

ADJUSTING THE COMPASS. THE QUEST FOR MEDITERRANEAN PARADIGMS

The focus of this stimulating conference volume lies on a period of Mediterranean history which has been decisive for the cultural development of this region, yet less known than almost any other phase from antiquity to modern times. In the present paper, I would like to discuss some important theoretical concepts relating to the Mediterranean, and explore their hermeneutic potential for the history of this area in the late 2nd and early 1st millennium BC.¹ Since the main scope of the conference was not to introduce new theoretical approaches in the study of the Mediterranean basin, but rather to provide a fresh look at old and new archaeological evidence – something very sensible and necessary indeed – I hope that my approach will, in some way, complement the efforts of the contributors to this volume in bringing Mediterranean archaeology one step further. Among the wide variety of issues relating to Mediterranean studies, I would like to highlight and briefly discuss the following aspects: 1. the current phenomenon of convergent interests in the Mediterranean among politicians and social scientists, 2. the crucial question about the unity of the Mediterranean region, 3. some important theoretical concepts and their current value as cases of applied theory, and 4. the question whether we really need theory for understanding Mediterranean phenomena. After answering the last question, I will close the present paper by using the paradigms discussed here to shape an agenda for the future study of the Mediterranean region in the Iron Age.

WHY NOW? THE MEDITERRANEAN »BOOM«

At the turn of the 21st century, the idea of the Mediterranean as a cultural entity has experienced a remarkable revival, both in international politics and social sciences. In politics, the programmes of Euro-Mediterranean partnership and the establishment of a Mediterranean Union set some very ambitious aims, which have recently found an abrupt end with the so-called Arab spring. The violent upheavals in the Southern and Eastern Mediterranean Arab states – which can certainly be regarded not only as an Arab, but also a Mediterranean phenomenon – are comparable, though actually unrelated, with the social unrest in the European Mediterranean states, especially Greece, Italy, Spain, and Cyprus, which was caused by the current financial crisis. This recent historical development across the Mediterranean basin, including financial collapses, protests, riots, uprisings, and civil wars may have brought to a halt the plans of a Mediterranean union, yet at the same time, made apparent how important the realization of this idea could have been or could be for the region's political, financial, and social stability. If we now leave politics aside and turn to science, we can detect a growing interest in the Mediterranean across different social disciplines in the past 20 years. This new scientific focus coincided with the aforementioned political interest in this area and its problems. The »boom« in Mediterranean studies is reflected in the significant increase of academic journals, which deal to some degree with the archaeology, history, society, and culture(s) of the Mediterranean.² But it is not so easy to explain the reasons which led to this contemporaneous awakening of politics and science since the 1990s. Even if it is not very plausible that a single force propelled the interest of both groups, we have to agree with Ian Morris that globalization played a significant role in this recent process of putting the

Mediterranean into focus.³ The Mediterranean fits into the new era of globalization, since it has no clear center nor periphery, being a region which can be better perceived as a network. It thus provides an ideal setting for studying globalization phenomena in a (Mediterranean) nutshell. For our disciplines, this rare convergence of interests among international politics and social sciences is undoubtedly a great opportunity. Despite the serious problems which impede field research in Arab countries in the last years, it is apparent that this convergence has ensured and will continue to ensure very favourable conditions for the funding of interdisciplinary research projects focusing on the past and present of the Mediterranean region.

WHICH MEDITERRANEAN? A REGION AS AN ANALYTICAL TOOL

The new scientific quest for unity in Mediterranean studies has several opponents, especially in the field of social anthropology. There are many critical voices which stress the heterogeneity of the region, and reject any attempt to regard it as a coherent spatial and cultural entity, and consequently as a homogeneous field of scientific inquiry.⁴ According to a rather extreme version of this line of thought, the Mediterranean is nothing more than a geographical expression. This critique, which is based on some sound arguments, is actually quite useful for relativizing the no less extreme position that the Mediterranean is one region, breathing the same air, and having a common destiny. The regional and cultural diversity of the Mediterranean world is undeniable; even the outer limits of this internally diversified zone cannot be delineated with certainty, no matter what criteria one may apply (geological, hydrological, climatic, ecological, cultural or national). Egypt, for instance, tellingly demonstrates the intricacies of a clear-cut definition of the region. The question of the extent to which Egypt has been part of the Mediterranean at any period of its history cannot be answered easily.⁵ The land, for one, lacks certain basic features of a Mediterranean landscape (such as mountains, or the two common elements of the Mediterranean triad: olive oil and wine). Despite these problems, there is a sensible way to overcome doubts against the considering the Mediterranean as a singular entity. If we agree that the Mediterranean is not a geographically and culturally coherent sphere, but a web consisting of several parts bound together by very strong ties of mutual dependence, the similarities or differences between these parts become less significant. Therefore, the conception of the Mediterranean, not as a homogeneous entity but as a network, legitimizes its utilization as a clearly defined field of study and, moreover, a valid analytical category in the study of cultural phenomena.

