
RESEARCH TRENDS

European Economic and Social History in the 1990s

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Preliminaries

Having been insufficiently punished for propheteering twenty years ago, we are indulging again.¹ In 1970 Richard and Charles Tilly prepared a long paper for the annual meeting of the [American] Economic History Association, delivered a short version orally, and published yet another version in the *Journal of Economic History*.² Those papers predicted and prescribed the near future of economic history in Western Europe. Anchored in recent and current trends, they sought to be neither utopian nor pedestrian, but viably visionary.

Our paired papers of 1970-71 stressed the arguments that a) the so-called Industrial Revolution started much earlier, took longer, and had less to do with the factory than standard accounts then said and b) economic historians were ready to take on the entire transition from peasant to industrial econo-

¹ A preliminary version of this paper was given by the authors and circulated at the Annual Meeting of the Economic History Society in Liverpool, 29 March to 1 April 1990 (as "An Agenda for Economic and Social History for the 1990s"). That paper, with more extensive bibliography, is available, from the center for Studies of Social Change, New School for Social Research, 64 University Place, New York, 10003, at cost. The hospitality and wisdom of the Society and Conference organizers are herewith gratefully acknowledged.

² C. TILLY and R. TILLY, "Emerging Problems in the Modern Economic History of Western Europe", (1970); "Agenda for European Economic History in the 1970s", *Journal of Economic History*, 31, (1971), pp. 184-198.

mics, and to derive influences that were relevant to the contemporary Third World. On the first point, we sniffed the scholarly wind correctly; confidence in a compact, unitary Industrial Revolution has declined radically. (Whether we actually helped blow the wind is quite another question). On the second theme, we surely have to plead excessive optimism; despite repeated efforts, economic historians remain far from a general account of the transition, and even from consensus on the proper approach to that general account. In the form of an invitation to discuss social and economic history for the 1990s, the [British] Economic History Society has now given us a chance to review the record and put ourselves once again on the line. This time we have become a trio, and have self-consciously added social history to our agenda. We have also strayed occasionally into areas outside Europe when the work involved seemed relevant to European concerns.

In 1970, we did not survey social history, although many of our discussions drew on social history. We recommended and to some extent predicted study of four topics: 1) protoindustrialization, 2) the demography of economic expansion, contraction, and change, 3) industrialism as a life experience, and 4) the political context, both national and international, of economic change. The shorter version of our paper only mentioned the political context *pour mémoire*, but the longer unpublished version included discussions of three subtopics under that heading: a) state intervention — direct and indirect, intentional and unintentional — in economic change, b) competition, emulation and trade, with special reference to Britain's influence, and c) imperialism. We did anticipate the 1970s' and 1980s' heightened interest in three of our four topics. But on the fourth — the political context — we expected and advocated more research than actually occurred; surely our failure to elaborate on the topic in our published version does not account for the profession's relative inattention to political context since 1970!

Looking forward from 1970, we failed to anticipate how much effort would go into the analysis of labour processes and labour markets during the 1970s and 1980s — in retrospect, surprising omissions for us to make; we smuggled references to them into our presentations of industrialism as life experience, but failed to anticipate the attention that capitalist strategies with respect to labour processes and labour markets, or the consequent struggles between employers and workers, would command during the next two decades. All in all, then, our forecasting in 1970 got a number of trends more or less right, but fell far short of a perfect score.

Some of 1970's agenda still needs attention: economic historians still lack a convincing general account of the western transition from peasant to industrial social life, have if anything become more reserved about claims to apply European experience to the contemporary Third World, and have much to learn about the political context, domestic and international, of economic change. Our 1990 mandate also calls on us to delve farther into the social-historical complements of these problems.

Current changes in western economies and social life, furthermore, put new problems on the agenda: the emerging European community, the startling transformations of socialist states, deindustrialization of substantial regions within rich capitalist countries, widespread shifts from manufacturing toward service industry, the relative decline of American — and perhaps even of western — economic well-being and power, the apparently increasing importance and mobility of international capital, the state-led labour-repressive industrialization of several Asian powers all raise new questions for western economic and social history, just as feminists have put women's history, family history, and gender inequality on the agenda, and students of mentalities and discourse have forced us to attend (however reluctantly) to their subject matters.

As we peer into the futures of economic and social history, our most general message is quite simple: *it is time to de-economize economic history and to re-economize social history*. The de-economization of economic history should include the analysis of rights, power, coercion, state action, and related "institutional" factors; it does not entail the abandonment of economic analysis, but its broadening from a single-minded application of free-market models. The re-economization of social history should include new treatments of the interdependence among different forms of production and reproduction, both material, biological, and social. It should challenge the surprising recent tendency either to treat the three as separate spheres or to reduce all of them to artifacts of discourse. In this limited but crucial sense, we call for the revival of materialist social and economic history.

Scholars long paired economic and social history as base and superstructure, cause and effect, underlying processes and everyday experiences. The last twenty years have spread increasing scepticism about the straightforward determination of social life by economic organization, without installing a clear alternative to the broadly Marxist base-superstructure scheme. Various forms of cultural analysis, in terms of mentalities, discourse, and ethnographic description, have washed over social history while leaving economic history almost unstained. We remain stolidly committed to a materialist position, seeing social life as occurring within strong limits set by the existing organization of wealth, production, and distribution. But we also concede more autonomous influence to political power than most Marxists of 1970 or before allowed for.

Economic history, for us, analyzes changing patterns of scarcity and inequality at four levels: 1) describing those patterns, 2) documenting their variation, 3) searching out their causes, and 4) identifying their effects. Social history, as we see it, concentrates on the intersection of large social processes and small-scale social life: life at the scale of the individual, the household, the community, the kin group. The large-scale processes include economic changes *stricto sensu*, to be sure, but they also include state formation, demographic transformations, and alterations in systems of households, socializa-

tion, kinship, friendship, community organization, workplace relations, and local forms of inequality, hostility, competition, and solidarity.

Social history likewise at four levels: describing, documenting variation, searching cause, and identifying effects. The link between economic and social history, then, connects changes in small-scale social life, on the one hand, to alterations in scarcity and inequality. This way of setting up the discussion differs dramatically from Alexander Field's authoritative recent survey of economic history.³ Despite disclaimers, Field and his collaborators ground their analysis firmly in the discipline of economics. Their agenda concerns the historical study of economically-defined problems. In contrast, we are not asking what history can do for economics, but what economic and social history can do for history in general and for our understanding of social life.

The Styles of Recent Work

Field and company are not alone. In a slim volume titled *Economic History and the Modern Economist*, William Parker and his colleagues offered hortatory and cautionary visions of the same future that we are exploring here. The papers in the book were all originally presented at a session of the 1984 annual meeting of the American Economic Association.⁴ Parker opened the collection with a mild complaint that in recent years theory had been "elevated to a central place while being itself more and more narrowly and 'rigorously' defined... The institutional content, the social concepts, the moral zeal implicit in the training which economists used to be given through courses in economic history, economic institutions, and applied fields have been pushed aside while these fields have themselves been partially transformed or distorted into playgrounds for the imagination of the theorist". Nevertheless, he concluded, economic history had "survived these decades [of the dominance of theory and quantification] with much of its original quality intact".

Elder statesman economists Kenneth Arrow and Robert Solow echoed Parker's defence of economic history,⁵ Arrow declaring that economics provides theory and method for economic history, while economic history offers context and evidence to economics. Solow was more disenchanted with recent developments in economics. Economics has wrongly modelled itself, he argued, on physical science; he recommended a more modest approach. Analytical economists ought (in his view) to "organize our necessarily incomplete

³ ALEXANDER J. FIELD, ed. *The Future of Economic History*. (Boston: Kluwer-Nijhoff, 1987).

⁴ WILLIAM N. PARKER, *Economic History and the Modern Economist*. (Oxford: Basil Blackwell, 1986).

