along the route Pettau (Ptujs)-Cilli-Laibach (Ljubljana)-Görz (Gorizia), although Vienna continued to be used for cattle to South Germany. Between 1577-83 over 120,000 cattle passed the south-west Hungarian customs posts on the way to Italy. The new wealth in cattle-ranching replaced the old wine staple. But once again, by 1600, Inner Austria was living off a Hungarian export handling trade, just as a century previously.

Laszlo Makkai devotes a whole article (pp. 483-506) to the Hungarian cattle-trade between 1550-1650, which he sees as the most important sector of Hungarian export until mid-eighteenth century. Crudely speaking, for this period — for Poland, read grain: for Hungary, cattle. Makkai estimates that over two million beef-cattle were reared on the Hungarian plains in the second half of the sixteenth century, of which five to ten per cent (about 100-200,000 head) were exported annually. The European (notably Croat) cow-boys drove them mainly to the markets of the West, via Bohemia, Moravia and Austria to Germany and Italy, where Vienna, Nuremberg and Venice acted as the chief markets and slaughter-houses.

Pavel Horvath retraces the South-Slovakian export route (pp. 507-513), and Miloslav Belohlavek reckons that up to 20,000 head of cattle a year passed the customs post at Pilsen (Plzen) in Bohemia on the way to Nuremberg in Franconia (pp. 514-524).

Most of the articles contain new material of a descriptive sort, as very briefly indicated above. They are of great importance to a preliminary understanding of east-central Europe to those of us who only have western languages. For east-central Europe greatly helped to feed the cities of the West and Centre of the Continent, also providing considerable mineral wealth that could keep a ring of German, Austrian and Italian towns markedly wealthy. Two articles of interest in an analytical sense that may be further noted by way of an ending are — Joseph Vozar (pp. 569-583), "The social consequences of mining for the population of the middle-Slovak region in the sixteenth century," and Franz Lerner (pp. 147-184), "The Imperial town of Frankfurt-am-Main and its fairs in relation to East and South-east Europe, 1480-1630."

This is an important introductory volume for the general student of early modern Europe, but the Editor has done little to help readers find a coherent way through it. There are no clear themes and each article has to be read as if it were written almost independently in a regular journal. But perseverance will be amply rewarded.

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Stone's English Aristocracy

Lawrence Stone. — *Family and Fortune: Studies in Aristocratic Finance in the Sixteenth and Seventeenth Centuries.* Oxford: Clarendon Press, 1973.

The author who gave us our noblest study of English aristocracy now offers us an engaging new book, *Family and Fortune,* as a "coda" to his earlier volume. The new book "focuses exclusively upon financial history, and... organizes the material chronologically around individual families, rather than analytically around general themes" as its predecessor did. It offers case histories "to demonstrate the practical working of the general theories" of the earlier work and believes its results support " the fundamental findings of the former book" (*Family and Fortune,* pp. xv-xvii).

The work which stands *in loco parentis* to this new volume is a study of vast scope and deep learning, attempting nothing less than the description of the total environment of the English aristocracy in the century leading up to the modern English Civil War. "Economic, political, legal, social and intellectual" developments are all included. The description is undertaken not for its own sake, however, but in order to investigate a key political problem: how to account "for the breakdown of monarchical and aristocratic government in 1640-1 and its reestablishment on terms in 1660 and 1688" (*Crisis*, pp. 17, 197). This breakdown of traditional modes of government occurred during a period in which English society as a whole underwent a basic transformation from a "medieval" into a "modern" state, so that the strains of this change form the context for Professor Stone's study of the English aristocracy. Indeed, it is the reflection of these strains in the economic, the political, the social and other aspects of the aristocracy's activities that constitutes the heart of the analysis of *The Crisis of the Aristocracy*. If one can explain these aristocratic difficulties one has explained an important part of what made the Civil War possible. Stone analyses the total environment of the élite, therefore, to uncover and examine the nature of the crisis in its affairs which will explain the temporary eclipse of its dominant political role in society. To this end he bends all his investigative and analytical talents and all his great learning. In its service he has combed aristocratic account books and scoured aristocratic archives for evidence on all aspects of noble life during his chosen period. In the process he has made himself an unchallenged master of the circumstances of that life. In the event he has written a massive interpretative study outdistancing all its competitors both in the range of issues it considers and in the depth and variety of learning with which it treats them. The scale of the book's ambition and accomplishment set it apart in the contemporary literature of the subject with which it deals.

