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German economic history and Cliometrics: A selective survey of recent tendencies

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This article discusses recent cliometric contributions by German and non-German economic historians to the field of German economic history. After a brief attempt to describe the field in Germany I survey recent work in four specific topical areas – which thus serve as rough indices of the spread and development of econometric and quantitative techniques in the field. I conclude that a German 'cliometric revolution' has not yet taken place, but that promising beginnings have been made.

1. Introduction

Many years ago I did a review of Germany economic historiography. It is now time for a second attempt.[1] The timing, obviously, follows from the nature of this issue of the *European Review of Economic History*. But beyond that, what makes the project worthwhile are some recent stirrings of change in the field – signs of growing acceptance of the 'cliometric' approach among German economic historians – that deserve comment. That points to one limitation of this survey: its object is not the entire field of economic history but that part of it which has been penetrated by serious attempts at quantitative research. Further related limitations are a concentration on the more recent period (since around 1850) and on a small number of themes which have attracted a great deal of scholarly attention. Within these constraints, the survey defines its topic broadly, to include non-German contributions as well as German ones. That gives us more to think about, though it may make the survey somewhat less focused.

I need to say what I mean by 'cliometrics'. I see it as economic history in which quantitative questions predominate, though the presentation of numbers alone

[1] The author published a survey in the *Journal of Economic History* in 1969. A German-language survey followed (Tilly 1980 and again in Tilly 1996). The last words (in English) were published in Lee's (1988) topically somewhat restricted review, and in the essays on German cliometrics by Komlos and Eddie (1997) and (1999). Quite a few discussions of the field have appeared in German, of course, but this essay makes no attempt at a complete compilation. See, for example, the essays by R. Walter or C. Wischermann in Schremmer (1998) and the discussion papers published in the *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte* (*VSWG*), vol. 82, pp. 387–423 and 497–510.

would not suffice, that is, analysis is also essential. This overlaps with what some of us call 'historical economics', where the use of economic theory, with or without quantification, may qualify for membership in the club.[2] In the rest of this article I include under the heading of cliometrics work belonging to both genres, as well as to what I call standard economic history having cliometric sub-sections.

The article has the following structure. It begins by briefly describing the field as it has developed in Germany in recent years, listing a few of the field's main organisational features, its main publication organs and its principal paradigms. To this it adds a brief comment on 'foreign' contributors to the field. The main body of the article follows. Its intent is to examine a few of the leading themes (paradigms) from the cliometric perspective. Four themes (or paradigms) are discussed: the measurement and explanation of Germany's long-run economic growth; the role of institutions (and the 'economic order'); the sub-field of monetary and banking history; and the so-called 'Borchardt Controversy' concerning German experience in the 1920s and 1930s. So far as the article offers critical judgements they appear within the context of descriptions of these four areas. Nevertheless, a brief conclusion does include a few general interpretative comments.

2. The field in Germany

We have no reason to envision the field of German economic history as an increasingly visible core of dedicated cliometricians, surrounded by a fading periphery of traditionalists. What we have, in fact, is a total of some 40–50 university professorships, adding up, if we include instructors, lecturers, full-time assistants and a rising number of retired professors, to perhaps 150 full-time practitioners. In addition, there are economic historians outside the universities, for example among the archivists or in a number of research institutes. Perhaps two-fifths of the professorships are domiciled in economics departments, two-fifths in history departments, and the remainder mainly connected to the fields of sociology or political science (or both).[3] Taken together, this field represents a spectrum of intellectual interests describable as no less than catholic. It covers numismatics, the history of technology, agrarian history, the history of the labour movement and of socialism, the history of economic ideas, the history of cities, historical demography, the origins of the state, the forms and causes of social conflict, cultural history, the history of war, of gender, and so on. Most of the persons involved are *economic* historians, even where they have a nominal obligation to cover social history. A small number of these are

[2] See on this Dumke (1992, pp. 25–6).

[3] Note that most of Germany's university chairs are chairs of Economic *and* Social History, which reflects a tradition of concern that the social and political ramifications and foundations of economic change be jointly considered in teaching and research. See also Tilly (1969, p. 298).

medievalists, a few are specialists on the early modern period, and the rest – the overwhelming majority – work mainly on the nineteenth and twentieth centuries; but only a handful, perhaps a dozen, work consistently cliometrically.

A look at the field's main publication organs confirms the picture just presented. In general, the publication opportunities in Germany may be characterised as abundant, if not super-abundant. Four journals devote their space largely to economic history and there are also a number of specialised journals. It is said of our field that no scholarly article need remain unpublished in Germany, a feature which, among other things, widens the range of treated topics and raises the costs of any attempt to characterise the field. Nevertheless, in recent years in all of this heterogeneous output there has been a general tendency to focus on the economic history of the more recent period, the age of industrialisation and the twentieth century. This is even true of the oldest journal, the tradition-rich *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte (VSWG)*, where both social history and concern with earlier periods have been in retreat for some time.[4] In its new guise, The *Jahrbuch für Wirtschaftsgeschichte*, up to 1990 an East German (GDR) journal, has strengthened an inherited bias toward recent economic history, while the *Zeitschrift für Unternehmensgeschichte* (*ZUG*), which publishes specialised articles on business history as well as general economic history, concentrates exclusively on the modern period. Of the cited four journals, only the *Scripta Mercaturae* has maintained rough parity between pre-industrial and modern economic history. Here, as elsewhere, however, articles explicitly addressed to social history are in decline. I find that observation interesting, if only because it contradicts – for one country at least – a prediction about our field for the 1990s with which I was associated some years back.[5] Finally, since this survey is about cliometrics, I should mention the *Historical Social Research* (*HSR*), a journal which has been publishing quantitative contributions to political, social and economic history since 1975, and which clearly overlaps with what this article understands as cliometrics.[6]

Certain organisational features of the field call for attention, not so much for completeness' sake as for the reason that they have certain methodological implications relevant here. The senior organisation is the 'Gesellschaft für Sozial- und Wirtschaftsgeschichte', to which most German economic

[4] In 'Soll und Haben' (Tilly 1969, p. 301), I noted that 60 per cent of the *VSWG*'s articles dealt with the pre-industrial age and roughly half were concerned with social history. In the 1990s those proportions are way down, to about one third and one fourth, respectively.

[5] See Tilly *et al.* (1991). Strictly speaking, we were making a normative argument, claiming that good economic history needed to incorporate more of the concerns of social history into its analysis. And to some extent, in Germany as elsewhere, economic history *has* increasingly built social history into its own arguments, for example, by explicitly considering the role of social institutions. I return to this theme again later.

[6] The HSR is published by Quantum, a Cologne-based organisation which also sponsors summer school seminars and conferences related to historical methods and especially those related to quantification.

and social historians belong, which has long stood for a policy of autonomy for the discipline economic history *vis-á-vis* its twin bases (of economics and history), which has in the past maintained close links to the traditional journal, the *VSWG*, and which has organised bi-annual conferences, the proceedings of which represent the field's intellectual focus (or eclecticism) and to which I return below. Economic historians also form a bloc in the 'parent' organisations of economics (Committee for Economic History of the 'Verein für Socialpolitik') and history ('Verband der Historiker und Historikerinnen Deutschlands'). These organisations also generate annual or bi-annual conferences and related publications. The intent of the work within these larger organisations seems to have been the promotion of closer links to those 'parents', for example, by treating topics of concern to economists with respect to historians, by stressing the use of economic theory, and so on. One motivation behind this was doubtless the hope that thereby economic history could demonstrate its relevance for economics in respect of history and thus gain institutional support to strengthen its chances of survival as an academic discipline.[7] My survey, finally, would not be complete without mentioning one of the field's most dynamic organisations, the 'Society for Enterprise History' (or 'Gesellschaft für Unternehmensgeschichte'), which is responsible for the publication of the *ZUG* (cited above) and also many specialised monographs and conference volumes, some of which deal with general questions of economic history, thus transcending the enterprise perspective.

These features add up to a characterisation of German economic history as a field marked by great heterogeneity. Thus, German economic history can be said to encompass highly varied interests. However one dissects it, the facts of its lack of unity and the absence, until very recently, of substantial support for a cliometric approach would seem to be indisputable.[8]

[7] The question of whether the discipline of economic history need be seen as an endangered species is not of central concern to this essay, though the issue cannot be avoided entirely. Note, however, that the discipline itself has generated widely differing, indeed diametrically opposed, solutions to the problem. See the discussion in the *VSWG* cited in note 1 above.