⁵ KENNETH ARROW, "History: the View from Economists"; ROBERT SOLOW, "Economics: Is Something Missing?", in PARKER, *Economic History*, pp. 13-29.

perceptions about the economy... see connections that the untutored eye would miss,... tell plausible — sometimes even convincing — causal stories with the help of a few central principles, and... make rough quantitative judgments about the consequences of economic policy and other exogenous events".⁶

The product of this type of low-key analytical economics (in which there would be no loss of logical rigour) will be models (plural) "contingent on society's circumstances — on the historical context, you might say — and not a single monolithic model for all seasons". For Solow, then, history makes possible a broader range of observations for the theorist. Economic historians can test the less ambitious models Solow recommends by observing similar processes or relationships in other times and places, calling on historians' broad knowledge of what was.

Comments on the papers by economic historians Donald McCloskey and Gavin Wright offered both enthusiasm and caution. McCloskey exuberantly claimed that there was no difference between economics and economic history; all good economists, he insisted, are historians, that is, "tellers of plausible stories about the past". "Economics is neither social physics nor social engineering: it is more like a peculiar variant of social history".⁷ Wright was less optimistic that preaching history to economists would convert them. He argued that economists have long given lip service to the notion that their theories are historically conditioned, but they do not take full account of historical evidence; instead they assume history, i.e., an uncomplicated, unchanging past. Wright prescribed the teaching of history so that its practice can become an integral part of economists' activity. No doubt economic historians came away from the session with a warm feeling of being appreciated. The warming appreciation came, however, from theoretical economists, not from historians.

For a clearer picture of what economic history can do for others sorts of history, let us turn to a more tangled set of connections — the relationships among economics, economic history and social history. Here our limited review of three social history journals and 2.5 economic history ones (*Journal of Interdisciplinary History*, *Past and Present*, *Social History*, *Economic History Review*, *Journal of Economic History*, and *Journal of European Economic History*) comes into play. If these journals stand for all, economic historians routinely use economic theory and logic, even though they do not always express it numerically.

The *Economic History Review* is, of course, distinctly less quantitatively inclined than the *Journal of Economic History*; nevertheless the *Ec Hist Rev's*

⁶ SOLOW, "Economics", p. 23.

⁷ DONALD McCLOSKEY, "Economics as an Historical Science" (p. 64 and 69), GAVIN WRIGHT, "History and the Future of Economics", both in: PARKER, *Economic History*, pp. 63-69 and 77-82.

authors consciously formalize their problems to a much greater extent than do social historians. Some insights may also be derived from perusal of the annual bibliography of recent books and articles about the economic and social history of Great Britain and Ireland in the *Economic History Review*. The list is at least partially conceived to inform economic historians about articles appearing in journals they otherwise would not read on a regular basis. Hence there are many more references from local history or narrowly specialized journals (treating the history of railways and canals, for example) than from the large-circulation general journals of social history, such as those reviewed here. (These general journals may also be less represented of course because they do not have the exclusively British geographic focus of the local history journals).

The list reflects a division of labor between economic history and social history similar to that between economics and economic history. Articles in the local and general social history journals provide evidence that can be used by economic historians to contextualize and test their accounts just as those by economic historians provide comparative empirical evidence for the theories of economists. Social history also contributes to economics, as does economic history, information about what Arrow calls the "historical conditioning" (the social conditions of its production) of economic theory and method.⁸

More important contrasts and commonalities exist between the two histories. Contrasts first. Notice how much of the economic history in the journals reviewed consisted of single case studies. Big comparative studies of economic development *à la* Rostow or Gerschenkron have become less common, despite their importance in getting interesting questions on the economic historical agenda. The journals reviewed contained few spatial comparisons even on a more limited scale; temporal comparison was not much more common. According to this sampling, social historians aspire to be more comparative in spatial or temporal terms than do economic historians. Moreover, social historians are much more likely to make internal comparisons, borrowing the sociological method of studying variation within social phenomena and testing sociological theory about the conditions under which variation occurs. This model of analysis fits well the central method of social history, collective biography: the painstaking accumulation of uniformly-described individual events or lives into collective portraits, as in political prosopography, family reconstitution, and analyses of social mobility; the method takes its justification from the belief that the aggregates so constructed will provide a more telling portrayal of popular experience than the recapitulation of general impressions, observers' commentaries or convenient examples.

Economic and social history nevertheless do sometimes converge in met-

⁸ ARROW, "History", p. 18.

hod and topic of inquiry. This occurs most often in the fields of demography, social structures and family history, labor history, and the history of entrepreneurship, all areas in which economic history employs the method of collective biography. Even in these areas, however, the questions asked by economic and social historians differ, based as they are most often on different bodies of theory.

Instead of surveying all fields of social and economic history, let us single out some strategic fields of inquiry, in a rough order of increasing scale: gender inequality, collective action, class formation, mobility of labour and capital, institutional change, state formation, economic growth, economic structure, and international variation. We concentrate on Europe of the past few hundred years, stressing changes that occurred in the course of capital-concentrated industrialization.

Entitlements and Inequality

Inequality concerns economic historians as both cause and effect of other economic changes. Only recent has gender inequality attracted much serious attention. With his ideas about entitlement, Amartya Sen has advanced the study of gender and made it easier to link with other forms of inequality. Sen's definition of entitlement is not the conventional one of legal rights to state welfare or social security programs. Rather he defines entitlement more comprehensively as "a semi-legal concept, focusing on the bundles of goods and services that a person (or a family) can legitimately use to establish command over food using the laws, regulations, conventions, opportunities and rights ruling in the society in question. In market economies, entitlements reflect ownership, on the one hand, and opportunities of production and exchange, on the other".⁹

Sociopolitical rights, whether they be religious or moral obligations of some groups to see that others are fed, or institutionalized government-sponsored welfare or social security rights, are also entitlements in this sense. In the short run, it may become impossible for some groups to acquire food due to the collapse of their endowment entitlements (their earned or otherwise acquired set of entitlements to food) or to an unfavorable shift in the distribution of exchange entitlements; this inability may develop into famine, a topic which has particularly concerned Sen, a development economist. Over the long run, entitlements change along with structural shifts, combine and recombine in different patterns. The distribution of entitlements among groups — political, economic, social, gender, racial — may be more or less secure, more or less equal.

⁹ AMARTYA SEN, "Food Battles: Conflicts in the Access to Food", *Food and Nutrition*, 10 (1984) pp. 81-89; see also: SEN, *Poverty and Famines. An Essay on Entitlement and Deprivation*, (Oxford: Oxford University Press, 1981).

The social history of entitlements and the way they have varied among groups is an interesting historical problem in itself. The simple statement in Sen's definition noted above about the way entitlements are acquired in market economies (through ownership of property and wages) suggests very different access to entitlements by women and men, for women have been legally precluded in most times and places from holding property, and their access to wages has usually been less secure (their wage-earning more intermittent, their wages consistently lower) than men's. Spinning out these implications over long periods, especially weighing changes in moral and other sociopolitical, along with the more easily identified and evaluated economic, entitlements would provide a valuable perspective on recent history.

Let us mention three more general historical problems that can be illuminated by conceptualizing entitlements as mechanisms linking large-scale structural change and outcomes for individuals and groups: collective action, the household division of labour, and class formation. (The examples all come from the modern history of Western Europe and the United States, but the great advantage of this approach is that it facilitates comparison across long periods of time and vast areas, for it does not specify economic system; nor is it evolutionary, nor does it posit transitions or directions of change).

Collective action. Especially important here are forms of collective action and its participants. Long-term changes in entitlements link forms of collective action to processes of structural change. The slow erosion of a peasant economy based on ownership or control of land to a proto-industrial wage (or partial wage) economy was characterized by conflicts over food supply, exacerbated by the elimination of state control over grain marketing (a critical entitlement). Popular collective action about food took the form of efforts to fix prices or prevent shipments of grain out of a locality. Once wage earning was the means of acquiring entitlement to food, conflict shifted to labour rather than consumer markets.

