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Towards a Social Anthropology of the Mediterranean¹

by Jeremy Boissevain

THE SOCIAL ANTHROPOLOGY of the northern shore of the Mediterranean was born in 1954 with the publication of Julian Pitt-Rivers's *The People of the Sierra*. Since then, increasing numbers of anthropologists from both sides of the Atlantic have focused their attention on the area. They have produced a growing stream of theses, articles, and monographs. With few exceptions, they have dealt with single villages, thus tribalizing the societies, stressing diversity rather than similarity. Moreover, they have treated the Mediterranean Sea as though it were 1,000 miles wide: no more than four attempts have been made to examine anthropological problems in a pan-Mediterranean perspective (Gellner 1968, Wolf 1969, Schneider 1971, Boissevain 1976). For many reasons, therefore, the publication of John Davis's *People of the Mediterranean: An Essay in*

Comparative Social Anthropology (1977) is an important event. It is a milestone that marks the coming of age of a Mediterranean social anthropology.

People of the Mediterranean attempts "not only to review the literature of Mediterranean anthropology but to suggest ways in which it might be improved" (p. 10). Davis succeeds rather better with his first goal than with the second, but because he castigates all Mediterraneanists for their lack of controversy, because he jabs at several of the notables, including Pitt-Rivers, Wolf, and Gellner,² because he thumps, slashes, and, on one occasion, disembowels³ lesser lights, his book will also achieve its second goal as colleagues provide the data he calls for and take up his challenges. The best way to do justice to this erudite, useful, and provocative book is to discuss it vigorously.

¹ Preliminary versions of this paper were discussed with the SSRC seminar group on anthropological research in industrial societies, with particular reference to Western Europe, University of Sussex (September 24, 1977), with the University of Malta's social studies research seminar (October 13, 1977), and with John Davis. I am most grateful to all discussants for their helpful comments, and also to Tom Nieuwenhuis and Victoria Vitale. All errors of interpretation, of course, are mine alone.

THE ARGUMENT

Davis begins by honestly setting out his own biases and assumptions, insofar as he is aware of them. He admits that his ethnographic interest is focused on the northwestern Mediterranean and that his reference to the literature is somewhat arbitrary. While he has limited his survey to anthropological studies, the works of colleagues from other disciplines are used in a somewhat haphazard way when they strike his fancy. For example, he discusses in detail the work on Moroccan political organization by political scientist Waterbury (1970), while ignoring the sound studies by Tarrow (1967) and Allum (1973) on Italian politics. Moreover, while he notes sociologist Lopreato's (1967) discussion of the impact of emigration on stratification in Italy, he declares the study of Israeli Jews "beyond the scope of this book" because it "has been undertaken mostly by sociologists" (p. 41). Finally, he justifies omitting a chapter on religion because the authors he surveys "have been singularly silent about the sources of religious forms of expression, and its relation to the standardised doctrines of religious communities" (p. 9). Yet he ignores completely the sensitive anthropological study on precisely that subject by Christian (1972).

² Pitt-Rivers, for example, is described as presenting information "in a tangle such as only a pioneer's licence could justify" (p. 93).

³ On Maraschini (1968): "unfortunately it is the second worst book on Mediterranean anthropology and, in ethnographic terms, admits no rivals in error and omission" (p. 73). Banfield (1958) is presumably the worst, for quite inexplicably its author is ignored completely, in spite of his having contributed more than most to the sort of debate the absence of which Davis deplures. (He must be aware of this debate, having himself joined in [Davis 1970].)

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The present paper, submitted in final form 16 III 78, was sent for comment to 50 scholars. The responses are printed below and are followed by a reply by the author.

Davis argues that Mediterraneanists, including himself, have failed in four notable ways. To begin with, they have failed to compare their results and hence have not entered into debate with each other. Secondly, they have failed to make use of the time variable, so that their work has been flat and ahistorical (Lisón-Tolosana [1966] and Blok [1974] are exonerated). Thirdly, they have overwhelmingly chosen to work in the rural, marginal areas of the region and have consequently failed to seek to understand the complexity of cities and the important relation between *bourgeois* and *villageois*. Finally, because of this rural bias, they have failed to come to grips with the whole of which the communities of peasants and pastoralists form part. The reason for these failures, Davis argues, may be the sense of professional insecurity from which Mediterraneanists suffer in comparison with their colleagues who have studied primitive societies in Africa and Asia. As a result (p. 7) they

have tended to underemphasize those qualities of the Mediterranean which might distinguish their work and lead them to differentiate themselves from their colleagues: they have ignored or abused history, and ignored those millennia of intensive interaction which have made Mediterranean societies. For that is what the failure to compare, the failure to establish simple patterns of concomitant variation amounts to.

Davis also examines in which sense "the Mediterranean" is a useful category. This is an area on which little work has been done, yet he airily dismisses what there is: Arensberg (1963) and Pitkin (1963) are too general and hence no controversy is possible. "Nor is it useful to perform the sort of pyrotechnics which Gellner (1968) and Wolf (1969) are able to carry off by wholly individual wit and charm" (p. 12). He also belittles the culture area approach ("a certain kind of agriculture, a certain respect for towns, a climate, a type of plough and a couple of syndromes" [pp. 12–13]) and concludes that it is inadequate to give anthropological meaning to the term "Mediterranean." Rather, he seeks this meaning in "those institutions, customs and practices which result from the conversation and commerce of thousands of years, the creation of very different peoples who have come into contact round the Mediterranean shores" (p. 13). The anthropologists' Mediterranean is thus a field of interaction, commerce, and conquest.

His survey is detailed and focuses on five major areas: economic activity, stratification, politics, family and kinship, and history. Inevitably, he examines economic activity and kinship in greater detail than the other activity fields. Yet these two chapters, which comprise half the book, are the most difficult to read, for comparison here consists chiefly of placing side by side the findings on pastoralists, migration, agriculturists, markets and shopkeepers, kinds of domestic group, division of households, dispersal of property and persons, systems of kinship, and patterns of marriage and godparenthood. Davis has few conclusions here, for there is little argument. The wealth of data leaves the reader a great deal better informed, if slightly bewildered.

The chapters on stratification and politics, particularly the sections on honour and patronage, do not suffer to the same extent from the absence of a systematic comparative framework. Hence they make lively reading. He argues that while most authors maintain that the people they have studied perceive themselves as "all being equal," they are far from so. He examines the degree of inequality in eight communities: the people of Alcalá demonstrate the greatest and the Sarakatsani the least. He then examines the assertion by Lisón-Tolosana (1966:108–9) that honour is an egalitarian principle that "has no connection with economic power," a position, he argues, that is shared by Pitt-Rivers and by Campbell (1964). This is somewhat unfair, for while Pitt-Rivers does indeed beg the relation of honour to class in his first study (1954), he subsequently (1965) examines it in some detail and arrives at much the same conclusion as Davis. In what is to my mind the best

section in the book, Davis demonstrates that everywhere honour is related to wealth. This is because performance of the roles the success of which determines honour, such as "feeding a family, looking after women, maintaining a following, can be done more easily when the family is not poor" (p. 77). In contrast with the lively materialist treatment of honour, however, the section on class is shallow, short, and disappointing, for he restrictively "requires that men be conscious of class before the observer can attach that label to the phenomenon" (p. 107).

Davis also examines patronage with a question in mind. He examines the degree to which there is a moral element in the patron-client relation under different forms of government. He finds that the client has more power in a democracy, where he can bargain his vote for a greater moral hold on his patron, than in a dictatorship. He concludes that patronage "affects more areas of life than formal politics or bureaucracy do: it is the bedrock of political life in most of these Mediterranean communities which anthropologists have studied" (p. 147). In spite of his enthusiastic treatment of patronage, Davis curiously does not make systematic use of the helpful distinction between the patronage and brokerage roles of patrons. Consequently he does not have the analytical tools to come to grips with the relations people in many areas have to patron saints, which often stand as model for earthly relations of patronage and brokerage. Similarly, he is unable to note the general shift from patronage to brokerage roles as governments, parties, unions, and industries grow powerful and complex and thus become the focus of lengthening chains of intermediaries mobilized to gain access to the resources they control. This gives his discussion a rather static texture and allows him to observe that "most Mediterranean people's experience of patronage does not usually involve intermediaries" (p. 149). The locus of power chances important to the people of the Mediterranean is increasingly shifting beyond the community to higher integration levels. The general increase in brokerage reflects this (cf. Boissevain 1975, 1977b).