[8] In the early 1970s I was able to induce a number of students to write 'cliometric-type' dissertations, for example, Holtfrerich (1973), and Fremdling (1975). But although these studies were recognised as useful contributions to economic history, the 'new technology' involved did not diffuse across Germany at this time, probably because the overall environment was insufficiently hospitable, as Dumke, Komlos and Eddie argue in recent assessments (see notes 1 and 2 above). Note also that in the *VSWG* statements cited above at least two authors (Buchheim and Schinzinger) explicitly warned against strong emphasis on cliometrics or even quantification; and there were no unqualified endorsements of the cliometric approach. Two recent positive signs of change are (a) the conference volume (cited in n. 1) edited by Schremmer (1998) and based on conference papers presented in 1997, and (b) the Toronto Conference on German Cliometrics which took place in September 1999. Yet a third shift in this direction can be seen in the work on 'anthropometric history' recently produced by John Komlos and his associates at the University of Munich. See Komlos (1998) and Baten (1999).

Addendum on 'foreign' contributions

An addendum is necessary to cover those 'German' economic historians who live outside Germany, for, as this survey and earlier surveys indicate – and as recent developments such as the volume on German cliometrics edited by John Komlos and Scott Eddie (cited in note 1) and the Toronto cliometrics conference organised by those same two colleagues with Jörg Baten in 1999 further document – this group has played a disproportionately important role in the field, especially in its cliometric segment. Just offhand, I could list about 20 persons in this category who have published regularly over the past decade or so, but there are probably a few more I have missed. Some of their contributions come up for discussion in the thematic sections which follow. Space limitations preclude a comprehensive treatment of the others here, but perhaps even an abbreviated comment can suggest their scope and nature. John Brown, for example, has investigated problems of German urbanisation such as housing and water supply using, among other tools, regression analysis.[9] Rolf Dumke's important University of Wisconsin dissertation on the German *Zollverein* (1976) qualifies as a cliometric import, even if the author has since then lived in Germany and published in German on that same subject.[10] Peter Temin is yet another sometime member of the group, most recently as author of a revisionist article on the 'Korea Boom' as key to West Germany's postwar 'Wirtschaftswunder'.[11] I close this section, finally, with references to two further 'foreign' contributors: one by Raymond Cohn applying the concept of a Full Employment Government Budget to Germany at the end of Weimar and beginning of Hitler's Germany, another by Larry Neal examining the importance of slowing population growth (and immigration) as an economic factor bearing on West German development after 1945, for both, in different ways, show how focusing on a delimited quantifiable problem can help change our perceptions of economic history, and help direct attention to new possibilities.[12]

3. Key themes

The best way to characterise the contribution of quantitative work to recent German economic history is to examine a number of concrete issues which

[9] Brown (1987, 1988, 1991).
[10] Dumke (1976, 1984).
[11] Temin (1995), reprinted in Komlos and Eddie (1997, pp. 351–69). Temin's strictures are especially directed at Abelhauser (1979) and Giersch *et al.* (1992).
[12] It is likely that 'outsider' social scientists are generally better positioned to see and exploit new research possibilities in a given country than native scholars, that is, German economic historiography may not be a unique case. See Cohn (1992), reprinted in Komlos and Eddie (1997), and Neal (1984).

have attracted more than negligible attention. In the following discussions I will offer some personal judgements and occasionally attempt to adjudicate among conflicting interpretations, but the aim throughout lies less in conflict resolution than in reportage and identification of methodological issues.

Germany's long-run economic growth

This is a good place to begin, for economic growth is an inherently quantitative phenomenon and its treatment in German economic historiography covers the full range of methodological problems of interest here. These are: the use of theory, in particular the relative merits of neoclassical and the 'new' growth theory; and the treatment of measurement problems, running, for example, from the application of new techniques of time series analysis to discussions of the quality of the underlying data. Some of the relevant arguments appeared quite a while ago, in an article published by Knut Borchardt in 1977.[13] Using largely aggregate data and graphical analysis, he raised the question: does modern German economic history – from, say, 1850 to the present – reflect an identifiable long-run trend of economic growth? Since Germany's growth experience over the period varied considerably, this posed a major challenge. His answer was to offer three competing trend concepts and to suggest that each corresponded to a particular theoretical interpretation of long-run growth, each relating the events and changes of shorter periods or phases to the long-run trend in different ways. Precocious, in the light of subsequent discussion of time series analysis of long-run growth, was Borchardt's emphasis on the interdependence of short-run changes and shocks with long-run growth trends and on the subjectivity involved in choosing among trend concepts (or models).[14] Nevertheless, Borchardt's interpretation did not fully anticipate econometric advances in historical time series analysis, especially the treatment of random shocks as phenomena having permanent effects on the long-term growth trend. Some of these new methods – which have collectively amounted to a revolution in econometrics since the 1980s[15] – have been brought into the historiography by Rainer Metz. In a series of critical articles applying the new techniques to German aggregate data he casts doubt on (a) the relevance of the constant long-run linear trend of the neo-

[13] Borchardt (1977). See also Spree (1991).

[14] One of his most important conclusions: no adequate interpretation of short-run change (or cycles) without joint consideration of the long-run trend; and no adequate modelling of the long-run trend without consideration of the short-run changes. At the same time, however, he warned that the choice of trend model could not be inferred from the statistical results, but required judgements concerning assumptions about behaviour, institutions, and so on, which inevitably contain a high degree of subjectivism.

[15] I am referring here to the analysis of unit roots and cointegration. See, for example, the Introduction in Rao (1994).

classical type and also (b) on the 'long-wave' curvilinear trend of the Kondratieff type. What he offers in their place is not a long-run 'stochastic' trend, but a temporally variable trend in which only a small number of strong, random shocks are seen to have had persistent effects on long-run growth.[16] This latter result, to be sure, is based only on statistical techniques and has no theoretical basis. Seen in context, however, Metz's contributions have certainly raised the level of cliometric discourse on long-run German growth and deserve serious attention.

Improvements in our understanding of Germany's long-run growth have come largely from the desire to use such knowledge to clarify specific historiographical issues. Rainer Fremdling's articles on the national output and income aggregates, for instance, stem from an interest in international comparison of productivity and *per capita* incomes.[17] These articles have consequences. For one major casualty of this work has been our faith in the utility of Walter Hoffmann's published estimates of Germany's aggregate output and income, 1850–1925, estimates which have been widely used.[18] Fremdling clearly shows that Hoffmann's procedures, for various reasons, underestimate the level of productivity and *per capita* income before 1913, though more for the 1850s than for the 1900s. The historiographical implications could be far-reaching: Germany's relative backwardness in the so-called 'take-off' period of industrialisation was quite likely significantly less, Germany's economic growth over the 1850–1913 period accordingly less striking, and the slowdown in the Weimar years more pronounced, than previously believed. Some of these same issues are also affected by recent revisions of the same aggregate output and income data by Albrecht Ritschl and Mark Spoerer.[19] Their work covers a somewhat later period, this being motivated by the ability to draw on official sources which offer a true alternative to the Hoffmann estimates. They make a good case for a downward revision of the latter, especially for the interwar period.[20] Once again, this has considerable importance for the historiography. If they are right, many judgments about the Weimar Republic and the

[16] Metz (1992, 1993 and 1998).

[17] See, for example, Fremdling (1988, 1995), and Broadberry and Fremdling (1990).

[18] Strictly speaking, reference here is to the collective work: see Hoffmann (1965). At the Toronto German Clio conference Ben Tipton presented a paper entitled 'Tales of Hoffmann' which offers, as the title suggests, severe criticism of Hoffmann's product and productivity estimates.

[19] Ritschl and Spoerer (1997); Spoerer (1998); Ritschl (1995).

[20] I note in passing that they seem to contradict Fremdling's conjectures on the pre-1914 years, since they suggest that Hoffmann's estimates overestimate the aggregate productivity level in 1913 by 10 per cent, while Fremdling is unsure of the direction of error. This may derive from the fact that they offer an alternative estimate of capital income, whereas Fremdling merely points to the need for re-estimating Hoffmann's dubious calculation of this magnitude.

Third Reich are wrong, including the nearly universally used estimates of Angus Maddison.[21] Among other things, their results place the achievements of the West German economy after 1945 in a much more favourable light, the 'Wirtschaftswunder' seems to get a new lease of life, and the deficiencies of the East German economy become all the more apparent.

