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6. Aubin and Doutté's *Morocco of Today* (London and New York: 1906).

7. For a full bibliography of Michaux-Bellaire, cf. R. Gerofi, "Michaux-Bellaire," Tinga, *Bulletin de la Société d'Histoire et d'Archéologie de Tanger,*'1 (1953) 79–85.

8. David M. Hart, "Clan, Lineage, Local Community and the Feud in a Rifian Tribe," in *Peoples and Cultures of the Middle East: An Anthropological Reader*, Vol. 2, Louise E. Sweet, ed., (New York: Natural History Press, 1970), pp. 3–75.

9. There are a number of worthwhile studies of Berber customary law in Morocco (even though the majority have not been written by professional anthropologists), notably: Marchy, "Le problème du droit coutumier Berbere," *La France Méditerrannéne et Africaine* (Paris: 1939, republished with preface by G. H. Bousquet in *Revue Algerienne, Tunisienne et Marocaine de legislation et de* jurisprudence (Algiers: 1954), pp. 127–170; Georges Marchy, *Le droit coutumier Zemmour, Publications de l'Institut des Hautes Etudes Marocaines*, Vol. 40 (Algiers: J. Carbonel, and Paris: Larose, 1949); a posthumous compilation of which the second volume on penal law was never published. Robert Aspinion, *Contribution a l'étude du droit coutumier Berbere Marocain: étude sur les coutumes des tribus Zayanes*, 2nd ed. (Casablanca: A. Moynier, 1946); G. H. Bousquet, "Le droit coutumier des Ait Haddidou des Assif Melloul et Isselaten," *Annales de l'Institut des Etudes Orientales* (Faculte des Lettres de l'Universite d'Alger), **14** (1956), 113–230; and in Spanish, for the Rifian tribes of what was Spanish Morocco, Emilio Blanco Izago, *El Rif*, 2a Parte—La Ley Rifena, II: *Los Canones Rifenos Comentados*, (Ceuta: Imprenta Imperio, 1939). In this case it was only the second volume of the work in question that was ever published. See also my ethnographically oriented critique of this last-mentioned work in David M. Hart, "Emilio Blano Izago and the Berbers of the Central Rif," *Tamuda*, **6**, 2 (1958), 171–237. And for an overall view, see G. H. Bousquet, *Les Berberes*, Collection "Que Sais-Je?" No. 718, 2nd ed. (Paris: Presses Universitaires de France, 1961). The subject of Berber custom may be an unpopular or even outdated one since Moroccan independence and the rescinding of the Berber Dahir of 1930, but it is nonetheless of great intrinsic interest, and a number of tribes have experienced real difficulties in various respects as a result of readjustment to the *sharī*ᶜ*a*.

10. There is, however, one excellent study of kinship and family structure in Arab Algeria, namely, Robert Descloitres and Laid Debzi, "Système de parenté et structures familiales en algerie," in *Annuaire de l'Afrique du Nord* (Paris: Centre National de la Recherche Scientifique, 1963).

11. There is a great deal of valuable material contained in the mass of unpublished North African tribal reports, mostly by French Army Officers, on file at the CHEAM (Comité des Hautes Etudes de l'Afrique Méditerrannenne), Paris, which was founded by Robert Montagne; and also evidently at the French Overseas Archives (Archives d'Outre-Mer) at Aix-en-Provence. In Spain the Garcia Figueras collection in the Africa Section (Section Africa) of the National Library (Biblioteca Nacional), Madrid, is very much worth consulting, and the same would seem to be true of the military archives at the Military Historical Service (Servicio Historico Militar), Madrid.

CHAPTER FIVE

Islamic Art and Archaeology

OLEG GRABAR

A fuller and more correct title of the "art" whose "state" is discussed here would be The History of Islamic Art and Islamic Archaeology. History of art and archaeology are related in that they both deal with man-made "things" as their raw material and that their primary or, at the very least, preliminary mode of operation is through visual observation. Beyond this generality the two disciplines diverge considerably, and their definition has given rise to any number of discussions. Without entering into the latter, I should like to propose the following working definitions for the purposes of this chapter.

