REVIEWS OF BOOKS

F.T. BLACKABY (ed.), *British Economic Policy 1960-74*, Cambridge, University Press, 1978, pp. 687+xviii.

In 1964 the National Institute of Economic and Social Research published J.C.R. Dow's *The Management of the British Economy 1945-60*; the volume under review is the National Institute's successor to that outstanding book. Like Dow, the authors have set out not only to describe the economic policy of the fifteen years 1960-74 but to tackle such questions as: " What was the government trying to do... and why did it select the methods it did? ... how far were its policies successful, and what were the reasons for the success or failure? ", concentrating particularly on major changes of policy (Introduction, p. 1). The framework of the book is, therefore, generally Tinbergen's targets and instruments approach (whose limitations are carefully spelled out in the editor's Introduction).

A major interest of this period to economic historians lies perhaps in the *experiments* attempted by the later two of the three governments — which were Conservative (under Macmillan and Home) up to 1964, Labour (Wilson) 1964-70, and Conservative (Heath) 1970-4. The period of the Labour government saw new choices of instruments, partly because of new, or rather additional objectives and partly because of new instruments, e.g. selective employment tax, investment grants, and the regional employment premium in the fiscal field, the Industrial Reorganisation Corporation and the Ministry of Technology in industrial policy. The second Conservative administration began with experiments that Prime Minister Thatcher is now trying again, and also introduced in 1971 the new monetary regime known in Britain as " Competition and Credit Control ", which at least initially brought the tech-

niques of British monetary policy more in line with those used in other Western countries. All these experiments are described very clearly and fully and the evidence relating to their effects surveyed. In some cases the authors also report unpublished empirical work they have carried out to fill gaps in the previously published tests of policy effects.

At the same time the book illustrates the continuity of British economic policy — realised if not intended. In the areas of public expenditure and taxation there have been persistent growth in public expenditure, absolutely and relative to the growth of output, and equally persistent difficulties in controlling public expenditure, a shift of taxation from direct towards indirect taxes, and an apparent reduction of the progressiveness of income tax. With respect to monetary and exchange rate policy, the " old spectre " of balance-of-payments difficulties was only temporarily exorcised by the 1967 devaluation (p. 304), imposing continuity of objectives though not of instruments. Other persistent preoccupations were industrial structure and industrial relations, and the attempt to become a member of the European Economic Community. With the exception of the last, the continuity lies in objectives rather than in instruments and in the lack of success of the various techniques adopted to deal with the problems. Despite the improvements in the conduct of conventional macroeconomic policy that were made, it is unfortunately difficult to disagree with the editor's final conclusion that " the problem of devising policies appropriate for a country with a relatively inefficient manufacturing sector and an unreformed pay bargaining system remained unsolved " (p. 655) under all three governments.

Turning to the structure and organisation of the volume, the discussion of fiscal policy appropriately comprises almost one quarter of the book, monetary policy one sixth of the whole; there are two chapters on each. R.W.R. Price's survey of public expenditure which begins with a discussion of the *management* of public expenditure, demonstrates with clarity the problems of control that any government faces; his chapter on budgetary policy well complements this with its careful analysis of the outcome of both discretionary and automatic changes in revenue. Measures of the weighted full-employment budget are also calculated and fiscal policy's contribution to stabilisation assessed. The monetary policy chapters are written by M.J. Artis and J.H.B. Tew (who also contributes a chapter on balance-of-payments policies): Tew neatly discusses the techniques of monetary control by dividing them into official regulations and official transactions, Artis takes on the difficult task of evaluating the effects of monetary policy. Despite the unexciting conclusion that " it seems unlikely that monetary policy did very much for good or bad in this period: it did not do very much at all, and was not supposed to " (p. 303), these chapters, and that on balance-of-payments policies, are, like Price's, consistently interesting and informative.

The five chapters already mentioned are preceded by a narrative chapter

by the editor and followed by separate chapters on incomes policy (Blackaby), planning (P. Meadows), industrial policy (P. Mottershead), policy towards the nationalised industries (K. Jones), commercial policy (A.D. Morgan), industrial relations and manpower policy (R. Elliott) and a general appraisal by the editor. By and large they share the outstanding characteristic of the earlier chapters, namely an awareness of and attention to the recent theoretical and empirical work produced in Britain and North America bearing on the issues raised by macroeconomic policies — though Blackaby's chapter on incomes policy is disappointing in this respect. The chapters on "structural" policies not only make the account of British economic policy more comprehensive but also serve to place the policy experiments (macro and micro) in context.

Overall, the volume is indeed a worthy successor to Dow, even though it necessarily lacks the finely-meshed coherence of single authorship. It will, like its predecessor, remain an invaluable work of reference and analysis of an important period in Britain's economic history; it deserves to be widely read outside Britain, either in the complete version or in the paperback edition of the chapters on demand management.

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D.F. CREW, *Town in the Ruhr. A Social History of Bochum, 1860-1914.* New York, Columbia University Press, 1979, pp. xiii, 289.

Crew's study of Bochum brings the paradigm of American social historians to the study of German society. Despite the enormous impact American scholarship has had on historians in Germany, in the main it is politics that forms the core of the bulk of German language studies. On the other hand, a significant body of English language literature from the last two decades has raised other questions about social change, in particular under the broad category of "social mobility." Strangely enough, it is the Americans who are saying that industrial society is not a species unto itself, while it is the Germans (e.g., Hans-Ulrich Wehler, Ralf Dahrendorf) who have spoken of an aberration in the normal course of industrial capitalism. These national lines are not purely drawn, but they help to put this historical work in its social context.

The outline of the book is straightforward. Crew first summarizes the social and economic background of Bochum in the nineteenth century. From being a declining small town in the 1840s, Bochum became a major center for the mining and metallurgical industries of the Ruhr Valley industrial complex. Crew utilizes the published state occupational censuses of 1882, 1895 and 1907, but he has also found some rarer archival data for 1871, data with allow him to develop a longer run picture than is common for this period. The broad outlines of Ruhr economic developments are well known, but the author fleshes out this economic skeleton with information on the economic

status of workers — e.g., household costs, age of entry into the work force workers' income over the life cycle.

"A social history of Bochum" means primarily a history of the workers in Bochum. When Crew discusses geographic mobility, he means the origins and destinations of workers. The information here is fascinating and suggestive, but it is extremely difficult to present definitive conclusions. Single and married workers acted differently; younger and older workers acted differently; newcomers had problems different from those of more established residents. We still do not know how such different experiences were related to other modes of social and political action.

Social mobility is another favorite topic of social historians, and Crew has drawn on this literature to evaluate information fished from the depths of the Bochum city archives: manual and nonmanual occupations, lateral movement within the skilled trades, intergenerational mobility, education as a route upwards, homeownership and savings accounts. In the end Crew refers to cultural norms that suggest that the mobility of the American dream is not necessarily applicable to Bochum, but he does not expand on this very difficult question of the needs, aspirations, and desires of the local workers.

These explorations into mobility form the basis for exploring intergroup conflict in Bochum among workers, *Mittelstand*, and industrialists. The *Mittelstand*, a vaguely defined lower middle class of artisans and shopkeepers, has received a lot of attention from students of the post-World War I era. Crew goes back into the nineteenth century to discuss the uneasy and ambivalent alliance between these self-employed and the big industrialists. Evidence is difficult to find; the picture of the *Mittelstand* itself and the relationship between it and workers and industrialists remains fuzzy.

Finally, the author provides two significant chapters on the foundations of worker protest and on the great miners' strikes of 1889, 1905, and 1912. By analyzing the demands of strikers, he hopes to show their priorities in work, be they wages, forced overtime, piecework or intensity of labor. In these sections Crew impressively and judiciously analyzes some common sociological explanations for worker unrest. He also talks of support for trade unions and Social Democrats, but these items remain secondary to an attempt to understand what was bothering workers at the workplace. In this regard Crew summarizes the prevailing sociological and historical approaches to labor unrest and strike activity, and he measures these models against the evidence he has unearthed about Bochum.