The reference to the 'Wirtschaftswunder' opens yet another chapter in the historiography of German economic growth: that concerning West Germany's celebrated 'economic miracle' of the post-1945 years. Perennially a hot topic, this success story began to attract renewed attention a little over 20 years ago – in the 1970s – as a slowdown in the growth of the capitalist economies and a related condition of 'stagflation' seemed to have become quite general.[22] In the Borchardt (1977) contribution cited above, the unique character of Germany's post-1945 experience stood out; and it cited the 'reconstruction thesis' (Janossy) – which saw high growth rates to be a reaction naturally following from wartime interruptions – as its most plausible explanation. This argument was then taken up by Werner Abelshauser and Dietmar Petzina in what was to become a classic reinterpretation of Germany's twentieth-century economic history.[23] Another rekindling of interest emerged about ten years later, this time related to the break-up of the East European socialist economies and German reunification, phenomena accompanied by the hope that history might repeat itself, especially to the benefit of the larger Germany's ex-GDR regions.[24] This was, in retrospect, a particularly propitious background for 'growth history'. Scientific discourse, however, follows its own rules, in economic history as elsewhere. In the 1980s, macroeconomics, for a number of reasons, became dissatisfied with its standard growth theory, that is, neo-classical growth theory, and developed the so-called 'new growth theory' in response. Increasing returns to scale through externalities and investment in human capital became instruments for endogenising the growth of Total Factor Productivity (TFP) and thus reducing the unexplained 'residual'. International differences in economic growth became, once again, a fash-

[21] Maddison (1982, 1991).

[22] See the treatment by Maddison (1982), and Olson (1982).

[23] Borchardt (1977, pp. 154–6); Abelshauser and Petzina (1980). Larry Neal's recent EHA presidential address called attention to the link between migration and 'Wirtschaftswunder' – a sizeable part of the Abelshauser and Petzina argument. See Neal (2000) and also Neal (1984).

[24] Chronologically, these political shifts coincided roughly with the 50th anniversary of the West German Republic, an event which in any case would have called forth a flood of historically-minded publications stressing the 'Wirtschaftswunder'. In fact, some of the publications which appeared at this time used the occasion to call for a return to the presumed economic virtues of the immediate postwar years. See, for example, Giersch *et al.* (1992); for a survey, see Lindlar (1997).

ionable topic, in part for criticising or defending the neoclassical model; and the inevitable appeal to history led economic historians to join in the task. The great paradigm became understanding 'the Golden Age' of Western European capitalism, of which West Germany's 'miracle' was naturally a substantial part.[25]

Integration of the 'Wirtschaftswunder' in the broader international discussion took a while, however. Rolf Dumke's 1990 article represented an important step forward in this respect. It offers a critique of three hypotheses which have been used to explain postwar growth: the (already cited) 'reconstruction thesis', the structural change (or 'structural break') hypothesis, and the more recent 'catching-up' hypothesis.[26] Table 1, taken from a recent Münster dissertation by Thomas Bittner, places these in the perspective of a choice menu.

The distinction between domestic and international factors is obvious. That between growth potential and structural change is not. The intent is to distinguish growth which results largely from realisation of a pre-existing potential from growth which follows mainly from specific institutional and policy changes. In Hypothesis (1), the reconstruction thesis sees postwar growth as realisation of a high potential based on overcoming war-created bottlenecks in the economy which temporarily raised the marginal productivity of non-human capital sky-high in relation to an abundant supply of human capital. The exploitation of this unique opportunity leads to falling marginal productivity of capital and slowing growth – a return to an assumed long-run growth trend. Hypothesis (2), catching-up, is based on

Table 1. Hypotheses explaining Western Europe's postwar growth.

	Economic growth potential	Structural change (or 'break')
Domestic factors	(1) Reconstruction thesis (Janossy)	(3) Institutions and policies: e.g. anti-cyclical stabilisation policy; break-up of national economic lobbies (Olson)
International factors	(2) Technological catching-up hypothesis (Baumol)	(4) Institutions and policies, e.g. governing international monetary and trade relations (Eichengreen)

Source: Bittner (2000).

[25] For some of these points and comments on the burgeoning literature, see Temin's (1997) survey. The 'golden age' label probably goes back to Marglin's and Schor's (1990) collection of essays. The emphasis there, however, is more on macroeconomic policy dilemmas and Keynesianism than on growth.

[26] Perhaps the first, or most succinct formulation of the 'catching-up growth' hypothesis was by Ambramovitz (1986).

international differences in technological levels which became exploitable after World War II, the technology and total factor productivity of the international leader (the USA) representing growth potential for the laggard countries. Institutions and policies might matter insofar as they could co-determine a country's ability to adopt the leader's technologies; but they do not drive the system. In Hypotheses (3) and (4), however, changes in institutions and policies *are* the main story. In Hypothesis (3), on the domestic level, reforms, such as deregulation, remove blockages and permit, say, higher investment rates or an improved allocation of capital and labour. Hypothesis (4), on the other hand, makes changes in international institutions or organisations responsible for higher growth, via, for example, changes which foster trade liberalisation, increased international specialisation and higher productivity in the affected economies.

Dumke's contribution emphasises Hypothesis (1), reconstruction; though it also discuses the other 'sources of growth'. The principal focus is on Germany, but the heart of his empirical analysis is a set of cross-country regressions based on international comparison: drawing on Maddison's (1982) aggregate data for 15–16 countries (1950–80), he estimates the contribution of reconstruction and catching-up growth potential to the growth of these countries. The interesting result is that both of these factors proved significant, that is, the postwar 'Wirtschaftswunder' was by no means a uniquely West German phenomenon. That is important – a qualification of the strong propensity of German economists, historians and politicians to treat their country's postwar success as the product of wise economic policies (related to Ludwig Erhard and the advent of the 'social market economy').[27] Thomas Bittner's dissertation, which compares French and German growth in this period, points in the same direction.[28] All is not well with Dumke's results, however. First, his econometric specification is not robust, for example, elimination of the quadratic term (GAP^2, which in any case has no theoretical justification), drastically reduces the significance of the reconstruction and catching-up growth variables.[29] Second, as Dumke himself points out, his approach assumes a constant, long-run growth trend which really needs to be empirically verified, a task beyond his paper.[30]

[27] See the discussion in Lindlar (1997, esp. pp. 43–51).

[28] Bittner (2000). Bittner even goes a step further by showing that neither specifically French nor specifically German economic policies had significant measurable effects on the two economies' respective growth performances.

[29] See the critique by Reichel (1998).

[30] Dumke (1990, p. 487). As indicated earlier (see notes 10 and 12), short-run changes and shocks can have a permanent effect on the long-run growth trend, a possibility which could eliminate any 'reconstruction effects'. Bittner (2000), stresses the need to test the 'reconstruction thesis' by means of time series analysis, whereas Dumke's approach is to assume a norm based on the average growth rates of the countries in the Maddison sample.

Despite such criticisms, Dumke's main point still stands as one part of the ongoing debate on Germany's postwar growth. I mean here the debate between proponents of the growth potential/reconstruction thesis and believers in the decisive role of institutional and policy changes (Lindlar labels this the thesis of 'the social market economy'). The two big changes which have caused most controversy are (a) the currency and price reforms associated with Ludwig Erhard and (b) the Marshall Plan. Whereas 'reconstructionists' like Abelshauser see (quantifiable) steps toward realisation of 'growth potential' taken as early as 1946, defenders of the 'structural break' thesis (like Ritschl, Buchheim or Klump) offer alternative quantitative estimates and some theoretical and methodological arguments which support the importance of a postwar shift in institutions and policy.[31] Readers interested in the details of this debate are invited to consult the growing literature.[32] For the purpose at hand, just one observation must suffice. The evidence that the strong growth of the 1948–73 period was related to a backlog of opportunities created by World War II seems irresistible, the evidence for a sharp change in policy regime after 1945 less so. Nevertheless, the generality of any appeal to the 'reconstruction syndrome' is limited by the difficulty that a similar 'backlog of opportunity' following World War I did not produce comparably positive growth results. Further cliometric action is needed which permits closer investigation of the effects of a change in policy regimes (or institutions), if necessary via the specification of counterfactuals and with the help of cross-country and intertemporal comparison, especially the latter.

Institutional change and policy regimes

The previous section closed with a hint that institutional and policy regime changes can be seen as one plausible determinant of West Germany's postwar economic growth. This could be a generally valid hypothesis, true of other periods of economic history as well. A closer look at the historiography, however, reveals some serious problems. For one thing, it involves more controversy than consensus. There is dissent concerning both the extent and nature of institutional change after 1945; and the same applies to earlier periods. For another, it has not attracted direct quantitative treatment. Nevertheless, the topic has sufficient weight to deserve attention here; and it does make a coherent story.

I begin at the end. Since the 1980s, economic globalisation has made

[31] For a critique of the literature see, once again, Lindlar (1997), ch. 3. The importance of quantification comes out in the exchange between Abelshauser (1979) and Ritschl (1985) concerning construction of a West German index of industrial production for the 1946–49 period.

