

Lokale Sonderinteressen schoben sich nicht selten in den Vordergrund, überlagerten die Bemühungen um Zentralisierung und beeinträchtigten nicht nur die Ansätze zu verbandspolitischer Homogenisierung, sondern auch die Bündnisfähigkeit und die Schlagkraft der organisierten Kleinhandelslobby überhaupt. Nach der Jahrhundertwende avancierten die verschiedenen Detaillistenorganisationen zu einem festen Bestandteil der politischen, nach einem kurzen Zwischenspiel im liberalen Hansabund mehr und mehr in agrarisch-konservatives Fahrwasser einschwenkenden »Mittelstandsbewegung«, die 1913 im »Kartell der schaffenden Stände« endgültig den langersehnten Anschluß an die übrigen »produktiven«, »staatstragenden« Erwerbsklassen in Industrie und Landwirtschaft fand.

Trotz vieler aufschlußreicher Einzelinformationen, die Gellately aus verstreuten archivalischen und publizistischen Quellen zusammengetragen hat, sind Versäumnisse zu beklagen, die nicht in jedem Fall durch das Fehlen von Vorarbeiten zu entschuldigen sind, zumal eine mehr als reichhaltige zeitgenössische Literatur zur Verfügung stand. Obwohl einleitend eine Untersuchung angekündigt wird, die ihren Schwerpunkt auf das gesellschaftspolitische Selbstverständnis der Kleinkaufleute und deren Perzeption einer sich in rasantem Tempo industrialisierenden Umwelt zu legen verspricht, bescheiden sich die nachfolgenden Abschnitte über weite Strecken auf eine Form der Verbandsgeschichte, die traditionelle Bahnen nicht überschreitet und zudem in etlichen Punkten nicht sonderlich originelle Ergebnisse zutage fördert: Das, was Gellately über die »politische Mittelstandsbewegung« zu bieten hat, ist seit Stegmans Forschungen zur Sammlungspolitik hinreichend geläufig. Es wäre vermutlich anregender gewesen, hätte sich der Verfasser noch stärker auf lokale Entwicklungslinien konzentriert und sich nicht nur mit den – in mancher Hinsicht doch recht untypischen – Hamburger Verhältnissen zufrieden gegeben. Dadurch wäre zwar das in der Tat schwer zu bewältigende Problem der Repräsentativität (S. XII) nicht aus der Welt geschafft, die Darstellung hätte aber insgesamt konkretere sozialgeschichtliche Konturen erhalten und an Dichte wie Differenzierung gewinnen können. Als Fazit bleibt daher zu konstatieren, daß Gellately in eine vielversprechende Richtung gewiesen hat, auch wenn er deren Möglichkeiten nicht immer voll auszuschöpfen vermochte. Jens Flemming

Charles S. Maier, *Recasting Bourgeois Europe. Stabilization in France, Germany and Italy in the Decade after World War I*, Princeton University Press, Princeton, N. J., 1975, XIV, 650 S., Ln., \$ 34.50.

Charles S. Maier's massive work is a history of all matters of high policy in Germany, Italy and France relating to the post-war settlement in western Europe after 1918. It is a resolute attempt to show how the major issues in foreign as well as domestic policy were not merely affected by fundamental divisions of opinion over the nature of society but also in their turn caused such divisions. The section in Chapter 3 on »The Coal Crisis« deserves to be singled out as an example of how the thorough and detailed mastery of a complicated and not vitally important set of issues in domestic economic policy may provide fresh insights into a much more important issue of international policy; it is the best account in print of the role of coal shipments in reparations. Indeed, the central theme of the book is that the major issues of policy were inextricably entangled with the attempt to »stabilize« the three countries concerned on the basis of a different socio-economic system. Stabilization could only be achieved by a transition to a more »corporatist« form of society and government. The reimposition of a social order based on »bourgeois criteria« was eventually achieved, he argues, only by a significant transformation of political institutions and of the way in which political power was wielded. Power, in effect, had to pass from legislatures and bureaucrats to the great organised vested interests in the economy and issues to be settled either by direct bargaining between these interests or by the influence which they exerted on government.

Corporate influence was often most powerful when exerted negatively because government, Maier argues, had to avoid the costs of prolonged struggle against it. The resolution of policy questions by trying to attain as nearly as possible the general good resulted after 1918 in divisions of economic and social opinion too fierce for liberal democracies to resolve. The fall from office in 1921/22 of Briand, Wirth and Giolitti marked, the author suggests, the clear failure of the parliamentary order built on the aggregation of individual preferences to solve the problems of the post-war world. The stabilization of western Europe after the First World War was thus not a re-creation of the world before 1914 but the ratification in power of a form of polity peculiarly identified with the twentieth century and foreshadowing the more successful stabilization after 1948. In this interpretation the First World War looms large, it is a constant theme that the shared central direction of the economy by government and corporate interests during that struggle did not come to an end in 1918 but had a pervasive influence in shaping post-war society and government.