WHAT COMPASS? THREE PLUS ONE MEDITERRANEAN PARADIGMS

In recent years, this field of enquiry – at least on a theoretical level – has been heavily dominated by the work of historians, to whom we owe the most important methodological advances on the subject. For this reason, it seems opportune to examine the past and future of Mediterranean research, and to discuss how archaeology can contribute to the ongoing interdisciplinary dialogue. Over the past two decades, the methodological significance of the latter has become increasingly apparent. As Irad Malkin states in a recent article: »Involving patterns that transcend national frameworks and structures that question conventional periodization and promote emphasis on networks of exchange, the Mediterranean provides a multilevel prism through which to view history«. ⁶ One could also add here »... and archaeology«.

Where do we currently stand with archaeological and historical studies on the Mediterranean region? In the vast and still growing body of scholarly work, one can accentuate only a small number of influential works and theories, which may serve as a common point of departure for our collective concerns. The most

important advances in the study of Mediterranean paradigms in the past have been made in the fields of ancient, medieval, and early modern history, such as Fernand Braudel's seminal *La Méditerranée et le monde méditerranéen à l'époque de Philippe II*.⁷ It is well-known how this work revolutionized historical thought, initiating one of the most dramatic paradigm shifts in historical studies of the 20th century.⁸ Regarding the Mediterranean as a distinctive and coherent unit, and studying a crucial period of its history, Braudel developed an intricate theoretical construct, which was of great interpretative value even outside of its own geographical context. This exceptionally imaginative concept contested the traditional understanding of history as a movement within one-dimensional time. Braudel defined three different rhythms of historical time (*longue durée*, *conjonctures*, and *histoire événementielle*, each corresponding to long-term geographic or environmental structures, medium term socioeconomic cycles, and short-term socio-political events).⁹ With this tripartite model of historical time, Braudel demonstrated that history possessed different rates of change and continuity, which were determined by different geographical and socio-historical parameters. Despite being available since 1949, Braudel's *Mediterranean* was hardly noticed by most Anglophone historians and archaeologists until the appearance of an English translation in the mid-1970s. Brent D. Shaw describes very vividly the late impact of this masterpiece of historical research in Britain: »I can still see the sudden arrival of the paperback edition of Braudel's *Mediterranean* in 1975 in the bookstores of Cambridge, and the ripples of excitement as the whole coterie of young research students in ancient history hurried to acquire their own copies«. ¹⁰

The reception of this magnificent work by archaeologists specializing in different periods of Mediterranean history was somewhat awkward, since most of them actually felt more comfortable with mentioning the book than with using it. In archaeological literature, Braudel's name hardly ever exceeded the level of footnotes; the names of those who mention Braudel without having read him could fill an entire volume. Until today, there has been no systematic attempt to apply the three rhythms of Braudel's historical time for interpreting archaeological data in the Mediterranean context. Despite the justified critique of Braudel's environmental determinism – a deficit that Braudel recognized in his later works – it must be stressed that his concept is still inspiring and valid. It provides a very fruitful and multi-levelled analytical tool for historians and archaeologists, and opens the possibility of a holistic approach to the interpretation of historical processes. One has to admit that, in our discipline, the preconditions for such an approach are given only in rare cases, since the archaeological record is, as a rule, very fragmentary. But that does not mean that Braudel's model is not applicable to archaeology. In this field, one should adopt a narrower perspective by keeping the tripartite model in mind, and by trying to file the extant data and/or analytical tools within one or more levels of Braudel's refined dialectic of time spans. In a second step, one could then explore how, for example, the available environmental or social/cultural data relate to those from other time levels. What makes Braudel's concept of time interesting for archaeology is the fact that in our discipline, we can normally detect or reconstruct the two first levels by applying processes referring to phenomena of continuity/discontinuity through different periods, being unable to write a history of events based on silent archaeological sources – even if the former generations of archaeologists tried to do exactly that.