[32] In addition to the works already cited, see Abelshauser (1983); Klump (1985); the essays by Berger and Ritschl in Dornbusch *et al.* (1993); and Buchheim (1990).

academics and politicians in the European economies more critical of national government intervention and institutions related thereto. In Germany, what has been dubbed 'the German model' has lost supporters. The future of the 'social state' seems at stake (Hauser 1998). That covers a great deal: a system of industrial production in which skilled labour and quality products dominate rather than capital-intensive mass production; a comprehensive system of compulsory social insurance; cooperative institutional arrangements in the labour market (for example, with co-determination by labour representatives); cooperative behaviour at the enterprise level (including cross-holdings of equity); relationship banking with interlocking directorates; organised interest groups having an active role in the formulation of economic policy; extensive protection of public enterprises (for example, the regional savings banks), and so on. This discussion has induced, among other things, an appeal to the historical record. A look at the postwar experience pitted, as reported, the 'restoration' and 'reconstruction' thesis against the thesis of institutional change (or 'structural break'). According to the former, the economic institutions of the Federal Republic were largely traditional. The organisation of industrial interests remained pretty much intact, still dominated by big business, especially heavy industry. The attempted break-up of the big banks proved ephemeral in its effects. The rebirth of strong industrial unions and a reinstatement of collective bargaining broke with the history of the Third Reich, but represented nevertheless a return to an earlier arrangement. These references to the vigour of traditional institutions were bulwarked by an explanation of postwar growth dominated, as reported above, by reconstructionist elements: the presence in 1945 of a pre-existing industrial capacity (instead of wartime depletion, wartime enlargement and rejuvenation, of the industrial capital stock); from 1945 on state-sponsored repair and modernisation of the country's damaged infrastructure; an influx of refugees which embodied a huge import of human capital; a large-scale industrial investment programme organised jointly by the state and business associations; and so on. This picture naturally contradicted the view of Wirtschaftswunder as the product of a postwar policy shift to neoliberalism. The latter's persuasiveness, it was suggested earlier, depended heavily on interpretations of the currency and price reforms of 1948 associated with Ludwig Erhard.

Controversy is not the chief problem of this historiography, however. The sober truth is that the more recent literature contains virtually no examples of quantitative proof that such institutions played an important economic role. Eichengreen (1996) offered a framework of analysis of institutions applicable to West Germany (as well as to other West European economies) which had institutions serving as the means for reducing informational asymmetries (domestically) between capital and labour and (internationally) between countries. German economic history, however, has not taken up Eichengreen's offer. Thus, to take one concrete example, the forms of labour organisation, such as co-determination by employee representatives, could have induced wage restraint, a policy based on trust in employers' willingness to honour such restraint (for example, with reinvestment of profits). There is also some evidence of 'wage drift' in West Germany which could signal wage moderation, to be sure (Giersch *et al.* 1992); but a study which connects it to the institution of co-determination and measures its impact is missing.[33] Unfortunately, this is the general pattern. A number of studies exist which mention particular institutions and correlate them roughly with 'leaps' in industrial growth; but the desired quantitative link between institutional 'output' change and industrial growth remains unidentified.[34]

The German tradition of emphasis on extra-market institutions, of course, goes back to the nineteenth century, especially to the last quarter of the century, when German economic liberalism came under fire and corresponding institutional changes took place. For the purpose of this survey it is not essential to recapitulate the literature which has discussed these changes.[35] One observation on the basic message can suffice. It was around this time that 'organised capitalism' became the principle institutional framework of the German economy – the 'German Model' referred to earlier.[36] Chandler's (1990) more recent treatment of Germany's largest enterprises as a system of 'cooperative managerial capitalism' actually strengthens that interpretation. In a recent study, Herrigel (1996) has challenged this line of argument, but the basic notion of a 'German Model' of development driven by powerful, non-market, cooperative institutions remains the central idea. The question of interest here, however, concerns the presence or absence of cliometric studies of such institutions. In fact, there are few such studies. In the rest of this section, therefore, we focus only on those few.

Perhaps the best example of cliometric treatment of an economic institution is Tim Guinnane's work on German rural credit cooperatives.[37] He has been able to find archival material on membership and financial performance for a sample of individual cooperatives operating in the late-nineteenth century and connect it to hypotheses which draw on the economics of

[33] See Eichengreen (1996). As Bittner (2000, pp. 133–36) argues, however, 'wage drift', which reflects the gap between negotiated and *de facto* paid wages, might simply reflect unanticipated productivity gains.

[34] See, for example, Heldmann (1996), and literature cited there.

[35] For an overview, see Abelshauser (1987).

[36] A brief review of the literature is in Hentschel (1978, esp. pp. 9–21), citing works by Kocka (1969) and Wehler (1975).

[37] Guinnane, (1994, 1997a and 1997b).

information. In Guinnane's account, these cooperatives appear to have been an informationally efficient response to information asymmetry; and there is even evidence that they contributed to a significant fall in interest rates wherever and soon after they had taken root (in the 1860s).[38] As Guinnane points out, however, there was something in the German socio-economic environment which allowed the institution to work well there, and to fail when transplanted (for example, to Ireland). That something deserves more attention.

But although credit cooperatives represented an effective institutional response to 'market failure', they have not played a leading role in accounts of the origins of the 'German Model'. Cartels are closer to the historiographical center. But here, unfortunately, problems loom larger than solutions. In Hentschel's (1978) sceptical description of the most successful coal syndicate and iron and steel cartels, for instance, the quantitative analysis is limited to (a) a look at swings in annual production of steel products, (b) a comparison of *ex post* steel prices in (cartelised) Germany and (non-cartelised) Britain, and (c) some rough, casual estimates of the extent of 'outsider' capacity in these branches.[39] Steve Webb's study of German steel output and productivity for the 1880–1913 period utilises the presumed price- and output-stabilising effects of cartels – augmented as they were by protective tariffs from 1879 – to explain 'Germany's overtaking of Britain' in steel. His argument implied that cartels did not hinder realisation of scale economies, but his study eschews an explicit analysis of cartel pricing and output controls.[40] Peters (1981) and (1989), finally, does examine the institutional structure and governance rules of the two most renowned cartels, the Rhenish-Westphalian Coal Syndicate and the Steel Works Association.[41] His conclusion is that the cartels stabilised output growth and price formation to a certain extent, and that they also succeeded in conserving smaller firms and slowing concentration. This contradicts Webb's interpretation somewhat, but an analysis of effects on productivity and growth is missing. From a cliometric point of view, therefore, we are left with an incomplete story. Cartels were probably important, but given the relatively large capacity which remained outside them, it is by no means clear that they were a decisive institutional factor for the economy's long-term development.

Cartels have often been associated with the advent of protective tariffs in Germany in the late-1870s; and the two have been seen as joint products,

[38] For example, in the annual reports of the Industrie und Handelskammer of Dresden, 1864–67 (Sächsisches Wirtschaftsarchiv, Leipzig).

[39] Hentschel (1978, pp. 99–125).

[40] Webb (1980). By drawing on archival materials (on prices) Webb can estimate enterprise profits.

[41] Peters measures stabilisation by estimating variances of prices and outputs by period.

so to speak, of the crisis of these years. Some economic historians have attempted to analyse the emergence of protectionism (and rejection of liberalism) in terms of political economy.[42] They have done this in a rather impressionistic way, however, using characterisations of Bismarck's domestic and foreign policy aims and major interest groups – 'iron and rye' – as the vehicle of interpetation. In his contribution to this issue Klug offers a new approach in the form of a comparative analysis of the German elections of 1877 and 1878 and the British election of 1906 – both elections in which tariff protection played a leading role. With occupational data from the 1882 Census, he characterises the German voting districts by sector and estimates how much occupational shares – which are also classified as gainers or losers from tariff protection – contribute to the election of protectionist candidates. Readers will have to judge for themselves how well the author's economic explanation of the elections holds up; but the idea of explaining an important institutional change – the shift between a free-trade and a protectionist regime – in this way is a promising one. In a sense, this is an exception to the main finding of this section, namely, that the literature has rarely well-specified the economic characteristics of institutions in a way which lends itself to quantitative treatment.

