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## REVIEWS OF BOOKS

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F. BRAUDEL, *The Mediterranean and the Mediterranean World in the Age of Philip II*, 2 vols., translated from the French by Siân Reynolds. New York: Harper & Row, 1972-73.

There comes a time in every historian's life — or at any rate every historian with some stake in early modern history — when he has to confront the question of what the work of Fernand Braudel means to him. Now that an English translation of Braudel's mighty volumes on *The Mediterranean* has appeared there is little excuse for avoiding such a confrontation. The first version of this work appeared in 1949. It was immediately pronounced a masterpiece; and it turned sixteenth century social and economic history into one of the "leading sectors" of our specialization. A rush of even greater, world-wide interest greeted the second and greatly revised edition of 1966. The high level of excitement concerning this work is being sustained, following the English translation, by so many commentaries on the work (and on Braudel himself and the "Annales school" in general) that not only *The Mediterranean* but also our reactions to it have become events of first-rate significance to our profession. Recently the *Journal of European Economic History* published a highly laudatory article on Braudel and the Annales school by Maurice Aymard [Fall 1972]. But the most extraordinary evidence of this keen interest can be found in the December 1972 issue of the *Journal of Modern History*, which carried three articles on the significance of Braudel's work, in order (said its editor, William McNeill) « to sound a fanfare on behalf of this majestic monument of twentieth century historiography ». Indeed, one could say that *The Mediterranean* already is an over-reviewed

book, and that the time has come to stop celebrating Braudel and start using him.

This situation has led me to decide that I should discuss how teachers of European economic history might want to present *The Mediterranean* to their own students. What guidance can we give a young scholar about to invest many hours in reading this long and complex work? In my opinion, we should try to explain that it constitutes a famous and most salutary challenge to our entire profession; that it claims to stress theory but instead offers little beyond method and not much of that; that certain sections provide us with precious detailed information and enormously useful insights; and that some passages are useless and even misleading.

Why did the Spanish government shift its chief international concerns from the Mediterranean to the Atlantic in the 1580s? This is the port from which Braudel sets out to explore what some historians are calling "le monde braudellien." In the course of enlightening us as to how such a voyage should be made, he has seen to it that no self-respecting scholar will ever again be able to navigate in the manner fashionable until the 1940s: that is, merely plotting political or foreign policy decisions and events in their proper time sequence ("histoire événementielle"). This work embodies Braudel's basic professional concept that the starting point of all serious historical labor should be some truly worthwhile question ("histoire problème"), and that in our endeavour to answer such a question we must immerse ourselves in every facet of the life of the societies ("histoire totale") in which these dramas are worked out.

One of the most engaging aspects of *The Mediterranean* is the way in which Braudel challenges us to accept or reject what he is offering but in any case to carry on the good work ourselves. Nothing is presented as definitive or decided for all time. This very lack of confident-sounding statements confuses students. Expecting a compelling lecture, they find instead a sort of free-wheeling seminar discussion in which a teacher exposes the interim results of his work, admits to the gaps in his knowledge, explains his doubts concerning his hypotheses, and demonstrates where students might concentrate their own efforts so as to build on the teacher's findings. For example, in raising the question as to why English shipping seemed to desert the Mediterranean for some years after 1552 Braudel demolishes the conventional explanation (the supposed dominance of the Turks at that time) and concludes, very tentatively, that the only reason that seems to make sense is that market conditions may have changed enough to make such voyages (temporarily) unprofitable for the English.

Perhaps the most useful preparation for a student about to tour Braudel's *Mediterranean* is to know he must resist being stunned into passivity by the marvellous vistas laid before him. The richness of detail in this work is in violent contrast to the modesty of its claims. What are students to make

of a work that employs African, Greek, and Turkish geography and history as well as the more familiar materials from the western Mediterranean? How are they to keep their balance when presented by dizzying changes of focus, from brilliant generalizations on the geo-history of the entire inland sea to glimpses of ordinary life in a tiny town? The magisterial vibrations emanating from Braudel's comparisons of transport costs out of Constantinople with those of other ports, for example, are likely to evoke awe rather than the sort of critical competitiveness that helps turn a student into a professional scholar.

Jack Hexter, in his adroit and amusing appreciation of Braudel written for the December 1972 issue of the *Journal of Modern History*, believes the richness of *The Mediterranean* is so overwhelming that a well-advised reader will not plough through the two volumes cover to cover, but rather will browse here and there as fancy dictates. For students with a great stake in early modern history, however, this is bad advice. Such students approach *The Mediterranean* hoping to find there models of scholarship and up-to-date procedure; so that dipping into it here and there is certain to increase their feelings of frustration.

The easiest way to invite students to enter into combat with Braudel is to point out how often he uses small clues to develop large generalizations. The vulnerability of his work to this sort of criticism has been commented on by many previous reviewers. Too much time spent on this sort of fault-finding with our students, however, would mean turning our backs on the great gifts Braudel offers. Students of history, especially those in medieval and early modern studies, find it all too easy to retreat into a rather narrow and barren "solid scholarship," which disdains to "go beyond the documents." Therefore I feel we should emphasize for our students the enormous service Braudel has rendered in not only pointing up the areas where more work is needed but also in demonstrating that useful additions to the literature can be made by publishing one's insights or even conjectures.

If *The Mediterranean* has indeed become a "majestic monument," the inscription carved on it is easy to read: long hours in the archives should stimulate, not stultify, one's imagination; and reading documents and framing hypotheses must go hand in hand.

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J. C. DAVIS, *A Venetian Family and its Fortune, 1500-1900: The Donà and the Conservation of their Wealth*, Vol. 106 of the «Memoirs of the American Philosophical Society», Philadelphia, 1975, pp. 189.

This is a noteworthy but uneven book. The author outlines the history of a noble family, chiefly with an eye to its ways of conserving wealth.

Numerical tables and demographic detail will attract the attention of quantitative historians. The author's large ambitions, however, force him continually to cut corners and get over great gaps in his documentation. This makes his treatment facile, as he puts his story and bits of evidence together by means of an artful splicing. He would have done better to take 150 rather than 400 years, for the longer stretch demanded more sustained work in Venetian archives. The book aims to be an empirical study of one branch of the Donà family, of their wealth, patterns of marriage, reproduction, and inheritance; but it betrays a persistent refusal (e.g., pp. 43n, 162-63) to invest in deeper research. Much here that is conjecture and impressionistic has the appearance, inadvertently, of being founded in archival research.