The wealth of material in the book generates a range of questions which will keep Mediterraneanists busy for some time, thereby achieving Davis's object of provoking discussion. Some questions are more or less explicitly raised by the presentation: Is a professional inferiority complex the only reason for the failures Davis lists? What is the object of comparison? To what extent is the Mediterranean more than just a field of interaction, commerce, and conquest? Other questions, such as the reasons for greater inequality in Alcalá than among Sarakatsani, lie implicit in the material.

MEDITERRANEANIST FAILURES

The failures Davis attributes to himself and his colleagues—failure to compare, to make use of history, to work in cities, to relate part to whole—are indeed the shortcomings of Mediterranean anthropology, but they are more than just the result of a mass professional inferiority complex among Mediterraneanists. The tendency to tribalize, to seek out the most marginal areas, to examine the present only in terms of itself or the very recent past, to treat the units of study as isolates, is inherent in the structural-functional analytical paradigm which dominated (British) social anthropology from the mid-1930s to the mid-1960s and still exerts considerable power, as Davis's own lack of concern for "why" questions demonstrates (cf. Boissevain 1974a, 1975).

Davis would be correct, however, had he said that the failures of Mediterraneanists were perhaps more pronounced and have persisted longer than similar shortcomings of other regional specialists. This persistence may indeed be due in part to professional insecurity, although I suggest it is also related to four further influences: (1) the relatively short period that

social anthropologists have concentrated on the Mediterranean compared to other areas: the Africanist equivalent of Peristiany's (1965) *Honour and Shame*, Fortes and Evans-Pritchard's *African Political Systems*, preceded it by a quarter of a century, and the region's first professional journal in English, *Mediterranean Studies* (to be published by the University of Malta), will not appear until 1978; (2) the allocation of research patronage to more traditional areas of anthropological attention; (3) the more conservative bent (at least by present standards) of the early Mediterraneanists who still dominate the field, many of whom came in contact with the area through military careers or leisurely holidays or were themselves members of Mediterranean professional families; and (4) the dominance of Oxford, where most British Mediterraneanists have studied and where structural-functional theory long held sway. Davis has shown that although Evans-Pritchard at Oxford advocated more history, his students, who made up the first generation of British Mediterraneanists, were more influenced by his structural-functional Africanist *The Nuer* (1940) than by his historical Mediterraneanist *The Sanusi of Cyrenaica* (1949). Nonetheless, while specialists in other areas may have a head start on Mediterraneanists, the failings Davis points to are still all too common everywhere. *People* is a sign that Mediterraneanists are rapidly catching up.

WHY COMPARE?

Davis rather gives the impression that by merely attempting to study a pattern of concomitant variation our task is completed. He has indeed carried out this essential step, and all Mediterraneanists must be grateful for his painstaking devotion. Our work really begins in earnest, however, once such variations or similarities have been established, for they must then be explained. It is certainly interesting to learn that newlyweds sometimes settle nearer the bride's family (p. 178), that descent is bilateral in the northwest of the region, patrilineal without kin groups in the northeast, and patrilineal with kin groups in the rest (pp. 197–98), that European patriliney disappeared in the last century (p. 218), or that egalitarian institutions have been reported only in the Moroccan Atlas and the Sierra Ministra of Castile (pp. 110–25), but the inquisitive reader will want to know why. Davis rarely asks this question (although he rightly praises Schneider [1971] for asking why there is such a preoccupation with honour in the region [p. 100]). Hence he leaves the reader with the feeling that he ascribes the variation and similarities he discovers to their function, or to free-floating values, or to both. He genuinely does not seem to perceive the many “why” questions his survey begs.

To answer such questions, comparison must have a basis. It must try to relate variations in, say, honour, exchange of property, marriage residence, descent, inheritance, or whatever, to something other than their imputed function. Depending on the problems the analyst is concerned with and his hypotheses, he will attempt to relate such variations to external variables. A number of these spring immediately to the mind of Mediterranean anthropologists: terrain, climate, mode of production, access to resources, dominant religion, degree of industrialisation, distance from urban centre, and so on. On the only two occasions that Davis has done this—when attempting to relate honour to wealth and form of patronage to type of government—he moves from description to analytical argument. Yet this is almost by accident: several attempts by Mediterraneanists to engage in this sort of analysis are either dismissed as useless (Gellner 1968, Wolf 1969) or simply ignored (Silverman 1968, Schneider, Schneider, and Hansen 1972).

Comparison with a view to answering “why” questions is no simple matter. It is fraught with methodological pitfalls which anthropologists in general have scarcely begun to face (for some exceptions, see Köbben 1967, Worsley 1966, and Blok

1975). Some of these problems relate to the differences in scale and differences in development of the societies the institutions of which are being compared. Can we compare, say, patron-client relations among Portuguese peasants or Berber tribesmen with those of North Italian or Maltese countrymen without also taking into consideration the differences in scale and development of the societies? Is it permissible to take data from various historical periods and lay them side by side without further ado? Davis touches on the problem when he criticizes Pitt-Rivers for creating conceptual confusion and an impression of “potpourri” (p. 253) by mixing different kinds of evidence culled from various historical periods in his classic discussion of honour (1965). Yet Davis is open to much the same criticism when he places events in his own Pistocchi and Blok's Genuardo, studied in the 1960s, alongside data from Chapman's Milocca, studied in the 1920s (p. 58 and *passim*: for other examples, see pp. 178, 187, 228, etc.), as though they all had the same atomic weight.

On the other hand, comparison that situates the institutions being compared in their historical context can easily end by stressing historical uniqueness. This makes generalization, or even simple explanation of different or similar development, highly problematic. Awareness of this very real problem can lead to defeatist abandonment of further comparison. Anthropologists will not help solve the problems inherent in comparison simply by becoming sensitive social historians and adding a stream of historical monographs to the hundreds of synchronic functional studies of the previous generation. They must face squarely the methodological problems raised by historical comparison as well as those highlighted by the sort of survey Davis has carried out.

THE MEDITERRANEAN

The Mediterranean is more than just a field of interaction, commerce, and conquest. In spite of his materialist analysis of honour, Davis, in my view, has missed the most obvious materialist parameters that together give the region its distinctive signature: sea, climate, terrain, and mode of production. The sea has facilitated communication, and hence political domination and unity at certain periods and rebellion, relative isolation, and concentration of power at local levels when force was relaxed. The steep mountains to the north and east and harsh deserts to the south have isolated the region from both Europe and Africa. A roughly uniform climate, in conjunction with the peculiar pattern of communication, has fostered the cultivation of similar crops by neighbouring peoples and obliged them to resolve analogous production problems the solution of which is reflected in the social organisation. These are some of the more obvious reasons that social historian Fernand Braudel begins his classical study *The Mediterranean* with a 300-page analysis of the role of the environment (1972:25–352). These materialist parameters, placed in a comparative historical framework, provide a basis on which various differences and similarities characteristic of Mediterranean societies may be usefully compared. In short, such a framework is useful as a starting point to answer “why” questions. It would be most unfair to criticize Davis for not being the anthropological equivalent of Braudel. Nonetheless, if he had made greater use of such materialist parameters, he might have been able to move farther along the path we must ultimately travel to resolve the type of question raised in the previous section.

Is it really a coincidence, as Davis implies, that matrifocal uxorilocal families are reported among landless labourers on Portuguese latifundia (Cutillero 1971:126) and among industrial workers in Malta (Boissevain 1969:36–40; 1972; 1976)? The facts that in both cases attachment to landed property for

economic purposes is minimal and that in both there is a village-outward economic orientation allowing freer play of the strong mother-daughter tie do not suggest coincidence. Is it chance that bilateral descent, patrilineal descent with kin groups, and patrilineal descent without kin groups in the region appear to be concentrated respectively in the northwest, the south and the east, and between the two in the Balkans; that these same areas form subgeographical regions with notable physical differences; that their respective politically dominant modes of production are, or were until recently, agriculture, pastoralism, and a combination of the two; and that they are also areas in which different world religions hold sway? Is it a coincidence that patrilineality disappeared in the past century from Europe when we consider that the area was characterised by growing pressure on land resulting in massive emigration and that it also has been industrialised longer? Perhaps, but surely the possible relation of descent to these materialist parameters should at least be explored, even if impressionistically. The potential pregnancy of the relation between kinship and economics must be obvious, especially to British-trained anthropologists who cut their analytical teeth, if not on Marx, then by debating elder statesman Leach's (1961a) ex-cathedra assertion that the constraints of economics are always prior to those of morality and law.

