

Detailed Description: Rural Commercialisation in Southern Germany, c.1200–c.1440

Objectives

This project is a study of rural commercialisation in southern Germany c.1200–c.1440, with a focus on the modern German province (*Land*) of Bavaria, but including some regions falling within the current *Land* of Baden-Württemberg, and Austrian Tyrol. The objectives of this project are (i) to track the changes in the extent and nature of socio-economic differentiation, market involvement, and market dependence; and the evolution over time in the relationships between commercialisation and ‘feudal’ social-property relations; (ii) to compare these developments in this region with other parts of Europe, primarily England, where it is now generally accepted that this was a period of widespread commercialisation; (iii) by this means to provide a new way of understanding processes of medieval commercialisation in Europe and elsewhere and whether, how, and to what extent we can understand the developments of this period as leading to the emergence of capitalism; and (iv) to make available to the scholarly community the information that can be gleaned from the bulk of the extant sources by means of a publicly accessible online database, and potentially some print and/or digital editions of the source base, thereby providing the material foundations for further comparative research on late-medieval European social and economic history.

Context

One of the most significant issues in global economic history is the process by which societies move from relatively simple and largely self-sufficient systems (in which households and small communities produce all or most of what they consume) to more complex arrangements in which exchange, markets, and specialisation of production begin to predominate, which in turn allow for and encourage further growth. Some form of such a complex socio-economic system is in turn arguably a necessary prerequisite for the evolution of modern industrial capitalism, which depends on a) a highly productive and specialised agriculture that allows for b) a specialised and growing non-agricultural labour force while also creating c) a large consumer base for mass-produced non-luxury commodities. In the context of post-Roman European history, it is generally agreed that there was a growth in such complexity from c.800 (van Bavel 2010; Kuchenbuch 1978; Kuchenbuch 2016; Loveluck 2013; McCormick 2001; Naismith 2014; Rösener, ed. 1989; Verhulst 2002; Wickham 2005), although development was comparatively slow until at least the eleventh century. In northwestern Europe, however, from around 1200, market relations grew in importance within an increasingly commercialising society characterised by higher and growing levels of interregional interdependence and regional specialisation (van Bavel 2016; Duby 1962; Hoffmann 2014). While this is acknowledged as a common development in much of western Europe, the issue has been studied in much more detail for England than anywhere else (Britnell 1996; Campbell 2000; Dyer 1998; Dyer 2005; Masschaele 1997), although recently the Low Countries have also received considerable attention in this respect (van Bavel 2010; Hoppenbrouwers and van Zanden, eds 2001; de Vries and van der Woude 1997).

In England, in the period of growing commercialisation landlords increased the levels of direct management of their lands, maintained by services or fees owed to landlords by their tenants, in many cases in the form of obligatory labour services (the ‘feudal’ or manorial system) (Britnell 1996; Campbell 2000; Dyer 2002; Fox 1996). In contrast, in the German lands (as indeed in many other parts of western Europe), direct management had been, by c.1300 at the latest, largely abandoned by the major landowners, as landlords leased out their properties for rents (Arnold 2004; Dollinger 1982; Duby 1962; Ghosh 2014; Ghosh 2016; Ghosh 2017; Henning 1994; Hoffmann 2014; Kuchenbuch 1997; Kuchenbuch 2004; Rösener 1983; Rösener 1985; Rösener 1991; Schaab 1983). It has been suggested that the reasons for the disintegration of the manorial system in the German lands were precisely increasing commercialisation, urbanisation, and monetisation (Arnold 2004; Rösener 1991), yet given

that the same developments are evident in England where the manorial system remained in place, and indeed arguably experienced its apogee in precisely the same period (*c.*1200–*c.*1350) when it was being dismantled in Germany, such a conjecture is clearly untenable. Comparative empirical study is therefore crucial in order to understand the relationships between economic developments (commercialisation of production and increasing market involvement and dependence) and changes in tenurial and socio-legal structures and forms of exploitation ('feudalism'); such a study has never been carried out.