Before leaving the topic of institutions, I would like to draw attention to a somewhat different perspective. The concept of 'organised capitalism' implied, among other things, the emergence of a strategy of cooperation between Capital and Labour, as opposed to a class conflict situation. Such a coming-to-terms between industrial enterprises and their employees was suggested in earlier work on the frequency and incidence of industrial strike activity.[43] The development of industrial unions and the beginnings of co-determination of industrial labour relations could also be cited as examples of institutional change which reflect an increasingly cooperative strategy, one possible source of Germany's late-nineteenth-century industrial success.[44] In their contribution to this volume Brown and Neumeier do not attempt an analysis of such institutions, but they do investigate a crucial aspect of industrial labour relations – job tenures – and their results shed light, at least indirectly, on the role institutions may have played. With the aid of the econometric tool of hazard functions, they are able to make sense of a vast mass of enterprise archival data on employment. What they find is that job tenures were largely determined by demand factors (the business cycle) or such supply-side factors as age, maturity and place of birth. These, in turn, largely reflect the course of the business cycle as well as the demographic characteristics of the German Kaiserreich. That seems to leave less to be explained by the concept of 'organised

[42] See Borchardt (1984), and also Tilly (1990, pp. 82–3, 111).
[43] Kaelble and Volkmann (1972).
[44] See Teuteberg (1961) and Lee (1978, pp. 442–91).

capitalism' and related institutions. No need here for emphasis on the 'German model' (or the 'Sonderweg'). In any case, if labour relations institutions were important, they should be shown to affect phenomena such as job tenures.

Money and banking

The role of money and banking in the German economy has attracted sufficent scholarly attention to warrant its treatment as a separate chapter in this survey. It begins with the discussion of money (and monetary institutions); and the following sub-section deals with the relevant banking historiography. I stress the word 'relevant', which in this context means 'quantitative'; and that implies an intended neglect of much banking history.

Research on Germany's nineteenth-century monetary history has faced the problem that, initially, 'Germany' consisted of many states and a number of only loosely connected, regional economies. On the one hand, that has encouraged studies of monetary integration, with the result that the latter could be causally linked to Germany's successful customs union (Zollverein).[45] Among other things, this discovery – of integration beginning as early as the 1830s – qualified the role of political unification (by means of Bismarck's 'blood and iron') as deliverer of German monetary unification. Nevertheless, more work needs to be done on the question. For this purpose, systematic use of the data on intra-German exchange rates published by Schneider and Schwarzer might be helpful; and a series on regional differences in bank behaviour still remains as a desideratum.[46] On the other hand, the plurality of sovereign states and persistence of regional differences in pre-1871 Germany raise doubts concerning the utility of German-wide money supply estimates for the earlier period. These apply to Bernd Sprenger's estimates of the aggregate German money stock from 1835 to 1871, less so to his estimates of that stock's structure;[47] and not at all to the 1871–1913 estimates, which can also be compared with other attempts.[48] Here, however, the relevant questions concern both the quality of the estimates and the purposes for which they can be employed. The quality of the available estimates suffers from uncertainty concerning (a) the data on international flows of specie (and coin), (b) the extent of specie/coin held by private and commercial banks as reserves, and (c) the extent to which the latter's liabilities can be viewed as 'money' and if so, to use a modern classification, of which type,

[45] See Holtfrerich (1989), Sprenger (1993), and Tilly (1998).

[46] For the former see Schneider and Schwarzer (1990). For the latter, which would involve study of bank balance sheets, a dissertation (or equivalent monograph) is needed.

[47] Sprenger (1982) and Sprenger (1995, chs. 9 and 10).

[48] See, for example, Tilly (1973).

M1, M2 or M3.[49] Unfortunately, I know of no satisfactory solution to any of these problems. Until one is found, economic historians will simply have to live with them, trusting in the usual assumptions and also in the fact that the closer one gets to 1913, the less serious the problems become.

Good money stock estimates, of course, are a *conditio sine qua non* for determining whether monetary policy has been excessively lax or tight, or 'on target'. The standard procedure has been to estimate money demand functions and compare these with the money supply data. Up to now, however, only one published set of estimates of money demand exists for pre-1914 Germany, that of Douglas Fisher.[50] In the comparative context in which they are presented, the estimates suggest a stable demand for money, in Germany as in other countries, and they seem plausible. I should add that their estimates reflect two defects. First, the econometric treatment does not face up to the problem of 'non-stationarity' of income and money.[51] Second, the broad definition of money they use probably exaggerates money stock growth, thus producing a high income elasticity of demand and also an unwarrantedly high degree of 'financial sophistication' for Germany before 1913.[52] Nevertheless, for the time being, they are 'all we have'. It remains to be seen whether the positive judgements which Craig and Fisher offer about German central banking and the gold standard will hold up when alternative series and estimates are available.[53]

This last observation takes us into a discussion of two important chapters of monetary history: the role of the international gold standard and the development of central banking. Since this is a survey of cliometric work, it can limit itself to the small number of studies having that orientation. My discussion begins with Marc Flandreau's work. Although not primarily concerned with German monetary history, it deserves mention here for two

49 There are actually two problems here. First, few private banking firms have left usable records, so the sampling problems are immense. Secondly, not even the joint-stock banks published consistently reliable information on their deposits. Sprenger (1982, p. 202), uses a broad definition, corresponding roughly to today's M3; but his estimates include savings accounts held in the Public Savings Banks, the degree of 'moniness' of which (before 1909) is doubtful.

50 See, for example, Craig and Fisher (1997, ch. 7, esp. pp. 175–76).

51 In some of my own unpublished work on the German data I have found non-stationarity and absence of cointegration between the variables money and income for the period, 1870–1913. I must mention here that Craig and Fisher do warn readers that their estimates may reflect data and unsolved econometric problems.

52 By reworking the money stock data, i.e. substituting M1 for M2, I found a much lower index of 'sophistication' (= M/Y × 52) for Germany on the eve of World War I, putting it on the level of Sweden.

53 Craig and Fisher (1997, pp. 129–30 and 185–86), find that prices are integrated among countries but money is not, while money demand is stable and similar across countries, thus suggesting that national differences between money demand and supply drove international gold movements. This result is (surprise, surprise) in harmony with the Monetary Theory of the Balance of Payments (MTBP).

reasons. First, it provides a strong reminder that currency convertibility did not begin with the spread of the gold standard since the 1870s and that the coexistence of bimetallic, silver and gold currencies served the international economy pretty well up to that time.[54] This observation encourages us to better understand the cost-conscious reluctance of contemporaries to 'move to gold' before the 1870s. Second, as Flandreau shows, it was only the German state's 'windfall' reparation gains in 1871 (the war indemnity of FF5bn paid by France) which allowed that country to take the plunge; and it was France's unwillingness to help Germany's transition to gold (by buying German silver) which led her in 1873 to limit silver coinage, abandon bimetallism and turn to gold as well. On this view Germany's move onto the gold standard was more an 'historical accident' than a result of conscious policy.

Once on the gold standard, Germany's experience with it, understandably, was connected with the operations of its central bank, the Reichsbank. The cliometric literature on this question, however, is ambivalent. Though gold standard rules should have limited central bank autonomy, the older literature found that the Reichsbank consistently violated 'the rules'.[55] More recent contributions stress a difference between credible commitment to the basic principle of convertibility and means to that end, recognising that pre-1914 Reichsbank policy never really threatened the mark's convertibility into gold. Differences of opinion concern (a) identification of the aims of Reichsbank policy and (b) interpreting its results. McGouldrick (1984), using weekly Reichsbank data, argues that the Reichsbank smoothed business cycles and offset gold flows through its discount rate. On this view, small violations of the rules did not endanger convertibility and even stabilised monetary growth. Sommariva and Tullio (1986), in contrast, think the Reichsbank followed the rules, properly understood as the Monetary Theory of the Balance of Payments. They strongly suggest that satisfaction of domestic demands dominated policy, often at the price of inducing losses of gold reserves and liquidity.[56] Eschweiler, finally, seems to agree with this last assessment of Reichsbank policy, preferring, however, to argue by means of a 'reaction function' which better specifies policy aims.[57] This

[54] Flandreau (1996, 2000); Eichengreen and Flandreau (1996, pp. 113–43). See also Milward (1996).

[55] Bloomfield (1959). Bloomfield, like many who followed his lead, made the 'rules' virtually synonymous with Hume's price-specie flow mechanism. Thus, a decline in gold reserves accompanied by a rise in central bank earning assets qualified as such a 'violation'. See, for example, Seeger (1968).

[56] On this, see McGouldrick (1984), Sommariva andTullio (1986); also Craig and Fisher (1997, esp. note 9, p. 290).