Finally, is it a coincidence that the only two reported cases of egalitarian institutions occur in mountainous areas where very strong traditions of pastoralism and irrigated agriculture are intertwined? It certainly does not seem like chance that people who share so strongly held a belief in egalitarianism as do pastoralists would establish institutions to protect this against assault by those who seek to accumulate water rights and offices in order to perpetuate their ascendancy. Both Davis (pp. 121-25) and Freeman (1970: *passim*), in fact, mention that some of these egalitarian institutions are linked to the distribution of water. Freeman even observes that the Castilian word for the egalitarian rotation of responsibilities (and formerly also offices), *adra*, meaning "turn," derives from the Arabic and that the verb which stems from the same radical, *adrar*, "refers specifically to the rotation of irrigation rights" (p. 33). While I certainly do not advocate a simple materialist causality, I do wish to plead for greater attention to the environmental parameters of the Mediterranean than Davis has shown.

In short, while we can agree with Davis that the anthropologists' Mediterranean is not a "discrete entity" (p. 11), it is certainly more than just "those institutions, customs and practices which result from the conversation and commerce of thousands of years . . ." (p. 13). Mediterranean men have done a great deal besides "converse" and "exchange" (are these refracted shades of totemic ancestors Mauss and Lévi-Strauss?). Men and women around the middle sea have also worked hard to solve similar problems of production under comparable physical conditions.

By his scholarly comparison, Davis has explicitly and implicitly set out some patterns and raised certain problems that Mediterraneanists must examine. In many respects, his study sums up the work of one generation and could form the point of departure for the next. There are, however, several areas of debate increasingly important to Mediterraneanists that Davis has not mentioned. One issue concerns the relation of the marginal areas anthropologists have examined to centres of power in and outside the Mediterranean.

CORE AND PERIPHERY

Viewed in terms of relative economic and political power, Europe has a strong, prosperous core and an underdeveloped periphery. If its core lies under the smog of the Ruhr, its periphery is washed by the Mediterranean. Recently some

Mediterraneanists (e.g., Schneider, Schneider, and Hansen 1972, Nikolinakos 1972, and Bodemann 1974) have explored some of the contrasts between core and periphery in the region by using models, among others, developed to explain the dependency of Latin America on the United States. They have argued that because countries and regions in the area are controlled by capitalistically oriented dependency elites, their dependency on northwestern Europe is growing. Poor countries and regions in the area are not *developing* in the sense of attempting "to acquire an autonomous and diversified industrial growth on their own terms" (Schneider, Schneider, and Hansen 1972: 340). Rather, they are *modernizing* in the sense of being caught up in a process "by which an underdeveloped region changes in response to inputs (ideologies, behaviour codes, commodities and institutional models) from already established industrial centres; a process which is based on that region's continued dependence upon the urban-industrial metropolis." Modernization and thus the development of underdevelopment in the Mediterranean and the expansion of Europe's periphery, it is argued, are made possible by the new wealth derived from welfaristic investment by the state (price subsidies, loans, land redistribution, health, unemployment, and retirement benefits, etc.) emigration, tourism, investments by multinational companies, and the employment generated by these activities. The increased purchasing power that these bring about gives rise to economic activities, "all more or less parasitic in terms of development" (p. 343).

The evidence from the region, however, does not always support the neat articulation of increasing marginalization that the advocates of this Mediterranean dependency model suggest (cf. Pi-Sunyer 1974, Boissevain 1977a). If the European periphery is expanding southward in search of cheaper labour, new investment opportunities, and new markets, the core is also growing, as the slow but steady expansion of the Common Market indicates. Since part of the periphery is thus absorbed by the core, marginalization is not necessarily continuous. Emigration has extended the periphery into the core area of northwestern Europe in the form of permanent colonies of Mediterranean people in all major industrial centres. Tourism is proving more resilient to the recent economic crisis than many industries in core areas. Moreover, northwestern Europe is becoming increasingly dependent upon the Mediterranean tourist industry to provide essential escape from the pressure and pollution inherent to the industrialized metropolis. Increasing relative regional autonomy for politically peripheral areas in Italy and Spain is evidence of loss of power by their centres. Finally, the development of energy resources in North Africa contradicts the notion of the automatic southward expansion of the European periphery. The north-south debate between Europe and the Mediterranean has hardly been joined by anthropologists.

This anthropological lack of involvement in Mediterranean development is not likely to continue for long. Increasingly, anthropologists are coming to realise that development and underdevelopment are not processes limited to the tropical, non-European world. This is partly a sign of the maturity of anthropology as a discipline that deals with all mankind, not just non-Western people. The growing interest in development problems closer to home is also partly a result of the closure of traditional areas of anthropological research. This has come inevitably as newly independent countries became aware that many anthropologists, developers, and other foreign-aid dispensers have been seeking their own fulfillment in very much the same self-interested, career-oriented, paternalistic manner as previous generations of missionaries and colonial servants carried around the white man's burden.

As anthropologists focus on problems of development and developments in the Mediterranean, they will increasingly turn their attention to the consequences of such long-term processes

as industrialisation, tourism, urbanisation, regional protest, the movement of North African workers to southern Europe, the consolidation of power by trade unions and parties of the left, and the growth of the relative economic power of North Africa. The impact of these processes can be studied by using the traditional local-community research niche of the anthropologist as a point of departure. Growing concern with such problems will raise new questions in addition to those that Davis's book has highlighted. It thus seems likely that there will be an explosion of research in this region and that "Davis on the Mediterranean" will become basic reading.

Comments

by JOSEPH B. ACEVES

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Boissevain's article divides itself neatly into two problem areas: (1) the merits of Davis's book and (2) the scope and possibilities of Mediterranean social anthropology. The first needs little comment. If anything, Boissevain is kinder to Davis's book than I would be. We have been telling ourselves for years what anthropology is doing wrong; Davis, like most critics, is strong on what is needed but weak on providing an exemplary body of work from which we may derive guidance. As for the work of Pitt-Rivers, most especially *People of the Sierra*, it is fashionable these days to heap scorn upon it. As a Hispanist, I've certainly heard enough of this nonsense. Pitt-Rivers's book is no longer a fully valid picture of contemporary Spain, as he would be the first to admit, but for its time it is justifiably a classic. I do not, however, agree that *People of the Sierra* marked the birth of the social anthropology of the north shore of the Mediterranean. In English, *maybe*, but what about European studies? In Spain, for example, we find the marvelous work of Julio Caro Baroja as well as earlier works by scholars affiliated with the Ateneo de Madrid. Foster's (1960) *Culture and Conquest*, while largely atheoretical, is a valid study of Spanish customs which antedates Pitt-Rivers's book. Specialists dealing with other countries can probably produce similar illustrations of pre-Pitt-Rivers social anthropology of Mediterranean peoples.

The second problem is more perplexing. Boissevain's call for a pan-Mediterranean perspective begs the issue of whether something so broad as the Mediterranean area can be included in any perspective save one so equally broad as to be useless for analytical purposes. At the heart of the problem is defining "Mediterranean" in the first place. Boissevain mentions Portugal at least twice. My maps fail to indicate any part of Portugal as bordering on the Mediterranean Sea. I would consider Portugal "Mediterranean" just as much as Spain, but what does that do to the geographical limits we must inevitably place upon the area we are concerned with? This is the "core-periphery" problem Boissevain cites. Until we have the area defined clearly on a geographical basis, studies of pan-Mediterranean cultural differences and similarities will be extremely difficult. This type of problem has long plagued social anthropology; North American anthropologists, e.g., Lowie, Wissler, and Kroeber, tried variously to define Amerindian culture areas but failed to develop good theory that could account for the differences among supposedly culturally similar tribes in the same geographic area. Again, geography as an operational limiting factor rears its head. The essentially ecological approach Boissevain advocates has possibilities which I would like to see elaborated, but even the most sophisticated ecological approach will explain little without integrating more intangible factors such as values and what Redfield called a "moral order" of the society under study.