The major strengths of the book lie in Crew's careful reading of his Bochum evidence while paying constant attention to other studies of urban developments. This book is not "urban history" in that it is not about city life as a variable in the historical equation; the book deals with specific groups of people, mainly industrial workers, living in an urban environment. In that context, two major problems engage the author's attention, namely mobility

measured in many ways and labor unrest. The interests of historians of the United States come out in the mobility chapters; those of historians of Germany in the labor chapters. Between the two lies a gap that Crew does not completely bridge, even though he tries valiantly. To connect social mobility with geographic mobility requires a number of assumptions about human behavior, and to move beyond these into group motivations for labor unrest is even more difficult.

Moreover, as in many studies of regions of heavy industry, miners and metal workers present different societies, different behavior patterns, and different historical evidence. Miners have traditionally been more distinctive, and it is sometimes easy to let metal workers slide from view when speaking of mass social action.

This is an important monograph. Crew does not hesitate to tackle difficult questions on the theoretical and the abstract level. From now on even historians who have not the slightest interest in Bochum will need to reckon with this book.

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A.R. DISNEY, *Twilight of the Pepper Empire: Portuguese Trade in Southwest India in the Early Seventeenth Century.* (Cambridge Mass.: Harvard University Press, 1978, pp. x+220, 17 tables, 2 maps).

The bulk of the literature dealing with the Portuguese in Asia concentrates on the sixteenth century before the English and the Dutch had challenged their monopoly of the sea-route via the Cape of Good Hope. Dr. Disney's study, concerned primarily with the first four decades of the seventeenth century when the Portuguese trading organization in the East experienced a great deal of strain, is a welcome addition to this literature. The study does more than what is claimed in the title: it is concerned not only with southwest India but indeed with Portuguese concerns throughout Asia in the early seventeenth century.

The crisis that overtook the *Estado da India* during this period was mainly financial — falling revenues in the face of rising expenditure. Throughout the sixteenth century, the customs revenue from intra-Asian trade had been reasonably substantial. Dr. Disney shows that between 1600 and 1634, the income at Goa from this source declined by as much as 40%. He argues that the real decline was even greater because of the inflation that had taken place in the meantime. The only branch of trade that continued to be remunerative was that between Macao and Nagasaki. But even that ceased to be the case after Japan closed its doors to most foreigners, including the Portuguese, in 1639. The income from customs duties charged from Asian vessels which were obliged to call at Goa was also on the decline. At the same time, expenditure was on the increase due partly to the necessity of countering the English and

the Dutch. It is another matter that that did not prevent the Dutch from demolishing the eastern wing of the Portuguese empire. The pepper trade between India and Lisbon was also posing problems. From the 1620's onwards the cost price in Malabar and Kanara had been rising while the sale price in Lisbon had been on the decline.

It was in this context that the Portuguese India Company was founded in 1628 in imitation of the English and the Dutch. But the resources made available to the Company were quite small and the expectation that private capital would flow in never materialized. The result was that the volume of trade carried on by the Company continued to be quite small. Dr. Disney suggests that the export performance by the Company compared most unfavourably with that achieved up to the first decade of the seventeenth century. The problems encountered by the Company did not stop at an inadequate availability of finances. There was also the curious fact of as much as one third of the tonnage, on average, being lost on the return voyages. The resultant loss was more than what the Company could be expected to bear and its liquidation became unavoidable.

Pepper was the most important item procured by the *Estado da India* as well as the Company. Dr. Disney has analysed the mechanics of the trade in this item in great detail. The price the Portuguese paid for pepper at Kanara and Malabar was probably somewhat less than what would have obtained in a free market situation but Dr. Disney rightly suggests that the main motivation behind the agreements entered into with the various ruling authorities was to facilitate a safe passage of the pepper through the territories controlled by these authorities. The pepper was paid for mainly in the silver received from home which was the principal item of import into India. Dr. Disney shows that a profit of as much as 50 to 70% was earned on this silver. These rates are truly remarkable because later in the century the Dutch and the English had to be satisfied with whatever little profit they could earn on the bullion imported by them.

Dr. Disney's study is based solidly on archival sources and is generally well-argued. But on some of the more general issues raised such as the possible inflation in southwest India in the early years of the seventeenth century, it would have been helpful if some more evidence had been introduced.

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G.D. FELDMAN, *Iron and Steel in the German Inflation, 1916-1923*. Princeton: Princeton University Press, 1977.

Historians and economists have not made much progress in advancing understanding of the complex effects that inflation can have on society. For

example, in examining the impact of inflation on the distribution of wealth and power in XXth century industrial societies, much scholarly work continues to rely on simplistic assumptions: e.g., that inflation benefits mainly the masses, or the opposite of this — mainly the rich. Surprisingly, little attention has been given to the indeterminacy of many socioeconomic processes, on the one hand, and to the importance of individual actors in influencing outcomes, on the other.

Building in several respects on his *Army, Industry, and Labor in Germany, 1914-1918* (Princeton, 1966), Feldman has produced an extraordinarily careful and probing study of industrial politics and public power during the period of Germany's wartime and postwar inflation. Intended by the author to be a "preliminary study" for a broader social history of the German inflation, it not only stands by itself; it should be read by everyone trying to understand the political and a socioeconomic history of Weimar Germany.

The volume contains an illuminating essay on the scholarly literature dealing with the German inflation that began during World War I and climaxed in the hyperinflation of 1923. Both the main themes and the gaps in this literature are carefully surveyed. Attention is called particularly to the relative absence of research by historians both on inflation *per se* as an historical problem, and specifically on the German inflation. "There is no adequate history of the German inflation, a situation that ceases to be suprising when one considers that, until recently, historians of Germany have tended to concentrate on political and intellectual history and have neglected socioeconomic development" (p. 4). He also notes that the statistical material needed to study the German inflation is not entirely satisfactory, and that economists have used the available material both to argue that postwar inflation enabled Germany to reap significant benefits — high employment, export advantages — and to argue that the postwar economic boom produced significant economic "distortions" by encouraging investment that later appeared to be wasteful. Feldman focuses attention on the political and economic gains, largely by heavy industry, and the losses, largely by workers, particularly their loss of the eight-hour day in 1923, accruing from the inflation. One of the central analytic concerns of the book is to explain why the iron and steel industry — no longer the technological leader or the most profitable part of German industry — was able to negotiate the difficult period of inflation better than other industrial sectors and to achieve greater influence on public policy than might be expected from its economic position alone.

A part of Feldman's answer is that the iron and steel industry had extraordinary leaders: owner-entrepreneurs of great energy such as Fritz Thyssen (1871-1951), Peter Klöckner (1863-1940), Gustav Krupp von Bohlen und Halbach (1870-1943), and Hugo Stinnes (1870-1924), or general directors with unusual talent, organizational abilities, and keen business sense such as Albert Vögler (1877-1945) of Stinnes' *Deutsch-Luxemburgische Bergwerks- und Hütten*

A.G. and Paul Reusch (1868-1956) of the *Gutehoffnungshütte* (GHH) controlled by the Haniel family. Yet the leaders of the iron and steel industry did not have a unified strategy during the inflation. Industrial self-government — the utilization of cartels, syindicates, and trade associations to coordinate production, manipulate prices, and divide markets — seemed highly desirable to most German industrialists. However, even before World War I there were constant strains on this system of cooperation, and new strains were introduced by political and economic events during and following the war. One major source of strain was the fact that the heavy industrial concerns had significantly different capacities to adapt to changing circumstances and to seize new opportunities for strengthening their market position and weakening their competitors'. Another source of strain was the split between the few large producers of crude and semifinished iron and steel products (ingots, slabs, billets, sheet bars, merchant bars, rolled wire, steel plate, pipes) and the thousands of manufacturers who bought these items to produce the broad range of finished industrial goods poured out by German industry.