[57] Eschweiler (1993). Eschweiler is more critical of the accommodating nature of Reichsbank policy than Sommariva and Tullio, stressing its conflict with the need to supply liquidity.

view, interestingly, harmonises with those historians who have stressed the Reichsbank's willingness to serve as lender of last resort, even at the risk of a greater dependence upon outside capital flows.[58]

From the long-run point of view of modern German monetary history, the First World War was *the* major discontinuity, mainly because of the 'Great Inflation' which followed from it. About 20 years ago that inflation formed the core of a large-scale research project, accompanied by the appearance of a modern classic on the subject by Carl-Ludwig Holtfrerich.[59] Holtfrerich's study is largely 'straight' economic history, but it contains a number of intriguing cliometric fruits as well. One is the cogently argued thesis depicting the inflation as the result of a policy choice favouring high employment, external disequilibrium and punishment of creditors as opposed to restoration of external equilibrium, high unemployment and punishment of debtors (especially the state). A second and related fruit is the empirical demonstration that (a) Germany was able to mobilise short-term speculative capital to finance public and private expenditures with the help of a newly emergent foreign exchange market (in which foreign investors were betting on an improvement in the German Mark); and (b) that subsequent losses suffered by these speculators from 1922 onwards more than offset such reparations payments as the German government had made up to that point. Other studies have picked up the story and developed it further, including a couple of econometric models incorporating inflationary expectations and stressing the decisive role of the Reich government's finances.[60] The new consensus would seem to be that (a) German government policy was the 'least bad' option, given domestic and international political conditions; and (b) the impossibility of anticipating hyperinflation before 1922 facilitated the distributional victory of debtors over creditors. Nevertheless, that 'victory' depended on domestic politics, including the pressures which led to currency reform and revaluation laws which followed in 1924 and 1925.[61] Moreover, there are still some unsolved puzzles, such as why commercial banks, which were both debtors and creditors, should have become major losers in the distribu-

58 See Borchardt (1976) and Tilly (1991). This may be the place to call, once again, for more work on pre-1914 short-term capital flows, though the data problems are daunting, possibly unsurmountable.

59 Holtfrerich (1980). Results of the 'inflation project' in Feldman *et al.* (1982).

60 See, for example, Webb (1986); also Jaksch (1982).

61 It may have had something to do with the speed at which different social groups adapted to inflation, as Pierenkemper (1998) has argued. An excellent quantitative study of profits in retail and wholesale trade during inflation, 1920–23, by Kiehling (1996), suggests the ongoing importance of political pressures for the distributional outcome.

tional struggle. That is, the consensus is by no means 'graven in stone', unchallenged.

I pass over the rest of the interwar period, which is the topic of a separate section, and turn to the post-1945 years. If we also skip over the currency reform of 1948, the central theme of Germany's postwar monetary history becomes the much-vaunted success of its central bank, the Bundesbank, as guarantor of price stability, a success which many observers attribute to its institutional independence. Two recent publications discuss this question. Helge Berger's book emphasises the short-run stabilisation policies of the 1950s.[62] It argues that in the European Payments Union (EPU) period (from 1950 to 1958) the West German central bank could and did act to stabilise the business cycle. Such a policy was possible thanks to the EPU institutional arrangements involving restraints on capital mobility, but which also had surplus countries extending automatic credits and which placed the burden of adjustment on the deficit countries. Interestingly, Berger estimates a 'reaction function' which is dominated by domestic economic indicators and which suggests that the central bank was motivated by the desire to stabilise the German business cycle and not constrained by balance of payments considerations. In the post-EPU phase, however, balance of payment surpluses undermined the domestic orientation, eventually forcing the Bundesbank, in 1960, to veer onto an expansive course of action. One of the most interesting fruits of Berger's study is its (non-econometric) demonstration that the West German central bank's independence evolved in the 1950s in large measure because West German governments (and their leading politicians) found it convenient to be able to assign the public odium of deflationary policies to an outside institution, to use the central bank, so to speak, as a scapegoat.

In contrast to Berger's book, Björn Alecke's study concentrates exclusively on West German monetary policy, considers both long-run and short-run determinants and effects, and covers the entire Bretton Woods Era (from 1948 to 1973).[63] A contrast also lies in Alecke's emphasis on the lack of central bank policy autonomy – a condition which he thinks followed from the relaxation of covertibility restraints as early as the mid-1950s. This is supported by his econometric model of long-run money market equilibrium, for his principal finding, which builds on cointegration analysis, is that money is endogenous, that is, not a significant long-run influence on prices, income or interest rates. Further support comes from the related error correction modelling of short-run changes, for here the interesting result is

[62] Berger (1997). Strictly speaking the book deals with fiscal *and* monetary policy, but since fiscal policy proved unsuitable for short-run stabilisation purposes, positive stabilisation effects, if there were any, will have derived from central bank action.

[63] Alecke (1999). Alecke does not analyse the political economy of central banking as Berger has done, but he does have some comments pertinent thereto (especially in ch. 3).

that, since 1957 at the latest, the key German interest rate depended on the US discount rate. This finding obviously downgrades the importance of central bank policy (and independence). Alecke shares Berger's (and almost everyone else's) assessment of the primacy of price stability as central bank policy target, but he does not believe that one can attribute West German price stability before 1968–69 to central bank behaviour. In fact, there is reason to believe that wage and price moderation exercised by the collective bargaining partners was more important.[64] In any case, adds Alecke, scepticism here follows from the difficulties of measuring causality; and, accordingly, he fails to produce an econometric analysis of the short-run effects of monetary policy.[65]

Banking history is a well developed subfield of German economic history, doubtlessly more prolific than monetary history; but it has a stronger qualitative bias, and cliometric studies have been few and far between.[66] Nevertheless, a few relevant contributions exist, and they focus on important historiographical issues. No question, for example, has attracted more historical attention than the role of the large, universal banks in the development of the German economy. In Anglo-American circles Alexander Gerschenkron's positive judgement on that role is well known.[67] In Germany, since the 1900s, many positive assessments have appeared, though often fused with emphasis on the large banks as a potentially dangerous concentration of economic power. In recent years, the application of modern concepts, for example the Capital Asset Pricing Model (CAPM) or the economics of information or the New Institutional Economics (NIE), to the problem has led to some restatements. The most persistent scholar in this reconstruction has been Caroline Fohlin, who has contributed a number of revisionist publications.[68] Her target is the historiography which claims that the German universal banks, by virtue of their combination of

[64] Alecke (1999, p. 100). Carl Holtfrerich's (non-cliometric) history of the West German central bank contains many references to that institution's legendary attachment to the goal of price stability, but he argues that this goal was no more than means to a 'mercantilist' end emphasising international competitive price advantage and export-led economic growth. The evidence for the effectiveness of this policy seems, in fact, to be quite weak. See Holtfrerich (1998, esp. pp. 348, 380–400 and the quote on p. 383). This volume is available in English translation (Oxford: Oxford University Press, 1999).

[65] Alecke (1999, p. 188), reports that his econometric modelling of short-term effects via error correction failed to overcome non-stationarity problems; but he quickly adds that almost all other studies of central bank policy effects implicitly suffer from the same problem.

[66] Banking history even has its own German journal, the *Bankhistorisches Archiv*. Much interesting work has appeared here, but a look at the issues published in the last twenty years turned up not one cliometric paper.

[67] The *locus classicus* is Gerschenkron (1962). See also the essays by Richard Sylla and Richard Tilly in Sylla and Toniolo (1991).

[68] Fohlin (1994, 1998, 1999a, 1999b).

short-term commercial with investment banking and their uniquely close ties to industrial and commercial enterprise, made a significantly positive contribution to German industrial development before 1914.[69] In its modern form this claim is based on informational and transactions costs: the close ties between banks and their customers, associated with a long-run current account connection, proxy voting of shares in client companies, and representation on the supervisory boards of those same companies, are seen as a means for overcoming informational asymmetry and for facilitating the finance of risky investment projects. Fohlin's strongest argument has been logit and panel regressions in which samples of non-financial joint-stock companies are divided into sub-samples defined by the presence and absence of bank representatives on their supervisory boards, the next step being examination of enterprise performance and particularly of investment behaviour. The basic finding is that bank influence, so measured, has no persistent, significant effect on enterprise performance and investment behaviour. That is a blow against what Fohlin calls 'the orthodox view'.

Criticism of the 'orthodox view' has built on other quantitative arguments as well. One such concerns the relatively limited size of the sector, 'big universal banks', for this makes it unlikely that their financial contribution could have been as decisive as often claimed. Edwards and Oligivie, in their survey of the topic, weight this argument quite heavily, and Fohlin has also adopted it.[70] It has an international variant, used by these and other authors, which stresses that German universal banks, taken as a whole, were not larger, more concentrated, or more involved in industrial finance than banks in other countries. Anglo-German comparison plays a key role in this respect, since British banks and capital markets have often served as negative examples showing German universal banks in a positive light. Revision here has the British banks even more concentrated and British capital markets more prolific in the intermediation of industrial finance than their German counterparts.[71] Nevertheless, the 'orthodox view' has not yet lost the battle on this front. My own attempt, some years ago, to use the CAPM to compare British and German capital market performance over the 1880–1914 period produced results strongly favouring the German institutions; and they have not yet been explicitly challenged.[72] In addition, both

[69] The historiography goes back to the pre-1914 period. Fohlin (1999b) offers a review. For an early formulation see Riesser (1910). This was translated for the US National Monetary Commission: *The German Great Banks and Their Concentration* (Washington DC: US Government Printing Office, 1911). For more recent statements see also, in addition to Gerschenkron (1962), Schumpeter (1939, 2 vols., esp. vol. I); Tilly (1986 and 1995).