The book's major findings and conclusions follow.

The Donà made much money from long-distance trade and shipping in the thirteenth and fourteenth centuries. During their heyday they had a family vessel and trafficked in a rich variety of goods. Not until after 1460 did they begin to acquire land. The later sixteenth century saw them retreat from trade and come to rely increasingly upon their income from land. And now, in an age of economic decline for the Venetian republic, the Donà had recourse to devices calculated to conserve the family wealth. They entailed real property, making it inalienable; they fixed hereditary succession, excluding females from inheritance; they restricted marriage in the male line to one per generation and they made sure that the one marriage brought in a huge dowry. Then came the far-reaching changes of the later eighteenth century. Daughters were less easily deposited in convents. The Napoleonic and reformed Austrian codes went to help alter the course of change: entail was much reduced, when not altogether eliminated, and henceforth women were entitled to share in family estates. Already in the eighteenth century the Venetian nobility practised birth control; all the more so in the nineteenth century, as more and more Donà and their peers married. In short, when a new age (1750-1900) put an end to the old ways of conserving wealth, the Donà resorted to more extreme measures. In the twentieth century, plunging back into money-making, they have been much involved in business enterprise.

Such is the pattern traced by Mr. Davis. It is neat. The Donà appear to illustrate fundamental trends in the history of Venice: first, success in shipping and commerce, followed by growing investment in land (later fifteenth and sixteenth centuries), retreat from trade, then a conservative holding operation lasting some two centuries, and finally the revolutionary readjustments of the nineteenth and twentieth centuries. Anyone can see that these are main lines in the history of the Venetian patriciate; yet the author, remembering but not always observing his empirical model, refuses outright to acknowledge or deny the representative character of the Donà (p. xiii). The ambiguity is one very much of his own making, for he seems

to have hit on what *is* typical, but how establish this on the basis of one family line?

I wish that Mr. Davis had seriously confronted the question of typicality, especially as historians who use his study will be forced into doing so. In this connection — if I may tender a suggestion — it seems to make historical sense to see the Venetian nobility (ca. 1450-1750) as a composite of three social groups: the rich and preeminent, the group of those who held middling positions in government, and the throng of relatively poor noblemen who lived on the fringes of public life. Their pride and canny obstinacy aside, the three groups pursued different life styles. The Donà had something of those at the top and those in the middle, but what the contrasting typical features were, or what forms these took in the consciousness of the three groups, is not something that we can sort out of this monograph.

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D. DOWD, *The Twisted Dream: Capitalist Development in the United States Since 1776*. Winthrop Publishers, Inc., Cambridge, Mass. 1974 pp. 303 + XVII. Bibliographical references.

The central thesis of Dowd's re-evaluation of American economic history is clearly set forth: The nation's remarkable development was marked by « expansion and exploitation, the alpha and omega of capitalist development. » These twin processes brought with them a wide range of social disorders and costs (exploitation, poverty, pollution, racism, imperialism). Material welfare was achieved *by means of* degradation of human values and waste of natural resources. The "dream" of the title was not only Americans' traditionally hopeful view of their future and belief in progress toward equal opportunity, but also a conviction that production of more material goods could somehow ameliorate social problems. Now, says the author, we must see how "twisted" that interpretation was, how the growth of capitalism embedded in the whole fabric of society those very ills which we had hoped to cure. In effect, he says, America has been the victim of its own illusions, and is only now beginning to awaken to reality.

From the very beginning, American institutions favored "free" individualistic exploitation and personal gain — but only on the foundation of slavery. In post-Civil War industrialism, corporate business acquired the lion's share of wealth, used it to forge instruments of power, controlled government, labor, natural resources, and subdued small business and agrarian protest. The monopolistic sector, controlled by a wealthy oligarchy, throws crumbs of welfare to the poor, and by adroit manipulation divided the

labor movement against itself. So inequality is institutionalized. Now the warfare state and the economics of control protect that oligarchy against economic instability. We are rich enough to solve problems of poverty; « ...something under \$ 14 billion annually would eliminate... money incomes below the poverty line. A society with a GNP well in excess of a trillion dollars... could not be economically strained by such a piddling amount. » But the "halls of power" are occupied by those who profit from the malaise, just as those who have the power to eliminate pollution are precisely those who caused it and gain from it.

Today the military-industrial imperatives and the costs of the welfare state impose a burden and generate resentment among middle-income taxpayers (including the elite of labor). This implies the risk of fascism: the drift is all that way. The "movement" of the 1960's — blacks, disillusioned white youth, anti-war forces — was vitiated by too much reliance on *moral* outrage. Its vision was "revelation," i.e., that same utopian belief in social justice *within* private property which historically has always ended in frustration and withdrawal. What can and must be done, Dowd says, is to weld a truly revolutionary socialist movement, but this is feasible only if the lessons of history are intelligently applied to our future.

This drastically compressed summary does not convey the full range of the author's analysis. From the opening chapter's critique of neo-classical economics — « it must come to terms with history » — to the essentially hopeful epilogue, all major facets of his main thesis are skilfully and vigorously developed. Dowd is that *rara avis*, an intellectual capable of altering former interpretations without hand-wringing or breast-beating. His earlier work was mostly in the "progressive" tradition; this book draws heavily from "new left" historians, from Marx and Veblen, and from Baran and Sweezy. The result can be faulted for eclecticism, perhaps: Dowd sometimes implies, *à la* Keynes, that the power of government *can* prevent unemployment, inflation and pollution if it only *will*. But this and other shortcomings are essentially minor. Overall, it is a fine, stimulating, useful historical analysis.

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P. EARLE (ed.), *Essays in European Economic History, 1500-1800* edited for the Economic History Society. Oxford: Clarendon Press, 1974, pp. VII-273.