Moreover, what unity there was among the large iron and steel producers was constantly threatened by their differing circumstances and interests. They were drawn together by their dislike of massive government supervision during the war and their fear of even greater direct intervention after it, for example in the vague but ominous form of *Gemeinwirtschaft* as envisaged by Wichard von Moellendorff in the confusion immediately after the armistice of November 1918. After the signing of the Versailles Treaty in June 1919, however, a free-for-all scramble for German and world market shares emerged. The situation for German heavy industry was in a variety of ways unpropitious, since it had lost much of its productive plant in the territories taken from the Reich, was carrying a heavy burden in reparation payments, and faced seriously sagging labor productivity in its remaining installations. On the other hand, the revival and expansion of heavy industry from its shrunken base was fueled both by continued depreciation of the German currency and thus low export prices, and by large government payments to industry to compensate for the properties lost as a result of the peace treaty. The intense competition among iron and steel men for markets and new plants often seemed to be mitigated only by the desire to beat back the efforts of the Reich Economics Ministry and other government offices to force a more conciliatory supply and pricing policy on the producers vis-a-vis domestic customers, e.g., the German railroads and the thousands of metal fabricating and finishing firms.

Feldman stresses the essential rationality of the intense concern with self-preservation and rapid adaptability among the iron and steel producers during the turbulent years of the inflation. The development of wartime and postwar inflation was not, he notes, " a smooth and continuous one, and it was anything but simple for state officials and industrialists to adjust to its imperatives. Institutions and policies designed to meet specific situations were condemned

to rapid obsolescence, and lessons learned from one set of circumstances had rapidly to be unlearned or modified to meet suddenly changed conditions" (p. 160). It is often said that German industrialists ruthlessly exploited the opportunities presented by the inflation to build vertically integrated empires, but in an impressive chapter devoted to "vertical concentration" Feldman examines a number of the pitfalls involved in the concentration movement. The chapter gives particular attention to the creation of two major *Interessengemeinshaften* during the inflation. One, formed by the alliance of the *Maschinenfabrik-Augsburg-Nürnberg* (MAN) with the GHH, was carefully constructed and proved permanent. The second, the huge *Siemens-Rhein-Elbe-Schuckert Union* (SRSU) formed by the alliance of Hugo Stinnes' Deutsch-Luxemberg concern, the *Gelsenkirchener Bergwerke* A.G., Siemens & Halske, and Siemens-Schuckert, was never really workable. The SRSU was dissolved in early 1926, approximately two years after the end of hyperinflation and the stabilization of the economy.

In assessing the effects of the push toward the greater concentration of German industry during the inflation years, Feldman notes that the main short-term concerns of German iron and steel men were inadequate raw material supplies and unreliable transport, not rationalization. The firms that integrated vertically often acquired a larger capital base and an expanded marketing network. However, much of the expansion that took place was redundant. It often involved the quick reinvestment of the funds received from the Reich in coal mines or simply willy-nilly in other production facilities. Two major iron and steel firms, Stumm and Rombach, effectively collapsed in 1925. But Feldman indicates that they "were not victims of excessive vertical concentration but rather of an irrational, helter-skelter reconstruction of their holdings following their wartime losses and of mistaken financial policies" (p. 456). Klöckner skilfully reconstructed its holdings and survived, as did the vertically organized GHH. After stabilization in 1924–25, the trend was not to vertical but to horizontal concentration, e.g., the United Steelworks (*Vereinigte Stahlwerke*) was created in April 1926. Yet the more viable patterns of vertical concentration, which began well before World War I, continue in German heavy industry today.

The economic gains of heavy industry during the inflation were of course made possible and reflected in the national politics of the Weimar Republic. Feldman stresses that heavy industry was committed to an authoritarian reconstruction of the constitution, and that its influence on national policy was disproportionately large in relation to its position in the economy. The harmful role played by heavy industry in the political and socioeconomic history of the Weimar Republic "would not have been possible had it not been for the advantages it derived from the war and inflation and the unwillingness and inability of the revolutionary and postrevolutionary regimes to challenge the pretended omniscience of the heavy industrialists, to take advantage of

the conflicts within German industry as a whole, and to shift the balance in favor of more progressive elements" (p. 468). With regard to the lessons for the present, Feldman points out that other producers of scarce raw materials, e.g., oil, have found themselves in a position in which they could maximize their advantages at the public's expense and shield their financial position from public scrutiny. Yet he suggests that the most important lesson concerns the economic role of the state in industrially advanced, democratic societies. The history of the German iron and steel industry during the years 1916-1923 indicates the immense problems — not limited to that time or place — that are involved in developing modes of regulation that do not persistently grant the claims of the interests being regulated. The efforts of the Weimar Republic to deal with the iron and steel industry also indicate how important it is for the state to maintain its authority vis-à-vis powerful interest groups without itself becoming authoritarian.

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B.A. HANAWALT, *Crime and Conflict in English Communities, 1300-1348*. Cambridge: Harvard University Press, 1979.

This is an ambitious but disappointing book. It is ambitious in that it attempts to portray the crime patterns of eight scattered counties in fourteenth century England on the basis of gaol delivery rolls, the records of prisoners brought before the circuit judges as they traveled around England. There is no particular reason for choosing the first half of the fourteenth century other than that the gaol delivery rolls were available and the Black Death, which struck in 1349 had not yet muddied the waters. The author has attempted to bridge criminology and history by immersing herself in the relevant criminological literature, and the organization of the book clearly reflects it She opens with a discussion of the social and judicial context of the eight counties, she defines the crimes she is going to focus upon — felonies — and charts their incidence. She describes the suspects — their sex, their age, and social position — and she speculates on the relationships between the suspect and the victim. Finally, she examines gangs and professional criminals, and estimates the impact of political conflict and economic and demographic change on crime.

Through it all, however, she leaves much out, commits a variety of analytic errors, and in general presents us with an unimaginative treatment of the issues she explores. In discussing the fluctuations of "crime", she considers the impact of political turmoil and famine. A crime-by-crime analysis would have been better, but she reports that political turmoil had little effect on crime and that wheat prices showed strong correlations with crime levels (0.67-0.95). These statistics are somewhat misleading, however, in that it is usually the most numerous crimes (larceny and burglary) that dictate the nature of the rela-

tionship. In terms of her finding it would seem that economic conditions as measured by wheat prices were the primary factor in crime in medieval England. This finding, of course, is not particularly startling; nor is the finding that medieval England was a violent society. Professor Hanawalt indicates that homicide was the third most common crime prosecuted (after larceny and burglary), and she estimates that the homicide rate in pre-plague London was about three times the ratio in Miami in 1948-52.

Her analysis of the problems of juvenile delinquency and professional crime, however, is particularly dismaying. She recognizes that juveniles as a social category probably did not exist in the middle ages. But in her analysis she proceeds as if the only distinctive quality of a juvenile is his age. "If medieval teenagers behaved like modern ones, then their large numbers would be partly responsible for the great number of simple larcenies and burglaries (p. 127)". But medieval teenagers particularly those with wives and families probably behaved more like modern day adultes than adolescents. The social category — juvenile — was all but unknown, and accordingly so was juvenile delinquency in any meaningful sense in medieval England. A similar misunderstanding plagues her treatment of professional criminals. Professional criminals are professional not simply by virtue of their expertise but also because of their acceptance of an ethic governing their specialty. I would hardly expect to find many such criminals in rural England even today, and predictably Professor Hanawalt finds very few. But if she were to explore the same question in medieval London, her conclusion might have been different.