[70] Edwards and Oligivie (1996); Fohlin (1999b, pp. 327–28).

[71] Fohlin (1999b), Edwards and Oligivie (1996).

[72] Tilly (1986). This drew on new issues data and argued that in Germany these reflected largely banker decision-making.

Charles Calomiris and I have found evidence on 'spreads' – the difference between the price shareholders paid to acquire new shares and the price the issuing companies received – which seem low for German securities in relation to those estimated for companies in the US and the UK in the pre-1914 period.[73] That points to the greater efficiency of German intermediaries.

Roughly the same kind of dialogue has marked discussion of twentieth-century German banking. Quantitative work has continued to focus on the information networks associated with bank representation on the supervisory boards of non-bank enterprises, suggesting their ongoing importance for the German economy.[74] On the whole, however, this applies less to the interwar period than to the postwar years. A number of studies have attempted to measure the extent of German bank influence on industrial and commercial enterprise since the 1950s – as indexed by equity holdings, proxy voting power and supervisory board membership – and assess its economic consequences.[75] I single out John Cable's contribution here because it draws explicitly on an informational framework of analysis emphasising banks' role as 'delegated monitors'.[76] His data base consists of (a) the annual reports and balance sheets of the largest West German industrial companies for the years 1968–70; and (b) materials on bank connections with these enterprises gathered by an official government commission. He then runs regressions using an indicator of enterprise profitability as dependent variable and bank linkages as well as indicators of other enterprise characteristics (for example, branch performance, size, market shares) as independent variables. The result is confirmation of 'the internal capital market' thesis, that is, Cable finds a positive connection between the profitability of non-bank enterprises and the presence of bank influence upon them. Cable's article carried the day for a while, but in 1994 Edwards and Fischer published a book-length study which included a critique of the Cable results.[77] They point out that only one of the three models used by Cable confirms the 'internal capital market' hypothesis with respect to shareholder voting rights and that another variable reflecting concentration among all shareholders registered a more persistent influence in the models tested. They conclude that the results offer 'only very limited support of the

[73] Calomiris (1995); Tilly (1996). Fohlin (1999b) has criticised this work on grounds of its limited sample size. Nevertheless, the argument stands; and the matter deserves more internationally comparative work.

[74] Wixforth and Ziegler (1994); Ziegler (1997). Also Pappi *et al.* (1987). This work, though insightful, does not explicitly take up the question of the banks' contribution to the economy's efficiency.

[75] See, for example, Böhm (1992); also the literature cited in Tilly (1993).

[76] Cable (1985) includes some comments suggesting the inferiority of British institutional arrangements for corporate governance.

[77] Edwards and Fischer (1994, esp. ch. 9).

view that banks use their control of equity voting rights to perform a corporate control function and to raise profitability by limiting management's ability to pursue objectives which are not in the shareholders' interests'.[78]

The Edwards and Fischer book is cited here also as a general contribution to revision of 'the orthodox view' of German banks, even though it is not, strictly speaking, cliometrics. Their investigation of the sources of finance of enterprise investment, however, produced two quantitative findings which bear on the Anglo-German comparison discussed above and which deserve mention here. First, the size of the corporate, non-financial sector of the economy, as measured by the weight of corporations (or joint-stock companies) in estimated turnover, was much larger in the United Kingdom (UK) than in Germany (in 1986, for example, 79 to about 24 for the latter). This contrast is substantiated when one compares only companies listed on the stock exchanges – arguably a better measure of the impact of capital market institutions on the supply of external finance – for the difference comes to a ratio of 35.2 to 10.6.[79] Second, comparison of the importance of bank loans as a source of enterprise finance shows, for the aggregate of all non-financial enterprises, little difference: in both countries internal funds dominated, and loans from banks and insurance companies were, if anything, relatively more important in the UK. Edwards and Fischer thus observe that 'there is no evidence to support the widely-held view that bank loans are a more important source of finance in Germany than in the UK'. Therefore, 'there is no more reason to categorise Germany as having a bank-based system of investment finance than there is to categorise the UK in this way'.[80]

On the political economy of the Weimar Republic

For over two decades, a debate about the Weimar Republic's political economy has stood high on the agenda of German economic historians. In a narrow sense it concerns economic policy. Oversimplifying somewhat, I see it as pitting a quite deterministic view stressing political constraints against a view emphasising policy choice and alternatives. Cliometric work has not been at the centre of attention, but it has played an important role, as we shall see. The 'deterministic' view has spawned a scenario which runs from defeat in World War I and the German Revolution of 1918–19 through the 'Great Inflation' of 1918–23, the limping recovery of the 1924–29 years into the economic crisis of the 1930s, and the collapse and Nazi takeover of 1933.

78 Edwards and Fischer (1994, p. 226).

79 For 1986. Edwards and Fischer (1994, ch. 4, esp. pp. 74, 84, 86–7).

80 Edwards and Fischer (1994), ch. 3 (quote, p. 68). Edwards and Fischer point out the approximate nature of their comparative estimates – which suffer from national institutional differences, for example, in accounting for company pension funds, and so on. This comparison covers the 1970s and 1980s.

At its core is a distributional conflict between 'Capital' and 'Labour' involving control over state policy. Once the war was lost, only a limited set of options 'made sense' to the relevant actors. Inflation was a rational response to unresolved distributional conflicts at home and reparation demands from abroad. To paraphrase Borchardt's words, it is not wrong to say that at this time a revolutionary movement turned into a wage-price spiral, which made distributional conflict the dominant factor in the German polity and kept the German economy vulnerable to 'exogenous' shocks.[81] The economic crisis of 1929–33 called forth an economic policy of deflation, once again, given the historical situation, a 'rational' response, though one which favoured the rise of National Socialism.[82]

Two issues are at stake: the nature of the distributional conflict of the 1920s; and the availability of alternatives to Brüning's deflationary policies in the early 1930s. Cliometrics came on stage in the guise of Borchardt's suggestion, which opened the debate in 1979, that since 1918 politically driven wages had outstripped (aggregate) productivity, a claim supported by reference to a modern concept, the 'cumulative real wage position'.[83] According to this view, the implied redistribution of income lay behind the high unemployment and low investment of Weimar; and only the shocks of the early 1930s restored 'labour market equilibrium'. Carl Holtfrerich soon questioned the suitability of Borchardt's wage and productivity measures and, after supplying alternative ones, concluded that productivity had risen faster than wages.[84] Albrecht Ritschl followed up with a critique of Holtfrerich's results based on a review of alternative measures of aggregate productivity.[85] He concluded that of four possibilities, Holtfrerich had utilised the only one – the value-added approach based on Hoffmann's data (net domestic product) – which showed a greater rise than wages; and the one he chose, moreover, raised more problems of weighting and biases than the others. Spoerer's 1997 contribution supported Ritschl.[86] Some econometric models of the labour market have been presented since the debate began, but they have not settled the controversy, which still turns, as clio-

[81] From the *locus classicus* of the debate: Borchardt (1979, reprinted in 1982, p. 180).

[82] It is necessary to add at this point that Borchardt's aim was not to depict Brüning's policies in a positive light but to question the availability of alternatives, for example, Keynesianism.

[83] This shows the extent to which, in relation to some base year, wages have diverged from productivity, and thus altered the distribution of income between labour and capital (or 'labour's share'). See Dumke (1986, pp. 484–6).

[84] Holtfrerich (1984). Here, as in most of the wage-productivity estimates, the benchmark year is 1913.

[85] This effort was apparently related to Ritschl's work on the German national accounts in the interwar period. See Ritschl (1990). It is not clear to me why the bias cited by Ritschl should plague only the Hoffmann series preferred by Holtfrerich.