The proposed project intends to fill this gap by means of an analysis of rural commercialisation in one German region, the results of which will then be subject to detailed comparison with studies of England in the same period. Furthermore, the years around *c.*1350 have often been presented as a period of 'agrarian crisis' (e.g. Rösener 1985), and indeed it has been argued that it is because of different reactions to this crisis that the feudal system broke down in England, but not in Germany (Brenner 1985a; Brenner 1985b). Extending my analysis past this date into the first half of the fifteenth century will provide an empirical basis for understanding how this crisis might have affected economic development in this region—and indeed the extent to which it is fair to characterise this period as one of agrarian crisis. Here I will draw on and provide a greater empirical refinement for the revisionist arguments of some recent German scholarship (Kießling 2005; Schuster 1999; Vasold 2003).

These issues lead us to the wider theoretical and chronological context of this project. After 1350, the manorial system, and with it the institution of personal bondage or serfdom, largely disintegrated in England (Bailey 2014; Britnell 1996; Dyer 2002; Dyer 2005; Whittle 2000), while personal bondage of various kinds remained legally in force in the German lands till the late eighteenth century, although the specifics of the manorial system—access to land in exchange for labour—had long been largely abandoned, and even the payment of natural rents had largely been commuted for rents in money. With regard to social differentiation, commercialisation, market dependence, and regional specialisation, however, it is debatable that there was any truly significant difference between England and at least some regions in southern Germany even after the end of serfdom in England (Ghosh 2016; Grees 1975; Koch 2007; Rebel 1983; Robisheaux 1989; Sabeian 1972; Scott 2001; Scott 2002; Scott 2012; Sreenivasan 2004). Robert Brenner's hugely influential theory proposed that in the fifteenth and sixteenth centuries in England we witness the origins of capitalism: a transformation of class relationships arising from the death of the feudal system was the prerequisite for this (Brenner 1985a; Brenner 1985b; Brenner 2001; Brenner 2008). Many scholars have rebutted the 'Brenner Thesis'; however, the critique has generally been limited to analyses of England alone, and has thus sidestepped one of Brenner's most important contributions, the comparative perspective (French and Hoyle 2007; Glennie 1988; Hatcher and Bailey 2001; Hoyle 1990; Mate 1993; Whittle 2000; cf. Dimmock 2014). Most studies attempting to refute the Brenner thesis have also been confined to undermining empirically Brenner's claims, without providing any alternative theoretical framework. Thus while Brenner's thesis may no longer appear plausible with regard to explaining the origins of capitalism, no other truly satisfactory theory has been proposed in its place. Furthermore, the scholarship on medieval commercialisation, like the debates on the origins of capitalism, have been largely concerned with England, and have generally ignored Germany—although Germany was clearly a highly successful capitalist economy since *c.*1870 at the latest. Only a few contributions have, implicitly or explicitly, questioned the Brenner thesis from the German perspective (Sreenivasan 2004; Wunder 1983; Wunder 1985). The long pre-history that led to German capitalism has not been explored with anything like the kind of empirical and theoretical rigour that has been applied to England.

Capitalism without markets and commercialisation is unthinkable, and thus precisely the developments to be found in England in the thirteenth and fourteenth centuries are necessary preconditions—though not a sufficient cause—for the later evolution of capitalism. While it is acknowledged that similar processes took place in Germany, commercialisation in medieval Germany has scarcely been studied as a subject in and of itself, and has never been empirically examined in a

comparative perspective. Given this gap in the research, it has been easy to retain a belief in English uniqueness in its path to capitalism. In a preliminary paper synthesising what can be gleaned regarding commercialisation and market dependence from a diverse body of scholarship (Ghosh 2016), I have argued that with regard to these factors, many German regions appear to be similar to England. The hypothesis advanced in this paper is that despite differences in tenurial and socio-legal structures, in terms of economic developments, and in particular with regard to market dependence and commercialisation, at least some regions in Germany seem to be as advanced as England in the thirteenth and fourteenth centuries, and thus the medieval prerequisites for the later evolution of capitalism are to be found in both Germany and England; commercialisation matters more than tenurial structures. Given the lack of rigorous empirical analysis of medieval commercialisation in medieval Germany, my hypothesis urgently needs to be refined on the basis of more detailed and comprehensive studies. In particular, there has been very little quantitative analysis of German sources laying out the extent of social stratification and market dependence in this period, and while my recent publications (Ghosh 2014; 2017) make a start on this, the proposed project would, because of its much greater scope, provide a truly robust foundation for an understanding in comparative perspective of the nature of medieval commercialisation and its relationship to the manorial system in Germany and England. In doing so, it would also provide a better starting point for understanding the paths towards capitalism taken in both countries. My proposed project thus builds on my recent work, while also serving as a basis for studies on an even larger scale addressing these issues in a very long-term comparative perspective.

