

Thus what we have here is a collection of islands, some high, some low, some with rather interesting topography, and some whose landscape is rather legalistic and bleak. Each could have served the author—and the scholarly and legal community as well—as a contribution to a particular legal journal. For example, I cannot help but think how much the *Journal of African Law*, oftentimes marked by a thinness of contributions, might have benefited had the pieces on African law been printed there instead.

After all, there are few comparatists who dare to range over such a wide terrain, or even admit that they try to, especially in such an important but restricted subject as family law. Obviously, the presentations found in this volume can inform the small, hardy band that works these fields, but even within the band the audience that is better served by a book of such isolated and far-flung treatments of particularistic law is very small indeed, particularly when no effort is made to be comprehensive or to draw the pieces together. The price of the volume, which is \$ 12.50, suggests this and underlines, in my opinion, the desirability of using other modes of publication. Wider readership and thus the diffusion of knowledge would be achieved, I am sure.

This, then, while there is meat for comparatists, is not a comparative study except as individual authors rather briefly compare systems within their respective contributions. But one might have expected, I would assume, that whatever legal links exists between Asia and Africa—for example, in the *adat*—would have been explored by one speaker. The title of one piece promises much, “Family Arrangement in Developing Countries”, and delivers only India—quite enough, you may say, but my hopes of finding something of value to the more general comparatist were falsely raised.

Even if I were to accept the framework of the conference and the volume, I am also disturbed by the contrast afforded between the article by Professor Z. W. Falk of Tel Aviv on “Religious Law and the Modern Family in Israel” and those of most of the other authors, excepting Professor Anderson’s treatment of “The Eclipse of the Patriarchal Family in Contemporary Islamic Law”, Professor Allott’s piece on “Family Property in West Africa”, and Mr. Irani’s contribution on “The Personal Law of the Parsis of India.” That is, the lack of discussion of the cultural-historical, sociological and economic context within which the marriage relationship subsists. Falk’s article is a mine of riches in this respect, and I found myself totally engrossed in it. By contrast, too many of the contributions are rather dry recitations of provisions of the Law, giving the impression that they were written at some distance and for practitioners and civil servants rather than for scholars. It is rather too late to analyze marriage as a contract, in terms of rights and duties of the parties and of the property consequences which flow from it.

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R. G. Saylor, *The Economic System of Sierra Leone*. Publication 31, Duke University Commonwealth Studies Center, Durham, Duke University Press, 1967, pp. 231. \$ 10.00

This book is a revised Ph.D. dissertation, hatched—the author tells us—in

Professor Calvin Hoover's seminars on economic systems at Duke University. The book brings together some scattered statistics—on export crop production, foreign trade, mining employment and wage rates, government budgets and and similar matters, though it is hardly packed with statistical series; Sierra Leone is not well-endowed statistically. Mr. Saylor gives us a general economic survey of the Sierra Leone economy, punctuated by several vigorous criticisms of government economic policy, notably agricultural price policy and related activities of the Sierra Leone Produce Marketing Board.

Since so little is written about actual economic policies in developing countries, these parts of Mr. Saylor's discussion are useful. Overall, however, there is not much that is new here. And while Mr. Saylor does a creditable job of analysis, the volume raises certain general questions about research priorities and techniques, questions which need airing.

The first concerns the suitability of country studies for Ph.D. dissertation research. It is terribly hard to write good country studies, especially where not much is known to begin with. No matter how long such surveys are, (and this one isn't long), they can't avoid a once-over-lightly quality. To avoid this they need cogent organizing principles, well-defined points of view; and to the extent that they have these they lose their character of general surveys. Mr. Saylor tries to meet the problem by writing that his purpose is to analyze "the economic system through a time series analysis (mainly 1950–1963) of its major structural components—agriculture, mining, manufacturing and foreign trade..." And he indicates his intention to test some specific hypotheses in his sectoral discussions. In fact, however, the notion of "an economic system" is nowhere given substance in the book, nor is any attempt even made to spell it out. Specific hypotheses are mentioned only in the chapter on agriculture, and they considered too briefly and broadly to pass for "tests".