Consequences for gender-linked political participation might be sketched this way. Because women were members of households and communities that claimed entitlements under the older spatially-organized system (of property-owners and communities with common rights), they were likely participants in food protest. Once individual wage earning became the norm, women's entitlements were more often indirect, through men who were wage earners rather than through their own wages. Occupations and jobs based on individual wage earning were more difficult for married women — especially those with children — to manage; employers seeking workers in industrializing sectors tended to assign them by gender, taking account of competing demand in labour markets and the extent of their needs for highly trained, lifetime workers. Highly gender-segregated, based on local conditions, jobs assigned to women were consistently lower paying, easier to enter and leave, and possessed few possibilities for advancement. As a consequence, women became much less centrally involved in popular forms of collective action, which

now revolved around labour markets. Over the nineteenth century, women gradually gained rights to spend their own wages and to own property, rights gained in the process of fighting for access to broader political participation, and in both these ways acquired direct entitlements. Because of the nature of the welfare-state entitlements, women as a group nevertheless continue to hold fewer direct entitlements than men in most western countries.

The household division of labour. As Sen himself has shown, entitlements affect men and women, boys and girls in the domestic as well as in the public arena, shaping the household division of labour in ways that have repercussions in labour markets and in politics. Wives' primary, or sole, responsibility for child care and housework actually increased with the removal of production from the household and the development of wage labour. This contributed to the emergence of the housewife, the most segregated occupation of all. Housewives' segregation, and the asymmetry and inequality that went along with it were exacerbated, he suggests, by their own weak bargaining position in the household, as access to resources was linked more and more to wage earning outside the households. Sen's sketch of household economics is, he argues, more realistic than the neoclassical translation of economic theory about rational behaviour from firms to families, a transfer which requires, he insists, "strong and farfetched" assumptions.¹⁰ As an alternative, he proposes a bargaining perspective which accepts:

both cooperative and conflicting elements in family relations... and is sensitive to various parameters, including the respective powers of the different members of the family... [Superior] bargaining power of men, related to better outside job opportunities... would lead to a correspondingly more favourable cooperative outcome for the men. Further, such differential advantages may feed on themselves. A better deal for the male in one period may, *inter alia*, include a better role in the division of labour with better training and more profitable job experience, and these may lead to a better placing in the next period's bargaining problem. Certain "traditional" arrangements may emerge, e.g., women doing housework and being able to take up outside work only if it is additional. These inequalities may solidify over time".¹¹

Here is a temporal process affecting and rigidifying the household division of labour on the macro and micro level. This household division of labour affects wives' access to opportunities in the labour market, and also women's access to politics, in which the weight of individual economic resources was (and is) so critical.

Class formation. By most definitions of class formation, women's relative lack of entitlement based on property ownership or wage earning would ex-

¹⁰ A. SEN, "Economics and the Family", *Asian Development Review* 1 (1983) pp. 14-27; see also Sen, "Food Battles" (previous note); and Sen, "Women, Technology and Sexual Divisions", *Trade and Development*, 6 (1985), pp. 195-223.

¹¹ A. SEN, "Economics and the Family", p. 18 (note 10).

clude them from the process. Nevertheless, several recent histories of middle-class women put women squarely in it, even though excluded from some of the more public aspects of middle class formation. Here women's participation in the social construction of roles and the norms of class is salient. Mary Ryan, for example, describes the women of Oneida County, New York, as actors in the ideological definition of class. Middle class women's reformism (defining appropriate roles for mothers and implementing governmental or private philanthropy), along with the Evangelical religion of preachers and the capitalist ideology of businessmen contributed to shaping the middle class of this region in the first half of the nineteenth century.¹²

Similarly, Leonore Davidoff and Catherine Hall, sensitive to gender differences, study middle class life through the lens of family. They show how "middle-class men who sought to be 'someone', to count as individuals because of their wealth, their power to command, or their capacity to influence people, were, in fact, embedded in networks of familial and female support which underpinned their rise to public prominence".¹³ Nevertheless, because of civil disabilities, women could seldom accumulate capital. They show, as does Ryan, how women's activity in the Evangelical reform brought them into the public sphere at the same time that they were constrained by the structures and customs sanctioned by domestic ideologies delineated by Evangelical social commentators. Women counted for still less in the world of formal politics. Women contributed to middle class formation, but its effect in the first half of the nineteenth century was to strip them of participation and citizenship.

Although it is more difficult to do because of the lack of written materials by working class women, studies of working class formation in a similar framework could usefully compare outcomes for women in that group with the middle class. Nevertheless, some themes have been raised in histories of working-class formation that echo those described for the middle class. Deborah Valenze, for example, writes evocatively about the conditions under which women helped to shape working-class religious practice and ideology in Methodist sects between about 1800 and 1850. As middle-class evangelicals within the Church of England and Wesleyan Methodists became more secure, their institutions came to resemble each other in form and practice; small groups of popular evangelicals seeking to preserve their communitarian and familial religious links broke away. "The locus of truly popular religious activity was the home", writes Valenze, "where generational, seasonal, and personal changes governed practice and belief". Women preachers played a public role in these sectarian churches, teaching of the 'preservative and resistant

¹² M.P. RYAN, *Cradle of the Middle Class: The Family in Oneida County, New York 1790-1865*. (New York and Cambridge: Cambridge University Press, 1981).

¹³ L. DAVIDOFF and C. HALL, *Family Fortunes: Men and Women of the English Middle Class, 1750-1850*. (Chicago: University of Chicago Press, 1987).

role of cottage religion', celebrating the integration of home and work of times gone by.¹⁴ Ellen Ross shows how women's networks provided the margin of survival for London working class families in the early twentieth century, and how women sought to provide a framework for working-class domestic respectability.¹⁵ The construction and reproduction of working-class domestic ideals are a topic for empirical exploration in other national settings as well.

The development of new forms of gender inequality based on differential access to training and education, occupations and jobs, and sex-linked wages has been the subject of sometimes acrimonious debate among feminist social scientists.¹⁶ This debate has been conducted largely in the realm of theory with only *ad hoc* empirical evidence, however. Whether it was a household decision based on class interest, the outcome of negotiation, or the consequence of patriarchal control jointly enforced by males in households and capitalist enterprise, women's labor force participation and — even more — their participation in the public side of working-class formation (in building organization and striking) were limited. These limitations (and women's consequent lack of resources) contributed to the gender inequalities in nutrition found in nineteenth and early twentieth century household food and time-budget studies.¹⁷

¹⁴ D.M. VALENZE, *Prophetic Sons and Daughters: Female Preaching and Popular Religion in Industrial England*. (Princeton: Princeton University Press, 1985).

¹⁵ E. ROSS, "Survival Networks: Women's Neighborhood Sharing in London Before World War I", *History Workshop Journal* 15, (1983), pp. 4-27. "Not the Sort That Would Sit on the Doorstep": Respectability in Pre-World I London neighborhoods", *International Labour and Working Class History* 27 (1985), pp. 39-59.

¹⁶ J. HUMPHRIES, "Class Struggle and the Persistence of the Working-Class Family", *Cambridge Journal of Economics* 1 (1977a), pp. 241-258. "The Working Class Family, Women's Liberation, and Class Struggle: The case of nineteenth-century British history", *Review of Radical Political Economics* 9 (1977b), pp. 25-41. "An Open Letter to the RRPE Special Issue on Women's Collective", *Review of Radical Political Economics* 12 (1980), p. 94. "Enclosures, Common Rights, and Women: The Proletarianization of Families in the Late Eighteenth and Early Nineteenth Centuries", *Journal of Economic History* 50 (1990), pp. 17-42; and H.I. HARTMANN and R. MARKUSEN, "Contemporary Marxist Theory and Practice: A 'Feminist Critique'", *Review of Radical Political Economics* 12 (1980), pp. 87-94.

¹⁷ Mortality differences by gender in South Asia, China and other countries in more recent years have been attributed to nutritional neglect of girls and women, but a recent review of the literature finds it unpersuasive. See RAY LANGSTEN, "Determinants of High Female Mortality in South Asia: Are the Data Consistent with the Theory?" 1987 manuscript quoted in Lillian Li, "Life and Death in a Chinese Famine", *Comparative Studies in Society and History* 33 (July), p. 501. For Europe see L. OREN, "The Welfare of Women in Labouring Families: England 1860-1950", *Feminist Studies*, p. 1 (1973), pp. 107-125; A. PUGLIESE, *Il bilancio alimentare di 51 famiglie operaie milanesi*, (Milan: Società Umanitaria, 1914).