Finally, she found that minor crimes like larceny and serious crimes involving family members were substantially less common than is the case today. Larceny, for example, outnumbers homicide today in most cities by a factor of thousands, and about half of all homicides involve family members. In medieval England, however, larcenies were only twice as numerous as homicides, and only about 2 per cent of all homicides involved members of the same family. Professor Hanawalt speculates that homicides are disproportionately common because of under-reporting of larcenies and that families were either too cohesive or too tenuous to provoke serious violence.

It could be, however, that different definitions of larceny coupled with different methods of dealing with the crime in the fourteenth century were responsible for the low rate. Some consideration of the peculiarities of fourteenth century courts and policing would seem to be in order here. It could also be that family violence was rare not because it didn't occur but because families dealt with violence in non-legal ways. Custom and not law may have been decisive in these cases.

Professor Hanawalt has assembled an impressive body of data on medieval criminality, but she has not used it in insightful ways to throw light on crucial issues, and for the most part her conclusions follow highly predictable

paths. Her data are too narrow to permit definitive statements on many of the issues she has raised, but her criminological imagination is insufficiently developed to permit interesting speculations. A dilemma that many young, ambitious scholars face these days without a happy solution.

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D. HERLIHY - C. KLAPISCH-ZUBER, *Les Toscans et leurs familles. Une étude du catasto florentin de 1427.* Paris: Editions de l'École des Hautes Études en Sciences Sociales, 1978, pp. 703.

This is a very important and exciting book. Its authors have utilized computer technology to examine the Florentine *catasto* of 1427, a unique series of late medieval assessment and tax registers. More significantly, they have posed numerous intelligent questions and sought to answer them by supplementing data from the *catasto* with information from a mass of secondary studies and from some other primary source materials. The result has been to supplement the static picture furnished by the *catasto* and to present studies of numerous topics in social and economic history that span the period from approximately 1300 to 1550.

The first part of the book examines the documents that comprise the *catasto* and to probe their nature, possible utilization, and shortcomings. Those documents include thousands of *portate*, individual declarations filed by each head of household with the *catasto* officials. (The *portate* then were abridged and copied into large volumes of *campioni*.) The returns included the composition of each household, with the names, ages, and occasionally occupations of its members, and a detailed listing of its taxable wealth, both movable and immovable. The tax then was determined by subtracting from that wealth certain allowable deductions and expenses, and taxing the balance. In all the *catasto* comprises almost 60,000 households and more than 264,000 people.

A review cannot do justice to this book and the many themes that it comprehends within its twenty chapters, with their 39 graphs, 85 tables, and five appendices. Any selection must be arbitrary and reflect the reviewer's own interests; and this one is no exception.

Numerous discoveries, both expected and unexpected, become the subject of stimulating short studies, and the work is studded with fruitful observations. On p. 71 the authors examine the advantages and disadvantages created by the exemption from taxation of homes and their furnishings, as those could include palaces and magnificent art works as well as humble artisan dwellings. The exemption of splendid dwellings and their contents removed capital from trade and industry (hence from future taxation), but it also stimulated artistic production. Pp. 268-283 contain one of the best available discussions of Tuscan *mezzadria* or sharecropping. (See also pp. 261-263, for relations between

mezzadri and urban proprietors.) In fact, the peculiar role of *mezzadria* and its effect on many facets of economic and social history is a theme interspersed throughout the book.

In Chapter IX, "The Distribution of Wealth," the reader is referred particularly to Table 29 (p. 243). This includes an analysis by persons and households of the percentages and values of investments held by those in Florence, in six major towns (Pisa, Pistoia, Arezzo, Prato, Volterra, and Cortona), in fifteen large villages, and in the countryside, in landed properties, movables, and investments in the public debt. In 1427 Florentines comprised only 14% of the lay population, but they owned two-thirds of the wealth. They possessed 86% of the movable capital in Tuscany and they held almost the entire public debt. The authors discuss problems created by the tendency of capital to remain with the urban patriciate and not to circulate, and those created by the shortage of capital in rural areas (pp. 259 ff.). Here too the *mezzadri* come in for special examination, and sharecropping is blamed for keeping much of the rural population in a state of poverty.

We could focus on any chapter and find it worthwhile. Chapter VI, on population movements, 1300-1550 (pp. 165-188) contains an up to date survey of scholarly literature on the subject. The authors have settled upon an urban population of about 120,000 for pre-plague Florence, about 1340 (p. 176). Table 17 (p. 186) offers a convenient survey of population changes within the Florentine state, 1427-1552. Herlihy and Klapisch-Zuber conclude that while a moderate Florentine population increase during that period permitted the city to strengthen its regional hegemony, it was insufficient to allow Florence to match its newer northern rivals in economic strength (p. 188).

Discussions of longevity in Chapter VII (pp. 189-215) perforce depend heavily on literary evidence, and especially the family *ricordi*, although the authors are well aware of the limitations of those documents. Herlihy and Klapisch-Zuber focus primarily on the upper classes, especially in the city, and the certainty of their findings is weakened by the nature of the literary evidence. The authors discuss the relations between the approximately constant number of households and increasing population, and they investigate the limitations imposed on population increase. These last include the impact of the spread of *mezzadria* in the countryside, as impoverished sharecroppers could not easily afford marriage and families. The lack of economic opportunities did not permit young men to leave their paternal homes, while rising dowry costs drove girls into convents as the religious dowry was markedly lower than that required for marriage.

Marriage itself is the subject of Chapter XIV (pp. 393-419). Florentine society was demographically unstable at the time of the *catasto*, "relationships between the different age groups, between generations, and perhaps those between the sexes, did not cease to vary from the outbreak of the plagues until at least the end of the fifteenth century. That signifies that the pro-

portionate number of marriages, deaths, and births did not remain constant" (p. 393). In the city Florentine women first married between 18 and 20, to husbands about twelve years their senior. Fewer than half of the urban Florentine males were unmarried, while two-thirds were married in the contado. The authors relate the differing percentages of marriages and marriage ages to the differing economic functions of the family and the obligations entailed in creating a family in the city and in the countryside, and among the wealthy and the poor in each area — with *mezzadri* again being an exception. They relate the propensities of urban young men for fights and factions, as well as the incidence of sodomy and prostitution, to the excess energies of unmarried young males. The text includes an analysis of dowries (pp. 414-417), a subject that has attracted much attention in recent scholarly literature. The authors conclude that "The important age difference between the number of young men and girls available" for marriage mean that men (especially upper class urban males) might hope to rise socially by marriage, while girls might expect to decline in social status (p. 417).

Chapter XIII (pp. 350-390) concentrates upon young and old people, and includes analyses of the numbers and percentages of people in each age cohort. There is a valuable discussion of the problems caused by the tendency of those reporting to the *catasto* officials to round off ages, and the authors try to ascertain patterns in that rounding off. They note that "the declaration of [one's] age seems to have been a real innovation," especially in the countryside (p. 360). Some reasons for reporting age as younger or older than the truth relate to the ages during which one was liable for taxation or military service. The authors conclude, however, that even allowing for an artificial exaggeration of the number of old people, at the beginning of the fifteenth century the Tuscan population was an old one in a demographic sense. The proportion of the elderly was highest in 1427 and then diminished slowly (p. 374). Towards the end of the fourteenth and the beginning of the fifteenth century the Tuscan population was unstable and was moving towards a new equilibrium after the shocks of the fourteenth century.

Chapter XV (pp. 420-442) treats births, a subject upon which the *catasto* is less informative than many others. The authors estimate that the period of the greatest fecundity for urban Florentine women was at about 20-34 years of age, while men particularly became fathers between 30 and 50. Pp. 439-442 contain a discussion of the means used to limit births.

"Death" follows in Chapter XVI (pp. 443-468), a subject for which the authors especially utilize the "Books of the Dead," records especially maintained by government officials charged with provisioning the city of Florence. Even in the fifteenth century the violence of the plague is manifest, and it was the number one cause of death for both women and man, 1424-1430.