I applaud Boissevain's efforts to bring forth debate and dialogue in Mediterranean social anthropology. In such a brief article as the present one, however, he cannot be expected to do so fully. Again, one hopes a more lengthy discussion will soon be forthcoming. One variable that needs examination is religious tradition; ecology alone does not explain the Moslem-Christian division of the Mediterranean populations.

A final thought—negative or positive, as the reader wishes—is whether it makes sense even to try for a contemporary "Mediterranean social anthropology." Is, in fact, the Mediterranean Sea a valid boundary marker of an area, and, if so, just how far inland does the maritime influence prevail? It is far simpler to see the influence of the sea as ecological feature, trade route, etc., some centuries ago than it is today. Mediterranean history, yes; Mediterranean social anthropology, not yet, maybe never. The necessary brevity of this comment prohibits my developing the possibilities of a Mediterranean social anthropology further. I congratulate Boissevain for trying, but before he—and I—go on we would be wise to examine closely whether the enterprise is being based upon assumptions that are invalid.

by JEREMY BECKETT

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Despite Davis's claims, it is hard to see his Mediterranean as anything other than a culture area, albeit with more than the usual complement of history. Correspondingly, some of his across-the-board comparisons remind one of a Human Relations Area Files survey. Nor can the zone be said to have a monopoly of the features that Davis takes as characteristic, for example, patronage or ritual kinship. This, however, is not the point. Over the last 25 years or so, some remarkably able anthropologists, working in the Mediterranean, have brought into focus social forms that had previously been but dimly perceived. As a result, researchers in other places—like myself—come to their writings for insight and direction. Inevitably, not everything is of such a quality. Much of the work, useful in its own way, could be described as anthropology in the Mediterranean rather than Mediterranean anthropology.

The refinement of the notion of honour is perhaps Mediterranean anthropology's most impressive achievement to date. Yet the survival of Banfield's aged straw man seems to support Davis's and Boissevain's judgment that there have not been enough real problems. Like them, I suspect that anthropologists have not been looking in the right places.

Davis, rather tentatively, and Boissevain, more forcibly, conclude that the village focus has proved inhibiting. If this is a survival from tribal anthropology, it is a degenerate one, for it amounts to a withdrawal from societywide problems. Yet many workers have settled for less, and some for very little indeed, when the "limits of naivety" have been too narrowly set.

It is by now clear that anthropologists can move out of the village without selling their professional birthrights. In this connection it is instructive to compare the study of Blok (1974) with that of Schneider and Schneider (1976) in western Sicily. The former magnificently depicts the village manifestations of the Mafia but sketches its wider ramifications only lightly. The latter conveys little sense of locality but compensates for this with its account of translocal broker-capitalist networks. Straightaway we recognise the grass roots of the *clientelismo* that Tarrow (1967) has described for the Mezzogiorno as a whole.

The Schneiders compare the structure of broker-capitalist families with that of ordinary Sicilians, explaining the differ-

ences in terms of the former's need for alliance and continuity. It is unfortunate, as Boissevain observes, that Davis did not apply this kind of insight to his chapter on family and kinship. If it is true that Sicilians, or southern Italians, or whoever, share the same familistic norms, it is also true that these get stretched, contracted, and finally modified by material conditions. In fact, rather than starting with morphologies, we might get farther by asking how and how far kin ties are mobilized at different levels and in different sectors of various societies. Family histories can tell us much because of the family's tendency, over the years, to span places, occupations, and strata. This technique has been all too little used, in the Mediterranean or elsewhere (but see Whitten 1965).

It would seem that family coalitions are most likely to emerge as part of a clientelistic system, and it is with the persistence of such systems that I think Mediterraneanists—and others—should be concerned over the next few years. Davis recognizes the importance of this problem, but by defining patronage in such a way as to make it apply across the board (p. 150) he has destroyed its analytic usefulness. The need is rather for more complex models having more limited application. As Kaufman (1974) recently observed, the patron-client relationship is, in itself, too limited a basis for a societal model, and it is when we start asking the "why" questions that Boissevain demands that such common-denominator concepts cease to be helpful.

by STANLEY BRANDES

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I agree with Boissevain's high overall estimation of Davis's volume. The book represents an advance for Mediterranean studies and is worthy of Boissevain's stimulating commentary. Both the book and the article raise a wide variety of important issues. I can only remark here on a single matter: the accusation that Mediterraneanists have confined their investigations to the boundaries of the small community and refused to relate parts to the whole.

First, it is clear that a large portion of the anthropological research in this part of the world has taken place within the context of the small community, and often in communities that are poor—or, as is fashionable to say nowadays, marginal. This aspect of the field could justifiably be termed a weakness or failure only if the scholars who engage in such studies claimed that these communities are representative of the nation as a whole. I cannot think of anyone foolish enough to make such an assertion. In the vast majority of instances, small traditional communities have been portrayed as representing just themselves, their region, or the village type of which they are unmistakably members. As long as the quality of ethnographic research remains high and no sweeping claims are advanced as to the representativeness of a particular settlement, it seems to me that marginal communities are as good a place as any to learn about humankind.

Second, despite dominant opinions to the contrary, there exists a good ethnographic justification for studying a single community in depth: to many Mediterranean peoples, the community is (or at least until recently was) the most important focus of identification, the center of their social world. If our mandate is to try to see the world through the eyes of our informants, then—depending on where we carry out our investigation—we may have to emphasize the small town or village upon which their thoughts and actions are concentrated. This does *not* mean that Mediterranean communities are or ever have been immune to outside influences. Nor is it the case that they have been portrayed in the anthropological literature as being hermetic isolates. Studies of migration, patronage, tourism, and economic change alone suffice to dispel that erroneous impression. Even with British social anthropology as the research paradigm, anthropologists working in small

Mediterranean communities have necessarily reached beyond those communities to explain what was occurring within them. The contrary opinion is stated frequently and glibly, with little basis in fact.

This is not to say that all the explanatory possibilities, whether within communities or without, have been uncovered. Certainly, the recent increase in regional and comparative studies, as well as the merging of documentary and field data, should add immensely to the quality of our investigations. Nonetheless, where communities are themselves critical social units, a fine community study—or a more specialized study based on data from one community—will always be valid and welcome.

by THOMAS CRUMP

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Boissevain is certainly correct in describing Davis's book as "erudite, useful, and provocative"; it is at the same time a book which could have been very much better if the author had shown more consideration for his readers. With English being the lingua franca of modern scholarship, it is inexcusable to use words such as "erogate" and "successible," which are not to be found in the *Concise Oxford Dictionary*. There are, furthermore, a number of grammatical faults in the book, a fact which almost gives the impression that the author does not really want it to be widely read.

Turning to matters of substance, although Boissevain is right to refer to the geophysical factors discussed by Braudel, he fails to note three critical sociocultural factors which give the area a quite distinctive character. The first, and perhaps the most important, is that almost every region of the Mediterranean has at some time in the past—generally more than 300 years ago—been very much more important than it is now. What Schnapper (1971:85) says of the Italians—that they live in the decor of the past—is both true generally and highly relevant to the present state of the various Mediterranean societies. The second factor is that the general hierarchical structure of Mediterranean societies has always been based on cities: Mediterranean anthropology, if it is to make any progress, must become, essentially, an urban anthropology, as Boissevain is clearly beginning to recognise. This makes it all the more regrettable that both Boissevain and Davis fail to note Schnapper's (1971) study of Bologna, in many ways the best single study of a Mediterranean society. The third factor is that for more than 2,000 years every Mediterranean society of any importance has had the use of a written language. There is no space to explore this factor, but Goody and Watt (1968) may be recommended as the best starting point for such an exploration.

Davis is almost certainly better aware of the importance of these three factors than is Boissevain, simply because his approach to the study of the Mediterranean is better integrated. Although he has the advantage of a historical background, however, he has some way to go before he attains the same high level of scholarship as characterises the best of contemporary Mediterranean historiography. He is, nonetheless, probably closer to attaining this level than any other anthropologist now working in the area.

by J. DAVIS

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I am grateful to Boissevain for his attention and think it would be a misuse of CA space to argue about those points where I disagree with his interpretation of *People of the Mediterranean*. Readers who rely on him for information about what is in that

book, however, could be misled by his comment that it is biased toward the northwestern Mediterranean. The passage he alludes to says that Egypt and Libya are not thoroughly covered and that there is a bias away from the southeastern corner of the region. That is rather different from what he says, and no one should conclude that there is nothing in the book about Greeks, Turks, Lebanese, Oriental Jews, Israeli Arabs, Algerians, Moroccans, and Maltese.

