

countries as the "Free World" (Greece, Portugal, Rhodesia, South Africa, and Spain included), which does not befit an otherwise objective study. The overall conclusion emerging from the book is that the Soviet merchant fleet does not present any serious threat to other major maritime powers, even though aggressive tactics have been, and are likely to be, resorted to on occasion.

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CLASS AND SOCIETY IN SOVIET RUSSIA. By *Mervyn Matthews*. New York: Walker and Company, 1972. xviii, 366 pp. \$12.50.

Both for what it accomplishes and for what it leaves undone this volume should mark a turning point in Western studies of Soviet social structure. Drawing heavily on the rich harvest of Soviet sociological studies of the 1960s, as well as on census data and information in Soviet statistical handbooks, Matthews presents much of the available evidence on income inequality, differences in life styles, occupational prestige rankings, and social composition of students at different levels of schooling. Although this book was apparently completed before the author could make use of some very valuable recent Soviet studies of social structure (the works of Shkaratan, Arutiunian, Rutkevich, and Gordon published in 1970-72), Matthews's volume makes it clear that a firm empirical basis for the analysis of Soviet social stratification now exists.

Unfortunately the author is much more absorbed in presenting and summarizing the available data (120 tables worth) than in analyzing them. Although the compulsory reliance on "models" and "analytic frameworks" to organize statistical materials may sometimes be an academic pose rather than a source of illumination, the sheer presentation of evidence of Soviet social and economic inequality also has its limits. This is said not to denigrate the usefulness of Matthews's volume but to suggest that future studies of Soviet social structure may now fruitfully turn to questions which the author does not ask. Why has Soviet income inequality been markedly reduced in recent years? How is Soviet social stratification related to Soviet economic development strategy? How do Soviet occupational prestige rankings and mobility rates compare with those of other industrializing or industrialized countries? It is curious that when Western sociologists had to rely on data drawn from samples of Soviet refugees, they nonetheless confronted some of these questions, while the vastly more abundant data currently available have yet to be used for this purpose.

Although Matthews mainly focuses on social and economic inequality, the volume also includes a useful discussion of recent Soviet demographic trends, a summary of rural-to-urban migration studies, and a heroic attempt to estimate the magnitude of Soviet youth unemployment. The discussion of this last topic might have been enhanced by a more elaborate attempt to explain how both "voluntary" and "involuntary" unemployment of youth of similar skill levels can exist simultaneously on the substantial scale indicated by the author. This section might also have benefited from an attempt to reconcile the chronic Soviet complaints of a labor shortage with the author's characterization of youth unemployment as "very substantial."

Finally, it is difficult to accept Matthews's view that the Soviet admission of "important divisions" (of an economic and social nature) among workers would

shake the ideological foundations of the party (p. 51). The most fruitful Soviet sociological studies have focused precisely on intraclass divisions. It is undivided political loyalty rather than social and economic homogeneity that party ideology now seems to require.

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DIE LIVLÄNDISCHE KRISE, 1554–1561. By *Knud Rasmussen*. Kopenhagen: Universitets Slaviske Institut, Studier 1. Copenhagen: Universitetsforlaget i Kopenhagen, 1973. 241 pp. 2 maps.

This book treats, once more, the diplomatic history of the end of the Livonian state. The story has been fully told before. If the author nevertheless retells it without using other sources than those his predecessors used, he seems to feel the need for reinterpretation. For "nicht richtig" and "falsch" are termed certain interpretations of Möllerup, Hubatsch, Kirchner, Donnert, Jasnowski, Novoselsky, Engberg, Koroliuk, and Arnell! No reference is made to them (or others) when they may be "right."

Most of Rasmussen's objections refer to interpretations of details in the diplomatic developments. Thus Hubatsch is called "incorrect" with regard to Prussia's role; yet Rasmussen must confess that his own interpretation is only a hypothesis. Rasmussen at one point denies that Denmark's negotiations of 1557 were a "fiasco," as others have stated; yet he must, a few pages later, agree that they were a failure ("im Sande verlaufen"); and still later he himself uses the word "fiasco." According to Rasmussen, as opposed to others, the king of Poland did not seek to dominate Livonian internal affairs through the treaty of Poswol of 1557; yet Sigismund Augustus must have reversed himself quickly; for, as Rasmussen states correctly, two years later he claimed the Livonians as his subjects and within another year demanded complete submission.

Despite his "corrections," Rasmussen comes, as he must, to the same results as his predecessors; and they, in turn, will find no major fault with his conclusions, though they may reserve their judgments in some details.

More serious is Rasmussen's failure to take adequate account of the internal divisions in Sweden and Livonia. Having not perused the Burwitz report of 1555, he neglects the effects of these conflicts on Denmark, Poland, and Russia. Even if Gustavus Vasa undertook little, Denmark, even more than others, could not ignore the aspirations on Livonia which many Swedes entertained, including Gustavus's own sons. Nor can the policies of Master of the Order Kettler be treated as a mere continuation of those of his predecessor Fürstenberg, when Kettler's aims were diametrically opposed to those of Fürstenberg. Since Sweden, Denmark, Poland, and Russia each had sympathizers in Livonia, and since the burghers of Riga and Reval played a major role long before Rasmussen considers them (about 1560), the fact is submerged that the need to prevent another nation's conquest of Livonia was no less a motivation for each of the contenders than their own desire to seize it.

What, then, is the contribution Rasmussen's book can possibly make? Perhaps a few points: (1) Although generally the Russian attack of 1558 has taken the central place in the histories of the Livonian crisis, it may be useful to focus attention on the initiatives of the other nations. (2) Although Prussia's role was played