It should be noted further that the results of this project would also open up new horizons of research by making up for significant shortfalls in the study of pre-modern German economic history: although there have been a number of regional studies and micro-histories that are of relevance to the issues discussed here, Germany is notably impoverished with regard to syntheses on these matters. There are no German equivalents to Britnell's work on the commercialisation of medieval English society (Britnell 1996), Dyer's studies of consumption and standards of living, and the later middle ages as an 'age of transition' (e.g. Dyer 1998; Dyer 2005), Campbell's study of the manorial system (Campbell 2000), or Bailey's recent reassessment of the chronology and causes of the decline of serfdom (Bailey 2014). From a theoretical perspective, while the work of Hatcher and Bailey (Hatcher and Bailey 2001) is obviously to some degree transferable to regions other than England, there is nevertheless nothing equivalent for Germany. Van Bavel's recent important synthesis of the medieval economic history of the Low Countries, which does not adopt a comparative perspective but does address some of the theoretical issues discussed above, and in particular stresses the importance of regional differences and interregional symbiosis, geography and ecology, and institutional endowments, is also without comparison for Germany (van Bavel 2010). A single five-year project with one PI obviously cannot hope to provide the equivalent, for Germany, of the seven monographs by six scholars listed above; but by beginning to address these issues, and doing so in a comparative perspective, it will be able to lay the foundation for a future, Britnell-inspired synthesis on the medieval commercialisation of German society. Furthermore, by creating a comprehensive database of the available evidence for a wider scholarly audience (which will stretch somewhat beyond the period and region covered by the monograph), this project will provide the raw material and incentives for both new research on medieval German agrarian and economic history, and the resources for scholars working on other regions to engage in comparative work without needing to immerse themselves in the original archival documentation.

In this context it is important to stress that one of the key further aims of this project is to build a bridge between the German and English scholarly traditions, and hopefully incite scholars other than myself both to undertake further study of these issues with regard to Germany, and equally to incorporate a comparative perspective when studying England. In particular, given that there is hardly

any scholarship available in English on the economic history of medieval Germany, and almost nothing that introduces the source material to an Anglophone audience, it is important to stress that the outputs resulting from this project will, in addition to presenting detailed empirical analyses of rural commercialisation in southern Germany as compared with England, also both provide an introduction to the context of German economic and agrarian history in this period, and present a database of the evidence and an overview of the available sources and scholarship including discussion of those not used for the analysis in this project. Thus hopefully the results of this project will stimulate not only further debate in the Anglophone scholarship, but also more comparative studies of the German material by scholars within the German-speaking countries and elsewhere.

It is thus envisaged that by bringing the subject within the scope of much larger debates in economic history and demonstrating its importance to the broader field of medieval and economic history beyond just the German lands, this project will also reinvigorate interest in medieval agrarian history among younger German scholars. In the last two decades, this has been a largely moribund field, and while there is still a lively tradition of scholarship on Swiss agrarian history (see e.g. Hürlimann and Sonderegger 2011, Rippmann 2001, Sonderegger 2012a, Sonderegger 2012b), this has mainly focused on the fifteenth and sixteenth centuries, rather than the period between 1200 and 1400. The vast amount of extant but largely untapped primary source material urgently requires a great deal of further research. By bringing different national scholarly traditions into dialogue, and by making more widely known the extent, scope, and possibilities for research inherent in the sources, it is to be hoped that the field may experience a new lease of life, and that there will take place some cross-fertilisation of the different questions and approaches, to the mutual benefit of both scholarly traditions.

Methodology

This project arises from my past and current research. In 2014 I published a paper on the estates of the abbey of Ellwangen in Baden-Württemberg in the mid-fourteenth century and in 2017 I published a similar study on the estates of the abbey of Scheyern in Bavaria (Ghosh 2014; 2017). The former paper was based on a polyptych (a type of source that provides details regarding expected services and renders) (Häfele, ed. 2008), and the latter was an analysis of a set of accounts (Toch, ed. 2000). The purpose of these studies was to understand the extent of market involvement and dependence, the extent of social stratification, and the control by the landlord of the labour and resources of its tenants. My paper on Ellwangen provided a snapshot from one phase of the abbey's economy 1337–44; my article on Scheyern examined change over the course of three decades in the middle of the fourteenth century. While the questions posed and methodology of the proposed project are similar to those of these two papers, it will greatly extend the scope of my earlier work both by examining sources from a large number of landlords, and also by including within its remit a much longer period in order to understand change over a broad span of time. There will also be far more detailed comparison with the scholarship on rural commercialisation in England in the period than is possible in individual journal articles.