After Braudel, there has only been one book that has used a comparable synthetic approach. In their Mediterranean panorama *The Corrupting Sea*, Peregrine Horden and Nicholas Purcell attempted to highlight people over geography.¹¹ *The Corrupting Sea* comprises an astonishing wealth of data from several periods of Mediterranean history, and provides a new methodological perspective that attempts to contest and supersede the Braudelian way of thinking. But the eye-catching book title is, in my opinion at least, irritating. The expression »corrupting sea« is based on Plato's disdain for the world of the sea, and his conviction that the Mediterranean was a source of moral and social corruption. Even after reading the book, the phrase does not stand as a common denominator for the Mediterranean past, even in the terms Horden and

Purcell set out themselves. The Greek and Roman belief of the Mediterranean Sea as »a medium resisting homogeneity, order, and social control«¹² is too narrow a perspective to be applied as a diachronic value, or to be used in a scientific analysis which encompasses the history of several centuries. The phrase »corrupting sea« merely exposes the real interests of the authors, that is the terrestrial parts of the Mediterranean region and the people populating them. More attention is paid to surrounding lands than the bonds that have linked the opposing shores of the Mediterranean basin. Nevertheless, a title is only a title, and cannot be taken as a decisive criterion for the significance of the authors' scientific accomplishment. So what about the book itself? *The Corrupting Sea* is a thorough study of primary production. Its premises are based on a series of regional case studies. It is thus primarily an economic or ecological history of the Mediterranean. The two final chapters on religion and social anthropology are only loosely connected to the preceding part. The economic/ecological core of *The Corrupting Sea* serves as the fundament of Horden and Purcell's new approach. Three key terms help us to grasp the essence of their study and theoretical vision. According to Horden and Purcell, the factors that determined Mediterranean processes are: a) topographical fragmentation, b) environmental risk and risk management, and c) connectivity¹³: It was the constant threat posed by the environmental risks that drove individuals or social groups to communicate and to connect with each other, in order to overcome the isolation and separation imposed by the exceptional topographical fragmentation of this vast geographical area and its regional diversity. It must also be noted that Horden and Purcell underline the primacy of connectivity over the two other factors¹⁴ – positing that connectivity is the key variable of Mediterranean history, the most important criterion for assessing the social and economic character of any Mediterranean microregion, at any given historical moment.

Leaving aside the indisputable value of the book as a rich collection of appealing case studies, we may concentrate on the applicability of this theoretical approach on two levels: a) in general, Mediterranean archaeology, and b) in particular, the study of Mediterranean phenomena during the Iron Age. How important may this theoretical construct be for archaeologists working on the Mediterranean? Does it really mark a significant step forward in Mediterranean studies, one that is worthy of being compared to Braudel's masterpiece? Despite the fact that the book was aimed to replace Braudel's vision of the Mediterranean with something superior, and although the authors seem to be engaged in a continual polemic against Braudel, a closer reading reveals an »undercurrent of harmony« with Braudel, as James and Elizabeth Fentress stress in their review of the book.¹⁵ They note that this work is definitely still Braudelian in its conception, with some theoretical adjustments. Horden and Purcell apply a refined Braudelian perspective that acknowledges the significance of human agency in Mediterranean history, restoring the right causal balance between physical environment and human decision-making. In their view, the unity of the Mediterranean is not a static thing, as Braudel claims, but a dynamic process: unity is driven by the impetus to connect the disparate parts, to overcome the fragmentation of regions and resources. Horden and Purcell take a bottom-up approach, placing strong emphasis on distinctive microregions and rural populations. Their focus on the importance of the microscale (local resources, short-distance exchanges) is an attempt to study Mediterranean distinctiveness on a small regional scale, conceiving in its case a Mediterranean in a nutshell. Like Braudel, they highlight connectivity, but at the same time, they demonstrate the complexity of this phenomenon. To them, roads and sea routes are not monolithic concepts which are only subject to geographical and climatic variables; instead, connectivity lines fade, intensify, or shift, depending on the larger networks of which they are part. *The Corrupting Sea*, however, has some serious shortcomings, which have already been discussed.¹⁶ I would like to stress some of these lacunae here, not because I want to question the significance of this work, but because I think that they provide several promising fields of enquiry for future research. Given its clear focus on ecological and economic history, the terrestrial parts of the Mediterranean figure more prominently than the maritime routes connecting them. Other important issues, such as social identities and cultural