There follows a complex and valuable study of the household in Chapter XVII, "*Le Feu*" (pp. 469-522). The average household in 1427 contained

4.42 people (with 59,770 households and a 264,210 population); but 52.5% of the population lived in households of six or more, with 10.8% in those of 11 to 25 people. Household size varied according to the occupation, social rank, and residence of its head. In Florence members of the wealthiest professions had the largest households (p. 476); while the 'typical' head of a household was a married man of about 50. Examining the composition of the patrimonies of urban heads of families of different ages the authors find that between the ages of 25 and 55 wealthier citizens especially increased their movable wealth, though converting a part of it into immovables. As they approached or reached 55 they gradually transmitted a part of their fortunes to their children, putting their sons into business and dowering their daughters (p. 494). Among both peasants and townsmen young men married at between about 25 and 30, and might expect to live some years (together with their wives) under the paternal roof — and authority (p. 499). For a variety of reasons this 'patrilocality' did not apply to masses of the urban poor, to landless peasants, or to rural artisans (pp. 511-512).

Part V, "Images of the Family" (pp. 525-613) containing the final three chapters draws very heavily upon literary evidence and often is both stimulating and controversial. Chapter XVIII includes a discussion of the origins of male lineages, of *consorterie*, of the use of *ricordi*, and of the use of surnames in Tuscany. Even at the time of the *catasto* only a small minority had surnames with the highest proportion being in Florence itself, and most particularly among the hundred wealthiest households. In Chapter XIX, "Childhood and Youth" (pp. 552-584) the authors tend on the whole to oppose Philippe Ariès' controversial thesis that denies that people in the middle ages viewed childhood as a separate phase of life (pp. 568-571). Chapter XX (pp. 585-613) examines "Maturity and Old Age." It contains interesting discussions of marriage customs, and the authors make reasonable psychological assumptions as to the relations between spouses. The discussion of family life, focusing as it does especially on the urban middle and upper classes, may give a different picture of family relations and attachments, particularly between children and parents, than might a study that focused on the lower classes, both urban and rural, that is, on the majority of the Tuscan population. Perhaps, though a sophisticated study of that type might not even be possible, as most of the extant fourteenth and fifteenth century literature was intended for those middle and upper classes — even if we set aside for the moment the problems inherent in utilizing that purely literary evidence.

It now should be apparent that *The Tuscans and their Families* is an important book, and one that is to be read. It should quickly be translated into English so that scholars in a variety of disciplines may profit from its approaches and findings.

WILLIAM M. BOWSKY
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I.S. KOROPECKYJ, ed., *The Ukraine within the USSR, An Economic Balance Sheet*, New York, Praeger, 1977.

In September 1975 a group of scholars gathered at Harvard for a symposium on the Ukrainian economy. The proceedings have been published in this volume under the editorship of Ivan Koropeckyj, with a foreword by Abram Bergson and a short overview by Holland Hunter. The result is work of solid scholarship and substance for which students of the Soviet economy will long be in debt. The papers are rich in factual material and in developing ideas and conclusions on the Ukrainian economy in general and on its relations with the Soviet overstate.

Four papers treat explicitly or principally the Union-Ukraine interrelationship. These are the papers by the editor, Bandera, Melnyk, and Wiles. Koropeckyj concludes that the Ukraine's political and economic rights in practice are extremely restricted although in principle Ukraine has what appear to be generous constitutional and administrative freedoms. Bandera's paper examines and estimates the net resource outflows through such devices as the turnover tax, a theme recurring in Melnyk's paper and also in Wiles's. All of these conclude that there has been a substantial net outflow. Cohn's paper, although concerned principally with Ukrainian macroeconomic growth rather than exploitation, bears on the latter issue as well. While the Ukraine has been a net capital exporter, he argues, this capital is destined to help develop the new and/or backward regions and presumably will one day be recompensed. (In this, Cohn seems to have in mind something more tangible than the general and vague gains from federalism such as are frequently invoked in Canada and other federal states.)

Shroeder's conclusions lend further support to the exploitation hypothesis, although this must be tempered by incompleteness of the available data. Thus, she concludes that per family earned income has been 10% below the national average. This contrasts with what might have been reasonable expectations based on comparative economic standings on the eve of industrialization. The republic does show up better in terms of per capita income; the Ukraine with smaller family size, ranked fifth in 1970. However, all the estimates seems to exclude private plot income, which is probably higher than average in Ukraine, owing to climate and large labour reserves. (Note that, while differential grain prices would work to keep incomes lower in relation to harvest than in regions with lower land yields, price differentiation would not be a factor in measuring the income received from, or the personal utility of consumption of private plot output.)

Zum Brunnen's paper shows a serious pollution situation in many of the republic's rivers and assigns an important role in this to upstream extra-republic sources, although as he is quick to point out, major Soviet rivers with industrial tributary regions are polluted almost everywhere.

The other papers will also be of value. Gillula's able analysis and extension

of the Ukrainian 1966 Input-Output table can be helpful in analyzing some of the issues here discussed, Dienes's is a helpful survey of Ukrainian resources and outlook. Finally, the Whitehouse-Bronson contribution is a survey and comment on population and labour characteristics. The authors make the point that increased rural education could be an important means for increasing labour productivity in agriculture, a view that this writer has also stressed, arguing that this rather than incentive structure on the farms is the main problem in agriculture.

The book is indexed and well produced.

ALAN ABOUCHAR
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C.H. LEE, *The Quantitative Approach to Economic History*. New York: St. Martin's Press, 1977, pp. 117.

M.M. Postan likes to quip that according to some historians the "truth lies between two errors." Lee's concise book introduces the reader to the intellectual current that has transformed such an apparent anomaly into one of its tools of the trade. The practicioners of the "new" economic history, known as cliometricians, emphasize the use of formal models based more often on microeconomic than on macroeconomic theory, and test the validity of these models by applying econometric techniques to the historical data. Conceptually, the cliometrician isolates the economy from its socio-political environment and assumes implicitly that cultural, institutional, and religious factors are unimportant arguments in the society's utility function. At most he concedes that they are random influences which cancel out in analyzing most economic problems.

For some, the propagation of this view has taken on the attributes of a crusade. This creates an atmosphere of intellectual challenge and incessant confrontation with exhilarating ideas. On the negative side, however, some drawbacks of a belief in a "one chosen faith" do manifest themselves. For instance, the cliometrician at times assumes the existence of long-run competitive equilibrium or the absence of uncertainty in decision making with about as much scepticism as a Marxist might question the ubiquity of class struggle.

What is a model? Based on the complex reality of individual action or motivation the researcher posits some behavioral postulates that seem to be logical and which a-priori are not contradicted by experience. A downward sloping demand curve for a good or for a factor of production might be a beginning of such a model. Add an upward sloping supply curve with the market clearing condition that demand equals supply and one has the basis of a full fledged model from which inferences might be drawn. One of Lee's examples is precisely such a simple model used by Donald McCloskey to draw interesting conclusions about England's old poor law. A model is accepted

as long as it is internally consistent, no known fact contradicts it, and its predictions appear valid.

Often the model used by the cliometrician takes the form $Y = a+bx+\varepsilon$, where ε is a random error term. The coefficients are estimated by minimizing the deviations of the sum of squared residuals from the regression line. If b is found to be significantly different from zero and several other criteria are fulfilled the hypothesis that Y is a linear function of x is accepted. One ought to keep in mind that the identification of a successful model does not imply that a true structural relationship was discovered. Rather, it makes more sense to use the model as a short-cut algorithm to organize human thought about a set of related events that would otherwise remain obscure if not incomprehensible. Some researchers, however, are carried away by their findings and pontificate as though the model they have tested had a one to one correspondence with reality. Not quite. As Milton Freedman argues in his essay on " The Methodology of Positive Economics ", " Factual evidence can never ' prove ' a hypothesis; it can only fail to disprove it."