DEFINITIONS

Archaeology can be understood in two ways. One is technical and refers to a procedure for the retrieval and ordering of "things"; in this sense it is akin to a field like statistics, which may have an exciting end in itself but which tends to be used most of the time for some other type of information. Archaeology in a broader sense is also an attempt to provide a complete description of the material culture of a time or place. It tends to be more effective synchronically than diachronically and, if utilizing proper techniques and done on a large-enough scale, it can come close to providing all available or possible information about its subject. Obviously enough, archaeology has been most successful and most developed in areas and times such as prehistory, where it is the only means to acquire any sort of information. When used in historical and highly literate periods like the Islamic Middle Ages, its functions have been modified and on the whole narrowed. At its most limited, it is a technique for the determination of an architectural monument's history; an excellent example of this type is the history of the Aqsa mosque worked out by Robert Hamilton, and similar enterprises are proceeding at the mausoleum of Oljaytu in Sultaniyah, at the Shah-Sindah sanctuary in

Samarkand, and in the Great Mosque of Isfahan. The ultimate value of the information received tends to depend on the importance of the monument.

At a somewhat broader stage, archaeology can provide the context, evolution, and contemporaneity of otherwise known features. For instance, we always knew that sugar existed in the Middle Ages but it is only through the discovery of a sugar factory in Susa that we know what an actual enterprise looked like; archaeology alone can provide the development of as typical a feature of medieval Islam as the bath. It is only through excavations that ceramics or glass, the most ubiquitous materials for objects of daily or unique use, can be seen both in their development and as contemporary groups. In all these instances the knowledge of the existence of sugar factories, ceramics, or baths precedes their archaeological investigation, but the latter makes knowledge more secure.

A third stage of archaeology is the discovery of new information. The most spectacular example of the sort in Islamic archaeology has been the revolution in our understanding of the Umayyad period that has come with the excavation and investigation of hitherto unknown and unsuspected princely establishments, and the concomitant disproof of the theory of the *badiya* developed by historians on the basis of texts alone is a striking legitimation of the activity as a whole. Though perhaps less striking, the Nishapur excavations did bring to light and into a reasonably assured chronological context a whole art of ceramics, until then little understood.

A last stage of archaeology is problem-oriented; i.e., it seeks to resolve through excavations or surveys questions raised through other means. For instance, archaeology can solve certain problems of trade by demonstrating the spread of traded objects and staples; it can delineate valid regional units at various times by showing the similarity of material culture between a variety of sites; or it can provide a physical setting for characteristic concerns of historians, such as urbanism or the expression of social differentiation. Although several archaeological enterprises such as the ones at Sirâf, Qasr al-Hayr, Balis, or Fusṭâṭ have some of the objectives in mind, the work itself is still not completed or not published, and it is too early to judge the results.

The first two kinds of archaeology are essential but in a way marginal to the existence of the monument and artifacts with which they deal, and the third one is frequently accidental; the last one, consisting as it does of the setting of problems and hypotheses to be resolved through the only technique that can do so, is obviously the most appealing and exciting. It presents one major danger, however. By being essentially quantitative, it can succeed only if it is done on a sufficiently large scale. Thus to know the urban structure of Fusṭâṭ does not solve the problem of Islamic urbanism, *unless* a large number of other cities are also investigated, and the spread of a certain kind of ceramic indicative of a discrete cultural entity can be assumed only with a large

number of samples. Though reasonably well solved in such instances as Hellenistic or Roman cities or Bronze Age Palestine, these methodological questions have simply not been raised in Islamic archaeology, with the possible exception of northeastern Iranian ceramics, where at least the elements for solution do exist.

The history of art is a rather different discipline, although some of its material derives from archaeology and almost all of it can be given an archaeological context. It is based on the assumption that there are qualitative variations in the "things" made by man, and its primary concern is to determine the nature and development in time of these qualitative modifications. In other words, the history of art makes choices in existing documents. For the most part the choice is made according to the time of the document on the assumption that every period had a range of qualitative achievement. At times the choice is made by the taste of the contemporary observer, as different sensibilities lead to new or different interests. All sorts of means have been developed over the past century for visual analyses defining and explaining the nature of and the reasons for the creation of works of art. They range roughly from technique of manufacture, or other connotations, to style, with its definition of manners of treating a subject (composition, proportion, color, etc.), to mode, a complex combination of style and subject matter. As in archaeology, there is a tendency to consider most of these techniques of study as universally valid, and the end result of the art historian's job is threefold. First, it is to evaluate the quality of any one monument, as was done by Ettinghausen in a group of iconographic studies of paintings and ceramics, and by S. C. Welch in discussing the Houghton Shahnameh. Second, it is to provide a history of taste, i.e., of those elements which a culture, a time, or an area tended to consider as good or beautiful. No such work has yet been done in Islamic art except by inference, or in some particular cases of dating coherent groups of objects. Third, it is—or at least can be—a means either to demonstrate some broader relationship between man (of a time or universal) and his creation or to develop methods of visual analysis which, for one reason or another, can best be formulated through the art of a given culture. Thus Western art is probably best equipped for the study of the representation of man or of space and Islamic art for architectural ornament.