exchange, are completely ignored. The latter is, especially for archaeologists, very disappointing, given the significance that processes of cultural transfer, adaptation, transformation, and resistance play in our disciplines. There is no need to stress that cultural transfer is the topic of this conference. In Horden and Purcell's book, archaeology is present only through a detailed discussion of Colin Renfrew and Malcolm Wagstaff's study on the Aegean island of Melos.¹⁷ This work can be regarded as having an exemplary character for Mediterranean history, only if we recall the real interests of the authors of *The Corrupting Sea*, which are related to the history of microregions and not to processes at the macroscale, which are relevant to Mediterranean networks. To the significant blind spots of the book also belong major phenomena of Mediterranean history, such as Phoenician expansion and Greek colonization. Horden and Purcell's approach offers too ruralised a view of the Mediterranean, which pays no attention to palatial centers, elites and cities, stressing, on the contrary, the significance of microregions, short-distance exchange and private enterprise.

It is exactly at this juncture, however, that *The Corrupting Sea* becomes relevant to this conference volume. What appears to be a methodological weakness from a diachronic point of view – i. e., the narrowness of the concept and its clear focus on the microscale – gains significance when applied to a period of Mediterranean history in which these very elements (regional diversification, short-distance exchange, and private enterprise) prevailed. A comprehensive and interdisciplinary study on Mediterranean history between 1250 and 750BC, and especially of the early part of this period, cannot be – or cannot only be – an archaeology of palaces, territorial states, and long-distance exchange but a study of exactly those themes dominate the core of *The Corrupting Sea*.

These two synthetic works are only the climax of an intensive engagement of historical disciplines with the Mediterranean. In the field of archaeological sciences, the interest in Mediterranean regions and the various patterns of exchange has also been unbroken. Theorization of the Mediterranean has been advanced by some very inspiring contributions of Bronze Age archaeologists, especially Bernard Knapp¹⁸ and Andrew and Susan Sherratt.¹⁹ An apparent lacuna has been, however, the monographic treatment of the subject. Mark Patton's *Islands in Time*,²⁰ which offers a sophisticated theoretical approach to cultural processes occurring on the Mediterranean islands in prehistoric times, still remains an isolated achievement. Among the recent attempts by archaeologists to provide a theoretical background for interpreting Mediterranean cultural history, Ian Morris deserves special mention for coining the term »Mediterraneanization«.²¹ In his view, the awakened interest of social scientists in the Mediterranean, a region characterized for most of its history by fluidity, interconnection and openness, has been, as already mentioned,²² a response to the modern phenomenon of globalization. Morris begins by defining two periods of scholarly thought, which oppose each other diametrically. The first, the traditional paradigm (or static cellular model), was – according to Morris – heavily influenced by Moses Finley, and stressed division and sectioning into places and periods in the Mediterranean.²³ The second, the new paradigm (or interconnected model), emerged in the 1980s, and emphasizes the connectedness of the Mediterranean basin and the fluidity of the movement of people, goods, and ideas.²⁴ Morris' conceptual dichotomy is not only very simplistic; it is also wrong, at least as far as archaeology is concerned. His observation that »scholars in several disciplines have moved towards looking at the Mediterranean as a unit since the 1980s«²⁵ may be partly true for ancient historians, but untenable in the case of our discipline. For instance, in Aegean archaeology, the idea of the interconnected Mediterranean had been a key issue since the time of Arthur Evans, and definitely since the publication of Helene Kantor's influential »The Aegean and the Orient in the Second Millennium B. C.«,²⁶ which focused on the interregional circulation of artefacts, images, and styles across the Mediterranean region. An equally pioneering interest in Mediterranean connectivity can be postulated in the field of Classical Archaeology. The cultural impact of the Orient on Greek culture – a genuine Mediterranean phenomenon – was certainly acknowledged long before Sarah Morris' »Daidalos and the Origins of Greek Art«,²⁷ or even Martin Bernal's