Adding Sen's notion of cumulative disadvantage to the scarce household resources and the limited civil and political rights of women — the latter also tended to exclude middle-class women from the public sphere — strongly suggests the constraints working-class women faced. Still, it would be valuable to look systematically at variation among women in various occupations with less conventional work and family relations to explore the conditions under which women were more active in workplace or popular politics. Did access to training and better jobs improve women's position in the household historically, as Sen would predict? The problem remains of finding evidence that can confirm or disprove these theoretical formulations.

The study of entitlements will not solve all the problems of inequality. We still need to get right the relation between inequality and changing labour processes; sustained historical work has given a drubbing to the idea of continuous deskilling à la Braverman from the invention of the factory onward, but we do not have an adequate alternative.¹⁸ Although we now know that the demographic-economic relations that Franklin Mendels discovered in Flanders were far from universal in regions of protoindustry, we still lack good general statements of the conditions under which a) industrial production expanded rapidly without substantial increases in the scale of producing units, b) that process led to population increase and/or impoverishment, c) protoindustrial regions became the sites of capital-concentrated industry. Despite the heroic demographic synthesis of Wrigley and Schofield, Wrigley's own further interpretations of his results, and econometric contributions by such authors as Peter Lindert and Jeffrey Williamson, furthermore, we still await a coherent account of European proletarianization — the separation of the vast mass of workers, rural and urban, from ownership and control of their productive means.¹⁹ These problems lead inevitably to the renewed study of mobility.

Mobility of Labour and Capital

As economic and social historians, responding to the multiple mobilities of our own time, turn away from fixation in the world of fixed capital and

¹⁸ M. GRANOVETTER and C. TILLY, "Inequality and Labor Processes", in N.J. SMELSER, ed., *Handbook of Sociology*. (Newbury Park, California: Sage, 1989).

¹⁹ E.A. WRIGLEY and R.S. SCHOFIELD, *The Population History of England, 1541-1871* (Cambridge (Engl.), 1981). P.H. LINDERT and J.G. WILLIAMSON, "Reinterpreting Britain's Social Tables, 1688-1913", *Explorations in Economic History* 20 (1983), pp. 94-109. P.H. LINDERT, "English Occupations, 1670-1811", *Journal of Economic History* 40, (1980), pp. 685-712. "English Living Standards, Population Growth, and Wrigley-Schofield", *Explorations in Economic History* 20, (1983), pp. 131-155. D. LEVINE, *Family Formation in an Age of Nascent Capitalism*. (New York: Academic Press, 1977). *Proletarianization and Family Life*. (New York: Academic Press, 1984); *Reproducing Families*. (Cambridge: University Press, 1987).

settled labour, they begin to notice flux everywhere: the teeming small-scale mobility of European agricultural workers before the nineteenth century, the constant shop-to-shop movement of eighteenth-century artisans, the vast labour migrations of the nineteenth century, movements of capital so grand they left large deposits of American silver in China. The nineteenth-century concentration of both labour and capital in large industrial and commercial centres looks increasingly like an exception in the grand sweep of European history.

It was, to be sure, a crucial exception, a time when the sheer distance and volume of migration circles and chains increased enormously, when British capital leaped continents as never before, when the deindustrialization of the countryside left many rural areas far more exclusively agricultural than they had been for centuries, when the implosion of population into industrial cities increased the separation of home from work and thereby caused surges of movement within cities corresponding to the beginning and end of the working day; small wonder that so many contemporary observers and subsequent historians mistakenly imagined an industrial revolution as the breaking up of a previously immobile European population.

Partly because of that nineteenth-century misconception, the longer-term fluctuations of labour and capital mobility have not yet found their historian. What were the grand rhythms of movement? To what extent did Europe alternate between periods in which labour migrated to the locations of concentrated capital and other periods in which capital migrated to concentrations of cheap labour and expanding markets? Alain Dewerpe's study of eighteenth- and nineteenth-century Northern Italy shows how easily capital moved to labour, just so long as the capital remained chiefly circulating rather than fixed, just so long as the labour remained both cheap and flexible. For decades, Milan thrived as the nerve centre for a silk industry the great bulk of whose production took place in rural households or small, dispersed shops.²⁰ After 1880, furthermore, the availability of commercial networks and seasoned industrial labour in the hinterland facilitated the implosion that made Milan and its satellite cities Italy's greatest centre of fixed capital.²¹ Given the salience of similar issues in the current experiences of Europe, the Americas, and Asia, we predict with confidence that economic and social historians will renew their interest in these mobilities during the 1990s.

Institutional Change

Understanding the sources of long-run economic growth depends on

²⁰ A. DEWERPE, *L'Industrie aux champs. Essai sur la proto-industrialisation en Italie du Nord (1800-1880)*, (Rome: Ecole Française de Rome, 1985).

²¹ L.A. TILLY, *Politics and Class in Milan, 1880-1901*. (New York: Oxford University Press, forthcoming 1991).

understanding economic institutions. Recent work in this area — known as New Institutional Economics (or NIE) — by such scholars as Mancur Olson, Douglass North and Oliver Williamson offer some promising bases for the historical analyses of institutional change and its connection with economic growth. There are pitfalls and dangers here, to be sure; readily quantifiable propositions will doubtless remain rare. Transactions costs, e.g., are one of the key (shorthand) concepts developed by NIE to explain “market failures” and there has been here a temptation to quantify them. However, we question the utility of locating certain activities in the National Income Accounts and calling them the economy’s transactions costs.²² The truly significant transaction costs may be *ex ante* hindrances to actions which are never observed *ex post*. Which gives rise to the opposite kind of danger — that of inferring the presence of (unobservable) transaction costs from market failures. Nevertheless, despite such problems, used with some care, the New Institutional approach has great promise.

Institutional change involves the role of the state. Take, for example, the British case. It is not always appreciated that British government activities, particularly military and naval spending, powerfully shaped the development of the British economy from the XVIIIth to the XXth centuries. In several respects this had positive economic effects; and it had to do with what has been called the “financial revolution”. A key NIE concept here is “credible commitment” — an obligation which one believes will be fulfilled. It was the clear subordination of the British crown’s spending decisions to the will of the country’s wealthholders, formally expressed, e.g. in the Bill of Rights, and concretely, by investing parliament with decisive powers over revenue raising, secondarily, in the abolition of the royal prerogative courts, which convinced taxpayers, and private wealthholders generally, that the crown deserved their financial support. Henceforth, from the early XVIIIth century, government borrowing built on a more elastic, politically less fragile base. Through the XVIIIth century, the tax burden grew; and along with it the British military and naval machine, which certainly was an important basis of the country’s increasing weight in international affairs — with possibly significant positive economic externalities. Certainly at home the British government’s borrowing terms improved, so much so that there were spinoffs — positive externalities — leading to the parallel development of a market for private debt through the XVIIIth century.²³

²² D.C. NORTH, *Structure and Change in Economic History*. (New York: W.W. NORTON, 1981). *Ibid.*, “Transaction Costs in History”, *Journal of European Economic History* 14 (1985), pp. 557-576. J.J. WALLIS and D.C. NORTH, “Measuring the Transaction Sector in the American Economy, 1890-1970”, in S. ENGERMANN & R. GALLMANN, eds., *Long-Term Factors in American Economic Growth*. (Chicago: University of Chicago Press, 1986).