Students of economic and social history unfamiliar with foreign languages will welcome this new collection of essays in translation just as they welcomed two somewhat similar collections (edited by Marc Ferro and Peter

Burke respectively) when they appeared in 1972. Unlike those two earlier volumes which reproduced articles from the French journal *Annales* on both social and economic history without limitations of either time or space, this new volume is deliberately restricted to economic topics and to Europe from 1500 to 1800 and draws from a variety of sources. The *Annales* and the *Annales* school, however, still loom very large here: out of eleven essays at least six are by members of that distinguished school of history — a tribute to the outstanding contributions it has made to our understanding of the economy of early modern Europe. The main themes of this volume are, in the editor's own words, « the dynamic changes which occurred in Europe during these centuries (1500-1800) as the long cycle of population and price movements had its differing effect on the economies of the various regions ». In this broad perspective the Mediterranean world is covered in a chapter from Fernand Braudel's *opus*. The editor has selected what is possibly the most debatable, but also one of the most challenging chapters in the 2nd edition of Braudel's *Mediterranean*, namely the one where he attempts to construct « a model of the Mediterranean economy ». It should definitely serve to stimulate interest in Braudel's book now available in English in its entirety. A long excerpt from E. Leroy Ladurie's *Les Paysans du Languedoc* summarizes with elegance and brevity the main argument of that impressive book and, along with an article by the Chaunus on "The Atlantic Economic and the World Economy", represents an excellent introduction to the work of the second generations of *Annales* scholars. From that same group the editor has also selected an essay on the crisis of the Spanish economy by Pierre Vilar and one on seventeenth-century Italy by Ruggiero Romano. The case of Poland is dealt with by Jerzy Topolski in an article which summarizes the vast body of recent literature on the agrarian history of that country. The Dutch economy in the period 1650-1750 is the subject of an essay by J.G. van Dillen emphasizing the structural changes and the adaptations experienced by that economy in an age which has too often been dismissed as the mere, unexciting aftermath of Dutch commercial supremacy. The Netherlands are also discussed in the final selection of the book, a long article by the late Max Barkhausen on "Government Control and Free Enterprise in Western Germany and the Low Countries in the Eighteenth Century". The title is somewhat misleading, for the article is, in fact, a penetrating comparative analysis of the origins of modern industrialization in the lower Rhineland and the Low Countries. Barkhausen's emphasis on the crucial role played by rural industries in preparing the terrain for the rise of the modern factory should serve as an antidote to the lingering urban slant of much historiography on the early modern period, and so should the detailed, prodigiously well informed survey of "rural industries in the West from the Middle Ages to the Eighteenth Century" by H. Kellenbenz. The still little known history of the inner workings of the rural economy of

early modern Europe is explored in Jean Meuvret's pioneer article on "Monetary Circulation and the Use of Coinage in Sixteenth- and Seventeenth-Century France" and in the lucid discussion of French agriculture in the seventeenth century by Jean Jacquart. Altogether this is an invaluable introduction to the economic historiography of early modern Europe. It is only to be hoped that the publisher will soon produce a paperback edition of it, thus facilitating its diffusion among as many students as possible.

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S. D. GOITEIN, *Letters of Medieval Jewish Traders*, Princeton, Princeton University Press, 1973, pp. xviii + 359.

Solomon D. Goiten needs no introduction for those at all familiar with his area of research. His scholarly monographs, which include *Studies in Islamic History and Institutions* (1966) and the first two volumes of *A Mediterranean Society. The Jewish Communities of the Arab World as Portrayed in the Documents of the Cairo Geniza* (1967-1971), are major works in their field. It is not surprising then that the present book is not simply a collection of well-known documents in translation intended for assignment to undergraduate students. Despite the author's caution and modesty this is far closer to being a scholarly, thoroughly annotated edition of English translations of eighty hitherto unpublished and untranslated letters (selected from a collection of some 1,200 pieces), ranging from approximately the eleventh though the thirteenth centuries and written by Jewish traders in Islamic lands ranging from Spain and Morocco to India.

The work is prefaced by an Introduction surveying the overseas trade engaged in by Jewish merchants and the nature of that merchant community itself — a community closely bound together by ties of religion and of blood. Separate essays precede each of the major sections into which the book is divided: « Geographical Setting », « [The] Eleventh Century », « [A] Merchant-Banker, Scholar, and Communal Leader », « The India Traders », « Twelfth Century and Later », « Accounts », and « Travel and Transport ». Each separate piece is then separately introduced, placed in its historical setting, and related to other selections in the book. For greater ease of comprehension each of the longer letters is subdivided into sections indicating the topics with which it deals. No. 38, for example, dating from 1138 and dealing with the India trade, includes: « Introduction, beginning in rhyme », « Shipments from India », « Shipwreck », « Household goods ordered », « Silk sent instead of gold », « Request to intervene with a Muslim notable », and « Presents sent ».

The inclusion of complete letters, rather than extracts such as those published in analogous collections, has the advantage of presenting issues in context, an advantage that outweighs the difficulties that the beginner may encounter in trying to focus on particular topics. One might also wish that the organization had been kept strictly topical, rather than alternating topical and chronological chapters; but these problems can in part be compensated for by the intelligent utilization of an excellent subject index. We may be grateful to Professor Goitein for producing a book that should prove valuable for both the neophyte and the accomplished scholar.

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E. LE ROY LADURIE, *The Peasants of Languedoc*, translated by John Day, University of Illinois Press, Urbana and Chicago, 1974, pp. xii + 370.

This is essentially the same marvellous book which first appeared in French in 1966. Ladurie supervised an abridged French version which appeared in 1969, and now John Day and the University of Illinois Press have given us the abridgement in English.

In 1966 *Les Paysans de Languedoc* seemed to cap the whole movement, begun by Pierre Goubert's book on *Beauvais*, towards a demographically informed yet synthetic history of French rural society in the seventeenth and eighteenth centuries. It still does. In this book the claim to "total history" is fulfilled as well as ever it will be. From births, deaths, and marriages, through the patterns of landholding and use, to the cultural momentum and ephemera of the times, Ladurie gives us all of Languedoc. Out of these materials comes a portrait of a rending agrarian cycle, nine generations long, which ran through its phases of population growth, crowding, shrinking holdings, engrossment of land by *rentiers*, demographic disaster, reduced population, and renewed growth, from the beginning of the middle ages until the middle of the eighteenth century. Where was the way out? There seemed to be none, and the limitations of a relatively fixed productivity and an extravagant high culture forced the cycle again and again to its disastrous, depressing conclusion. Then, in the middle of the eighteenth century, for a dozen tenuous reasons — viticulture, a base of literacy, an evolving commercial mentality — production edged slowly up. It was enough to care for a growing population, whose growth was now augmented by a half-understood fall in mortality, and so the modern era began. In the nineteenth century the cycle became open-ended growth.