»Black Athena: The Afro-Asiatic Roots of Classical Civilization«;²⁸ it has been around since at least the coining of the term »Orientalising« to describe an important period of Greek culture. Yet, this is just the history of research, not the heart of the matter. Morris' contribution cannot actually be compared with Braudel's or Horden and Purcell's paradigms, since it lacks the complexity and methodological depth of either. Its significance lies in Morris' successful attempt to accentuate some diachronic aspects of Mediterranean history. The key word in Morris' Mediterraneanization is process: much in the same way as Horden and Purcell, Morris stresses the dynamic and constantly moving interactions between different factors, and pleads for an understanding of Mediterranean phenomena as constantly changing and not fixed categories – processes of movement, interconnection, and decentralisation. Morris shares Horden and Purcell's view that connectivity (Morris uses the term »connectedness«) is the quintessence of Mediterranean history.²⁹ He regards globalization not only as motor for the new interest in the Mediterranean, but also as a powerful analogy for exploring Mediterranean phenomena. This construction of a pre-modern and regionally-focused equivalent to modern globalization is certainly not unproblematic, as Morris' attempt to explain Greek colonialism as a Mediterraneanization phenomenon makes obvious.³⁰ Yet, it is exactly this provocative analogy that opens up a new perspective into the history of the Mediterranean. His observation that Mediterranean processes created winners and losers, and had consequences just as traumatic and uneven as contemporary globalization, is a very fresh and promising approach to well-known phenomena.

Furthermore, Morris suggests that some periods of Mediterranean history were more Mediterraneanized than others (an insight that he believes can already be attributed to Polybius).³¹ In an article published two years later, Purcell makes an almost identical statement relating not to periods but to regions: »Some parts of the Mediterranean, moreover, were always less »Mediterranean« than others, especially in the terms of our analysis – it was always normal for them (?) to be un-networked, uncharacteristic bits, less embedded in the Corrupting Sea paradigm than others according to their degree of connectedness or intensification«.³² To sum up, Morris suggests a dynamic approach to Mediterranean phenomena, putting an emphasis on processes and providing a criterion for their historical evaluation. This criterion is their degree of Mediterranean »compatibility«, their position within an imaginary Mediterranean gradient, which helps us to distinguish between more or less Mediterraneanized regions and periods.

Morris' focus on processes leads us to a further paradigm which I would like to discuss here, if only very briefly. Even if it is not an exclusively Mediterranean one, this paradigm has a high applicability to Mediterranean history and a strong relevance to Heidelberg. In the course of the ongoing Heidelberg Cluster of Excellence, »Asia and Europe in a Global Context«, an interdisciplinary group of social scientists is dealing with the traditional and problematic notion of culture. One of the main objectives of this group is to demonstrate that traditional narratives based on supposedly clearly defined cultures located in specific territories with boundaries – in other words, cultures which form closed and separated entities coming into contact to each other and exercising different forms of influence – are a very static and, in many cases, misleading explanatory model for understanding cultural processes and interaction. The alternative model, which is currently being developed at Heidelberg, is based on the concept of transculturality, which introduces a dynamic understanding of cultures and their plurality of forms.³³ Cultures cannot be defined as holistic units or systems of distinct social affiliation. The focus of the Heidelberg Cluster of Excellence lies therefore not on territories or cultural entities, but on border crossing processes. The main objective is to analyze a large range of historical forms of mobility and connectedness from pre-modern and modern cultures, highlighting circulatory patterns, issues of processuality and shifts in practices, ideologies and social values.

This alternative way of viewing cultural processes aspires to overcome the simplistic level of maps showing cultures with delineated boundaries interacting with each other, an interaction which can be assessed by imports/exports. It highlights instead the dynamic processes that linked these territories to each other.

Under this wide theoretical umbrella of transculturality, one can isolate and employ more specific analytical concepts, which are very relevant to the subject of this conference. One good example here is the influential work of Arjun Appadurai. Appadurai has stressed the importance of these culturally formed circuits or networks that shape and cover the multiple paths of circulation, and are of course not identical with cultural territories or nations. For a more dynamic understanding of cultural phenomena in the era of globalization, Appadurai has introduced the concept of »scapes«, which are virtual, deterritorialised spaces that are shaped and structured by a variety of flows and processes.³⁴ Appadurai defines different types of »scapes«, such as ethnoscapescapes, technoscapescapes, financescapescapes, etc. For example, migration phenomena constitute ethnoscapescapes: They are virtual spaces consisting of the movement of people through different regions. A further example demonstrating the hermeneutic potential of this model is the Phoenician culture: It could be better perceived as a »scape« and not a territory, a scape consisting of the routes of Phoenician trade, the regions with Phoenician presence, and the different forms of their interdependence or interconnectedness.