Given the inherent difficulties involved in saying anything very new or detailed in broad country surveys, it can legitimately be suggested that the time, energy and money that go into Ph.D. dissertations should be directed elsewhere. The intellectual community (and the countries concerned) need research of a basic, dirty-hands type, and this is the king of research we should urge graduate students to undertake.

A second question relates to the abuse of regression analysis. In one part of the book, Mr. Saylor does a regression analysis of the relationship between price changes and palm kernel sales. He wants very much to have the data show price responsiveness of palm kernel production. To this end he performs a number of simple regressions, analyzing the data for the years 1922–1963, broken down into three periods. The results, at first blush, are not convincing; palm kernel supply elasticities were very low (though positive) in the 1920's and 1930's and in the 1950's, they were actually negative, according to the raw regression (i.e., producers responded "perversely" to pure changes). Saylor remedies this latter inconvenience by deflating producer prices by a consumer price index. The results then come out o.k. In fact, the price index used as a deflator is nowhere described, though a footnote indicates that it is highly unreliable. He also uses some (extremely uncertain) data from Liberia to compare price responsiveness there, and finds it much higher. Finally, he relates palm kernel sales to a ratio of palm kernel to cocoa prices. This, he admits, "does some violence to reality since the two products are not

directly competitive in all parts of Sierra Leone. However, the crop areas are congruent enough to allow for meaningful comparison." This relative price measure "explains" a larger proportion of the variation in output of palm kernels than any other.

One can be charitable about this sort of analysis on the grounds that everyone does it these days, and it's supposed to be better to run the data through the machine than not do so. But it is grossly misleading to argue that this kind of statistical hanky-panky proves anything – in this case producer responsiveness. First of all, for two of the periods in question (1922-1929 and 1930-1939) the price data used are especially questionable (average wholesale export prices FOB), since radical changes in the transport system (and probably transport costs) occurred during these periods, making for uncertainties in the relationship between producer prices and average FOB prices in Freetown. In these periods, in any case, the degree of responsiveness indicated in Mr. Saylor's data is very low. Secondly, it is only by pulling a consumer price index out of the hat to deflate money producer prices for palm kernels that a positive response is found in the post World War II years. No deflator was used for the previous periods. Thirdly, the use of (very rough) data for Liberia to buttress a conclusion about Sierra Leonean behavior is obviously objectionable. Fourthly, the use of a palm kernel-cocoa price ratio, when the degree to which these two crops are alternatives on the input side is not known (and is probably low) is hardly meaningful. Finally, it is hard to see how any discussion of palm kernel supply can avoid dealing with the "disturbance" caused by the diamond rush of the 1950's, when – as Saylor tells us elsewhere – perhaps 50-100,000 men went after diamonds. It would be very surprising indeed if an event of this magnitude did not weaken the simple price-output relationship for palm kernels.

It is unfair to take Mr. Saylor to task for statistical adventurism when it is indulged in so freely everywhere. He after all gives only 15 pages or so to the regressions, and he does occasionally suggest that the conclusions are not all that firm. The trouble is that regressions take on a life of their own, and as they recede in time and space they gain in credibility. In later discussions of Marketing Board policy, Mr. Saylor's hesitations about the degree of reliability of the regressions seem to fade away; the qualifying adverbs are dropped.

I have perhaps been unduly harsh on Mr. Saylor, whose book is in fact, better than other similar efforts published in recent years. It's not his particular study so much as what it indicates about how we go about studying development problems which is the source of these grumbles.

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J. R. Crawford, *Witchcraft and Sorcery in Rhodesia*. London, Oxford University Press (for International African Institute). pp. 312. \$ 10

The magico-religious system of the Shona of Rhodesia for some curious reason has been studied and reported only by non-anthropologists: Michael