The world renowned econometrician Henry Theil suggested the same when he wrote that it " ... require(s) maturity to realize that models are to be used but not to be believed." Lee might have pointed to examples of researchers who have disregarded this admonition. The Neuburger-Stokes study cited by him is one that in the opinion of this reviewer emphasizes excessively the technicalities of the model using the " kitchen-sink approach ". (Trying various specifications until one is found to work.) They tested a model to ascertain the effect of the allocation of current account credits granted by the German big banks on Germany's economic growth between 1880 and W.W.I. They reported that the amount of current account credit had a negative effect on non-agricultural output in Germany with a one year lag without discussing why one would expect the impact of these loans to have been negligible in the contemporaneous period, without discussing why the shift-parameter should have been exponential and without presenting any evidence that heavy industry actually received longterm credits at short-term rates. In fact Eistert's data show that the end-of-year balances on these line-of-credit accounts were miniscule, hence the loans do not appear to have been of long-term duration. Neuburger and Stokes conveniently accepted the testimony of others instead of presenting evidence to the effect that the rates on current account credit were in fact lower than the free market rate of interest. In any event, the alleged lower rates need not have necessarily led to economic inefficiency. If the lower rates extended to heavy industry were due to lower information costs as they, in fact, suggested, then the banks were merely passing on their savings to these customers rather than discriminating. If the banks were equating their marginal cotss of doing business with the rate of interest then resources were not misallocated. After all, the acquisition of information on the unfamiliar enterprises would have consumed real resources. In sum, therefore, the model became the focus

of the researchers' attention to the exclusion of important other considerations. They became fettered to their own mental construct. Lee's disregard of these kind of problems within the profession renders his work less critical than it might have been.

Yet Lee capsulizes well the methodology of the discipline and discusses many representative achievements for the benefit of the uninitiated. That the problem of slavery was disregarded because of space limitations must, however, be lamented. Within a hundred pages a more successful introduction to modeling, regression analysis, and counterfactual history could hardly have been attained. However, the reader is forewarned that were he to become a disciple much more economic and regression theory would need to be assimilated before usefully applying the method to a problem of his choosing.

Regardless of how the new economic history will develop in the future or what manner of ideology will supersede it, the majority of European economic historians have dated their work by failing to consider seriously the tenets of this approach to understanding the past. This reviewer concurs with Lee's impression that they generally formed their judgements with insufficient information at their disposal probably because of the relatively high initiation fee. Lee's book seeks to lower the costs of these investments. His efforts will no doubt bear fruit and win converts to this school of thought.

JOHN KOMLOS
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SH. MARRINER (ed.), *Business and Businessmen: Studies in Business, Economic and Accounting History*. (Liverpool, Liverpool University Press, 1978, pp. xiv+300, £ 11.50).

The quality and diversity of the late Professor Francis Edwin Hyde's scholarship are reflected in this *Festschrift*, which ranges from maritime and business history to subjects as diverse as Dr. Johnson and the teaching of business management. Delightful as this makes it to the reader thumbing through (I recommend it as a varied diet for a long journey), it poses serious problems for the reviewer, who can give only a brief foretaste of each contribution.

Professor G.C. Allen provides an opening dose of cerebral applied economics, comparing institutional factors behind the contrasting growth experience of Britain and Japan. His challenging approach will already be familiar to readers of, for example, his other interpretative works written in retirement such as *The British Disease*. More conventionally historical, B.L. Anderson examines the investment portfolio of the Union Marine Insurance Company of Liverpool in the decades before the First World War: a much needed study of this sector of the capital market. His inferences on strategy from snapshot views of the portfolio are fascinating but tantalisingly incomplete, and his

discussion of its relation to modern portfolio theory would perhaps have benefitted from directly confronting W.P. Kennedy's broader work in this same area. Professor T.C. Barker examines some interesting links between Lord Salisbury's political life and his time as chairman of the insolvent Great Eastern Railway. Railways are also the subject of Professor F. Crouzet's examination of the engineering firm of Fives-Lille between 1861 and 1914. Professor J.R. Harris pursues a theme he began in a contribution to B. Ratcliffe, ed., *Britain and the World, Essays in Honour of W.O. Henderson* (Manchester University Press 1975), with an interesting analysis of the lags in adoption of steel technology in France. Interesting as it is, it does raise the question, in rather acute form, of whether "continued in the next Festschrift" is really the optimal publishing regime from the reader's point of view.

P.N. Davies, in comparing the careers of shipowners Sir Alfred Jones and Lord Kylsant, produces some interesting comparative analysis; though his brave subsidiary aim of analysing the usefulness of this for business history teaching does not quite come off. The shipping trade is also assessed by Professor R. Davis in an historiographical survey, which is extremely useful for those of us who have not managed to keep up with the voluminous maritime history literature of recent years. Finally, an accountant, Professor T.A. Lee, looks at the information which can be gleaned from the published accounts of the Distillers Company between 1830 and 1950.

Dr. Marriner has produced a fitting tribute to Professor Hyde, one of the founders of the Liverpool journal *Business History*, whom she succeeded as editor in 1975.

LESLIE HANNAH
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A. NOVE, *The Soviet Economic System*, London, Allen and Unwin, 1977.

I am often asked — what amazed me the most in the West? Not very much, for educated Soviet people know much more about the West, than let us say, educated Americans know about the Soviet Union. But what really amazed me were the huge streams of literature about the USSR in general and about the Soviet economy in particular. I then went to the opposite extreme; I decided that Sovietologists know everything and understand everything. Fortunately I wasn't right. If everything about the Soviet Union was already known, then we Sovietologists would be without work. On the whole, however, it is indisputable that the best of Sovietologists are very good specialists and proof of this is Professor Alec Nove's latest book.

It happens that a man knows his subject, invented (discovered) much, but is not a good writer; (otherwise how can one explain that not all specialists publish books.) This is not the case in the given instance and Nove more than successfully managed his task; the book is well organized and written.

I am trying to avoid the common standards for a review. Simply to retell the book is boring. One can point out mistakes, but they are scarce here. One can disagree with the author, but it is not for me to compete with Nove in expressing his ideas. Finally, one can use the book as an occasion to speak about something of one's own, but I will try to avoid this and within the limits of the place allotted to me, will say why precisely, in my opinion, this book has turned out so well; what are its merits.

First, it is an extraordinarily " dense " book; there is much material in it. It contains a detailed description of the organization of the economy, including its organizational structures, its branches and Soviet statistics. It explains how " macro " and " micro " decisions are applied and where they lead, reviews conceptual differences of Soviet economic theory and much else, including so-called alternatives to the Soviet system; (by the way, the latter is perhaps the best section of the book.)

I am not speaking simply of the broad scope of the problems, but of the fact that the book contains many carefully selected details, those seemingly petty things which in total explain the given economic system. An immense difficulty in any general description of Soviet society is that there are there many distinctions not only in large, but in small daily things and if the writer overlooks them, the reader unconsciously fills in the " missing links " with mistaken associations from his own experience in Western life. I mentioned above that the general level of Western literature about the Soviet economy is quite high. This is so but with one reservation. The level is high because there are relatively few mistakes, because the analysis leads to interesting and often correct conclusions. Yet in many conversations I recognized with amazement that the reader received a distorted idea. Why? Because the authors overlooked these " links " which mainly related to daily life experience; they wrote their books rather schematically. To the readers of this journal — to historians, none of this has to be explained in detail, but certain economists don't understand this.

It is understandable that Nove's attention to such details produced such a large volume. For this reason, in contrast to many textbooks on the Soviet economy, almost nothing is said about Russia's long history.