Materialist parameters (or environmental parameters, as Boissevain also calls them) seem to require more work and harder thought than they have received. It is disappointing that the discussion here is so impressionistic. Should "Is it really a coincidence . . .?" or "It certainly does not seem like chance . . ." be acceptable in a deliberated article in an international journal? In the present state of social understanding, it is unwise to exclude any of the possible connections among social phenomena, or between social and other kinds. Anthropologists should accept that some functional connections have been established as firmly as anything is likely to be established (but this would not make them functionalists), and they may also accept that some institutions are created by the intention of intelligent humans (but they would not say that all social phenomena are to be explained in this way). But the gap between those who have done detailed work on the relation of ecology to social organisation and those who make bold generalisations about it needs to be bridged: at the moment it seems fair to say that the onus lies on the bolder spirits to introduce some precision into their discussions. For example:

1. It would be useful to confine the discussion in the first instance to things which always constrain or which constrain in specifiable conditions. Later on, perhaps, investigation could extend to industrialisation, population growth, religion, and "mode of production" (and in Boissevain's usage this can mean "agriculture" or "pastoralism" without qualification), which have a tendency to appear variously as constrainer or as thing constrained. No anthropologist would attribute to mountains, say, this chameleon character, but some might think that religion, one of Boissevain's examples, could be either explanans or explanandum. For the time being it might be sensible to stick to the mountains.

2. If the investigation is to move beyond diffident assertion of connection ("Is it a coincidence that . . .?"), some attempt should be made to say what kind of connection. One of the problems is that the connection is a very remote one with many intervening steps which, when specified, might seem to detract from the force of the argument. Boissevain's example of the sea is a case in point: once one has said how the water sometimes facilitates the expansion of empires and at other times the assertion of local autonomy, one's readers could wonder what the elaboration of theory is for. It is not that the connection does not exist, but that exposition of it, Braudel by no means to the contrary, necessarily emphasizes the concurrent importance of human ingenuity and inventiveness. While some people are quite ready to accept that the sea can be so ambivalently potent, others find this difficult to swallow without further specification; neither group seems prepared to elaborate the stages in the connection.

3. If someone were to undertake this task, then it would further be useful to try to devise a way of saying that the constraint is stronger in some circumstances than in others, so that other workers could compare their own results.

4. It would be a pity if, in such innovative work, researchers were to discard all notion that their discipline is cumulative. Boissevain's generalisations, for example, ignore the valuable work of specialists in the Mediterranean regions. For example, one of the achievements of North African specialists has been to identify and to explain (in many cases partly by invoking ecology) the different kinds of patrilineal organisation. Gellner's (1969) work identifies two distinct types of lineage and relates their interdependence to the politics of the exploitation of

pasture. Peters (1968) relates variations in rainfall and terrain to a social organisation in which patrilineally organised patrons dominate clients who may be as much as 40% of the population and one of whose important characteristics is that they are not patrilineally organised. Vinogradoff (1973) is illuminating on *leff* alliances and variations in terrain, Hart (1973) on the variation of Berber organisation among pastoralists and farmers. The creation of bold generalisation seems to entail high-handed treatment of evidence, for similar points could be made, with only slightly greater difficulty, about the studies of the different kinds of family organisation in areas of what Boissevain calls bilateral descent and about household organisation in the Balkans, which he curiously characterises as "patrilineal without descent groups" (see Campbell's [1963] work on bilateral groups, or that of Hammel [1968], Hasluck [1954], Whitaker [1968], Durham [1928], Cozzi [1910], and Black-Michaud [1975] on Albanian patriliney, which, incidentally, still existed in the 20th century).

In short, fruitful discussion of materialist or environmental parameters will need to be rather more rigorous than impressionistic if it is to attract a more than muted response among those who have recorded the essential evidence for such discussion.

by DAVID D. GILMORE

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Having just read Davis's new book with a pleasurable sense of encountering many of my own observations and impressions more eloquently formulated, I am ready to agree with Boissevain that *People of the Mediterranean* is a useful contribution to Mediterranean studies. Whether it represents a major breakthrough I am not sure, but Boissevain is certainly right to call it a "milestone"; it should mark the coming of age of this relatively new anthropological specialty.

I think one may even go farther and defend Davis from some of Boissevain's criticisms; for example, the beauty of the book is synthetic: it is a work of patient compilation and codification. Boissevain faults Davis (albeit in a generally admiring vein) for not tackling the "whys" of Mediterranean cultural phenomena, but the author's stated objective was rather an overview and critical assessment of the vast and disorganized literature with an eye to extrapolating major areas of agreement, convergence, and weakness, thus indicating areas for future research. In this, Davis has succeeded remarkably well, so that taken together with this review the book will no doubt become indispensable for consultation in Mediterranean anthropology. Also, I think the book represents a needed upgrading of the generally low level of culture theory in Mediterranean ethnography. Lastly, Davis gives us a very useful and complete bibliography of Mediterranean studies.

The most important critical points to emerge from the book and article are the following: First, something must be done to remedy the utter lack of comparative concern among anthropologists working in the Mediterranean area. Because of its uniform climate and topography and its many historical, cultural, and commercial convergences, the Circum-Mediterranean region offers a uniquely auspicious field for the testing of comparative social science hypotheses: how and why, for example, do people develop different cultural adaptations to similar environments? Here is an area where Islamicists and Europeanists should work collaboratively, yet to date there has been almost no dialogue between them (Peristiany's anthologies present data from both littorals, but, again, there is little synthesis or comparison). Moreover, as Davis notes, *not a single published monograph* has compared its statistical data with those of others. Why is this so? Why have we so little

interest, for instance, in comparative social stratification? I think much of the neglect can be blamed on the shocking lack of scientific preparation and forethought that goes into the initial selection of communities to be studied, partly a result of a rampant romanticism bordering on dilettantism—at least among the Europeanists. For example, in Spain, ethnographers have chosen to work in villages because their ancestors came from there, or because the local wines tasted better than in other places, or because they happened to know someone who owned land in the vicinity, or simply because they liked the view. Moreno (1972) has discussed this problem of site selection, attacking Anglo-American anthropologists for their cavalier attitudes in choosing places to study. No one has yet responded to this charge. Aside from the initial handicap of naiveté in site selection, there is a tendency to overspecialize in one country or even in one small region of a country: to become a Riffian specialist or a West Andalusianist and to ignore even adjacent areas because they are thought to be outside one's field of specialization. Davis and Boissevain (both of whom have worked in two or more areas) are telling us to overcome regional narrowness and its unavoidable superficiality and to become "Mediterraneanists" in a true sense of the word. Some, for example, Schneider, Schneider, and Hansen (1972), are meeting this challenge, but much more work is needed, and we must have a dialogue between Arab and European specialists.

A second major point is the need to examine more closely the causes of the extreme variations in degrees of social inequality in the Mediterranean. This of course requires a comparative approach, but it also demands more attention to history and to the political and economic realities of the modern Mediterranean countries. The almost total lack of interest in class analysis is a case in point. In southern Europe, even in the rural areas, social class is *the* ultimate reality in social dynamics; certainly the modern history of Spain, Italy, Greece, or Turkey cannot be understood without reference to class, party politics, labor organization, and so on; yet class is usually left to sociologists using macrolevel techniques. In the ethnography of the Islamic Middle East, class analysis is almost nonexistent, although Islamicists have repeatedly told us that social class in the Western sense has always been important in the more developed areas, such as the Nile Valley (Ayrout 1963), and that it is becoming so in the less developed areas (Gulick 1976:36; Waterbury 1970). We have done better with the honor-shame syndrome and with patronage, but these pan-Mediterranean themes are closely linked to social stratification and cannot be analytically separated from it.