Family and Fortune is cast in a less heroic mould. It confines its scope to the finances of a few peerage families — the earls of Salisbury, Rutland, Southampton, Suffolk, and the lords Berkeley — in the sixteenth and seventeenth centuries, with an occasional excursion into the eighteenth. It would be impertinence in anyone but the author of the two books himself to suggest that the new volume might be used to test the "fundamental findings" of the earlier one. *Family and Fortune* talks family finance pretty exclusively. It considers "fertility, marriage, court service, conspicuous consumption, estate administration, debt, legal provisions, and industrial and commercial entrepreneurship," all as they affected the finances of the fortunate families included in the study.

The book falls into two major divisions, the first dealing with the Cecils and taking up rather more than half the total length of the volume, the second dealing with each of the other four families. This inequality of scale in the treatment of the families reflects the state of surviving archives rather than any judgement on the importance — political, social, or economic — of the families concerned: the Howards receive scarcely more extensive treatment than do the Berkeleys.

Each family estate and its incumbent made their own way through the variety of forces Stone has selected for special consideration. Sir Robert Cecil's success in creating his family estate is bounded by the economic perquisites of political service to the court, including the profits from the silk farm, balanced against the imperatives of conspicuous consumption, including the construction of Hatfield House. His son, the second earl, maintained his solvency and style with the help of an efficient estate administration at work developing the family's London land, as well as surveying and keeping "efficient records" of the country estates. Political upheaval, a category not included in Stone's initial list, played an important, and uniquely quantifiable, part in turning the tide of prosperity on these estates. A family demographic crisis after 1660 deepened the difficulties. "The root problem of the Cecils in the late seventeenth century was how to provide for widows and the horde of younger children produced by three generations of exceptionally fertile wives" (*Family and Fortune*, p. 152). It was, as the reader will know, a problem that the Cecils solved — albeit slowly, and beyond the period here considered in detail. Marriage launched the Manners family into the sea of riches, monastic lands kept it afloat through the 1540's. Careful estate management at home by one earl, the most extravagant expenditure abroad by another, successful exploitation of the iron ore at Rievaulx, and a divided inheritance, determined the fortunes of the Manners' estates into Stuart times. Marriage with an heiress and the failure of male heirs in the direct line reshaped the estate thereafter. The tales of the three remaining families are told more quickly. Office and spendour enriched and established the Wriothesleys; splendour and political opposition laid them low, long lived dowagers adding to the strain on revenues. London property and energetic industrial development helped to salvage the family. Office, extravagance and "biological accident" raised and ruined the Howards. Personality emerges as the dominant factor playing on the shape of the Berkeley fortune in this period. Good estate administrators and heiresses in an hour of need helped to sustain the family.

The book is not concerned exclusively to measure economic quantities; occasionally it turns aside to describe human qualities. The great earl of Salisbury, sick unto death and fearing for his soul while suffering in his body, hears his chaplain exhort him to thank God for his mercy in granting him this period of suffering in which to repent his sins, instead of striking him down in "full criminal career" as he had King Herod before him. Perhaps the king's musician, who also attended him, offered more soothing service. A young lord Berkeley seeks escape from prodigality in his mother-in-law's household at 17s. a week. Frantic as their children mysteriouly die, the earl and countess of Rutland seek the cause in the witchcraft of a discharged servant. The unfortunate woman pays with her life for their despairing superstition.