There is yet another level of dealing with a work of art, which is that of criticism. Its objective ranges from an attempt to explain to others the esthetic or qualitative point of a given monument all the way to a Ruskin-like personal, almost moral, statement inspired by looking at a work of art. Whether such criticism is really possible without a deep knowledge of the surrounding culture is a moot question, as can be shown in any number of ludicrous picture books with "personal" essays, but it can be assumed, at least as a methodo-

logical hypothesis, that something of value about a given monument can be said by considering it by itself.

SPECIFIC APPROACHES

There are instances or works by historians of Islamic art or by archaeologists dealing with Islam which illustrate characteristic procedures of either field, but such is not the case with all scholarship in Islamic art and archaeology. When compared to Italian or even Chinese art, the study of Islamic art is primitive and underdeveloped and certainly Islamic archaeology cannot be compared in achievement to classical archaeology. Part of the reason is its novelty, for as a definable concern it is hardly older than this century, and its practitioners have been and still are remarkably few. In addition, the field has been affected by the fact that it grew and still grows under the impact of a broad range of factors. Some of these are simply motivations, the reasons why an individual works in a field or a certain problem becomes studied; these are often unique to the field. Other factors are methods of work and can be shared by several areas of art history, but they are affected by peculiarities in the study of any one tradition. Still others are more personal preferences or even prejudices. In most scholarship more than one of these approaches or moods can be discerned, but they are interesting and important to identify, partly because they explain the growth of the field, its accomplishments, and its failures, and partly because they may best enable us to define what could and ought to be done. For each one of them has values and limitations. I should like to identify nine such factors, differing a great deal from each other in importance and impact, yet at times very much interrelated.

1. Without in any way acquiescing to the occasional pejorative aspects of the terms, I should like to call the first approach *antiquarian–scholarly*. Whether it started with collectors or with philologists, it was the first one formed, some time in the first half of the nineteenth century when Lanci, Reinaud, Coste, Dieulafoy, Khanikoff, and others began to study ruined or standing monuments all over the Muslim world and to notice Muslim objects in private and public collections. Although the first major monuments of this tradition were the pertinent volumes of Napoleon's expedition to Egypt, the tradition came into its own around the turn of the twentieth century, when Max van Berchem, Friedrich Sarre, Gaston Migeon, and Henri Saladin, to name only those whose works are best known, provided the first more general studies illustrating its point of view. The strength of this tradition was primarily its detached disciplinary rigor and secondarily its amazing spread of coverage. The knowledge of Oriental and Western languages or, alternatively, passionate collecting of objects, was essential to it, and Spain

and Central Asia as well as intermediary lands were felt to be understood together. It concentrated on making available and known monuments which were not in the normal circuit of educated men; it often tended to be descriptive, although the genius of Max van Berchem, for instance, did go much beyond the purely descriptive. The weakness of this tradition was first of all its very detachment; superbly proud in the quality and purity of its search for knowledge, it often failed to descend from its ivory tower and seemed at times arcane in its concerns, requiring a type of acquisitive, linguistic, and intellectual background which reduces its impact, as many of us have found as we confront modern students with its achievements. There was an elitist "clubbism" about this tradition, as there still is today among private collectors, "Orientalist" scholars, and world travelers. Another weakness is that it was a tradition that was very much aloof from the living culture of Islamic lands, even though one could argue that its high ideals made it independent of the relevant political and social contingencies. With the possible exception of Khalil Edhem in Istanbul, no Muslim or Near Easterner was involved in it in its early stages, and very few have been since then. In short, it serves as an example of the noble ideal of nineteenth-century scholarship, but as Herzfeld wrote so well in a sadly nostalgic obituary of Max van Berchem, there is some uncertainty about the likelihood of its value in today's world. Yet its successes, its dreams of large corpora, its ambition of making everything known and available, and its moral and intellectual rigor are permanent and will never be replaced.

