In these and other ways, the meta-source compiled for a particular community can be tested for reliability and representativeness, to ensure that the quantitative cross-checks on the historian's initial, subjective response to the sources in their

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[130] There is therefore a crucial role to be played by studies that juxtapose and appraise numerous micro-exemplary projects (the micro-exemplary equivalent to the 'meta-analysis' employed in medical research and other fields). A good example of this is Ehmer's brilliant synthesis of a number of detailed local studies with the theories of early modern German intellectuals and bureaucrats on the necessity for the early modern state to control access to marriage and full economic rights by 'economically and morally weak persons' in his *Heiratsverhalten*. Cf. also Sabean, *Kinship*, pp. 398–448, and Schlumbohm, 'Sozialstruktur'.

qualitative aspect are as robust as possible. Of course, nothing will ever guarantee that the data are absolutely reliable, just as no particular observation in biology or physics is ironclad; we are not dealing in certainties. And in history—why deny it?—we are considerably less certain than in theoretical sciences. History is messy. But this is no more reason than in medicine not to do as well as we can. Applying quantitative cross-checks to our qualitative, hermeneutic, holistic grasp of the documents minimizes our chances of going drastically wrong.

The good news is that once we have established a meta-source with a reasonably high degree of internal consistency (as well as external consistency with other, comparable communities), we can then leverage the robustness and reliability achieved and 'transmit' it, so to speak, to other databases, like the work database used in the study of gender-specific work patterns discussed above. So the (by now) age-old technique of turning qualitative into quantitative evidence (and using them in dialectical feedback with each other) can be greatly expanded, it turns out, far beyond the relatively narrow confines of 'naturally' countable sources: quantitative data can in fact be generated even from the most 'purely' qualitative data (like court records), and can parasitically derive the reliability and robustness they need (to function as an effective cross-check on qualitative immersion) from a meta-source relying on more 'naturally' countable sources. If these cross-checks confirm the reliability of the new meta-source, then they also, of course, give additional support to the trustworthiness of the 'naturally' countable sources; the support gained is mutual, if (as in the women's work study) the data come from mutually independent sources.

Even without the vast perspectives opened up by this new extension of the micro-exemplary method, it has brought enormous progress to the study of past societies in many parts of Europe. But the prospect of using what had seemed purely qualitative sources to generate quantitative data vastly expands the possible applications of the method. It suggests that a much wider range of issues could be addressed by it than has ever been thought possible. There has of course never been any reason why it could not be applied to societies outside Europe, and in later periods than the pre-industrial, such as the long-running debate about the impact of the industrial revolution on the standard of living, the origins of the nineteenth-century demographic transitions in France and England (and the twentieth-century ones elsewhere), or how quite different institutional systems in non-European societies affected the process of economic growth and the distribution of its gains. The new possibilities offered by turning a wider range of purely qualitative evidence into quantitative evidence, though, extends this list far beyond the scope of what has traditionally been considered 'social and economic history' to almost any imaginable sphere.[131] The 'community' to be studied in micro-exemplary style need not be a physical community, in the traditional sense. It can also be an ethnic community in a modern city, a nineteenth-century university, a scientific discipline, a government department, a business corporation, or, to cite

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[131] Like any other approach, this one, too, would eventually come up against limits. Its great advantage clearly lies in the *depth* it gives to an investigation; where breadth is required, other methods are needed. Our message here is that it is much too soon to say where the limits of the micro-exemplary approach might ultimately lie; only trial and error can give the answer.

a specific example that has recently been studied in something like a micro-exemplary style, the prosopography of the Communist Party of Great Britain.[132]

The only obstacle to the much more widespread use of the micro-exemplary method to address such questions is the narrowness imposed by the disciplinary organization of the social sciences. The two perspectives to which the micro-exemplary method is fundamentally committed reside in different disciplines, and there is presently no single discipline that rewards the very labour-intensive conjunction of them required by the micro-exemplary method. The holistic immersion in qualitative data is the province of history and anthropology, while the collection and statistical manipulation of quantitative data is the province of economics, econometrics, and social psychology. (Sociology, geography, and political science are split.) Max Weber's hope for a fusion of these two perspectives is realized in the micro-exemplary method, but the vast majority of historians and economists are contented within the self-imposed restriction to only one of these two. It is to be hoped that a sufficiently large minority on both sides can overcome these barriers. The work is hard, but the rewards are great. If our institutional limitations can be overcome, there is every reason to believe that, for all its achievements so far, the micro-exemplary approach is still in its infancy.

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[132] Cohen and Morgan, 'Stalin's sausage machine'. The authors have also reached methodological conclusions remarkably similar to those set forth in this article; see G. Cohen, A. Flinn, and K. Morgan, 'Toward a mixed-method social history: combining quantitative and qualitative methods in the study of collective biography', paper delivered at the European Social Science History conference, Berlin, March 2004. Cohen, 'Missing, biased, and unrepresentative', discusses some of the technical challenges peculiar to such an approach.

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