The brief outlines of the scope of each book make it apparent that *Family and Fortune* offers something other than, and less than, the opportunity to explore the fundamental propositions argued by its predecessor. It offers instead an opportunity to compare its carefully constructed and documented analyses of the economic activities of selected families with the broader, generalised treatment of the same families within the analytical framework of *Crisis.*

One area in which such a comparison can be made is that of gross rentals. *Crisis* estimates the gross rental incomes of all the families in the peerage for three different years, 1559, 1602, and 1641, and summarizes the results in tables (*Crisis*, Appendix VIII) illustrating, and demonstrating, its thesis that the peerage suffered an economic crisis between 1558 and 1641, with the 1590's probably representing the turning point in their fortunes (*Crisis*, p. 327). *Family and Fortune* provided accounts and explanations of rentals for several families which we can compare with the figures and explanations for the same families in *Crisis.* Such comparisons reveal several distinctions in approach between the two books. In the first place, *Family and Fortune* puts into practice a rule of method preached, but also breached, in *Crisis* — to wit, that "attempts to classify individual peers or gentry by the numbers of manors they held are bound to fail" (*Crisis*, p. 146). The new book nowhere uses manorial counts to provide data for computing landed income, while the earlier one relied on this method for fully one-fifth of its peerage income estimates in the 1602 survey (*Crisis*, p. 139, n. 1): the "hinge" survey as far as the thesis of the peers' economic decline and recovery is concerned. The new study also raises questions about the earlier work's calculations of gross rental figures for several of the peers in the 1559 survey. While *Family and Fortune* offers no figures for the incomes of the earls of Rutland, Southampton, or lord Berkeley in the years near to 1559, its discussion of the rental evidence for these families at this time questions the figures that *Crisis* presents for the same families. *Crisis*, without citing its source, attributes to lord Berkeley one of the eight largest gross rental incomes in the country in 1559. The new book states frankly (*Family and Fortune*, p. 245) that "we do not know" what Berkeley's gross rental in this period was. Calculations of Wriothesley's income in *Family and Fortune* suggest that the *Crisis* figures for his income in 1559 derive from an unreliable valuation of the estate made in 1550 for the court of wards (*Family and Fortune*, p. 211); a similarly full discussion of

Ruthland's finances (*Family and Fortune*, p. 171) leads us to conclude that *Crisis* based its figures for that family's income in 1559 on a Crown survey of 1563, a survey "which admittedly may omit a few items, and probably undervalues those it includes." The two books' treatment of the rental income of the Cecils raises a different query: whereas *Crisis* p. 408) declared that 20% of the earl's gross rentals in 1612 came from leaseholds held of the king and Establishment, *Family and Fortune* nowhere in its apparently exhaustive enumeration of the earl's important sources of revenue documents them. The New Exchange land, on *Crisis's* principles of reckoning (*Crisis*, p. 150), should be considered freehold land.

Three points may, perhaps, be made without pursuing further these comparisons of rentals as handled by the two books. First, the new book does nothing to allay the criticism, and may be seen partially to confirm it, regarding the usefulness of manorial counts as a source of quantifiable statistical data. Secondly, it must be clear that the usefulness of the estimates of individual incomes in Appendix VIII of *Crisis* would have been considerably enhanced by systematic descriptive references to the accounts used to construct them, glimpses of which we now appear to be given for a few cases dealt with in the more recent book. The third point, raised by the comparison of the handling of the 1612 Cecil income by the two studies, suggests the difficulty faced by even a master of aristocratic accounts when he sets himself the task of uniting the multifarious data on a great and complicated estate into a complete, coherent individual case history.