2. The second point of view is of more recent vintage and, though it is not entirely correct to see it as a reflection of nationalism, there is little doubt that it was affected by nationalism and by the emergence of real or artificial nations all over the Muslim world. I should like to call this point of view *local*, and at times it has become parochial. Its sources are less in the elite universities of the Western world than in local initiatives, at times teachers in primary or secondary schools, at other times administrators, architects, or other professionals born in a certain place or sent there through some vagary of fate. Such were the French administrators of Algeria like General de Beylié, to whom we owe the first excavation of an Islamic site, or the British and Indian sponsors of the Archaeological Survey of India, or the Russian officials of the Society of Lovers of Archaeology in Turkistan, or Mubarak Pasha reporting on every street of Cairo with Maqrîzî's description at hand. At times inspired with tireless enthusiasm as in the case of A. U. Pope, who almost single-handed made Iranian art known to a wider public, it is a tradition that can be as dully descriptive as annual reports of local departments of antiquities or as ridiculously nationalist-parochial as some earlier works done in Turkey and more recently in Central Asia. Its main strength is that it examined one area in depth, through constant familiarity; it pro-

tected its own immediate heritage and collected objects in new museums whose frequently pitiful looks should not hide their importance. The works of Ayverdi in Turkey, of Irâj Afshâr in Iran, of L. T. Bretanitski and others in Azerbaijan, and of Abû al-Farâj al-ᶜUsh in Damascus are examples of this mode of activity at its most useful and, at times, its best.

By being local, this interest became supported by new governments, in ways that vary enormously from country to country but with the principle of a *national heritage* throughout. Much of its effort has been to preserve and to count. Thus one knows that there are nearly half a million objects of Islamic art registered in Turkey, and mechanisms for the repair of monuments exist. Bulletins, annuals, and annals are associated with this tradition in increasingly frightening numbers, but each issue brings to light something new and of value. It is also a tradition that involved Muslims in the understanding of art of the Muslim world, and in recent years it has adopted excavations and surveys as a useful way to increase knowledge about one's own patrimony. A striking achievement of these activities lies in the mass of usually unpublished archives on architectural monuments lying in archaeological and archival offices all over the Near East.

An approach to Islamic art and archaeology that can by extension be related to the local one is the approach of certain collectors and of a large body of literature issued from collecting. It involves concern with a single technique, regardless of its source of origin. Best known among students of carpets, it exists also among collectors of ceramics, glass, and coins, but its passion is not always matched by equal intellectual abilities.

The weaknesses of this tradition as a whole, however, are considerable. First the practical task of discovering, maintaining, and making available has almost throughout been beyond the financial, technical, and often intellectual possibilities of many countries. Whereas the Berlin Museum and the Metropolitan Museum can control their collections, the Cairo and Istanbul museums cannot, because their responsibility is national rather than selective and, though Western scholars may complain that the minarets of Herat are falling down, only the local administrators are obliged to do something about it. In theory this first weakness can easily be made up through funds and training, but of course matters are more complicated, for funds do not exist and personnel ready to be trained are even scarcer. A second weakness has been national compartmentalization, often of a ridiculous nature when combined with linguistic antagonisms. Books and knowledge do not move easily from Syria to Iraq, from Iran to Afghanistan, or from Soviet Central Asia to Iran. Much of this is the result of political realities that may change, but in addition there is inherent in this kind of concern the weakness of national distortion, which exists as well (or at least existed) in the study of French, German, or Italian art. Altogether, then, though the local approach

is potentially highly desirable, it has not yet made its mark, for it has failed to meet some of the basic criteria of this field. It has not succeeded in generalizing, and it has not been able to define with adequate precision regional differentiations or else it has imposed improper ones. Some exceptions exist, as in North Africa and Spain or in Soviet Azerbaijan and Central Asia, partly also in Turkey, but in all cases, except possibly the city of Cairo, completeness of information has not been achieved in any accessible form and conclusions are often weakened by insufficient or improper knowledge of neighboring lands.

As one dreams of Dehio's work on German art, of Van Marle's on Italian painting, even of Siren's on Chinese painting, it is an approach that is still at an elementary level. However, in contrast to the preceding one, it is an approach that not only still exists but that is constantly growing.