²³ D.C. NORTH and B. WEINGAST, “Constitutions and Commitment: Evolution

The rest of the British story is well known — as is the contrast to contemporary developments elsewhere, e.g. in France. The thesis advanced here, as North and Weingast write, is that the credible commitment by the government to honour its financial agreements was part of a larger commitment to secure private rights — with positive effects for capital accumulation and the development of a market economy in Britain. A somewhat different story, with different chronology but some strong similarities, can be told for the German states and especially Prussia. In all cases it was a development with positive impulses for growth, but not without costs. Comparative financial history which discusses such benefits and costs is thus an important part of the agenda we propose.²⁴

Financial revolutions, however, do not exhaust the connections between institutional change and the state. Governments have generally been involved in defining and enforcing property rights, e.g. those regulating the use of land. Here one of the interesting questions has been and will continue to be the identification of the conditions under which private negotiations “failed” and state intervention was induced. The lively debate of the 1970s on English common fields and enclosures turned on this issue. The debate examined, among other things, the role of relative product and factor prices, the “initial” relative resource endowment of the actors — great landowners, smallholders, cottagers and others — and their interaction with the structure of government.²⁵ One wonders whether that debate’s generally positive assessment of the English solution to the “agrarian question” — a solution which is said to have created the institutional bases for the remarkable rise in agricultural labour productivity already noted — would be upheld by closer comparison with the agrarian reforms of other European countries. Some corroborative evidence for France, where enclosures came only haltingly,

of Institutions Governing Public Choice in Seventeenth-Century England”, *Journal of Economic History* 49 (1989), pp. 803-832. J. BREWER, *The Sinews of Power. War, Money and the English State, 1688-1783*. (New York: Knopf, 1989). P.K. O'BRIEN, “The Political Economy of British Taxation, 1660-1815”, *Economic History Review* 41 (1988), pp. 1-32. L. NEAL, *The Rise of Financial Capitalism. International Capital Markets in the Age of Reason* (Cambridge, 1990).

²⁴ C. KINDLEBERGER, *Financial History of Western Europe*. (London: Allen & Unwin, 1984); R. BRAUN, “Taxation, Sociopolitical Structure, and State-Building: Great Britain and Brandenburg-Prussia”, in C. TILLY, ed., *The Formation of National States in Western Europe*. (Princeton: Princeton University Press, 1975).

²⁵ D.N. MCCLOSKEY, “The Economics of Enclosure: A Market Analysis”, in W.N. PARKER & E. JONES, eds., *European Peasants and their Markets*. (Princeton: Princeton University Press, 1975); S. FENOALTEA, “Risk, Transaction Costs, and the Organization of Medieval Agriculture”, *Explorations in Economic History* 13 (1976), pp. 129-151; R. ALLEN, “The Efficiency and Distributional Consequences of Eighteenth-Century Enclosures”, *Economic Journal* 92 (1982), pp. 937-53.

suggests the potential fruitfulness of such an extension; but much work remains to be done.²⁶

The enclosures debate raises the question of institutional economics and political economy associated with the work of Mancur Olson. Owing to the free rider problem, there is no reason to expect that individual actors operating in relatively competitive markets will be likely to produce efficient solutions, i.e. those generating net social gains and equating marginal social costs with marginal social benefits, to broad, large-scale problems of economic organization. On the other hand, tightly-knit interest group organizations are well able to press for institutional change in their own favour, even if it means increasing their net gain at the expense of an aggregate net loss in welfare. Under the relatively pluralistic political conditions of the Western European countries in the XIXth century and since 1945, that produces a bias for government actions favouring "distributional coalitions" at the expense of overall efficiency. To be sure, rapid technological progress, wars, or revolutions, could alter the political situation and destroy the power of distributional coalitions. Nevertheless, over a fairly wide stretch of modern economic history, the hypothesis of distributional coalitions — villages, labour unions, cartels, trade associations, and the like — restricting competition, distorting relative prices, and restraining innovation and other sources of productivity growth, is worth entertaining.²⁷ It corresponds roughly to that negative part of Eric Jones' great dichotomy — rent-seeking activity which has the effect of offsetting that other universal demiurge, the striving for material betterment.²⁸

Institutional change, we suggested earlier, is not exclusively a matter of government action. And private and small-group agreements need not be inefficient by comparison with competitive "free-market" solutions. Such diverse institutions as cartels, employee codetermination in large-scale enterprise, or even universal banking control over non-bank enterprises, can be seen as responses to high information costs or asymmetric information. Recall the concept of "credible commitment". Under conditions of asymmetric or incomplete information, such as those which characterize most credit markets, a borrower may offer a lender a share in control over his enterprise. A

²⁶ G. GRANTHAM, "The Persistence of Open Field Farming in Nineteenth-Century France", *Journal of Economic History* 40 (1980), pp. 515-531; *Ibid.*, "Agricultural Supply during the Industrial Revolution, French Evidence and European Implications", *Journal of Economic History* 49 (1989), pp. 43-72. P. HOFFMANN, "Institutions and Agriculture in Old-Regime France", *Politics and Society* 16 (1988), pp. 241-264.

²⁷ M. OLSON, *The Rise and Decline of Nations*. (New Haven: Yale University Press, 1982).

²⁸ E.L. JONES, *Growth Recurring. Economic Change in World History*. (New York: Oxford University Press, 1989).

credible commitment by the borrower to honour his obligation can overcome the asymmetric information problem. Similarly, codetermination in the management of large-scale enterprise by employee representatives can be seen as a credible commitment by owners and managers to conserve or protect (at least not to plunder) the firm-specific human capital (or quasi-rents) of their workers, the presumed aim being to evoke higher levels of labour effort and limit the danger of "shirking".²⁹

In both examples risk problems are raised by the existence of asset specificity, another ubiquitous economic phenomenon not well covered by conventional neoclassical economics. Finally, along the same lines, we observe in most industrial economies in the late-XIXth century the emergence of very large enterprises in industry and elsewhere, enterprises which were vertically integrated and diversified across several product lines, and which were thus responding to asset specificity and the inability to obtain credible commitments from suppliers or customers, in addition to the lure of economies of scale and scope. The economic historian who has contributed most to our knowledge of these matters is surely Alfred Chandler, and further work along the comparative lines he has pioneered belongs to that part of our agenda which we term, for lack of a better term, business enterprise.³⁰

Historical treatment of the role of institutions in economic growth, we have argued, ought not to focus exclusively on the state and should include in its treatment the development of private agreements and arrangements. Nevertheless, even here, in the area of private arrangements, the role of the state is important. Indeed, in an important sense, the impact of the state on long-run economic growth may have been greater on this level than on any other. For this reason, consideration of the structure of the state and of the determinants of its rule-making activities is in order. Our description of this part of the agenda therefore concludes with a few remarks on some potentially fruitful lines of inquiry. The main point is this: we see the state as the social arrangement through which the general rules and especially the legal framework of economic activity are negotiated, established and enforced. This we need to explain. But explaining the role of the state means developing an appropriate model of political change.

This is not the place to attempt to sketch out such a model, but in order to be useful as part of an explanation of the modern economic growth of nations it should probably cover at least the following needs: 1) the need to explain how regional, sectoral and socio-economic class interests are mobilized and organized and what determines their effectiveness at the national level. Is

²⁹ O. WILLIAMSON, *The Economic Institutions of Capitalism*. (New York: Free Press, 1985).

³⁰ A. CHANDLER, *The Visible Hand*. Cambridge: Harvard University Press (1977). *Scale and Scope. The Dynamics of Industrial Capitalism*. (Cambridge: Harvard University Press, 1990).

it the economic weight of their basic constituencies, their social homogeneity, the particular kinds of coalitions they enter into, ideology, or some combination of all of these? 2) the need to see the state as more than the collective expression of organized interest groups but as an autonomous actor (or collection of autonomous actors) pursuing independent aims.

Autonomy may have a purely traditional base, corresponding to no more than the inherited prerogatives of a small but highly cohesive minority. On the other hand, autonomy can be maintained by the adept exploitation of differences among contending socio-economic interest groups. What explains autonomy and with what kinds of state behaviour is it associated? That leads directly to 3) the obvious need to monitor the "output" of state action, to assess its efficiency in promoting or hindering modern economic growth in nations. This has been a traditional field of work for economic historians, so little more need be said. Nevertheless, if more attention is paid to the relationship between the political bases of government — the kinds of coalitions and bargaining its stability depends on — and the contribution of state activity to modern economic growth (net of resources absorbed by government), that work will doubtless yield higher returns.