The book has many hidden virtues, such as Ladurie's persuasive and eloquent voice, which comes through nicely in this translation. The serious consideration given to the weight of culture is likewise remarkable.

In some ways the sections on high culture, on religion, and on other popular movements alone would justify the book, and would guarantee wider use if the book were in paperback. Yet in the end what Plato called « the recalcitrance of the medium » takes its toll. The bulk of the research ends at the beginning of the eighteenth century, and the skillful speculations on how the great breakthrough subsequently occurred are, alas, no more than speculations. Why did this upswing of the ancient cycle continue on into a "modern" era? We still do not, perhaps cannot, know.

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J. E. KNODEL, *The Decline of Fertility in Germany, 1871-1939*, Princeton, Princeton University Press, 1974, pp. XIX-306.

*The Decline of Fertility in Germany* is a physically well-constructed \$ 14.50 book. It contains seven chapters, five appendices, a list of official statistical sources and an additional bibliography. Textual material is amply accompanied by tables, charts and maps. These account for approximately 23 per cent of the book's space; appendices consume about ten per cent more. Textual material takes most of the remaining 67 per cent. Of these, the appendices will probably prove most useful and enduring. Possibly the charts, tables and maps will be next most useful. On these grounds, this volume will be most interesting to librarians and demographers interested in statistical sources.

Knodel's work is the second volume published in a series projected by the Office of Population Research in Princeton. These studies, and those that may follow, seek to explain the European fertility decline. As put by Ansley Coale, who wrote the book's forward, the Knodel study is a « building block in our eventual understanding of the circumstances under which a major decline in fertility occurs ».

Knodel, however, is not interested in presenting an historical analysis which draws together the hitherto disconnected threads which, once united, would explain Germany's fertility decline. He tests hypotheses. His test is neither logical validity nor plausibility but solely confrontation between thesis and data. His contribution, therefore, depends upon our interest in those theses tested, upon his selection of statistical accoutrement, and upon his data's adequacy.

Knodel presents his data in appendices 1A, 1B, 2A, 2B, 3 and 4. They appear sound. They are shown for various dates, generally for Germany's 71 middle-level administrative areas. Unfortunately they usually are presented as weighted averages. And neither the weights nor the raw data are shown. Since others may desire different numerical summaries of the German expe-

rience, this method of conserving printing costs is unfortunate. Those tables, charts and maps given in the text generally further compress the data. Thus, while summaries of summaries are useful for argumentative purposes, they do not bring us closer to the material we desire. Yet, withal, Knodel's appendices on overall fertility, marital fertility, illegitimate fertility, proportions married, urban and rural differences in these indices and infant mortality rates are a welcome addition to our statistical inventories.

Knodel's selection of testable theses is less daring. Central to this inquiry is his interest in and commitment to Transition Theory. Perhaps most important is his commitment to the proposition that there is but a single decline in fertility from the high, pre-industrial, primordial level to the low, modern level. Knowing there is but one such fall, Knodel's test of this proposition is less exacting than most he presents. In some places, chapter two for instance, he seems as interested in maintaining the thesis of one decline as in testing it. Evidence of declining rates in the early 19th century « appear to be largely a product of greater completeness in the population counts » (p. 42) In general the evidence « is more or less consistent with the contention that a prolonged decline in marital fertility had not been taking place... in the vast majority of villages... ». But Knodel protects the thesis of one fall most effectively with an essentially tautological definition of "significant" fertility decline. This permits him to note, rather disinterestedly, that « only a few small societal segments (most notably the Jews and possibly the highly educated occupational classes) may have preceded the general population in fertility decline by a generation or more » (how much more?) (p. 147) or that « in Germany prior to the fertility decline, as in most of Europe, the average age at first marriage was... over 25 for women... » (p. 249) and still maintain « the fertility decline that occurred in Germany during the latter decades of the nineteenth century and the first half of the twentieth century was a unique, clearly identifiable, and irreversible phenomenon in the course of German demographic history. » (p. 9) At least we have yet to see it reversed.

Many other theses are presented or tested. But none are treated as graciously as the tenet of one unique, clearly identifiable fall. It is worth noting, however, that many which are discussed are not tested because the data are not adequate to the task. And others, so it appears, are tested but not discussed. One such must be related to his continual use of analysis of variance to show the overall variance is greater than the variance within his seven regional grouping. This repeated conclusion may have some bearing upon a diffusion as opposed to an innovation explanation for the fertility decline, but we are exposed to no discussion connecting this statistical finding to diffusion, or for that matter, to anything else.

Knodel's difficulties in "hypothesis" testing are compounded by the interrelated nature of many variables. A case in point is his series on percen-

tage of workers in primary industries (predominately agriculture) and his series on urban-rural residence. Knodel escapes the multicollinearity problem deftly, however, by computing numerous zero-order correlations, for the most part. Thus, we learn that both a) percentage in primary industry and b) percentage in rural determine the rate of fertility decline. Fortunately he finally exposes us to partial regression coefficients. But they are treated both late and lightly as well as rather in the manner of an afterthought. This atmosphere of neglect for interaction and mutual determination is fostered by the preponderance of tables presenting zero-order correlation (23 or 16), depending upon whether you count from chapter I (23) or from the chapters which associate independent variables with various fertility rates (16). These compare with five tables which show partial correlation coefficients.

But even were the statistical tests more adequately mated to the problem, and even if Knodel were less wedded to the thesis of one unique, clearly identifiable decline, the study may have produced few different conclusions after all. For at heart, this book may reveal nothing so much as the poverty of current population theory. Consequently, we perhaps should study his summarized material carefully, well aware that it does represent a major addition to our data base. And had he done no more than that, his book would have been most welcome.

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P. MASSA, *Un'impresa serica genovese della prima metà del Cinquecento*, Milano, Giuffrè, 1974, pp. xv-318, Lit. 4,800 (Università di Genova. Pubblicazioni dell'Istituto di Storia Economica).