It is appropriate to mention that one would not feel the beating pulse of Soviet life in this book, if Nove did not know Russian like a Russian; if he had not read, in addition to economic literature, Soviet periodicals and Soviet belles-lettres; if he had not been so strikingly inquisitive. (He has asked Soviet emigrants more questions than anyone else.)

The second unquestionable quality of Nove's book is his position when evaluating various phenomena. I am not speaking of political — ideological ideals; I do not know what Nove believes, but rather I am speaking about something else. Soviet life is so ugly that the more one knows it, the more one is imbued with aversion towards it and it is understandable therefore that

it is hard to find among Sovietologists those who are pro-Soviet. At the same time it is no longer fashionable " to curse Soviet power ". Our audiences demand so-called objectivity, a balanced position from us. In principle no hone is going to oppose objectivity, the question is only what really is objective, or, to be more precise, what degree of subjectivity in evaluations can be sufficiently objective.

Nove's position here seems to me to be the best. It is not only that he speaks about both bad and good, as all now do. What is principally correct is that in many cases he demonstrates the real complexity of various economic problems, shows that such problems are tackled in ineffective ways not only within the bounds of the Soviet system, but in the West, and that often this is not a matter of the ill-intentions of the Soviet government or even its inability negligence, but exactly of these objective difficulties.

Why is this author's position so important? There are those who think that the well-known and increasing failures of the Soviet economy are the result of the fact that there is " no real " or " correct " socialism in the USSR. Others propose that certain reforms could significantly improve the economic situation. And there are those who believe that the only way out for the country is the quick restoration of capitalism. All are correct and incorrect. It is understood, that the " Soviet variant " of socialism is quite far from the ideals of many, although even the best of these ideals are for me, for example, far from ideal. It goes without saying that by means of certain reforms Soviet rulers could somewhat improve the economic situation, although in my opinion there is within the confines of the Soviet political regime no possibility of serious improvement. As for the options of restoring capitalism, a so-called new NEP would be without doubt beneficial, but on the one hand it is absolutely unrealistic to speak of the real restoration of capitalism in the country, and, on the other hand, capitalist economies themselves have huge problems and failures.

Here I have expressed my own views; Nove acts more wisely. He shows different pluses and minuses and allows the reader to formulate his own position. Professor Gregory Grossman told me that he begins his course on comparative economies with a declaration: " There are bad, very bad and impossibly bad economies. The task of the economist is to show what system is impossibly bad, and which is only bad." This is not only well said; it is true (alas!) in essence also. It is to Nove's merit that in his analysis of the Soviet economy he consistently followed this line.

I would like to mention also that it is the detailed analysis of all that related to the pluses and minuses of the Soviet economic system (in particular in " Assessments ") which was the most interesting part of the book for me.

Third point. One can regard Soviet economic theory in various ways, but the fact is that the entire system is based on it and without a clear under-

standing of this theory it is difficult to figure out the logic of the system. More than that it would be at the very least careless to reject all Soviet theoretical ideas as incorrect from the start.

Nove not only shows at significantly greater length the connection between Soviet economic practice and Soviet economic theory, but even speaks out against certain common Western ideas. In particular, I am in full agreement with his position on what exactly must be considered as social product (national income). The well-known works of A. Bergson, A. Bekker and also economists of the CIA showed how one should estimate the social product of the USSR within Western concepts and I value these works highly. But as Nove perfectly correctly observes, these Western concepts are not without question; here, Soviet theory has its reasons.

The question discussed here has even broader significance. The methodological achievements of various sciences in the West, whether it be economics, history, sociology and the like, undoubtedly should be used when studying the USSR, but to limit oneself to only these would be incorrect. There is a tendency to take from Soviet literature only facts, relating in a haughty manner to any theoretical-methodological attempts of Soviet scholars. In my opinion (I can only speak here about economics) we thus impoverish the analysis; our conclusions risk becoming really non-objective.

And now to my final point, and here I think that many will not agree with me. For the last couple of decades in the West and the USSR economic science has become strongly mathematical and, as Nove notes on p. 310, among the Western "mainstream" theorists "institutional economics" is regarded as unworthy of attention. I by no means intend to speak against the application of mathematics and computers to economic science. While still in the USSR I wrote several books about linear programming and carried out computations on the computer. But all the same, it seems to me that we crossed that necessary border: formulae and explanations about them began to replace ideas.

Four years ago Paul Samuelson asked who, in my opinion, is now the Soviet Keynes. I answered with a joke, but if he were to ask me now the same question, I would, in turn, ask him — but who is the contemporary Western Keynes? With all my deep respect for the outstanding minds of the Pleiade of contemporary Western economists, I must unfortunately state, that with all the abundance of models and theories, with the numerous calculations on superfast computers, new ideas somehow are not visible. Economists have not yet offered radical solutions to the crisis situation which is growing more critical. Mathematics has not helped to transform economists from being analysts of the past into prophets.

What has been said fully relates to the study of the Soviet economy. One can and must apply here mathematics and different models, (for example, it is difficult to overestimate the calculations of Professor Trèml' and his co-wor-

kers on the model of interbranch balances), but using the so-called " model approach " to study the Soviet economy does not give good results. Here, above all, " institutional economists " are needed. Only they can explain how various mechanisms and institutions work; only they can solve various statistical enigmas (about which, by the way, Nove writes very well).

Thus, it is to Nove's indisputable credit that he avoided the model approach, which ultimately oversimplifies and impoverishes the picture. He showed the Soviet economy in its living flesh.

In addition, the book is completely accessible to non-economists. Alas, there is an ever-increasing tendency to believe that scholarly books cannot be understood by the unitiated. This is harmful nonsense and Nove's book once more demonstrates it — there are many interesting fresh ideas and fine analysis in it. It is a professional book in the best sense of the word and is fully accessible to the non-specialist reader.

Introducing a critical note I should say that the organization of the book could be much improved; but, it is easier to point out the inadequacies of a given structure, than to propose a better one.

A year ago I told Nove, that what was needed was a kind of " Das Social ", that is a detailed, broad, deep investigation of the roots and basis of centrally-administred economies with corresponding theoretical examination; also taking into account the experiences of various countries. Nove said nothing. Having now read his book, I see, that it is a clear step to the creation of just such a " Das Social ".

IGOR BIRMAN
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C.R. PHILLIPS, *Ciudad Real 1500-1750: Growth, Crisis, and Readjustment in the Spanish Economy*, Cambridge, Mass.: Harvard University Press, 1979, pp. 190.

Shortly after the reconquest of Andalusia, Alfonso X founded Villa Real to act as a counterweight to the growing power of the military order of Calatrava in La Mancha. A pawn of royal politics, the town was declared a city in 1421, and Ferdinand and Isabel briefly made it the seat of the new *chancillería* for southern Castile and the regional inquisition. When these institutions were withdrawn, Ciudad Real lapsed into relative obscurity, which its promotion to provincial capital in 1691 did little to relieve. In 1591 its population reached about 9000, a figure not surpassed until the nineteenth century. Its élite were locally based hidalgos, without titles or ties to the aristocratic houses of Spain. Except for participation in the pogroms of the fourteenth and fifteenth centuries and the Comunero revolt of 1520 (the last must have been

of little consequence, I find no mention of it in the standard accounts of the revolt), it saw no strife, and its most noteworthy news was of natural disasters: floods, droughts, locusts, plagues.

In selecting so unremarkable a community for a study *de longue durée*, Carla Phillips hoped primarily to shed light on the general history of Spain. She concentrates on the crisis of the late sixteenth and seventeenth centuries. According to her account, the period before 1570 witnessed the growth of Ciudad Real's economy, based on agriculture, crafts, and the export of wine and fine gloves. Its ability to absorb 700 Morisco households expelled from Granada in 1571 demonstrated its economic vitality. After 1600 a series of bad harvests and the expulsion of the Moriscos brought its population down to a low point in 1621, 41 percent below the 1591 figure. Ciudad Real experienced a " reversion to a subsistence economy " (p. 65). Slow demographic recovery followed, hampered by plagues, until the economy began to expand about 1680. By the time of the catasto of 1751, population had reached a plateau of about 80 percent of the 1591 figure, which Phillips describes as its " natural maximum " (p .35).