Appadurai also stresses the relationship between the forms of circulations and the circulation of forms, and the importance that their relationship may have for cultural history; whereas forms include things, styles, techniques, beliefs, etc.³⁵ The different forms of circulation (diplomatic gift exchange, trade, tribute, migration, etc.) determine the nature of the forms circulated within them and vice versa. Finally, Appadurai draws our attention to the difference between the problem of connectivity and the problem of circulation.³⁶ There are periods of Mediterranean history in which both connectivity and circulation were at a height, and others in which we can detect a high connectivity and low circulation, or vice versa. The recognition of this difference can be decisive for understanding archaeological evidence. This rich variety of circuits, scales, and speeds determined the circulation of cultural elements in every period of Mediterranean history and has to be acknowledged before any attempt to evaluate archaeological data. Summing up, the concept of transculturality does not necessarily aspire to replace, but actually to enrich the older models of acculturation, cultural contact, mixture, and influence, by providing new and more complex concepts relating to flows and networks.

DO WE REALLY NEED THEORY?

Let me begin the fourth part of my paper with a provocative question: do we really need theory to understand Mediterranean phenomena? Not necessarily, as the following example makes evident: In the early 1960s, Shlomo Dov Goitein began to study the huge Geniza archive in Old Cairo, which is a vast collection of sacred and secular documents preserved in one storeroom of Cairo's Jewish synagogue, dating from the 11th to the late 19th century. These documents referred to an unprecedentedly wide range of activities of a prosperous class of Jewish merchants, covering virtually all aspects of daily life. Goitein published the results of his monumental study in six volumes between 1967 and 1988 under the title *A Mediterranean Society*, without applying any theoretical construct.³⁷ He structured the mass of historical data and his own observations according to a very simple and traditional concept: economic foundations, community, family, daily life, and the individual. The scientific importance of this purely empirical study can be compared with Braudel's *Mediterranean* and Horden and Purcell's *Corrupting Sea*. Goitein's descriptive and theory-free approach offers the reader an admirable panorama of Mediterranean life observed through the prism of the works and days of a mercantile class.

There can be no doubt that theoretical constructs are powerful methodological instruments. However, they cannot be regarded as an indispensable prerequisite for archaeological and historical research. Theory can prove very useful when seeking a framework of reference to evaluate archaeological data at hand, especially

when written evidence is scarce or non-existent. Theory helps to overcome the descriptive level which is so often imposed to us by the fragmentary and silent nature of our sources. It helps us to decipher things or practices, to fill gaps. However, its use has to be rational. Before its application, a theoretical concept must be chosen carefully among many others and the arguments which led to this choice must be explained. Moreover, such a choice has to be taken not a priori but a posteriori, or in other words, only after a thorough knowledge and treatment of the pertinent primary evidence. Only in this case, we can talk about an applied theory, which can exploit the full explanatory potential of archaeological data.

Looking at theory from the perspective of those who are employing it – in our case archaeologists – the application of an explanatory model can be perceived as a sort of »investment« to increase the value of empirical evidence and the »intellectual capital« that any of us possess (of course, to different degrees). Theory enables us to acquire good scientific results with a modest investment of scientific energy and time – modest when compared to the decades of work Goitein spent in writing his *Mediterranean Society*. Theory accelerates the pace of scientific production and increases, as well as ensures, its quality. However, there can be no investment without an appreciable portion of capital. This »intellectual capital« is, in our case, nothing else than an in-depth knowledge of one specific field of enquiry. This knowledge can only be acquired by the command of more than one language which enables a basic acquaintance with the vast body of secondary literature accumulated during the intensive scientific works of decades – if not centuries – in several academic traditions.

An archaeological approach to a given period of Mediterranean history can thus refrain from applying some modern theoretical constructs. In an ideal case, however, one should try to bridge the gap between empirical archaeological knowledge and theoretical vision by providing a sensible equilibrium between the thorough study of primary evidence and the thoughtful application of explanatory models. After some decades of intensive research, we are certainly capable of proposing hermeneutic models of approach, which are produced a posteriori, guided from the evidence at hand.