The book does not minimise the differences which existed in the three countries; much of its immense detail arises from the attempt to illustrate them. Whereas, argues Maier, corporate interests operated comfortably behind the screen of parliamentary institutions in France they publicly dominated the Weimar legislatures, and in Italy the whole apparatus of government had to be changed to accommodate them. It is at this point that the further implications of the argument lead into deeper waters where navigation becomes more difficult. If Italian fascism was the corporate state one degree more triumphant than the Weimar Republic, what was the National Socialist state? Maier implies that it was in essence a populist revolt against over-mighty corporate power. Such an argument would explain the fundamental differences of economic policy which sometimes appeared between the National Socialist régime in Germany and the fascist régime in Italy, but in doing so it would at the same time demolish the whole concept of ›fascism‹ as an ideology with certain common political and economic ideas setting it apart from other systems of government. It would not merely refute theorists such as Kühnl, Clemenz or Hennig but also, at the other extreme, less ardent exponents of ›Faschismustheorien‹ like Nolte because even the few similarities between the régimes which he insists on would hardly be historically significant if those régimes sprang from utterly opposed motives and groups. Maier's characterisation of the two régimes might well be correct and, certainly, the tendency of historical research is to confirm his view of National Socialism. But do we yet know enough about the economic history of these countries, and the impact of economic policy within them, to accept so sweeping a characterization? One part of the Italian fascist party, represented by Farinacci, was every bit as ›populist‹ as the most vehement Nazi inveighers against the corporate state and in its social and economic decrees the Republic of Salò returned to the elements of radical populism in the first fascist manifesto. Whatever the Italian fascist party did in power some of its intellectual and ideological origins were strikingly close to those of the NSDAP. And, of course, many corporate vested interests eventually found a satisfactory home within the Third Reich.

That much caution is needed in the use of the word ›corporatism‹ is a point the author willingly concedes; he is himself unhappy with the word. But the point at stake, I would suggest, is a more important one than the unsatisfactory nature of our vocabulary for an accurate description of historical processes. It is this. Is it possible properly to characterize the nature and policy of any régime, and, furthermore, is it possible to write adequate social history, before the basic economic history of the period has been written? In trying to do so the author is following the current American and German fashion of utilising as far as possible business archives to amplify what was previously discovered from government records. There is a natural tendency that, the more evidence is gathered from its records, the world of business will seem to have had a greater influence on government,

and the same is true for trade union records also. But that is not a reproach that can fairly be levelled at Maier's work, for it is a work based on what must have been a long and exhausting period of comprehensive archival research. The question is whether even the most comprehensive effort of archival research can by itself establish the sort of argument which the author is trying to make. Before Maier's characterization either of the similar socio-economic foundations of the governments of Italy, Germany and France after 1922 or of the sharp distinction in the socio-economic bases of the governments of fascist Italy and Nazi Germany can be entirely accepted far more knowledge is needed of the impact of economic policy and of the economic events. What were the relative movements of prices and wages? What were the changes in the pattern of income and wealth distribution? How were they affected by economic policy? What were the relative changes in the composition of the national product? Only by the answers to a long series of such related questions can the sort of characterization of government into the categories used here be properly and convincingly founded on historical evidence. Otherwise the attempt remains a matter of verbal impressionism no matter how huge the archival resources deployed. Can we really accept on the basis of documentary research alone that Poincaré and Stresemann before 1913 represented all the ›nonproletarian elements comprising the bourgeois nation‹, whereas after 1923 the nationalism which they represented enhanced ›only the power of business and industry‹ and thus fragmented ›the bourgeoisie‹ and rewarded ›organized capital‹ (p. 414). We may suspect that this was indeed so; but we do not know and it is possible to get nearer to the truth. The same point might be made about the effects of inflation on German society, even though in judging them the author achieves his happiest literary conceit.

The danger is, that not being tied down to systematically-ordered economic data social history takes off with spasms of verbal joy into soaring flights of social interpretation which are then used by others to rearrange the data to substantiate arguments over interpretations which need never have been formulated in the first place. It is only necessary to think how large a superstructure of interpretation of the social and political history of Germany is based on the concept of a ›Great Depression‹ after 1873, a concept which has no worthwhile foundations in economic history, to see how great the danger is. The historiographical tradition to which this book belongs has already had a most valuable result in bringing the history of high policy down into the arena of social evolution and conflict. But there are distinct limits to what can be achieved in this way. Important historical judgments will remain nebulous and unconvincing if social history is built downwards from the evidence of policy discussions and decisions rather than upwards from its macro- and micro-economic foundations.