Family and Fortune also furnishes evidence which can be used to test the argument in *Crisis* that large rental rises on selected pieces of land demonstrate a generalized swing to high prosperity on most great landed estates. *Crisis* chose as its sample for the purposes of analysis parcels of land on seventeen estates which remained parts of their respective estates throughout the period of the survey; that is, the rentals used in the survey refer to the same land, on the same estate, at the start and at the conclusion of the survey. The beginning dates for the survey vary on the various estates, from as early as 1566 to as late as 1631. From the rental rises observed on these seventeen samples Stone concludes that "on most estates rents at least doubled between about 1590 and 1640" (*Crisis*, p. 327 and Appendix XVI). Evidence in the new book suggests that, for some estates at least, this index of rental rises on selected lands is a very inaccurate guide to the level of overall increases in estate income. The Manners estates furnish an example in point. *Crisis* (p. 772) sets the rise in rentals on lands in Lincolnshire and Leicestershire remaining in the family between 1608 and 1658 at 80%. *Family and Fortune* (p. 206) in a more detailed and comprehensive consideration of the Manners' landed income, documents violent fluctuations in their income levels during this period, from an average of £4733/a. between 1653-6 to an average of £11,158/a. between 1657-60, neither figure giving a rise in overall income of anywhere near 80% when compared with the £5151 income cited for the year 1608-9. The new table strongly suggests that the rising rent levels contain at best a limited relevance for the problem of whether or not the earls succeeded "in increasing their landed incomes to keep pace with the price revolution" (*Crisis*, p. 324). The great ebb and flow of land out of, and into, their estate was decisive in this respect. The new table provided for the Howards (*Family and Fortune*, p. 295) similarly emphasizes that the family's ability to maintain its style of life — indeed here to create that style — depended primarily on the flow of land and favours into the family, and only secondarily on the increase in rental yields on the land the family held. The *Crisis* figure of a 433% increase in that rental yield betweeen 1576 and 1641 presents its own problem, apart from its inadequacy as an index of increasing Howard income. Such a gigantic figure raises the problem of that change "in the balance from fines to rents" of which, in general terms, Stone has warned us to beware.[1] As it stands this figure cannot serve as a useful index of increased rentals, even

[1] *Family and Fortune*, p. 293, speculates on the reasons for the increase.

for the land which produced the accounts used to calculate it. *Family and Fortune* provides an extensive rental account of the earls of Salisbury which again challenges the argument that the rental increases on the sample properties stand as a reliable guide to overall increased estate income. The figure for the increase from the fixed Cecil landholdings is 80% (*Crisis*, p. 772), the figure for the increase from their whole estate over the same period is 48% (*Family and Fortune*, p. 142). Where the two books enable us to make the comparisons, they demonstrate that the increases in rental income on the selected parcels of land are not an accurate measure of the movement of income on the estate as a whole. At best the sample's figures indicate the direction, not the magnitude, of the movement of rents. In a period of price inflation magnitude matters greatly.[2]

On the vexed question of the use of manorial counts to measure rise and decline in the real wealth of the élite *Family and Fortune* disappointingly adds nothing to the debate. It avoids the problem by avoiding the practice. Stone was driven in *Crisis* to adopt the counting of manors as the only source of evidence which could provide convincing figures for a statistical description of the shifting patterns of landholding which he needed to measure the long term economic health of classes in society. The importance of this evidence to Stone's argument is matched only by the misgivings over the method used to obtain it. Because the new book ignores this ambitious, contentious and crucial dimension of the statistical argument of its progenitor I would like to turn briefly to the consideration of a book which approaches the problem from an important, and insufficiently considered, perspective, E. A. Kosminsky's *Studies in the Agrarian History of England in the 13th Century* (1956).