It is clear that the ledger book has become a historical protagonist of major importance. It was Marcel Maus who understood that on account of the complementary nature of the relationship between its compiler and the different social and economic events with which his work and business brought him into contact, the ledger book provided a « phenomenon of total history ». It is still striking however to discover the full extent of the information, often of the most varied type, which the author has in this case been able to draw from an account book. As a result the figures cease to be simple factual accountancy records, and achieve a wider significance as keys to a living reality, from the needs, tensions and inter-relations of which they originate. The author has concentrated her attention on the research begun earlier in the century by Broglio d'Ajano, Cessi, Ciasca, and then further developed since the war by Violante, Bologna and Santoro. Previously she has been concerned with the organization of the guilds, and in particular on the silk guild (e.g. *L'Arte genovese della seta nella normativa dal XV al XVI secolo*, reviewed in this Journal in 1972). The present work is in fact a practical examination of certain aspects of the same sector,

and also provides an important assessment of the effectiveness of the statutory norms, regulations and decrees of the silk guild.

The author's experience enabled her to bring to light a double-entry ledger in the Genoese communal archive which had been wrongly positioned (both by virtue of its date (1538) and contents) among the papers of the silk guild. The ledger in fact deals not with the silk guild as such, but rather with the business of the silk merchant Vincenzo Usodimare di Rovereto in the period between 2nd January 1537 and 2nd January 1542 during which he was fully engaged in both manufacturing and selling silk. An unusual feature of the ledger is that it also contains information on the private possessions of the owner, although these are not regular entries. This makes it possible to obtain a good picture of the personal situation of this merchant and entrepreneur, who can also be taken as a typical example of the middle class of his day.

Naturally the document which Massa has discovered sheds much light on Rovereto's commercial activities, although without providing a comprehensive picture. This is because the "*cartulario manifattura*", or the current account book, in which Rovereto would, by custom, have recorded the quantities of silk which he consigned to spinners and weavers for manufacturing has not survived. Nevertheless one must acknowledge that the author has succeeded in filling these gaps by drawing on the records of other concerns, and by using the records of the guild consuls' ruling in cases where consignments were not made in full or where the workmen were suspected of stealing some of the material. She has also been able to use the entries in the main ledger which refer to the payments to weavers for their services in order to establish the length of time required for various manufactures, for this constituted an important factor both in terms of immobilization of capital and of the risks arising from possible contractions in demand.

The silk merchant's account book, despite these gaps, provides us then with a clear outline, but it is also a source which is criss-crossed by others, so that the events which it records lead on to others, and these to others in a permanently expanding and multiplying manner. The object of historical research is to distinguish within the field of human activities, as far as is possible, those points at which whole series of different activities come together and meet, and in this sense Massa's work admirably covers a wide range of fields. Behind this lie her extensive studies in the libraries and archives of Genoa, and the research which she has carried out on the papers of the silk guild (*Atti dell'Arte della Seta*), on the civil and criminal cases in which members of the guilds were involved, and on other archival collections such as the *Pratiche diverse dell'Arte della seta* and the *Istrumenti* of the same body. The *Cartolari delle Colonne dei "luoghi" di San Giorgio* and the *Cartolari del Banco primo e del Banco secondo di numerato di San Giorgio*

have also been consulted, while notarial material has also been used to throw light, even if indirectly, on references found in the account book. Full use has also been made of contemporary and modern works on the political, social and economic life of Genoa in the 16th century.

The period covered by the Rovereto ledger was one of relative tranquillity in Genoa both at home and abroad. Nine years before the opening date of the accounts Andrea Doria had confirmed the new constitution which restricted political power to a group of only twenty-four noble families, and Vincenzo Rovereto had entered the ranks of the nobility becoming a member of the "*albergo*" of the Usodimare, a patrician clan whose wealth was based not on feudal estates but on industry and commerce. It would be interesting to know something of Rovereto's political views in this period, because not only did Doria subsequently limit the powers of the new nobles because of the favour shown the Fieschi, but also damaged the commercial interests of the of the Genoese merchants in France as a result of his pro-Hapsburg policies. Due to the absence of any narrative evidence (in particular of any commercial correspondence) Massa is unable to tell us anything on this, although it may well be possible to glean something from the private archives of other Genoese families who are now much better disposed than in the past to make them available to scholars.

Once a member of the "*albergo*" of the Usodimare, however, Vincenzo Rovereto does not exactly seem to have felt himself to be a *gentilhomme*. From the notarial documents Massa has discovered that the "*volta di seta*", that is the shop or rather the offices from which the merchant directed his business, were rented premises, as was the house in which he lived with all his family. The silk merchants clearly did not share the pretensions of the old nobility who were in this period building themselves huge residences outside the city. As the author rightly points out, by renting his business premises, and by renting out two-thirds of his own property and investing his capital in the "*luoghi*", or banks of San Giorgio, from which he drew the so-called "wage" or interest (in face of the evocative terminology adopted and glossed by the author, Febvre's remarks on the relationship between semantics and economy spring to mind) Rovereto created an income and a fortune which provided a basic « safeguard against the risks of commerce, a guarantee for his creditors and a means of financing his own entrepreneurial activities ». All these things enabled Rovereto to work on his own account, something which was quite unusual in Genoa where businessmen preferred to form associations and seek financial assistance even from individuals who were not guild members. Rovereto's character seems to have been well suited to his work. He had gifts of initiative, together with a sense of caution and professional ambition, and at times showed an intuitive skill and sense of timing worthy of any modern entrepreneur. Simply to take one example, he succeeded in cutting out the

Genoese intermediaries by buying his supplies of raw silk directly from source. He often paid his artisans in kind, so that by the end of the five years covered by the accounts the payment in part with grain which he had purchased earlier as prices began to rise left him still with a profit even after he had met the expenses incurred.

Following March Bloch's exhortation, Massa has not hesitated to treat economic and social problems as interdependent and interrelating throughout her study and her conclusions also rely on this assumption. From the columns of figures she has been able to draw out a slice of social history, which is presented in the fullest and most detailed way. The figures of the spinners, winders, weavers and dyers provide a picture of men at work which is both poetic and nostalgic in a technological age such as our own. But even then it is clear not all went well, and there were already certain signs of conflict between capital and labour which gave rise to concern. It is apparent from the account book, for example, that the length of time before which accounts were settled for wages was left to the discretion of the entrepreneur, and that while the views of the workers and employees differed the contractual power of the guild only served to highlight the weakness of the artisans. Massa has also shown, however, that it was not easy for the entrepreneur (at least in Rovereto's case) to undercut the wages and payments fixed by the guild statutes, and in fact he often paid as much as 20% more than these.