These foundations are still partly lacking. As far as Germany is concerned the situation is slightly better than in the other countries although Hoffmann's tables of data are liberally speckled with blanks for the early 1920s. There are, however, excellent recent macro-economic syntheses by Petzina and Borchardt. For France, everything still depends on the valuable but idiosyncratic volumes of Sauvy and his collaborators. As for Italy, its economic history in the inter-war period remains virtually virgin territory. There are occasions when Maier descends into the economic field to settle detailed questions of fact and it is impossible not to feel that the book would have benefitted if he had done so more often because the sheer mass of documentary evidence tends often to bury the argument rather than illuminate it. Because there is so little recourse to the basic economic information the withdrawal of the United States from the scene as a major political actor means that it gets relatively little consideration even though as an economic actor its role was decisive. It was on the unstable pillars of the American economy and its even more unstable connections with the developed western European economies that the so-called ›stabilization‹ of bourgeois Europe was erected.

The paucity of economic history of the period must often have meant that the author was making bricks without straw and it would be grudging indeed not to compliment him on the industry and intelligence which he has used to illuminate his chosen period. I hope he will take these comments not as a criticism but as an exhortation. The business archives which he has used to illustrate attitudes and policy in the violently-shifting circumstances between 1918 and 1924 must also contain much economic information which could be used to help in the assessment of how wellfounded these attitudes and policies were. It is only a matter of shifting the focus of inquiry and a man with the determination to write such a big book would surely not be deterred by that. It would be by such enterprise that a more genuine social history might emerge, one more nearly approximating to a true history of society, and one which would replace more adequately the older, limited concept of 'political history'. And it is only when a social history of that kind has developed that the characterization of government and society attempted here, a characterization which is important to Maier's argument and important also to any history of the inter-war period, can be accepted. To your tasks, social historians!

A. S. Milward

Henning Grund, »Preußenschlag« und Staatsgerichtshof im Jahre 1932 (= Studien und Materialien zur Verfassungsgerichtsbarkeit, Bd. 5), Nomos Verlagsgesellschaft, Baden-Baden 1976, 167 S., kart., 36 DM.

Ein Desiderat, möchte man meinen: Eine fundierte Würdigung des Urteils des Staatsgerichtshofs vom 25. Oktober 1932 in der Streitsache Preußen kontra Reich wegen des Papenputschs vom 20. Juli 1932 fehlt bisher. Immerhin ging es, so Bracher, um den »größten Verfassungstreit der neueren deutschen Geschichte«¹, und dem Verfasser der angezeigten Göttinger juristischen Dissertation ist zuzustimmen, wenn er in der bisherigen Literatur, namentlich auch bei Bracher, überwiegend eine »allzu pauschale Verwerfung« des Urteils (S. 12) konstatiert. Auch das Inhaltsverzeichnis erweckt Erwartungen: Es verspricht nicht nur eine eingehende Diskussion der StGH-Entscheidung, die die Macht in Preußen salomonisch zwischen den Reichskommissaren und der legalen Regierung Braun aufteilte, sondern darüber hinaus eine Darstellung der Vorgeschichte des Putschs vom 20. Juli 1932 und der weiteren Entwicklung bis zum Urteil.

Das Vorhaben ist also gut, aber das ist auch das Beste, was über diese Arbeit mitzuteilen ist. Um mit dem historischen Teil zu beginnen: Wie ist es möglich, eine Darstellung der innenpolitischen Entwicklung im Reich seit dem Amtsantritt Brüning und darüber hinaus der Beziehungen Preußen-Reich in der gesamten Weimarer Zeit anzubieten, ohne auch nur einen Blick in die einschlägigen Archivalien getan zu haben? Dem Quellenverzeichnis sind ganze zwei Bestände zu entnehmen, die der Verfasser benutzt oder auch nicht benutzt hat; Zweifel sind angesichts der gravierend falschen Zitierweise angebracht. Um die Sammlung Brammer und die Handakten Arian einzusehen, hätte der Verfasser das Bundesarchiv aufsuchen müssen; wie ist es dann möglich, daß er nicht einmal einen Blick in die Akten der Reichskanzlei geworfen hat? Ohne die Kabinettsprotokolle, ohne die umfangreichen Sachakten zum Papenputsch und zur Entscheidung des Staatsgerichtshofs in diesem Bestand sind fundierte Aussagen zu diesem Thema ausgeschlossen. Auf alle Fälle hat sich der Verfasser den Weg in das Geheime Staatsarchiv in Berlin erspart; die Akten des Preußischen Staatsministeriums und des Finanzministeriums sind ebensowenig genutzt wie die einschlägigen Nachlässe in Koblenz, Bonn, Berlin und Amsterdam.

Da die historische Darstellung also ausschließlich auf der längst bekannten Primär- und Sekundärliteratur beruht, steht hier schlechterdings nichts Neues, dafür aber viel Falsches. Die Beurteilung des Falls Abegg zum Beispiel (S. 33 f.) – der Staatssekretär im Preußischen Innenministerium Wilhelm Abegg hatte Anfang Juni 1932 mit zwei kommunistischen

¹ Karl-Dietrich Bracher, Die Auflösung der Weimarer Republik, 5. Aufl., Villingen 1971, S. 557.