The theoretical background is provided by a recent publication (Ghosh 2016), in which I suggest that in order to understand both the nature of the socio-economic system of this period, and the prerequisites and processes by which capitalism eventually came into being, it is crucial to examine more closely, and in comparative perspective, the extent and nature of market dependence and the numerical weight of the 'sub-peasant classes': that is, those persons lacking a full holding—sufficient land to support a family—for which reason they are dependent on some form of exchange, whether of their labour for subsistence, or, as is increasingly most likely the case, market exchange. My project will provide a robust empirical examination of the hypotheses proposed in this paper, with refinements as necessary, and lay the foundations for adumbrating a new, overarching theory explaining in global perspective the processes leading first from autarky to commercialisation, and then from commercialisation to capitalism.

The principal types of sources to be used are rent or income lists, which provide information on money and commodities rendered; polyptychs; and accounts, which provide details regarding both actual incomes and expenditures (on these kinds of sources, see Bertelsmeier-Kierst 2008; Bünz 1995; Bünz 2002; Fossier 1999; Lübbers 2009; Sonderegger 2012b; Volkert 1966; specifically on Bavarian polyptychs, see Wetzel 1995). Apart from the accounts of Scheyern (Toch, ed. 2000), the account books of the abbeys of Kaisheim and Aldersbach from this period have also been published (Bruch 2013; Lübbers, ed. 2009). Manorial accounts are a relatively little-used source in the German scholarship, in part because they are comparatively scarce in Germany, whereas in England large numbers of manorial accounts are extant until direct management by landlords began to be abandoned in the later fourteenth century (Campbell 2000). Nevertheless, although fewer, and less standardised in form and content than their English counterparts, a good number of hitherto largely unexamined German manorial accounts do exist, and therefore, not least because of the comparative aims of this project, studies of extant south German accounts will be a significant component of the resulting monograph (a study of the accounts of Aldersbach, co-authored with my student research assistant, will be completed by September 2018).

In contrast to accounts, there are many more polyptychs extant from this period, and similarly many more of them are available in print editions; there are also a number of other serial sources listing rents and other forms of income, of which relatively few have been published, though thousands of pages are extant in manuscript for this period. The earliest such sources from seventeen ecclesiastical institutions in Bavaria have been published (Busley, ed. 1961; Geier, ed. 1969; Geier, ed. 1986; Gruber, ed. 1985; Hofmann, ed. 1983; Hoffmann, ed. 1959; Holzfurtner, ed. 1985; Klose, ed. 2003; Mai, ed. 1966; Maidhof, ed. 1933–9; Müntefering, ed. 1986; Schlögl, ed. 1970; Stephan, ed. 1988; Thiel, ed. 1958; Thiel, ed. 1996; Thiel and Engels, eds 1961; Weißthanner, ed. 1957). Many of these published sources, however, cover only brief periods and are relatively limited in size and scope. As in England, most available sources are from ecclesiastical institutions, but from Bavaria we also have series of ducal polyptychs, of which the earliest, from the 1230s, exists in a good edition (Heeg-Engelhart, ed. 1990), and there is an extensive series of unpublished ducal polyptychs and rent lists from the thirteenth through to the end of the fifteenth century (Bertelsmeier-Kierst 2008; Volkert 1966).

The proposed project arises out of research funded by my Insight Development Grant. For that grant, I proposed to work primarily with published sources listed above, along with a limited amount of manuscript material, to arrive at a preliminary analysis of commercialisation in Bavaria alone *c.*1250–*c.*1400. The project I am currently proposing intends to expand the temporal scope by about one century; it will also vastly increase the amount of unpublished material incorporated into the analysis. This is made possible on the basis of my preliminary work in 2016 and 2017 (funded by my IDG) at the Bayerisches Hauptstaatsarchiv in Munich and at archives in Augsburg and Nuremberg, which revealed an untapped source base roughly ten times larger in scope than initially expected, including materials pertaining to some of the landlords mentioned above for whom some sources have been published. I have been able briefly to inspect all the relevant unpublished sources up to 1440 that are in the state archives of Munich, Augsburg, and Nuremberg, and have acquired an overview of the extant sources up to *c.*1500. A detailed overview of the source base, providing a typology of the published and unpublished sources for *c.*40 landlords (most of which I have inspected *in situ*), and laying out the methodology to be used in tapping them for questions relevant to economic history, is to be published in 2019 in *Zeitschrift für Agrargeschichte und Agrarsoziologie*. On the basis of this preliminary research, I estimate that in addition to the published material, the total extant source base for these landlords comprises *c.*15,000 manuscript pages for the period *c.*1200–*c.*1440, with information altogether on *c.*10,000 individual rural tenant holdings.