A third point that emerges is the need for a more scientific ecological approach, the argument being that the primary underlying unity of the Mediterranean Basin is climate. Here Boissevain wisely refers to Braudel's long introductory treatment of Mediterranean ecology, calling for the "anthropological equivalent." While I agree with this, I also think that Boissevain is missing the most important implication of Braudel's work, which is not simply that the Mediterranean is ecologically homogeneous, but rather that this unity derives from the consistent juxtaposition of opposites within nations, the close proximity of rugged topography with fertile riverine basins. It is this intranational contrast between remote, inaccessible mountain peaks and rich agricultural valleys that lies at the heart of Mediterranean ecosystems. Throughout the region, one finds independent, egalitarian communities of peasants, tribesmen, or pastoralists in the marginal hills and in the adjacent plains something vastly different—the *latifundium*, the great estate, the commercial farm, heir to the Roman villa (Pitt-Rivers 1961:39), often worked by day laborers under harsh conditions. This is true not only for Europe: the Nile Valley and the Sa'is Plain of Morocco provide interesting parallels. I think that if we are to get anywhere in Mediterranean culture study, we will have to concentrate more on these intranational contrasts, and, of course, in comparative

perspective. For example, a comparative study of rural society in the Guadalquivir Valley of Andalusia and on the Atlantic Plain of Morocco or in the Lower Nile Valley would be invaluable. The most fruitful direction for future research—a comparative approach bringing both Islamic and Latin worlds under scrutiny—has now been clearly and compellingly outlined for us by Davis and Boissevain. It is now up to us "Mediterraneanists" to take up the challenge.

by C. C. M. GRIFFIN

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Commenting from distant parts without access to Davis's book, I beg indulgence—especially Davis's.

Firstly, Boissevain is right to appear suspicious of Davis's neglect of Israeli material, and if Davis justifies this by saying that such work "has been undertaken mostly by sociologists" he tests our credulity. This is not to deny that much of this "sociology" may lack "the ethnographic concreteness which characterises anthropology" (Goldberg 1976), but Davis surely is too able to fall for that hoary sociology/anthropology distinction. Is Israel not excluded because it is difficult to assimilate into what is usually regarded as "Mediterranean society"? This concept undoubtedly needs reexamining, and Davis in one way or another invites it, but the exclusion of Israel (and possibly other societies) does have serious implications. To start with, in the face of Davis's claim of concern with "the conversation and commerce" marking "the Mediterranean," Israel and Jewish culture are shelved and, worse, ignored as to their impact on other Mediterranean communities. Furthermore, research is ignored which makes theoretical and ethnographic contributions to those areas Davis may value most: urbanization, migration, ethnicity, and tourism (Eisenstadt 1951, 1954; Weingrod 1965; Cohen 1971).

Secondly, Boissevain's scepticism of Davis's notion regarding "professional insecurity" is justified. His own suggestion that the absence of comparative work is more likely attributable to the newness of Mediterranean studies is sensible and obvious. On structural-functionalism's role, he is on more vital ground. There is no doubt that it is the overriding influence of this tradition in British anthropology, and its consequent failure to equip Mediterraneanists with the new analytic tools necessary for analysing the special structural character of Mediterranean society, which has hindered development. Lacking methods and models equivalent to those which functionalism once provided for "tribal" regions, and the latter themselves hindering development, Mediterraneanists have moved relatively slowly. This situation also explains that "tribal," village-bound sociology of which Davis complains. Elsewhere, Boissevain (1974b) has described his own problems and one solution via "network analysis"; concurrently, I struggled far less successfully along similar lines (Griffin 1973). Briefly, Mediterranean fieldwork holds special problems, conceptual and practical.

The poverty of functionalism also explains the dearth of urban studies which Davis bemoans, for Mediterraneanists have not failed to value or tackle such work (Magnarella 1970, Griffin 1973). Boissevain is again correct to stress functionalism's failure to ask why. Here one urges, with Leach (1961b), both more generalization and less functionalist ethnocentrism. In this last respect, much credit goes to Bailey (1971), whose cognitively oriented school suggests new methodology. Perhaps the time has now also come for the structuralist analyses such work signals.

Finally, a comment on Boissevain's "core" and "periphery." If these work, it is at different levels and not at just one as suggested. Cores and peripheries are relative. Thus when Boissevain argues that Europe's core is getting closer to the periphery, he should be saying there is no *one* core—that conceptually every community, factory, village, or EEC, has one. In short, men inhabit many cognitive communities at once.

Thus, where rural and urban communities have particularly dynamic relations, as in this region, rural-urban interplay is both structurally and cognitively in need of closer examination.

by VINCENZO PADIGLIONE

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I agree with Boissevain as to the fundamental inadequacy of British social anthropology in coping with the problems which emerge from the study of the Mediterranean area. This is, I believe, the major criticism which can be put forward with regard to Davis's statements. For example, while he correctly points out some of the main theoretical failings of Mediterranean studies (the lack of historical perspective, the limited interest in the analysis of urban cultures and of the complex relationship between *bourgeois* and *villageois*, etc.), he explains them by attributing to the scholars of the Mediterranean area "a sense of professional insecurity." Boissevain, on the contrary, tends to believe that the reasons for these failings must be sought in traditional structural-functionalist theory and in the "conservative bent" of the social scientists of the older generation. I would again agree with Boissevain when he seems to prefer the study of complex societies and their mechanisms of social integration to more traditional studies. He is aware that no anthropology of Western societies is possible without a radical rethinking of its constituent theoretical parts and that a reduction of anthropology to social history or a wild comparativism is no solution to the shortcomings of Mediterranean studies.

The *pars construens*, however, unfortunately seems weaker than the *pars destruens*. The road towards a renewal of our anthropological discipline seems to lead, for Boissevain, to an environmental and materialistic concern. This is an important point, especially if we consider the abundant idealistically oriented literature on Mediterranean societies, but we should not forget that quite a lot of theoretical work still has to be done on this topic if we want to arrive at satisfactory results. Boissevain, on the contrary, seems to be satisfied with a sort of reductionist and vulgar materialism. He cites, for example, a set of variables such as "sea, climate, terrain, and mode of production" (the latter is a concept with very specific connotations in Marxist theory; since it refers to the relationship between Man and Nature in the process of production of material goods, environmental conditions are necessarily part of it) without mentioning the theoretical limits of their application. The combined action of these variables has produced, according to Boissevain, a homogeneous Mediterranean context, presenting the same problems as far as the organization of production is concerned. It seems to me that this position may be too vague; it does not take into account the unequal development of the productive forces and of the relations of production, which has had important consequences with regard to cultural differences and similarities.

It is, moreover, unclear how Boissevain intends to utilize the categories "core" and "periphery." Though he brings in relevant elements (which, however, are not sufficient to contradict the hypothesis of an increasing marginalization of the periphery), his conceptual contribution is inadequate to explain ongoing phenomena. He does not take into account, for example, a typology of community or cultural areas, the articulation between the dominant mode of production and other modes of production, class conflict and antagonism, and so on.

Last but not least, how can we analyze "the consolidation of power by trade unions and parties of the left" or even (why not?) the culture of the urban working class with the local-community approach as "a point of departure"?

I cannot but agree with the author as to the importance of developing the debate on these theoretical and methodological problems.

by JULIAN PITT-RIVERS

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I have commented elsewhere (1978) on Davis's book, giving a rather different view to Boissevain's. His reticence regarding Davis's methods of comparison is more than justified, however, and he is right to insist on the importance of geography, the scale of societies, and the significance of the historical period.

The problems of fieldwork in the Mediterranean are indeed different to those encountered in Africa or Oceania, and those of us who were taught anthropology with reference to those areas have had to modify some of our premises on going into the field. The other factors Boissevain enumerates to explain what he sees as the "failures" of Mediterranean anthropology do not, however, convince me. Poor allocation of patronage? Do the "best" brains get the grants or merely the least original? The conservative bent of the early Mediterraneanists? On the contrary, we were all aware that we were doing something that the traditionalists in anthropology disapproved of, and we made our choice of field because we were dissatisfied with their strictures. For this reason we were mostly in sympathy with Evans-Pritchard's condemnation of functionalism as "adding to a naïve determinism a crude teleology"—which dates overtly from the Marett Lecture of 1950 (1962) but was inherent in his writing from the beginning, as Louis Dumont pointed out in his introduction to the French translation of *The Nuer* (reprinted in English in Beattie and Lienhardt 1975). Long before he published *The Sanusi*, he drummed into would-be Mediterraneanists that we could understand nothing of such societies without their history and encouraged us to read Marc Bloch, Vinogradoff, Collingwood, Patrick Gardiner's *The Nature of Historical Explanation*, and other theorists in the field of history. Our discussions turned round the problems of continuity in society, and the revival of the interest in culture in British social anthropology dates from this period. Both Boissevain and Davis are writing from the point of view of that branch of British social anthropology that did not follow Evans-Pritchard's rebellion against Radcliffe-Brown and Malinowski and the scientific pretensions of anthropologists. Perhaps this is why they get him and his students so wrong.

by DIMITRA SCHÖNEGGER

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Davis's global view of the Mediterranean forces the reader to reconsider his own statements, observations on the field, beliefs, and principles.