Phillips shows technical skill in the use of her sources, especially church registers for demography and notarial records for land transfers. She does not attempt family reconstitution but notes trends in vital registration, to fill in a long gap in censuses between 1621 and 1751. A central concern stands out: the reasons for the long drawn out economic stagnation at subsistence level in the seventeenth century. On the one hand she sees it as part of a general collapse of the Spanish and even broader European economies. Lowered agricultural production and demand for industrial goods led to contracting markets and communications networks, forestalling the possibility for growth. On the other hand, royal demands, which had been relatively light in the sixteenth century, became excessively burdensome in the seventeenth, discouraging productive investments. Nobles put their money in unproductive but remunerative credit instruments: censos (mortgages) and juros (royal debt). Even more, they donated large sums to religious funds. Despite the depression, this period saw the greatest religious endowments. Wealthy non-nobles, meanwhile, bought wheat fields and olive groves from the nobles, but since agriculture was not expanding, this was an investment in prestige, not economic growth. On balance, Phillips seems to blame the disheartening effect of royal tax policies above all for the economic stagnation, and these were responding to the needs of foreign wars.

This account fits so well with the accepted picture of early modern Spain that it is hard not to be convinced by it. It is, however, based on few hard economic data, especially in long runs. Phillips has the royal tax figures for the sixteenth and seventeenth centuries, but because of the way they were levied, taxes are not a reliable index of economic activity. The picture of agriculture and industry comes largely from the catasto at the end of her

period. There is no comprehensive discussion of the archival sources available, or even a bibliography of those consulted, but several footnotes indicate that sources exist that are not used. Someone is working on the tithe records "which will allow us to trace year-by-year harvests with some precision"; Phillips has seen only partial tithe returns for 1555 and "eighteen years in the early seventeenth century" (p. 150). She has studied land transfers only in the seventeenth century and has not compared them to the detailed records of landownership in the catasto because someone else is working on these too. The overall result is rather like an attempt to describe 250 years of urban history from snapshots taken at different times of different subjects. To fill in the picture, Phillips calls on information drawn from other parts of Castile, especially Noel Salomon's study of Phillip II's *relaciones topográficas*, which are lacking for Ciudad Real, but in doing so, she weakens her study as an independent analysis of the Castillan economy.

Specifically, I see little evidence for the central assertion that the economy was closer to subsistence in the seventeenth than the sixteenth century. Global economic activity does appear to rise and decline with the population (especially the arrival and departure of the Moriscos), but it is hard to see that its nature changed or per capita income varied appreciably. Ciudad Real appears to have continued exporting large amounts of wine and its famed gloves throughout the seventeenth century; it was still involved in a broader market economy. If non-nobles bought land, some nobles engaged in commerce. These facts make me question whether royal policies were imposing new, non-productive investment practices on the upper class. Even gifts to the church might be a psychological response to repeated plagues and harvest failures rather than to heavier taxes. Phillips dates Ciudad Real's economic revival to the last decades of the seventeenth century, one hundred years before David Ringrose finds a general recovery in central Spain. Might this be a response to the city's establishment as a provincial capital in 1691, similar to Madrid's expansion after it became a national capital? We are not told the reasons for this royal favor; it is not unlikely that they included a desire to help the city's economy.

The book deals with a number of other subjects, especially the urban social structure and the nature of the elite. It is a pioneering study and provides a valuable basis for future comparisons with other Castilian cities. I have concentrated on the theme closest to economic history. In this area I cannot help thinking, however, that, hampered by a lack of information, Phillips has been unconsciously inclined to accept a standard picture of the Spanish evolution and fit her findings to it.

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CH. TILLY (ed.), *Historical Studies of Changing Fertility*. Princeton, N.J.: Princeton University Press, 1978. Pp. ix+390.

This work is one of a series in the quantitatives analysis of history sponsored by the Center for Advanced Study in the Behavioral Sciences, of which the advisory committee in history includes three contributors to this book — Charles Tilly, chairman of the committee and the book's editor, as well as Richard Easterlin and Ronald Lee, both of whom are identified as economists more than as historians. The other contributors include historians of the family (Lutz Berkner and Maria Vinovskis), of economic structure (Rudolf Braun and Franklin Mendels), and of population (E.A. Wrigley and Etienne van de Walle). Recruited for the project in 1971, the participants met for three weeks in 1972 to discuss the papers they had written, and finally their revised articles were published six years later.

As one would anticipate from so distinguished a group, most of the papers are stimulating and one or two are outstanding, but they hardly comprise even as much of a unit as most such composite efforts. Apart from the introduction and conclusion by Tilly, the essays range in length, and thus approximately in intended impact, from 16 to 78 pages. Each participant was permitted to go off on his own, with a consequent wide range not merely of empirical example but of underlying assumptions, method of analysis, and important conclusions. In short, the book is like one issue of a good journal in economic and social history, one in which a number of papers on fertility happened to be assembled.

None of the participants uses the family reconstitution that Louis Henry developed, and several point out the limitations that such a microscopic analysis imposes. The populations studied range from England or France to Massachusetts or Zurich — nation, state, or canton; but in every case the orientation is to seek broader generalizations from a macroscopic perspective. Some decades ago the debate over population growth in early modern England, for example, reflected much concern about how to repair the gaps and the manifest faults in the data. In this book the statistics available are generally used with barely an apology for their inadequacy; supposedly the more or less elaborate methodology that is found in most of the papers balances the deficiencies of the data. The measure of fertility that each analyst uses is whatever his sources suggest; the convenient index devised by Ansley Coals, based on comparing any level with the highest ever recorded, is adopted to advantage in van de Walle's paper but not generally.

One convenient way of testing the historical knowledge of these historians is to look at their references to Malthus — which, as is all too typical in demographic writings, are as often faulty as not: Early in the book he is introduced by Tilly as "Thomas Malthus," rather than T.R. Malthus, his invariable public designation, or Robert, as his friends called him. If someone referred to the man elected American president in 1912 as "Thomas Wilson,"

his readers might suspect a gap in the author's knowledge, an impression soon reinforced here by such phrases as " the crude Malthusianism which has underlain so much previous writing on fertility." Wrigley informs us that " Malthus took the view that the normal propensity of population was to rise unless checked by adverse circumstances " — an adequate summary of the first edition but not of the second to seventh editions of the *Essay*. From Lee we learn of " the Malthusian conclusion that economic progress cannot benefit the laboring class except temporarily "; Malthus wrote, on the contrary, that " before a rise of wages can be counteracted by the increased number of laborers, ... time is afforded for the formation of... new and improved tastes and habits " that will cut the rate of population growth. Braun notes that Malthus, " using data provided by M. Murat, describes contrasting demographic patterns " in Switzerland; in fact, Malthus sharply criticized the conclusions that Murat drew from his not very accurate statistics. If today's practitioners of historical demography took the trouble to acquire a genuine understanding of what Malthus really wrote, in many cases their own analyses would be improved.

I do not want to leave the impression, however, that these faults detract substantially from the overall worth of the book. Many of the authors use the concept of " natural fertility," which French demographers introduced a generation ago and which is a more useful base than fecundity for measuring changes in family size. Especially Easterlin but also several others try to transcend the boundary (one might almost say barrier) between the economic and the sociological analysis of fertility. Perhaps the book's main virtues are negative, in its questioning such perennial theses as that " urbanization " brings about a decline in fertility or that " modernization " never results in a higher number of births. Unfortunately the conclusion to be reached from such an excellent collection is that we still know rather little about the determinants of fertility either of the past or, in fact, of the present.

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