It would not be feasible to analyse all of this material within the scope of a five-year project. Nevertheless, for the project to fulfil its aims, it will be necessary to work with a sufficiently large and diverse source base. It will thus be based on the published and unpublished sources for 22 landlords:

Benediktbeuern, Dießen, Ebersberg, Formbach, Fürstenfeld, Geisenfeld, Heilsbronn, Hochstift Freising, Indersdorf, Kaisheim, Neustift, Obermünster in Regensburg, Osterhofen, Raitenhaslach, Scheyern, St Emmeram in Regensburg, St Johann in Regensburg, Schäftlarn, Tegernsee, Weißenstephan, and Weltenburg, as well as the Duchy of Bavaria. These landlords have been chosen both because the existence (in some cases) of printed texts will greatly speed up the process of analysis, and because of a relatively high amount of continuity of available, and relatively detailed, source material over the proposed time period. The terminal date for this project is partly determined by the explosion of the source base from the 1430s (similar to what has been observed for northern Switzerland: Rippmann 2015): my preliminary researches in the archive suggest that roughly as much survives for the period *c.*1440–1500 as for the previous two centuries. This terminal date will also, however, allow me to gain a sufficiently large slice of time after the demographic crises of *c.*1350 that have played a large role in the English scholarship, thus enabling a more thorough comparison of the two regions. The geographical scope is determined by the exceptional richness of the archival deposits, the relatively large number of published editions of these sources, and equally by the diversity of landscape and landholding and cultivation practices across the regions to be examined.

It is not my purpose to undertake a comprehensive study of the social and economic organisation on all the lands of all of these landlords. My interest is in understanding the relative proportions of holdings of different sizes, and the relative importance of different types of dues from which the extent of market involvement may be inferred (e.g. grain rents, money rents, labour services, services or rents explicitly commuted to money). Based on my recent preliminary studies, I am devising a template for a database that will incorporate all the data from these sources in searchable form (including, but not restricted to, rent types and amounts, incidences of servitude, arrears and cancelled debts, disputed renders, landscape features, and length of tenure). On the basis of this database, I will be able to provide descriptive statistics to illuminate my research questions. This will enable an analysis in which I will lay out the extent of social stratification, market involvement, labour services. This core data will be contextualised by evidence for incidences of wage labour, personal bondage, forms of tenurial relationship, and the existence of markets in the regions surrounding the holdings; this evidence will be drawn both from my principal source base as described above, as well as a much smaller corpus of published and unpublished charters and customals (some of which I have already identified in the archive), and the relevant regional scholarship. The resulting monograph will contain (i) a lengthy introduction to the context and the German and English historiography of the topic; (ii) a detailed presentation and empirical analysis of the data collected; (iii) a comparison with what can be gleaned from other studies of rural economic and social history in southern Germany, Switzerland, and Austria in this period; (iv) a comparison with the scholarship on England and a discussion of the wider theoretical perspectives; and (v), a broader theoretical section in which scholarship on other parts of Europe and other world regions will be drawn on to provide an initial synthesis on pre-modern processes of commercialisation in rural societies.

The second objective of this project is to prepare a publicly-accessible website that will make the database on which my monograph will be based available to the wider research community. In addition, this website will provide detailed surveys of the extant archival deposits in Bavaria and Baden-Württemberg through to the end of the sixteenth century, describing their nature, extent, and potential for future research; this will be supplemented by discussion of the published sources and scholarship on these and neighbouring regions. In preparing this website, therefore, I intend to lay the groundwork for the next project, extending my researches up to *c.*1600; what is equally important is that the online database and surveys will also provide a source base for other researchers and stimulate further scholarship. The website will also make available the historical GIS data that is to be used in my monograph to allow scholars to map forms of land exploitation onto geological and topographical features.

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