[86] Spoerer (1997).

metric debates often do, on the underlying data and estimating procedures.[87]

It would be misleading, however, to leave the issue at that. For a more fundamental dissonance is at stake here, namely the nature of the distributional conflict. Hitherto our discussion has followed the original point of departure and focused on Capital and Labour. In Carl Holtfrerich's world, however, 'capital' consists of both enterprises employing capital and labour *and* of financial capital. He (and others) have identified evidence showing that World War I and the Great Inflation produced a substantial redistribution of income against owners and purveyors of financial capital. This extinction of financial wealth lay behind the weaknesses of German financial markets in the 1920s, characterised as they were by supply scarcity and high interest rates.[88] The situation was extreme enough to support the hypothesis that capital shortage constituted a major structural weakness of the German economy of these years. Could it be, one is tempted to ask, that interest rates, rather than wages, were 'too high'? The question itself provokes two observations. Answering this question, most economic historians now seem to agree, calls for recourse to a macroeconomic model of the German economy.[89] Second, it leads directly to consideration of the second part of the debate on the Weimar economy, the question of alternatives to the deflationary policy pursued by Brüning's government in the critical 1930–32 period.

Borchardt's original strictures aimed at the widely-held belief that Keynesian policies could have had a significant positive effect on the 1929–33 crisis. The most powerful of his arguments was that not even the modest proto-Keynesian experiments discussed at this time had anywhere near sufficient political support among the Weimar Republic's political parties and interest groups to tempt the government to try them.[90] A second argument, which received more weight as the debate progressed, was the financial weakness of the government, which forbade large-scale deficit spending. This related to a third argument, namely that international agreements associated with the reparations question, restricted the central bank's ability to finance government deficits via money creation. This, in turn, related to yet a fourth argument, also discussed earlier by Borchardt, that Germany's heavy borrowing from abroad in the 1920s made it virtually

[87] See, for example, the contributions to the Borchardt Festschrift: Broadberry and Ritschl (1994); and Tilly and Huck (1994).

[88] See, for example, Holtfrerich (1980, 1985); and Balderston (1993).

[89] See, for example, Voth (1995). Voth's cliometrics, however, are weakened by the fact that they build on just six (annual) observations (parlayed by the Chow-Lin method and monthly production data into a nominally larger sample) and are limited to consideration of investment alone.

[90] Borchardt (1982, p. 173). This was repeated about ten years later in Borchardt's resumé of the debate. See Borchardt (1990, esp. p. 122).

impossible for her to consider devaluation or free floating of the Reichsmark, such as to free the central bank of its gold standard obligations and permit monetary expansion.[91]

In the meantime, some cliometric work has been done which bears on all of these arguments. Some of it involves presentation of new (or forgotten) data on specific aspects, for example Harold James' estimates of the money supply, or Holtfrerich's demonstration that the Weimar economy realised a substantial programme of public investment from 1925 onwards which, however, broke off radically after 1929, thus producing negative, pro-cyclical effects.[92] Such addenda and corrections, however, fall short of Borchardt's 1990 call for well-specified counterfactual arguments.[93] Of these there are few, and they point in different directions. Thus Norbert Huck and I, using a conventional Keynesian framework and a partial equilibrium (or sector-by-sector) approach, found monetary contraction and fiscal austerity in the early 1930s to have speeded recession and braked recovery and the plausible (counterfactual) alternative – estimated at between 3 and 4 per cent of net national product – to have offered more than negligible relief.[94] Borchardt and Ritschl, in contrast, used a complete, if small, Keynesian model of the German economy to generate simulations based on differing policy scenarios.[95] Their results are interesting because the scenarios combine 'proto-Keynesian' fiscal measures (the maintenance of public spending at the 1929 level) with Brüning's wage-price policies, showing, not surprisingly, substantial effects. It is not clear, however, whether the assumption of positive net public borrowing used here is a plausible counterfactual. Ritschl offered a new point of departure some years later by interpreting Germany's economic crisis as a foreign debt crisis: a period of generous foreign borrowing becomes unsustainable, the turning point related to the advent of the Young Plan and an end to 'transfer protection' to foreign holders of German debt. His key counterfactual is a Germany in the 1920s which exercised borrowing restraint – thus suffering no credit crunch and debt crisis after 1929.[96] The result is to see Brüning's austerity measures as the logical response to an economy

91 Borchardt saw 'fear of inflation' as an additional reason behind official German reluctance to contemplate devaluation and domestic monetary expansion. Borchardt (1982, p. 17); and Borchardt (1990, pp. 106–112).

92 James (1984), Holtfrerich (1992). Holtferich's target here, to be sure, was an element of Borchardt's 'crisis before the crisis'.

93 Borchardt (1990, pp. 103–5).

94 Tilly and Huck (1994, esp. pp. 80–83, 90–92). We believed that holding the line on 'high-powered money' from July 1931 onwards was not a political impossibility and would have sufficed to have the effects mentioned.

95 Borchardt and Ritschl (1992).

96 Ritschl (1998). This thesis was anticipated a few years earlier in an article by Broadberry and Ritschl (1994, pp. 38–41).

suffering a debt crisis – the price it has to pay for its previous binge of borrowing.[97] This supports Borchardt's view.

A final point concerns Borchardt's point about the absence of political support for Keynesian-like contra-cyclical policies. If one sees voters'preferences as an indicator of such support, however, the electoral results of the 1930–33 years force one to conclude that the opposite of contra-cyclical expansion – Brüning's deflationary policies – enjoyed very little of it. Years ago Frey and Weck took up this question and analysed it within the framework of a pooled regression model relating political parties' voter shares to social and economic indicators (applied to the Reichstag elections, 1930–33). Now Christian Stögbauer, in a contribution to this issue, has taken the analysis a step further.[98] On the basis of a widened data base and panel regression, he presents a strong case for the decisive role of unemployment as the factor turning voters against the Weimar government and toward the Nazi party. If one believes, as some of the counterfactual exercises have suggested, that contra-cyclical expansion would have raised income levels above, and unemployment levels below, their historically observed levels in 1931–33, then, by Stögbauer's results, both NSDAP and KPD would have received fewer votes. Could the difference have been a critical one? That is a question worth pursuing further.

4. Conclusions

The family of German economic history, as this survey has tried to show, has a young, but vigorous cliometric member. It has made important contributions to the fields of interest covered here, but in others as well. Thus one of the articles selected for this issue – by Bauernfeind, Reutter and Woitek – exploits a very long time series on Nuremberg grain prices (1490–1855) to test (and reject) the hypothesis that medieval and early modern grain storage patterns reflected 'rational investor behaviour', as has been claimed for other parts of Europe.[99] This reference to their work can serve as a reminder that the writ of econometric history, so to speak, is not limited to the modern era surveyed here, and that the old argument, that modern econometric methods presuppose the predominance of modern market relationships, is wrong.

97 With refreshing immodesty, Ritschl credits himself with thus reconciling Borchardt's arguments with those of his Keynesian critics. See Ritschl (1998, pp. 69–70).

98 See Frey and Weck (1981); Stögbauer (this issue, pp. 251–79). The improvement he offers involves a much finer disaggregation (830 township districts) rather than the 13 districts used by Frey and Weck); and it develops an income indicator, whereas Frey and Weck worked with unemployment alone, a resort which forced them to interpret the latter both as individual fate and 'system signal'.

99 By McCloskey and Nash (1984) for England. See also Komlos and Landes (1991).

What further conclusions may be drawn from the work surveyed here? I venture two observations.

First, one deficit in the literature, already mentioned, deserves emphasis: the paucity of attempts to analyse economic institutions in a manner facilitating quantification. There is a consensus, already noted, that institutions are important and that the 'New Institutional Economics' represents a promising road to their treatment; but up to now there have been few positive examples. Tim Guinnane's work is a notable exception. The closest thing to a meaningful attempt in this issue is the article by Klug, for in his methodology, easily measurable electoral majorities are interpretable as a key force behind a shift between free-trade and protectionist regimes.

Second, on the other hand, there is much to praise. As in other countries, the German literature also offers some examples of cliometrics generating new perspectives on old questions, for example, through the use of new econometric techniques which facilitate the exploitation of long-known but little-used sources, thus reopening research doors which seemed to have fallen shut. I see the Brown and Neumeier paper as a case in point. As in other countries also, cliometrics has had its greatest impact where it has questioned widely-held hypotheses by exposing defective or incomplete quantification or unwarranted inferences from quantitative data. One example is Fohlin's critique of 'the orthodox view' of German banking history, another Thomas Bittner's dissection of the debate on the 'Wirtschaftswunder'. And finally, the German literature also contains some examples sustaining the general cliometric belief that 'numbers matter', for instance, that the estimated losses on speculative foreign exchange transactions in the Berlin market between 1920 and 1923 exceeded estimated German reparation payments or estimates of the counterfactual effects of an expansive monetary policy in Germany, 1931–33. Of course, though the value of cliometrics for economic history does not depend exclusively on such results, it is worth keeping our eyes open for the possibilities that exist.

To sum up: it would be premature today to speak of a 'German cliometric revolution', but if this survey is on the mark, a basis for that kind of economic history does now exist.

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