The wealth of information which Massa has provided us with makes it possible to ask to what extent Rovereto was similar to a modern capitalist. While it is true that the spinners and weavers worked in their own homes and therefore enjoyed a certain autonomy, it is also the case that their independence was more fictitious than real. In fact neither the spinners nor the weavers could work on their own account, but had always to work for the silk merchant, and in order to have some continuity of employment and security they were forced to behave in a suitably subordinate fashion toward him so that he would continue to give them commissions. As a result they were in fact really precocious wage-earners who had nothing but their labour to sell.

In the five years in question Rovereto employed at least 47 artisans, and, bearing in mind that he had no partners, this means that his business was one of considerable size and scope. In particular the capital which was employed in it, as Massa notes, « exceeded the average of even the larger investments in silk shops effected by the leading Genoese capitalists of the time ». This was almost certainly due to Rovereto's ability to continue sending goods to Lyons, even after Genoese imports had been banned by Francis I, and also by his decision to sell goods in Antwerp in view of the difficulties which affected the Genoese market. Operations such as these are carefully reconstructed by the author, who once again provides more

important and new information on topics such as: the lack of insurance for goods; the routes used for transalpine trade; the activities of middlemen in Lyons and Antwerp etc.

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J. F. SHEPHERD and G. M. WALTON, *Shipping, Maritime Trade and the Economic Development of Colonial North America* (Cambridge, England: Cambridge University Press, 1972), pp. ix + 255.

Shepherd and Walton divide both the title, and this book itself, into three; they give us a study of ocean shipping in the eighteenth century colonies, an analysis (though only for the years 1768-1772) of overseas trade and the balance of overseas payments, and a suggested model of colonial economic development. Inevitably, the divisions show, and even in the concluding chapter the authors do little to bring together the results of their empirical studies or to relate them to the theoretical framework which they set out at the beginning of the book. They do not pretend, it is true, that the object of the book is to test that framework, but still the lack of any explicit connection between framework and fact leaves the reader unsatisfied. The major problem, as it has been in the historiography of the British industrial revolution, is how to relate the very considerable increase in overseas trade in the late seventeenth and eighteenth centuries to an explanation of the overall increase in the wealth and welfare of the European peoples on both sides of the Atlantic.

In the British case, the connection was traditionally seen as both a matter of the extension of sources of supply of raw materials and foodstuffs, and an extension of the market for British manufactured goods, and finally as increased opportunity for profits through trade and invisible earnings which could be invested in British industry. In the case of colonial North America, trade is seen by Shepherd and Walton as helping to develop an integrated market economy, and in particular declining shipping and distribution costs benefit the economy by bringing « into focus the relative comparative advantages in production among regions with widely different factor endowments. The consequence was advancing regional specialisation and division of labour, which increased output and raised living standards » (p. 23).

Clearly trade, on both theoretical and practical grounds, had an effect in all these ways, but the problem which has to be faced is that of weighing up its contribution against other factors, and in particular against the possibility of growth within the home market, stimulated by technological change and internal improvements in distribution. In the British case, the work of Deane and Cole, Eversley, Pollard and E.L. Jones has tended, in recent years, to emphasise the growth of the home market, and the expansion of agricultural production through technical, though not mechanical, inno-

vation; the role of foreign trade has consequently been diminished and relegated in one recent account to that of bringing about small shifts in the factors of production at the margin. In colonial North America, on the other hand, Shepherd and Walton still feel that their model has to give primacy to trade; they argue that agricultural productivity did not increase, so that agricultural growth had to come through extensive rather than intensive development, and that extensive growth was only possible because reductions in distribution costs allowed the products of the marginal lands, further from the coasts, to be sold in Europe. In addition, the shifts in short-run supply curves, caused by shifts in the demand for colonial products, are seen as motivating population migration which allows this extensive development to take place.

Colonial North America, a poorly integrated, agriculturally based, economy, with artificial restrictions on the development of manufacturing, is likely to have placed more reliance on external trade than did Britain or the European economies. Yet, as Shepherd and Walton suggest, if *per capita* output in the colonies rose, « overseas trade was a declining proportion of total economic activity over the century » with total earnings from overseas trade at about 14-15% of total output in 1768-1772, not a great deal more than equally speculative estimates for Britain at this period. In addition, Shepherd and Walton argue that the growing stock of capital in the colonies was due to « domestic saving rather than to foreign investment » although this conclusion is largely unsubstantiated, and, of course, domestic saving may well have stemmed from the profits of trade. As in the British case, any real judgement is difficult because of the absence of data on internal trade flows, although Shepherd and Walton's recent work on coastal shipping suggest that these flows were important to the colonies, but the evidence is not inconsistent with the possibility that the authors are ascribing too much to trade. Agricultural technology may not have seen spectacular changes, but nor did it in England or Holland, and yet substantial growth was achieved.

None of these comments detract from the great importance of the empirical work reported in the other two sections of the book. The calculations of the colonial balance of payments, the careful though somewhat speculative partitioning of the causes of reduced transport costs, and the painstaking estimates of trade flows, have all added immensely to our knowledge of the colonial economy. Shepherd and Walton are also model authors in the careful way in which they state whether, and in what way, their conclusions support, modify or differ from the earlier views. Their model is difficult to apply, with its strict equilibrium approach and a basic lack of data which might help to identify supply and demand shifts, and it may turn out to over-emphasise the role of trade, but the book as a whole is a very good start to the new economic history of colonial America.

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A. TEICHOVA, *An Economic Background to Munich: International Business and Czechoslovakia 1918-1938*, Cambridge: Cambridge University Press, 1974, pp. xx-422.

For many years economic research dealing with East-Central Europe has focused almost exclusively on the post World War II period and Western economic historians have seriously neglected interwar economic developments in the area. For this reason Alice Teichova's book is a welcomed addition to the scant literature available on this interwar period.