Boissevain gives the right reasons for the failures of Mediterraneanists, thus mitigating Davis's severe criticism. Moreover, he deserves praise for discussing the main shortcoming of Davis's book, the "lack of concern for 'why' questions," and for mentioning the serious problems involved in comparing anthropological data.

The obsession of anthropologists with "honour" ever since Peristiany dealt with it is justified by the subtlety of this notion and the multiplicity of its aspects. I disagree with Davis when he invariably relates honour to wealth. Boissevain does not tell us whether he accepts Davis's (1977:91) definition of honour as "a system of stratification." In my experience in a Greek-Turkish village in Thrace, honour must be kept separate from wealth because the people themselves distinguish between "a rich man" and "an honourable man." I personally prefer to use the word "prestige," for among the Turks the highest prestige is vested in the richest man, while the Greeks choose the best educated one, each in himself being a man of honour.

On the other hand, I share Davis's opinion on class (1977:107); class consciousness is primarily an urban phenomenon. Rural populations have their own differentiating terms and criteria.

Boissevain speaks of the obvious "potential pregnancy of the relation between kinship and economics." My study in the Thracian village has shown that not only economics but, to a greater degree, acculturation leads to substantial changes in marriage system and kinship organisation.

Boissevain points to a series of problems connected with the *Gastarbeiter* as carriers of north-south communication. In fact, such contacts have always existed. In the past, however, they were confined to a few merchants, wealthy travellers, artists, writers, etc., whereas in the last two decades they have involved masses of lower-income groups, both urban and rural. This new development leads Boissevain to the conclusion that "marginalization is not necessarily continuous," thus refuting Davis, who dismisses anthropological investigations in rural and marginal areas. Indeed, half of the Greek population of the village I studied was at work in or around Stuttgart. Nonetheless, this village, with its continuously fluctuating population, its steady contact with a Western industrial society, and its two religions can stand as a model for half the villages in the province. I therefore welcome Boissevain's view that "these processes can be studied by using the traditional local-community research niche of the anthropologist as a point of departure." Comparison is possible only if we have detailed small-scale studies to work with. Campbell, Lisón-Tolosana, and Blok have set the example.

by ROBERT WADE

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I wish to address my remarks to the "core and periphery" section of Boissevain's paper. I believe core-periphery analysis to be a promising perspective for anthropologists as for other social scientists, but it is wide open to fuzzy thinking. The use of that central word, "dependency," for example, is often procrustean to say the least. *Dependence* on the core is said by many, including the Schneiders in Boissevain's quotation, to explain South Europe's (and Latin America's, and South Asia's) lack of development, its "distorted" structures. Then Boissevain, by way of counterexample, cites northwestern Europe's growing *dependence* on the Mediterranean tourist industry. The first step to clarity is to see that dependence is a necessary but not sufficient part of the explanation of the patterns of social change that "underdevelopment" theorists have in mind. The question is why dependence leads to such "distortions" in the case of these areas, while other areas in advanced capitalist states may be (in some sense) similarly dependent but not show the same "distortions." (For one attempt at clarification, see Lal 1975.)

The fact that major industrial centres in Europe have permanent settlements of Mediterranean people Boissevain interprets as a shift of the periphery into the core. It seems to me useful to define the core in terms of a *predominance* of production *processes* (not products) which are relatively highly mechanized, highly skilled, high-wage, and highly profitable. Such a core will also contain activities which have more "peripheral" characteristics, particularly those of the subproletarian positions in the economy, but neither the existence of these activities within the core nor the fact that they may be carried out by migrants from the periphery alters its core place in the world division of labour. Similarly, the exploitation of energy resources in North Africa need not, and probably will not, shift the peripheral status of that region towards a core status, as Boissevain implies. Having said that Boissevain's use of the concepts needs clarification, one has to add that this is a general problem facing all who use this perspective. The task is twofold: to define the criteria differentiating core and periphery (and, in more complex formulations, the levels of regional hierarchy) and to map the geographical extent of the regions over time.

In another sentence, Boissevain blurs another critical distinc-

tion, that between states and the world division of labour. He says that increasing relative regional autonomy for politically peripheral areas in Italy and Spain is evidence of loss of power by their centres, and he takes this as another piece of evidence that the European periphery is not being further "peripheralized." "Core" and "periphery" refer essentially to production processes, to a division of labour. Regional political movements refer to juridical units. A change in the allocation of power within a juridical unit (a state) has no necessary connection with a change in the place of its constituent juridical parts in the world division of labour (that is, in the relative concentration of core-type and periphery-type processes within their boundaries). Indeed, the opposition between integrating production on a world scale and forming strong states is one of the basic dynamics of the perspective: states may try (if that reification be excused) to increase the concentration of core production processes within their borders (to "develop"), but the world-scale division and integration of production tends to move according to different and partly opposing patterns (see Hopkins and Wallerstein 1977, Murray 1972).

We can now raise the question of how useful it is to take "Europe" (or Western Europe) as a unit, as a relatively closed system. "Dependency" theorists have tended in practice to concentrate attention on one particular peripheral part of the world (especially Latin America) in terms of its relationships with the Northern European-North American core; they begin from Latin America and look outwards. The "world-system" theorists (such as those who write in *Review*) still use core and periphery as the basic idea for zoning the world economy, but they try more systematically than the "dependency" theorists to find concepts which will encompass the world system looked at from the outside, as a whole. It seems to me fruitful to situate core-periphery relations in Europe within this body of theory.

The more peripheral parts of Europe do have a number of special characteristics, however, and by exploring these we can go beyond the crudely dichotomous core-periphery formulations. For one thing, that the peripheral areas (those in which there is a high concentration of peripheral production processes) are physically *close* to the core helps (together with the declining cost of transport in relation to core wages) to explain the large (and opposite) flows of migrants and tourists between these areas and the core (see Seers, Schaffer, and Kiljunen 1978). The importance of migration and tourism for the European periphery has distinct consequences for state policies and for social life. Further, there are pressures to enlarge the market for the products of core industries, pressures which are stronger than in other sorts of periphery (e.g., the Caribbean and northern Mexico in relation to the United States, where the pressure to keep down wages in export industries is more important than the size of the internal market; see Portes 1978). That entails, amongst other things, flows of transfer payments (pensions, subsidies, etc.) to people living in peripheral areas. How these transfer payments are allocated, who gets hold of them, is critical for understanding the political processes of local communities and of nation-states (see Wade 1978). How, whether through transfer payments or other means, rural households are induced to depend more than previously on the market (thus expanding the size of the total market) and how they respond to contractions in the world market are critical questions for understanding the cycles of contraction and expansion of the world economy and the changing place of Europe in it.

In the last paragraph I have hinted at one or two ways in which the concerns of Mediterranean anthropology (focussed on "community studies") might be linked to broader questions of our age. A further set of questions to which anthropologists might well contribute concerns the study of *cultural* communities of various sorts, an aspect of the world-system perspective to which little theoretical attention has been given. Anthro-

pologists should have much to say by way of qualification of one of its central propositions—that the role of a region in the world economy is a determining influence on its class and productive structure and the form of its states, not the other way round. Boissevain is quite right to draw attention to the neglected potential of core-periphery analysis in anthropological studies and of anthropological studies in improving core-periphery analysis.

Reply

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The comments are most stimulating. Collectively they deal with most of the points I tried to bring forward. More important, they demonstrate that the discussion on the social anthropology of the Mediterranean that both Davis and I have tried to provoke is well under way. They touch upon the social history of Mediterranean anthropology, the characteristics of the region, the limits of ecological parameters, the relevance of community studies, the use of core and periphery as analytical categories, and, of course, the problem of generalization.

Aceves disagrees that Pitt-Rivers's *People of the Sierra* marks the birth of Mediterranean social anthropology. He is right, of course, in assuming that I was referring primarily to works in English. Obviously, there are works on the Mediterranean that pre-date Pitt-Rivers's, but, for example, neither Caro Baroja nor Foster established a base line against which later studies were compared. Pitt-Rivers did.