State Formation

John Brewer's recent study of the British state between 1688 and 1780 signals a remarkable revival of a concern that dates back to Schmoeller, if not to Jean Bodin — a concern with what we might call the political economy of state formation.³¹ Contrary to widespread misconceptions, Brewer shows that under the pressure of eighteenth-century war finances, the British state expanded, centralized, and tightened its grip on the national economy; by the time of the Napoleonic Wars, indeed, the British state was probably extracting a larger share of national income for its wars (past and present) than was its French neighbour, so long reputed to be the strong state *par excellence*.³² Therein lies a dual problem: how great an impact did war have on European national economies, and to what extent did the structure of any particular national economy constrain the organization of the state for war?

³¹ C. BRIGHT and S. HARDING, ed., *Statemaking and Social Movements*. (Ann Arbor: University of Michigan Press, 1984). In addition to JOHN BREWER's, *Sinews of Power* (note 23) see also his *Party Ideology and Popular Politics at the Accession of George III*. (Cambridge: Cambridge University Press, 1976).

³² M. MORINEAU, "Budgets de l'Etat et gestion des finances royales en France au dixhuitieme siècle, *Revue Historique* 264 (1980) pp. 289-336. P.K. O'BRIEN, "The Political Economy of British Taxation 1660-1815", *Economic History Review* 41 (1988) pp. 1-32, "The Impact of the Revolutionary and Napoleonic Wars, 1793-1815, on the Long-run Growth of the British Economy", *Review* 12 (1989) pp. 335-395.

Many links run from war and preparation for war to national economies: not only the obvious effects of invasion, destruction, and blockade, but also the channelling of resources (men, food, supplies, money) to the armed services, the creation and transformation of financial administrations, the effects of military service on young men. Using a general-equilibrium model, Jeffrey Williamson has concluded that the British state's mighty military efforts after 1750 crowded out investment in civilian production, but the doubts Charles Feinstein has recently raised about Williamson's wage data call forth caution in accepting the Williamson estimates.³³ However that discussion comes out, it should draw salutary attention to the economic and social impact of what was for centuries, after all, by far the state's bulkiest and most expensive activity: the mounting of military efforts.

More generally, economic and social historians ought to look much harder at the ways a wide variety of state activities shaped the lives of citizens. Susan Watkins has shown how state boundaries became sharper demographic frontiers after 1870: greater homogeneity within states with respect to fertility, nuptiality, and other vital characteristics, greater heterogeneity between adjacent states. The creation of national educational systems, national markets, national systems of migration and communication, national military service, and national languages no doubt all played their parts; European states promoted all of them. Similarly, J.M. Winter has shown how the British mobilization for World War I actually improved the civilian population's health and life expectancy by organizing the food supply; Winter's findings provide an indirect confirmation of Amartya Sen's line of argument. In earlier centuries the mobilization of food to serve expanding armies surely had the opposite effect, pushing the civilian population toward high-priced famine. Because of an unfortunate insulation of military and political from social and economic history, we know far less about these connections than we should. Historical demography, maturing with work like Wrigley-Schofield's great survey of English population history, can become a major tool of political and economic analysis.³⁴

Links in the other direction — from the economy to the character of war and the state — remain even less explored, and at least as interesting. Every political historian understands, to some degree, that maritime-commercial states such as Genoa and the Dutch Republic conducted very different kinds

³³ J.G. WILLIAMSON, *Did British Capitalism Breed Inequality?* (Boston: Allen & Unwin, 1985). C. FEINSTEIN, "The Rise and Fall of the Williamson Curve", *Journal of Economic History* 48 (1988), pp. 699-729. R.A. BLACK and R. MANN, eds., "Crowding Out during Britain's Industrial Revolution", *Journal of Economic History* 50 (1990), pp. 109-132.

³⁴ S. WATKINS, *From Provinces into Nations* (Princeton: Princeton University Press, 1990); J.M. WINTER, *The Great War and the British People*, (Cambridge: Harvard University Press, 1986).

of warfare and created very different governmental organizations from great agrarian powers such as Russia and Prussia. The texture of regional economies greatly limited the viability and organizational consequences of alternative fiscal strategies; on the whole, the attempt to extract money from relatively uncommercialized agriculture depended on driving producers into markets, and tended to produce both bulky tax-collecting apparatuses, extensive involvement of tax farmers and other middlemen, and political understandings in which landlords received considerable autonomy, power, and/or fiscal exemption in return for their collaboration in squeezing the peasantry. Where cities, bourgeois, and commercialized production thrived, on the other hand, merchants and financiers tended to accumulate great political strength, and to block royal bids for absolute power.³⁵ These are only the crude lines of what should become a fine portrait of relations among state activity, routine social life, and ambient economies in Europe.

Economic Growth

Recent economic historiography and current events — in particular the collapse of the socialist economies of Eastern Europe — give us reason to hope for continued, nay, enhanced interest among economic historians in the question of long-term economic growth. On those same grounds we anticipate ongoing attention to the related question of institutional change, especially to what one might call “property rights transitions”. This involves, though not exclusively, the role of the state.

These are mere topic headings, of course. Before going on to specify some of the essential details, it would be exhilarating to offer a grand synthesis, a general, unifying framework. Alas, we have none. Perhaps the closest thing to such a synthesis is the work of Eric Jones, first in his *The European Miracle*, and more recently, in *Growth Recurring*. Those works do a superb job of conveying the pressing relevance of the growth paradigm, not only for the modern period, but over vast stretches of history. They also clearly show its links to institutional change, and the value of a broad geographical and temporal approach to its treatment. In this contribution we make no attempt to follow Jones closely. His canvas is world history; ours is modern Europe; and there are other differences. Nevertheless, some common ground exists. The distinction between a demiurge to material improvement and rent-seeking behaviour is fruitful, if problematic. We also make use of the distinction between “extensive growth”, an increase in total real output of an economy, and “intensive growth”, which refers to increasing real output per head of population. In general, however, our contribution discusses “inten-

³⁵ C. TILLY, *Coercion, Capital, and European States, A.D. 990-1990*, (Oxford: Blackwell, 1990b).

sive growth", an obvious reflection of the fact that our concern is mainly with the XVIIIth and XIXth centuries.

Our hopes for improved understanding of long-run economic growth focus on three of the latter's interrelated aspects: its timing, its structure and its international variation.

Timing. Twenty years ago, we noted — and applauded — a then-recent tendency to relocate the beginnings of modern economic growth (MEG) further back in time, say, from the late-XVIIIth century to the XVIIth. We modify that observation here in two important ways:

1. We now have a considerable accumulation of quantitative evidence supporting it. One of the proudest products of recent quantitative research is a flattening-out of the time shape of economic growth. It is appropriate no doubt to begin with the British case, for which the work of Crafts, Feinstein, Harley, Lindert and Williamson, and Wrigley has had significant revisionist effects: more growth before ca. 1750, slower growth and more gradual acceleration thereafter. A case can now be made for placing Britain's MEG beginnings in the XVIIth century. Moreover, revision of British growth has had ramifications for growth estimates of other countries. Thus the Dutch Netherlands now appear to have grown more before 1700 and declined less in the XVIIIth century than had been thought on the basis of Gregory King's estimates. In general, however, the same flattening-out of growth profiles as Britain's is observed in the growth history of other countries. French growth, e.g., unfolded wave-like, it seems, but there is no unanimity on a starting point, and certainly no reason now to deny that significant manifestations of growth marked the XVIIIth century.³⁶ Acceleration of German economic growth, once located in the 1850s, then in the 1840s, now belongs to the

³⁶ E.A. WRIGLEY, "Urban Growth and Agricultural Change: England and the Continent in the Early Modern period", *Journal of Interdisciplinary History* 15 (1985), pp. 683-728. N.F.R. CRAFTS, "Gross National Product in Europe, 1870-1910: Some New Estimates", *Explorations in Economic History* 20 (1983), pp. 387-401. *British Economic Growth during the Industrial Revolution*, (Oxford: Clarendon Press, 1985a). C.K. HARLEY, "British Industrialization before 1841: Evidence of Slower Growth during the Industrial Revolution", *Journal of Economic History* 42 (1982), pp. 267-289. C. FEINSTEIN, "Capital Formation in Great Britain", in P. MATHIAS & M.M. POSTAN, eds., *Cambridge Economic History of Europe*, Vol. 7, No. 1 (1978). A. MADDISON, *Phases of Capitalist Development*, (New York: Oxford University Press, 1982). F. CROUZET, "Angleterre et France au XVIIIe siècle: essai d'analyse comparée de deux croissances économiques", *Annales, Economies, Sociétés, Civilisations* 21 (1966), pp. 254-291. P. O'BRIEN and C. KEYDER, *Economic Growth in Britain and France 1780-1914*, (London: Allen & Unwin, 1978). J. DE VRIES, "The Decline and Rise of the Dutch Economy, 1675-1900", in Gary Saxonhouse & Gavin Wright, eds., *Technique, Spirit and Form in the Making of the Modern Economies. Essays in Honour of W.N. Parker* (Greenwich: JAI Press, 1984b).