SHAPING AGENDA AND THE CONTRIBUTION OF THEORY

The three-plus-one paradigms already discussed can provide a backbone for constructing a common theoretical ground and defining common objectives and concerns for current and future research on the Iron Age Mediterranean. Connectivity emerges as the principal feature of Mediterranean geography and history. This insight corresponds to the significance of networks and flows in current research in different fields of social science. Appadurai's theoretical concepts make the necessity of such an approach even more apparent. However, it would be unwise to restrict ourselves to a single key term, when we envisage explaining the history of a region, which was characterized by its »paroxysm of factors«, as Purcell aptly expresses it.³⁸ What appears to be a more efficient analytical tool for future studies is Horden and Purcell's dynamic hermeneutic model, their triangle of factors referring to (a) a sophisticated network of connecting microregions, as a (b) main risk strategy against environmental risks, which helps to overcome (c) the fragmentation of space and resources. Given the fact that this basic triangle corresponds to a rather narrow and one-sided focus on economic and environmental history, it is necessary to expand it by the addition of further elements, which would enhance its applicability in the context of archaeological research. An essential addendum would be the cultural dimension of connectivity, the impetus of Mediterranean people to experience, acquire, adopt, and transform foreign cultural values. We can use this extended version of Horden and Purcell's triangle to form the core of a new coherent conceptual matrix for interpreting Mediterranean phenomena, a sort of »toolkit of Mediterranean characteristics«, which I would like to call the »Mediterranean formula«. This

formula, which can be compiled in the future only after a collective, interdisciplinary work, may help us to circumscribe and explain Mediterranean processes, to grasp the basic dynamic forces that have set in motion the core elements of Mediterraneanism, and to explore the manifold ways in which the human history of this region was expressed through social, economic, cultural, and religious interaction.

Let us finally turn back again to the topic of this conference volume. The published papers promise to offer valuable insights to cultural transfer in the Mediterranean by exploring both the maritime and terrestrial parts of this region in their manifold interdependence, and focusing either on far-reaching networks or small-scale processes. After the *Corrupting Sea*, we can be confident about the potential of a microregional perspective. No less promising appears to be the encounter of different area studies and different academic traditions, which was evident during the conference and can be seen in the published contributions. This encounter can be very decisive for the future, not only for analyzing important evidence from different angles, but also for setting a future agenda for the study of the Iron Age Mediterranean. Building on these decisive assets, the present volume has the potential to not only effectively apply but also to challenge the theoretical models which have been discussed in this paper.

Notes

- 1) The present paper is a revised version of Panagiotopoulos 2011 with a stronger emphasis on Mediterranean paradigms and their applicability in archaeology.
- 2) Alcock 2005.
- 3) Morris 2003, 33. 37. 40; see also Malkin 2011, 13.
- 4) Some crucial aspects of this problem are discussed by Horden/Purcell 2000, 7-49; Shaw 2001, 419-424; Harris 2005, 4-5; see also Purcell 2003; 2005; Molho 2002, 490-491; Fentress/Fentress 2001, 203-204.
- 5) See Harris 2005, 12 (with note 28). – Bagnall 2005.
- 6) Malkin 2003, 2.
- 7) Braudel 1949.
- 8) See for instance Burke 1990, 32-42; Aguirre Rojas 1999; Clark 1999. On the impact of the Annales School on Archaeology see Bintliff 1991; Knapp 1992.
- 9) Malkin 2003, 2-3.
- 10) Shaw 2001, 419-420.
- 11) Horden/Purcell 2000.
- 12) Purcell 2003, 16.
- 13) Purcell 2003, 10.
- 14) See Purcell 2003, 20-21.
- 15) Fentress/Fentress 2001, 204.
- 16) See among others Fentress/Fentress 2001; Harris 2005, 29-34.
- 17) Renfrew/Wagstaff 1982; see also Horden/Purcell 2000.
- 18) Knapp 1998. – Blake/Knapp 2005. – Knapp 2007.
- 19) Sherratt/Sherratt 1991; 1998.
- 20) Patton 1996.
- 21) Morris 2003.
- 22) See above note 3.
- 23) Morris 2003, 30-31.
- 24) Morris 2003, 31.
- 25) Morris 2003, 32.
- 26) Kantor 1947.
- 27) Morris 1992.
- 28) Bernal 1987-2006.
- 29) Morris 2003, 32. 37-40.
- 30) Morris 2003, 46-50.
- 31) Morris 2003, 44.
- 32) Purcell 2005.
- 33) See Welsch 1999.
- 34) Appadurai 1990.
- 35) Appadurai 2010, 7-8.
- 36) Appadurai 2010, 8.
- 37) Goitein 1967-1988.
- 38) Purcell 2003, 13. 23.

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