In retrospect, it was a mistake to perpetuate Davis's categorization of the shortcomings of Mediterranean anthropology as "failures." As Pitt-Rivers quite rightly implies, they are failures only in the light of present theoretical predilections. The legitimate theoretical puzzles of yesterday are viewed as failures by today's standards, just as the radicals of one generation are seen as conservative by the succeeding generation. Pitt-Rivers's study was pioneering precisely because he broke with the tropical tradition which had obliged anthropologists until then to study African and Asian societies. Moreover, his excursion on the relevance of history for anthropology and his own analysis, presented as an appendix to *People of the Sierra*, was also a radical departure from the ahistorical climate prevailing at the time. In short, to speak of failures is to commit a human but most unscientific error: to judge the scientific activity of a preceding generation by present canons.

Aceves has problems viewing Portugal as a Mediterranean society because it does not border on the Mediterranean Sea. The severe geographical criteria that he seems to advocate are too restricting. Large areas of Portugal that do not touch the Mediterranean share the climate, topography, language, and political and commercial history of the countries that border the Mediterranean. They are part of the region. Is France Mediterranean? Lille is certainly not a Mediterranean city, but Aix-en-Provence most certainly is. Similarly, there are extensions of Mediterranean societies in the form of enclaves in northern Europe, the Americas, and Australia. I would argue that these are also part of Mediterranean society, though not part of the Mediterranean region. Ecological and cultural boundaries do not always coincide with political ones.

Crump has noted several additional sociocultural factors, which should join those Davis and I listed as providing the distinctive signature of the Mediterranean. While agreeing with Gilmore that another characteristic of the Mediterranean is the juxtaposition of its rugged topography with fertile basins, I certainly would not go as far as he in maintaining that Mediterranean unity derives from this juxtaposition. The unity is

provided by a *combination* of characteristics; no single one is the key.

Most commentators appear to accept, with Beckett, the notion that the traditional village focus is inhibiting, though it can, as Schönegger and Wade note, provide a point of departure to study macro-processes. Brandes and Padiglione disagree. Brandes offers a curiously dated plea for the continuation of in-depth community studies in order to learn about humankind. I do not agree that our mandate is as limited as Brandes suggests: "to try to see the world through the eyes of our informants." I am interested not only in *how* informants view the world, but also especially in *why* they hold those views. One can indeed answer the more limited "how" questions by looking at a village as a thing unto itself; one cannot answer "why" questions in this way. Nor do I agree that "where communities are themselves critical social units, a fine study . . . based on data from one community . . . will always be valid and welcome." What is critical? The very viability of many Mediterranean communities—their criticality, if you like, to the people who live in them—is dependent upon injections from outside in the form of government subsidies, remittances from migrant labour, educational and health provisions, and so on. The relevant decisions for these essential elements are taken outside the community. Holistic community studies which are built up from data drawn from one community and which lack a clear problem orientation are increasingly less welcome. This certainly does not mean that a local community, whether city, town, or even neighbourhood association, cannot form the point of departure for a problem-oriented study. Quite the contrary. To understand the operation of macro-processes, it is essential to anchor the study at a level at which people interact. This is something Padiglione does not appreciate. How, he asks, can we analyze "the consolidation of power by trade unions and parties of the left" or . . . the culture of the urban working class with the local-community approach? The "local community" can be a neighbourhood in a city. If social scientists conclude that there are aggregate shifts in voting behaviour, culture, or union affiliation, these must also be manifest at the face-to-face community level at which real people live. Unfortunately, people and their day-to-day problems are all too often missing from the analyses of political scientists and economists precisely because they do not anchor their studies in local communities.

I cannot take too seriously Gilmore's impatience with anthropologists who have studied Europe for their "rampant romanticism bordering on dilettantism" in selecting their communities. Since the object of most of these anthropologists was, and according to Brandes still should be, to see the world through informants' eyes, then obviously pleasant people, picturesque scenery, and good wine are excellent criteria by which to select a village for study. Why not enjoy your field-work? On the other hand, if a study is problem-oriented, then, depending on the problem, other criteria become important. The point is that in the past Mediterranean community studies by and large were not problem-oriented, hence the criteria Gilmore frowns on were perfectly valid.

Padiglione, Griffin, and Wade note that I use the terms "core" and "periphery" rather loosely. Wade in particular provides a useful discussion of the applicability of the core/periphery concept to Europe from an economist's perspective. He advocates restricting "core" and "periphery" to specific production processes. Yet, for better or for worse, the term has escaped from economic strictures and is used to indicate not only division of labour, but also division of political power. The fact that Mediterranean people form colonies in northern Europe has helped to sensitize the population of these core states to the problems of the periphery. In other words, it has had political repercussions. Similarly, while the exploitation of

energy resources in North Africa need not shift the peripheral status of that region towards a core status in the economic sense of production processes, it most certainly does shift it towards core status in a political sense. A unidimensional economic interpretation of core/periphery misses this important development completely. Nevertheless, Wade is correct to argue for greater precision in the use of these terms. While it is essential to be explicit about the sense in which they are used, it would be a pity to restrict them to the incidence of certain types of production processes. They are sensitizing concepts that indicate the process inherent in asymmetrical economic or political relations.

While I view the ecological parameters as certainly among the most important characteristics of the Mediterranean region, I have by no means argued that they are the only ones. I developed the argument because Davis, while stressing the importance of materialist parameters in discussing honour, did not go on to apply this viewpoint to other areas of inquiry. He could have obtained much more mileage from it had he done so. I have tried to indicate how this might be done. While Aceves is right to note that religious tradition needs further examination, I would argue, against him, that an ecological approach might indeed go a long way in explaining the Moslem-Christian division in the Mediterranean region. Terrain and sea channelled and provided obstacles to Moslem expansion.

Padiglione finds me satisfied with a sort of reductionist and vulgar materialism. He complains that I indicate variables—sea, climate, and terrain—without mentioning the theoretical limits of their application. At this stage of the use of these variables by anthropologists, it would be as pedantic as it would be impossible to attempt to limit the relevance of ecology so as to exclude, say, religious experience. Until this field has been explored, how can one limit it? Who but the most naive would dare to argue *a priori* that there is no relation between the religious conceptualization and experience of fishermen, peasants, and pastoralists and their sharply differing physical environments? To my mind, at this stage even a vulgar materialism is better than the safe position of those who prefer to thrust their heads deep into idealist sand and maintain that their analyses are independent of the crude material world of differing climate, terrain, and economic power relations.

Davis raises an interesting methodological point. He suggests that queries on the order of “is it really a coincidence that [matrifocal, uxorial families are reported in Portuguese latifundia and among industrial workers in Malta]?” are not legitimate in a deliberated article in an international journal. Obviously, I consider that they are. They are the sort of “why” questions Davis eschews. The reader will have noted that I have not merely posed the question, but stuck my neck out by suggesting an answer in the form of a testable hypothesis (where attachment to landed property for economic purposes is minimal and there is a village-outward economic orientation, there the mother-daughter tie is allowed freer play and matrifocal uxoriality results). I think that such speculation is quite legitimate, too.

Davis's four points place me at a loss. At one stage he notes a distinction between things that constrain, like mountains, things that are constrained, and things that both constrain and are constrained, like industrialization, population growth, religion, and mode of production. He recommends focusing initially on the first. That the dual character of religion complicates analysis does not mean that we should stick to mountains. Again, this is a suggestion that we adopt a question-begging safe position. At another point he suggests that in generalizing I ignore the valuable work of North African specialists on patrilineal organization. Perhaps this is so, in which case I err. Davis himself, however, does not indicate which of my generalizations he takes exception to. The only one that seems remotely relevant to his point is my statement that North African societies may be characterized as having patrilineal descent with

kin groups. This statement was in fact based on one of the few generalizations to emerge from his book (pp. 197–98): “the rest of the mediterranean studies—the whole of the eastern and southern sections—are patrilineal societies with a preference for endogamy.” He now observes, among other things, that up to 40% of the population of the Cyrenaican nomads studied by Peters are not patrilineally organized. Indeed, as he notes, bold generalization often involves high-handed treatment of evidence! We must be aware of this danger so that we can take steps to avoid it, but this awareness must not so frighten us that we retreat behind infinite human variety and beg the “why” questions. It is now time to seek explanations for the concomitant variations Davis has noted, the “why” questions of the social anthropology of the Mediterranean.

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