The book is an attempt to evaluate the role of foreign investment in the economy of Czechoslovakia in the pre-Munich years from a Marxist perspective. On the basis of substantial archival sources not previously accessible, the author traces the extent and ramifications of foreign capital holdings in Czechoslovakia's joint-stock companies. Much of the book is devoted to detailed descriptions of the background and changes in foreign capital ownership in several major industries and of the intricate web of cartel agreements. Comprehensive tables show the scope and origin of foreign assets and detailed charts trace the interrelationships between parent and subsidiary companies in Czechoslovakia and other countries. Considerable attention is paid to the movement of this capital in and out of Czechoslovakia. The author stresses particularly her finding that after Hitler's rise to power in Germany, non-German foreign investors, though refraining from new long-term investment in Czechoslovakia, did not withdraw their capital until forced to do so after the German occupation of Czechoslovakia in 1939.

The balance of benefits and costs of direct foreign investment which accrue to the investors and host countries has been and continues to be a controversial subject. Alice Teichova chooses to concentrate her primary attention on one aspect of the problem, namely on the dependency in production and marketing decisions created in the host country for those enterprises controlled by foreign investors (which may not be in the best interest of the host country); and also dependency of domestic enterprises on the verdicts of the international cartels regarding the volume of production, trade, and prices of those commodities subject to cartel agreements. Throughout the book there is an underlying assumption that foreign capital and cartel connections are fundamentally detrimental to the host country, manifested in sentences such as «... foreign investors regularly skimmed off almost the whole net earnings...» (p. 88), remarks about the "infiltration" of capital, and explanation of events in terms of the "competitive struggle" between national bourgeoisies. National governments are assumed to represent primarily the interests of big businessmen and bankers in their policy objectives.

It is impossible in a brief summary to give an idea of the amount of encyclopaedic information contained in the book. The book is mainly a descriptive narrative and is most interesting where it provides a clear insight into the relationship between foreign ownership and the control of decision-making (as in the case of the manufacture of explosives where foreign companies refused to go along with the Czechoslovak government's strategic policy in the years preceding World War II) or where it describes changes in the origin of foreign ownership as related to general economic and political changes in Europe. On the whole, however, the overwhelming amount of detail makes it difficult for the reader to maintain continuity or to gain a sense of the general thrust of the book.

The initial discussion dealing with a general survey of international investment in the interwar period is followed by a chapter presenting the author's estimates of direct foreign investment in Czechoslovak joint-stock companies in industry, commerce, transport and banking in 1937, and by chapters focusing on the three industries which absorbed close to 80% of foreign capital invested in industry: mining and metallurgy, machine building, and chemicals. However, joint-stock companies to which the author limits her investigation, represented only a part of Czechoslovak industry. There was also an important non-incorporated business sector which operated mainly with domestic capital and which embraced as much if not more capital assets than the joint-stock companies (an empirical judgement derived from data on profits of incorporated and unincorporated enterprises). Her generalizing from foreign control of some important joint-stock companies to foreign control of Czechoslovakia's industry cannot, therefore, be taken at face value. Nor can the author's assertion that « foreign investment was able to exert a direct influence on about half of the basic nominal capital of the Czechoslovak economy » (p. 41). Among other things, « direct influence » remains undefined in terms of any observable phenomena.

The book is written from the viewpoint of a historian rather than an economist. The absence of a macroeconomic framework precludes any meaningful conclusions as to the impact of foreign investment on employment, production, and trade. The author treats materials in isolation without establishing essential relationships with other available data, so that her conclusions are not always well founded. For instance, she argues that dividends transferred abroad by the Vitkovice Metallurgy Works in 1937 of 80 million crowns amounted to a « significant part of the Czechoslovak national income » (p. 88). In 1937 the Czechoslovak G.N.P. was over 70 billion crowns so these dividends amounted to about one tenth of one percent of the G.N.P. The balance of payments shows that in 1929 the total transfer of private dividends and interest abroad represented less than one percent of the G.N.P. and if account is also taken of such revenues

flowing into the country, the net outflow was insignificant. These relative magnitudes remained very much the same during the "thirties".

The book is replete with superficial and unsupported generalizations too numerous to list. Particular problems of inference arise because most of the book refers to the period from 1930 to 1937 which represented an abnormal period of dismal economic performance in most European countries, a marked increase in trade barriers, a decline in multilateral trade, and a mushrooming rise in cartel activity. Generalizations based solely on this period (especially concerning cartel agreements and their impact on the Czechoslovak economy) do not necessarily hold for the entire interwar experience of Czechoslovakia. In addition, it is not always clear which period the author has in mind, e.g. she mentions « decreasing import figures » (p. 184), but Czechoslovak imports of iron and steel products in fact doubled between 1925 and 1929 and declined only in the depression years.

The book also suffers from some serious factual errors, of which one example must suffice: Alice Teichova writes that after 1918 Czechoslovakia had « a shrinking demand on the domestic market and diminishing export opportunities . . . » (p. 25). However, between 1920 and 1929 Czechoslovakia was among the fastest growing countries in Europe with an annual rate of G.N.P. growth of 6% and an annual rise in the volume of exports of 11%.

Finally, economic terminology is used extremely unrigorously and the language abounds with vague expressions, such as « deepening of contradictions », « intensified penetration » which are used in place of careful analysis. Tables are not always adequately labelled and the sources of data cited are often secondary without any evaluation by the author as to their reliability.

Alice Teichova has extensively researched the literature on the subject she presents, but the lack of analysis and the extensive narrative makes the book of limited usefulness to the general student of European economic history. Nevertheless, it contains a great deal of information of interest to specialists on enterprise histories.

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I. TURNAU, *Skórnictwo odzieżowe w Polsce w XVI-XVIII wieku* (The Leather Clothing Handicrafts in Poland in XVIth-XVIIIth Century), Ossolineum, Włocław, 1975, pp. 198.