Kosminsky, on the basis of unrivalled research in thirteenth century patterns of landholding, argued that substantial land sales by the generality of the greater landlords of the period constituted a normal part of their estate activity. As neither he nor others considered the thirteenth century to be a period of crisis for great landholders, he was faced with a pattern of abundant land alienations without crisis in landed fortunes. The evidence from his own research led Kosminsky to conclude that, for the thirteenth century at least, alienability of land was a prevalent practice and not an emergency expedient among freeholders. Inalienability of land, in theory and practice, constituted a peculiar burden of unfreedom, a limitation imposed on villein tenures by manorial administrations for their own convenience (*Studies*, p. 224). Vigorous selling of their freehold estate did not, in the thirteenth century, connote crisis among the baronage.

Kosminsky's evidence that the magnates sold land widely during a period of prosperity insists that the evidence of land (manorial) sales needed to demonstrate an economic crisis of the aristocracy must be comparative evidence: the volume of its sales in one period must be compared with the volume of its sales in another period for which the nature of the evidence is roughly similar if it is to be used to judge the economic well-being of the elite. How convincingly does *Crisis* handle this comparative approach to the evidence of manorial sales it presents? From the first comparison, between the number of manors held by the aristocracy in 1558 and in 1641, Stone concludes that the 1558 peerage held 10% more manors than the 1641 peerage, with a +10% margin of error (*Crisis*, p. 152). In other words, it is possible that there may have been no change between the number of manors the peerage held in 1558 and the number it held in 1641. Next,

[2] The conclusion that the sample shows a doubling of rents even for the lands in the sample is open to argument. The figure seems to have been computed by adding up the percentage increases in rental income on each of the parcels examined, without taking into account that the figures for the same property at various times are not in all cases comparable. Specifically, the figure of the 433% rise on the Howard estates has simply been addded in, "unimproved," along with the other sixteen figures, to arrive at the average figure for rental increases. To include in such a small sample a figure so out of line with expectations and most of the other figures is to come perilously close to invalidating the result.

Crisis selects for analysis the manors of a group of families who survived through the whole period between 1558 and 1641. A "very accurate" calculation of the numbers of manors they held in several counties plus a "fairly accurate" one of manors they held throughout the country suggests that this peerage sample in 1641 held 59 +10% of the manors they had held in 1558. Sales by these families were at their peak between 1585 and 1606, "during which time the net losses were so alarming that one may reasonably talk about a financial crisis of the aristocracy" (*Crisis*, pp. 154-8). But while the comparative evidence of manorial sales for this particular sample suggests economic crisis from 1585 to 1606, or even to 1620 (*Crisis*, p. 198), other evidence in the book refutes the suggestion. From figures for the rises in rents on a different selection of lands, Stone concludes that the period from 1590 to 1640 "must have been an exceedingly prosperous" one for landlords, aristocrats included (*Crisis*, pp. 327-8). Such mutually contradictory handling of the statistical evidence cannot encourage confidence in the conclusions the statistical comparisons are designed to prove.

In another area of the comparison of the pattern of landed transactions in different periods of time the discussion in *Crisis* also raises problems. This concerns the role of law and custom in stimulating or inhibiting the turnover of land on the great estates. Stone argues that from the *De Donis* statute of 1285 onwards land alienation had become more difficult for the possessors of the greater estates for two reasons. *De Donis* gave legal sanction to the conditions donors imposed on donees in conditional grants of land and led directly to the development of entails as an effective means to prevent the selling up of the family's estates. This legally enforced immobilization of entailed land remained effective until a judicial decision in 1472 in Taltarum's case opened the way to owners who wished to alienate land to do so, in spite of earlier stipulations inhibiting such actions. At the same time that the legal restraints on land sales were crumbling the "moral" inhibitions over "the dismemberment of the family patrimony" were also losing their force. These changes were "one of the most important of all causes for the extraordinary activity of the land market at this period," activity which marked the period as one of distinctive crisis for the aristocratic estate.[3] This argument presents several difficulties. Stone first substitutes statute prohibition of a particular type of transaction for other (statistical) evidence that the law effectively presented that kind of land transaction between 1285 and 1472, and then chooses an interpretation of the legal evidence that specialist opinion scantly supports. Holdsworth and others argue that, far from representing novel doctrine or law, the decision in Taltarum's case demonstrates a device to get around entails which was already "in full working order." The case illustrates, rather than creates, the ability of those possessing entailed land to defeat entail and hence to dispose of the land as they wished. Thus, on consideration of the only evidence advanced to support it, the thesis, that from 1285 to 1472 legal barriers effectively curbed land alienation, collapses. It also lacks confirmation from at least bits of other mid-fifteenth century evidence, notably from the Percy estates, where statute on entail has been found to be a misleading guide to the practical, if not the legal, devices open to powerful landowners for the disposition of entailed land. Finally, there is no evidence to indicate what percentage of land on the great estates was subject to entail between 1285 and 1472, even should the laws of entail be proven to have been effective during some part of the period.