1820s and 1830s.³⁷ The Hapsburg Danube economies, once regarded collectively as "latecomers" of European economic development, are now seen to be prototypes of gradual, cumulative progress with clear beginnings in the XVIIIth century.³⁸ In fact, in a recent publication John Komlos propagates an "Austrian Model" of development as the European norm. By this Komlos means two things: a) emphasis on the gradualness of growth; and (b) focusing on overcoming obstacles to recurring growth rather than on beginnings.³⁹ That takes us to the second modification of our 1970 view.

2. Recognition of the possibility that "beginnings" of MEG may be so diffuse as to offer no sensible basis for systematic research. The proposed alternative is to cease to search for beginnings of growth acceleration and instead to focus on the disappearance (or weakening of the causes) of breakdowns or growth reversals since the late -XVIIIth century. Note in passing that the results of recent revision just mentioned — coupled to other evidence such as that on the spread of steam power — could offer additional justification for striking that great misnomer, the Industrial Revolution, from our conceptual vocabulary; any such hope is no doubt vain.⁴⁰

In one respect, moreover, the Industrial Revolution period will retain its claim to uniqueness: it provided the context within which, for the first time in history, surging population growth did not lead into a major crisis and a long period of stagnation. That is, the idea of industrial revolution as another long-cycle upswing not followed by a comparable downturn — in contrast, e.g., to the expansion of the XVIth century — will encourage continuing re-

³⁷ R. TILLY, *Vorne Zollverein zum Industriestaat*, (Munich: Deutscher Taschenbuchverlag, 1990); and *Ibid.*, "German Industrialization and Gerschenkronian Backwardness", *Rivista di Storia Economica* 6 (1989), pp. 139-164.

³⁸ D.F. GOOD, *The Economic Rise of the Habsburg Empire, 1750-1914*, (Berkeley: University of California Press, 1984). J. KOMLOS, *The Habsburg Monarchy as a Custom Union: Economic Development in Austria-Hungary in the Nineteenth Century*, (Princeton: Princeton University Press, 1983).

³⁹ J. KOMLOS, *Nutrition and Economic Development in the Eighteenth Century Monarchy*, (Princeton: Princeton University Press, 1989). "Nutrition, population Growth, and the Industrial Revolution in England", *Social Science History* 14 (1990), pp. 69-92.

⁴⁰ J. MOKYR, *The Economics of the Industrial Revolution*, (Totowa: Rowman & Allenheld, 1985). "Has the Industrial Revolution Been Crowded Out? Some Reflections on Crafts and Williamson", *Explorations in Economic History* 24 (1987), pp. 293-319. R. CAMERON, "The Industrial Revolution: A Misnomer", *The History Teacher* 15 (1982), pp. 377-384. E.A. WRIGLEY, *Continuity, Chance and Change. The Character of the Industrial Revolution in England*. (Cambridge: University Press, 1988); also I. WALLERSTEIN, *The Modern World System*. (New York and Orlando: Academic Press, Vol. 3, 1974-1980).

search into the factors sustaining population growth without sharply falling living standards as part of economic history's ongoing agenda.

The particular phase or change of pace of growth known in many countries as their "industrial revolution" is by no means the only change of pace worthy of economic historians' attentions. Interest attaches to moving backwards and documenting origins of MEG and "how it all began", but in our agenda for the 1990s we see a fruitful movement forward in time as well. Here, the idea should be to examine how one phase of growth in one country may have affected subsequent ones, i.e. to see how "history mattered". The best example of what we have in mind can come from British experience.

The relatively high rates of Britain's economic growth over most of the XIXth century were associated with the development of a unique pattern of specialization in manufacturing and manufacturing exports. Economic historians have hypothesized that the late-XIXth-century slowdown in British growth — the precise timing of which is still being debated — had to do with the inability to alter the inherited economic structure and pattern to fit new technological and market possibilities. One may go even further: deficiencies of adjustment in the Victorian and Edwardian periods may have had effects which restrained growth in the interwar period and beyond — in fact, right up to the post-Thatcher present. It seems to us that the quality of discourse going on in a number of chapters of XXth-century economic history — at present highly compartmentalized — could greatly benefit from the knowledge of earlier periods and from the perspective of long-run economic growth. In Britain one thinks about current assessments of Thatcher economic policies, or about the debate — recently described as a "counter-revolution" in economic history — on the condition of the British economy and the nature of economic policy in the interwar period.⁴¹ But the same notion applies to the similar debate on German economic policies in the late-twenties and early thirties (known as the "Borchardt Controversy").⁴² Several reasons favour inclusion of such a perspective on our agenda. On the one hand, the significance of shocks and discontinuities is only identifiable in a long-run context. On the other hand, there may have been a trade-off between short-run cyclical recovery programmes and long-run growth. Assessing that possibility should be kept open; and it definitely requires a long-run orientation.

⁴¹ P.K. O'BRIEN, "Britain's Economy between the Wars: A Survey of a Counter-Revolution in Economic History", *Past and Present* 115 (1987), pp. 107-130.

⁴² Above all: KNUT BORCHARDT, "Zwangslagen und Handlungsspielräume in der großen Wirtschaftskrise der frühen dreißiger Jahre: Zur Revision des überlieferten Geschichtsbildes", reprinted in Borchardt, *Wachstum, Krisen, Handlungsspielräume der Wirtschaftspolitik* (Göttingen, 1982). See also *Geschichte und Gesellschaft*, 11 (1985), entire issue.

Economic Structure

Discussing the timing of economic growth inevitably involves discussion of its structure. Let us revert to the British case. The significance of British growth in the XVIIIth and early XIXth centuries can only be grasped by looking at its structure. In the following, five points are listed.

1) Many earlier studies had exaggerated the modern sectors and underestimated the weight of the slow-growing traditional sectors. That is an important point identified by the quantitative revision mentioned earlier. More work on these traditional sectors is still badly needed.

2) Those earlier studies also underestimated the productivity growth of agriculture. It now appears that high labour productivity in agriculture — reflecting a remarkable ability to reduce the agricultural labour force — was one of Britain's most striking structural features. It is this feature, moreover, which underlies this country's other structural peculiarities: its relatively large non-agricultural labour force in the countryside, which formed the basis for its protoindustrial development, its precocious, self-sustaining urban growth; and finally its high overall rate of population increase without a sharp decline in living standards. To some extent, to be sure, the British situation was shaped by a unique factor: access to Ireland's agricultural surplus.⁴³ The high priority our agenda assigns to investigating the sources of British agricultural productivity growth, should not preclude consideration of the role of Irish agriculture and its institutional bases. More on these later.

The envisioned agenda on the structure of economic growth, then, calls for particular attention — in all of the industrializing countries — to the nexus between agricultural productivity growth, on the one hand, and protoindustrialization, urban growth and changes in overall living standards, on the other. A critical condition of progress here will be the ability of historians to mobilize sources permitting credible estimates of agricultural output and inputs. Recent work and available techniques are encouraging.⁴⁴ The nexus mentioned, however, is one of interaction, and the markets for agricultural products still deserves close attention.⁴⁵

⁴³ B. THOMAS, "Food Supply in the United Kingdom during the Industrial Revolution", in J. MOKYR, *Economics of the Industrial Revolution* (note 40).