Irene Turnau's monograph caters for the specialist in the economic history of Poland, but is a valuable addition to the growing literature dealing with Polish handicrafts and commerce in the fullest period of development

of the Polish economy in the 16th century, and its decline in the 17th-18th centuries. The author has concentrated on the technical aspects and organisation of Polish leather-clothing production, shoemakers, fur-coat and fur-cap makers, trimmers, purse and belt makers, glovers etc. After analyzing the main sources (rich archival and iconographic materials) the author deals with the supply of raw materials and the production of raw hides, skins and pelts, as well as the supply of imported skins and furs (Poland was a centre of the highly-developed international trade, with skins and furs coming from Scandinavian countries, Russia etc.). Estimates for the 16th century show that the quantity of raw hides produced in Central Poland amounted to 2.10 units—over 3 kg per head per annum and the situation in the second half of the 18th century was only slightly worse. These figures are calculated from fragmentary statistical materials, nevertheless they show both the production and consumption of the inhabitants of the region. The most important part of the book explores the productivity of various branches of the Polish leather industry in the 16th-18th centuries. The author's main task is to estimate to what extent the needs of the population of Poland were met by the supplies of foot-wear and leather-clothing. In the second half of the 16th century in most of the Polish territories, Great Poland, Little Poland, Mazovia, there was on the average one shoemaker's workshop per 499 inhabitants, but only one furrier's workshop per 2,568 inhabitants; shoemaking was evidently the most wide-spread leather handicraft in Poland, and these workshops were scattered all over the country, in both towns and villages. The author calculates that on the average at least one pair of shoes was available per head per annum (if children and old people are omitted the figure is two pairs), and underlines that these figures are minimum due to the lack of sources. Turnau uses interesting methods to provide as broad a picture as possible of output and consumption in the Polish Commonwealth, and makes it clear that the scale and technical level of the Polish handicraft industry have been underestimated by earlier scholars.

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T. S. WARD, *The Distribution of Consumer Goods, Structure and Performance*, University of Cambridge, Department of Applied Economics, Occasional Paper 38, Cambridge: Cambridge University Press, 1973, pp. xii, 236.

The rapid spread of large retail outlets and self-service trading during this century has formed one of the more visible and important technological changes on both sides of the Atlantic. The phenomenon of the large depart-

ment store is, of course, by no means new, but together with the supermarket innovation, characterized by the reliance on customer self-service and initially confined essentially to the sale of food products though subsequently extended to include a wide variety of non-food items, has persistently eroded the market shares of smaller specialized stores. Both of these developments have been spurred by rapidly increasing consumer mobility, which increased market size and the elasticity of demand faced by existing stores, and hence also the pressure on profit margins in attempts to maintain market share.

In this volume, the author examines some of the economic issues arising from structural changes in the distribution of consumer goods in the light of British experience over the years 1954 through 1970. The study is divided into two parts. The first concentrates on the changes in labor productivity in the retail trades, while the second is concerned with changes that have occurred in the distribution structure at both retail and wholesale levels; particular attention is given to the effects of the abolition of resale price maintenance (R.P.M.) and the introduction of the Selective Employment Tax (S.E.T.).

Ward devotes Part One of this study to an examination of the three principal explanations of productivity change that have appeared in the literature. These explanations emphasize, respectively, the use of labor saving technology in the store, efficiency gains from a reduction in the number of stores, and apparent gains in efficiency due to a reduction in the amount of service provided. Tests of hypotheses are made with the help of both time series and cross-sectional regression analysis.

The author first reexamines the explanation of total employment changes in the retail industry over the period 1954-65 via a variation on a model used by Reddaway.<sup>1</sup> The results are consistent with a slightly less than unitary elasticity of employment with respect to sales volume associated with a time trend in productivity and a secular growth in sales, combined with a cyclical variation in sales per employee. This last factor is explained in terms of labor market tightness, measured by the level of unfilled vacancies, but is also explicable on the basis of a lack of willingness on the part of management to hire new workers until sources of additional labor input such as overtime working have been exhausted.<sup>2</sup> A labor demand interpretation of these cyclical factors would seem more in line with the emphasis placed elsewhere on the S.E.T. as a fixed cost of labor.

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<sup>1</sup> W. B. REDDAWAY, *Effects of the Selective Employment Tax, First Report*, H.M.S.O., 1970.

<sup>2</sup> Because of the lack of reliable data on hours worked, the author explicitly relied on employment data as a measure of labor input. Ward recognizes the important implications of this approach, which, in fact, makes a definitive interpretation of the cyclical variables in the regression almost impossible.

For the six years 1965 through 1970 following the introduction of S.E.T., Ward finds his model consistently over-predicts employment, reaching an error of over 12 per cent in 1970. The remainder of the chapter is devoted to possible explanations of this result — principally the effects of S.E.T. and the end of R.P.M. No attempt is made to measure the statistical significance of the phenomenon described, so it is not strictly clear that there is anything to be explained.<sup>3</sup>

In the remainder of Part One, census data is used to perform cross-sectional analyses by both product and town for the years 1957, 1961, and 1966. The author finds that productivity increased most in those trades which experienced the largest growth in sales over the decade, and thoroughly dissects several possible explanations of this result. A study of the changes in efficiency by type of outlet (independents, cooperatives, and multiples or chain stores with over 10 units) suggested that while structural change had some effect, productivity increases could be most positively ascribed to the spread of self-service trading.

Intertown variations in sales per employee are searched for a variety of influential factors, including town size as measured by gross retail sales, the market share of multiples and cooperatives, income per head, and population. Again the possible relationships are set out with clarity and detail but, as throughout the book, the models used are rudimentary, generally amounting to little more than a series of simple correlations, and one has impression of a host of multicollinearity problems waiting to be unmasked. The test of a properly constructed model which accounts simultaneously for all relevant independent explanatory variables might show revealing differences in the values and significance of the parameters from those presented in this work.

Part Two of this volume is concerned with recent changes in the structure of distribution in Great Britain, encompassing both the retail and wholesale levels, and stresses both the effects of the erosion of R.P.M. and the interdependence of the structures of the retail and wholesale trades. Six commodities are discussed: tobacco, confectionery, domestic electrical appliances, hardware, carpets and pharmaceutical preparations. This section is descriptive, but written with the author's usual attention to organization and clarity.

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<sup>3</sup> As in GREGORY C. CHOW, *Tests of Equality between Sets of Coefficients in Two Linear Regressions*, « *Econometrica* », 28 (July 1960), pp. 591-605. The general criticism of the presentation of the regression results might be made here that neither those significance levels of the estimated parameters nor, in many cases, the degrees of freedom for the estimate, are given explicitly. Either one or the other would facilitate a more informed reading of the results.