We lack acceptable proof that the statute of 1285 seriously interfered with the great landowners' ability to alienate land. We have strong arguments that they both enjoyed and exercised that right before 1285. We are, therefore, left in doubt about the condition

3 *Crisis*, pp. 178, 181, 197. The argument on p. 181 that the period was exceptional for the granting of land away from the heir male is contradicted on p. 591 where it is stated that the sixteenth century marked the "extreme" point in the movement to reserve most of the property for the eldest son.

of the land market between 1285 and 1472. Uncertainty here creates uncertainty over the contrast *Crisis* draws between the operations of the land market in the fifteenth century and its operations later in the sixteenth. The contrasts based on manorial counts between 1558 and 1641 are inconclusive or contradicted by other evidence on the question of aristocratic crisis sales. With none of these issues resolved there can be no firm answers to the vital questions of what class-wide level of land transactions should be considered normal for the aristocracy and what level of such transaction should be deemed exceptional, a sign of crisis. Failure to establish a convincing standard of comparison by which to interpret movements in the land market between 1558 and 1641 is made more urgent by Kosminsky's finding of abundant magnate sales without baronial economic crisis in the thirteenth century. The comparative arguments Professor Stone presents to support his thesis of aristocratic economic crisis are unconvincing.

Kosminsky again speaks, while *Family and Fortune* is silent, on the difficult question of comparisons between different classes in the same time period, in this case on the problem of who gets the land that someone else loses. The question is important for *Crisis*, because the book's argument links the decline of the peers' economic power, in absolute terms and relative to that of other groups, to the shifting focus of political power in the period. *Crisis* handles the question of who benefits almost dismissively, declaring that few manors passed by sale from landlords to tenant farmers, so that "the property held by the middle and lesser landlord groups... must necessarily have increased" (*Crisis*, p. 161). The statement is unsatisfactory on three counts: it provides no evidence for its validation; it does not exhaust the list of established groups who might take up the property lost to the peers; and it ignores the possibility that the property might in fact be going to no established grouping in society. It is Kosminsky above all who alerts us to the necessity of considering this latter possibility. His analysis of the movement of property in terms of its beneficiaries as well as its losers reveals the creation, out of the breakup of large manors and the buying up of small-scale freeholds, of a new and hitherto undefined type of holding, the "submanor," and of a new and hitherto undefined type of landholders, a group, a class eventually, to be named "the gentry." Kosminsky's method is, for our purposes, his message. By investigating destinations as well as origins he has securely identified in economic events the seeds of societal change. Stone's exclusive concern with the origins of the property that came on to the market leaves his conclusions on the social implications of the sales much less convincing. They need not be so. The Victoria County Histories have the data needed to determine precisely who got the manors the peers lost. Indeed *Crisis* (p. 180) used this type of data on the minor question of how much land was alienated to peers' younger sons. To ask for even more analysis of property movements of a study which already gives so much may seem churlish. The point stands, however, that where conclusions consider the destinations as well as the origins of sale land, as Stone's do, they are secure only if they refer to the actual and not merely to the probable destinations of the land in question. Kosminsky's work brings home the practical implications of ignoring this theoretically impeccable proposition.