⁴⁴ R.C. ALLEN, "The Efficiency and Distributional Consequences of Eighteenth Century Enclosures", *Economic Journal* 92 (1982), pp. 937-953. *Ibid.*, "Inferring Yields from Probate Inventories", *Journal of Economic History* 48 (1988), pp. 117-126. M. OVERTON, "Estimating Crop Yields from Probate Industries: An Example from East Anglia", *Journal of Economic History* 39 (1979), pp. 363-378.

⁴⁵ On this G. GRANTHAM, "Agricultural Supply during the Industrial Revolution. French Evidence and European Implications", *Journal of Economic History* 49 (1989), pp. 43-72; and A. KUSSMAUL, "Agrarian Change in Seventeenth-Century England", *Journal of Economic History* 45 (1981), pp. 1-30.

The factors just mentioned in connection with agriculture are structural features important in their own right. They thus have an independent place on our agenda.

3) Mentioned twenty years ago, a few pages ago, and thus still on the agenda is that bugbear of some, protoindustrialization, important as an early form of specialized non-agricultural activity *per se*, as a basis for the development of specialized urban industry, and as a recruiting ground for factory labour and for entrepreneurs, not only in textiles.⁴⁶

4) Urban growth is important as a basis of modern industrial growth, a possibility perhaps unduly neglected in our development scenario of 20 years ago but now increasingly recognized.⁴⁷ One question deserving consideration here is when and how cities, demographic parasites in Europe until the XVIIIth century, became self-sustaining growth centres. Is public health policy a factor here?

5) The question of living standards is seen to reflect the structure of economic growth in the sense of its distribution of product between consumption and investment, or as a reflection of income distribution. Apart from the never-ending task of mobilizing relevant income and expenditure data, a number of promising lines of investigation suggest themselves. One Malthusian hypothesis which merits attention concerns the extent to which the low-income proletariat resulted from the relatively high fertility of proletarian families. The growing inequality of income distribution and declining living standards among workers as a whole — reflecting the picture we have emerging in a number of European countries, say, in the first half of the XIXth century — could derive from just such a demographic pattern.⁴⁸

One of the useful dimensions of this kind of study is that it offers a possible feedback to explanations of economic growth and could involve an interesting growth-equity trade-off with implications for the evaluation of con-

⁴⁶ Now available in English: Herbert Kisch, *From Domestic Manufacture to Industrial Revolution*. (New York: Oxford University Press, 1989); also D. LANDES, *Revolution in Time: Clocks and the Making of the Modern World*. (Cambridge: Harvard University Press, 1983).

⁴⁷ J. DE VRIES, *European Urbanization, 1500-1800*. (Cambridge: Harvard University Press, 1984a). L.H. LEES and P. HOHENBERG, *The Making of Urban Europe, 1000-1850*. (Cambridge: Harvard University Press, 1985). P. CLARK, *English Towns in Transition, 1500-1700*. (Oxford: Oxford University Press, 1976).

⁴⁸ C. TILLY, "Flows of Capital and Forms of Industry in Europe, 1500-1900", *Theory and Society* 12 (1983), pp. 123-143. "Demographic Origins of the European Proletariat" in D. LEVINE, ed., *Proletarianization and Family Life*. (Orlando, Florida: Academic Press, 1984). Jeffrey G. Williamson, "Was the Industrial Revolution Worth It? Disamenities and Death in 19th Century British Towns", *Explorations in Economic History* 19 (1982), pp. 221-245. *Did British Capitalism Breed Inequality?* (Boston: Allen & Unwin, 1985).

temporary social and economic policy. Some European countries — or at least parts of countries — may have been “labour-surplus” economies (Flanders in Belgium and some parts of Germany). However, data problems abound. For example, property income in most countries is only incompletely identifiable; and wages are not available for many occupations. For this reason, among others, some scholars have turned to other kinds of data for indirect evidence on living standards or real incomes, e.g., to certain standard consumption items, such as bread, meat, etc.; and one controversial school has been collecting data on human heights as an indicator of nutrition and, hence, an important component of living standards. Despite some enthusiasts, it is not yet clear how much help this set of data will yield. There is, e.g., the anomaly that periods of rapid economic growth seem to coincide with falling average heights in the early stages of industrialization. Does this reflect, as Komlos suggests, diminishing returns to labour on fixed land supplies? Could it mean that population growth was highest among the poorly nourished proletariat? Or does it reflect certain biases in the data themselves (Söderberg)? No doubt time will tell.⁴⁹

International Variation

Our growth agenda obviously embraces cross-country comparison. It is virtually a truism that the significance of particular national growth patterns and structures only becomes readily apparent within an international comparative context. In any case, we certainly sympathize with O'Brien's plea for explicit cross-country productivity comparisons as a basis for rewriting modern economic history.⁵⁰ We have hopes that other scholars will continue to follow the model his own work — on Anglo-French productivity differences — has set here. His criticisms of the older paradigms of industrialization — those having Britain as the technological leader and the the rest of the world developing through emulation — has considerable merit. For one thing, Britain's unusual degree of specialization in coal, iron, textiles, and manufactured exports on the basis of comparative cost advantage was not a feasible target or model for the other industrializers of the XIXth century.⁵¹ Our

⁴⁹ J. KOMLOS, *Nutrition and Economic Development in the Eighteenth Century Habsburg Monarchy*. (Princeton: Princeton University Press, 1989). L.G. SANDBERG and R.H. STECKEL, “Heights and Economic History: The Swedish Case”, *Annals of Human Biology* 14, (1987). R. FLOUD, K. WACHTER and A. GREGORY, *Height, Health and History. Nutritional Status in the United Kingdom, 1750-1980*. (Cambridge, Engl. 1990).

⁵⁰ P.K. O'BRIEN, “Do We Have a Typology for the Study of European Industrialization in the XIXth Century?”. *Journal of European Economic History* 15 (1986), pp. 291-333.

⁵¹ N.F.R. CRAFTS, *British Economic Growth during the Industrial Revolution* (note 36).

agenda, however, would not go far as to abandon the paradigm of economic leadership and "catching-up" growth. Indeed, it is an essential part of agenda. As Maddison's work shows, it facilitates explication of long-run growth differences among countries — from the XVIIth century era of Dutch leadership down to the present "Japanese Age".⁵² And as more recent work on the post-1945 period shows, "catching-up" growth is a fairly robust variable in explanations of country growth differences and one which permits a more careful weighting of other sources of growth — educational policy, degree of monopoly, etc. — than would otherwise be possible.⁵³ Its importance is undoubtedly related to international links: flows of goods and services and information. These items thus belong on the agenda too.

Conclusions

De-economizing European economic history does not mean expunging old economic concerns with scarcity, efficiency, inequality, accumulation, and growth; on the contrary, it means placing them properly in their historical context instead of extrapolating backward from models — however adequate — of contemporary market economies. Economizing European social history does not mean returning to straightforward base-superstructure determinism; on the contrary, it means taking seriously the interdependence of different kinds of production and reproduction. After decades of differentiation, the two together will promote a merging of economic, social, and — yes — political history. Their proper pursuit will eliminate the old ambivalent quest after separate economic and social realms outside the world of power, coercion, and the state. We no longer need the specious justification of separate realms to establish the legitimacy of economic and social history. The future lies, precisely, in a historically situated, socially rich political economy.

⁵² A. MADDISON, *Phases of Capitalist Development*. (New York: Oxford University Press, 1982).

⁵³ W. BAUMOL, "Productivity Growth, Convergence, and Welfare: What the Long-Run Data Show", *American Economic Review* 76 (1986), pp. 1072-1085. M. ABRAMOWITZ, "Rapid Growth Potential and its Realization; The Experience of Capitalist Economies in the Post-War Period", in E. MALINVAUD, ed., *Economic Growth and Resources*. New York, Vol. 1 (1979); *Ibid.*, "Catching Up, Forgoing Ahead and Falling Behind", *Journal of Economic History* 46 (1986), pp. 285-406. R. DUMKE, "Reassessing the *Wirtschaftswunder*: Reconstruction and Postwar Growth in West Germany in an International Context", *Oxford Bulletin of Economics and Statistics* 52 (1990), pp. 451-91.