In *Family and Fortune* Professor Stone has narrowed his concerns to the economics of fortunes, thereby, as we have seen, sacrificing a primary dimension of the *Crisis* discussion. I, for one, regret this exclusiveness. *Crisis* vindicated its claim that patterns of economic behaviour are separated from patterns of cultural and political behaviour only with a loss in significance to both. The earlier book's social and political theses raise a host of questions one would like to see explored from the family perspectives of the new book. Is it not, for instance, the military strength of the aristocracy, rather than their weakness as argued in *Crisis*, that needs explanation in order to understand what made the Civil War possible? English aristocratic military obsolescence is after all at least as old as twelfth century scutage. Does the geography of the Civil War sustain the thesis that by 1620 the central government had succeeded in bringing the North and West into the national orbit? Does the political history of England between 1629 and 1660 bear out the contention that the House of Commons had become the dominant political body in the country? Had enough of the "old" landlord-tenant relationship really survived the vicissitudes of the fourteenth century to be further significantly undermined in the late sixteenth? Has *Crisis* defined the distinctions between "medieval" and "modern" with sufficient respect for the diversity of developments in the earlier period or their ambiguity in the latter? The new book turns its back on all these questions to which its parent addressed itself.

Family and Fortune, for author and reader alike, comes after *The Crisis of the Aristocracy*. It will probably be read more for what it says about its predecessor than for what it says about itself. While we must not take too seriously its author's claim that it tests the greatest arguments of the earlier book, we should be clear about what it does in fact accomplish vis-à-vis *Crisis*. It provides an independent treatment of statistics, many of which also appear in *Crisis*, which enables us to reconsider a number of the specific statistical propositions of *Crisis* where they employ data involving the families that the new book investigates. Where we can carry out these comparisons the results do not encourage us to treat the earlier statistical arguments as settled; indeed, they would seem to reinforce the conclusions of several of the earlier book's reviewers that the case for an economic crisis and recovery is not proven. Once this has been said it is necessary to emphasize once again that the importance of *The Crisis of the Aristocracy* does not depend, alone or most importantly, on the correctness or the proven correctness of its thesis of economic crisis. *Family and Fortune* takes up the argument of *Crisis* at one of its least convincing points and does little to strengthen it. The new study should not for that reason detract from an appreciation of the central merits of the earlier book. Those merits were, and are, that it brings to the question of how social change may help to explain the Civil War a breadth of vision and a depth of specialist learning which are unique among recent interpretations of the "causes" of the mid-seventeenth century conflict.

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Wilfrid R. Prest. — *The Inns of Court under Elizabeth I and the Early Stuarts 1590-1640*. London: Longman, 1972.

Those of us who had used Dr. Wilfrid Prest's work previously were very pleased at the appearance in 1972 of his *The Inns of Court under Elizabeth I and the Early Stuarts, 1590-1640*, a work which grew out of his valuable D.Phil. thesis. Members of the four inns of court, both those who became barristers and those who passed through these societies untainted, or relatively untainted, by the legal education available there, appeared in almost every walk of Tudor-Stuart life. Therefore an understanding of the life and workings of those institutions and their relationship to English society is very important for an understanding of the period. This Dr. Prest provides. In ten chapters he discusses the structure of the inns, their membership and administration, the education attained there, the cultural and religious life of these societies, and the role they played in the events leading up to the civil war.

The inns are never seen as isolated from society. They were, after all, surrounded by the bustle of London life and the activities of the royal court and parliament. Dr. Prest connects, in most suggestive ways, the trends he finds in the inns with those found in society in general. Thus, for example, the rebellions in the inns against their governors, the benchers, are seen as examples of a difference in attitude which separated the generations, a difference which has been traced in other institutions at this time, most importantly in parliament. Again, the decline in violence among the members of the inns is