3. A third approach is theoretically quite easy to define. It consists in starting from an accepted disciplinary problem or method of the history of art, and applying it to Islamic art. In this pure and simple form, as in attempts to identify the authorship of paintings, it has been rarely used, although the work of I. Stchoukine and S. C. Welch, among others, comes reasonably close to it. A series of iconographic studies by R. Ettinghausen can be used as examples of typical art historical methodology, and there are other examples as well. An important contribution to this approach has been made by scholars in other periods of the arts who sought in Islamic art some proof of or examples for a point raised elsewhere. E. Baldwin Smith in his *Architectural Symbolism* or Henri Stern and André Grabar in dealing with Christian art have utilized Islamic examples and have contributed to their understanding. But it is perhaps in the Vienna of the turn of the century that the formidable talents of Alois Riegl and of Joseph Strzygowski have sought to illustrate in the most interesting manner their own art historical theories through monuments of Islamic art. Although one still occasionally finds references to works of Islamic art in more recent manuals or theoretical works, such references are rare and usually limited to a few topics such as ornament or miniatures.

The strengths of an art historical approach are obvious. On the one hand, it removes the exoticism or parochialism attached to the first two points of view and furnishes a presumed universal system of investigation. On the other hand, it makes the knowledge of the Muslim world accessible to all who are interested in archaeology or in the arts. In theory it puts it on the same level of investigation and of value as any other artistic tradition. Perhaps most importantly, it introduces the Muslim world within the curriculum of all students rather than of a selected few, and almost all teaching commentators have pointed out that their audience consists much more frequently of students of other arts than of actual or potential specialists. The setting of

monuments of Islamic art within generally accepted procedures and concepts of the history of art is thus a highly desirable goal.

The defects of this point of view are equally obvious. First of all, it tends to exclude those who belong to the Near Eastern world or to make them strangers within their own culture, for it is rarely realized how much the basic concepts of the history of art issue from a fundamentally European visual and intellectual tradition. A second defect is somewhat more insidious and applies particularly to the use of Islamic art for other traditions or for broadly theoretical purposes. It tends not only to abstract a monument from its setting but at times even to abstract a single feature in a monument for the demonstration of some general point. This has been very true, for instance, of the treatment of Islamic monuments by Strzygowski and by Baldwin Smith. Yet without denying the validity of any attempt at general patterns, an obvious objective of a historian, and a fortiori of an art historian, is to understand the uniqueness of a moment or of a single work of art. Finally, a defect of this approach concerns its practitioners. Ideally they should acquire mastery in two areas, as disciplinary specialists and as area specialists. In reality, one or the other tends to predominate in a frequently uneasy balance.

4. An ideal archaeological approach to Islamic "things" is easy enough to define, for it would simply consist in the gathering and analysis of the sum total of available information about a given place or time. Much of the acquisition of the information is to be made through excavation, but its interpretation requires other kinds of documents as well.

Up to World War II, excavations of Islamic sites were carried out by small teams, often only two or three staff members, and their objectives were the reconstruction of architectural settings (usually of a single monument) and the retrieval of complete or almost complete objects. Very few of these excavations were fully published, although their most notable finds were rapidly made available. In addition a very large number of excavations of pre-Islamic sites encountered perforce a lot of Islamic material. When not discarded altogether, these remains were only occasionally made public.

After World War II and especially in the past decade, archaeology has undergone extraordinary changes. It developed masses of new techniques of fieldwork and recording, and a large number of specialists became necessary for the proper evaluation and definition of its results. Staffs of 20 or more are no longer a rarity. Alternatively, survey techniques utilizing aerial photographs and a variety of sampling devices have made it possible, at least in theory, to provide reasonably accurate reconstructions of such features as town plans, canal systems, and other basic elements of traditional life. Furthermore, a whole branch of archaeology has become concerned with its own methods as an abstract phenomenon and seeks to use a site for the illustration of broad theoretical questions of knowledge and of interpretation.

Finally, mention should be made of an activity which is not archaeological in the sense that it is not carried out by professional archaeologists and that its excavation procedures are not systematic, yet is very common and has provided or can provide significant results for the understanding of an area's material culture. This activity is the restoration of major monuments, usually done by specialized architects with little or no training in the area.

Although the earlier, more artisanal, manner of operating still continues and will always exist (not necessarily with worthless results, though almost always without the expected completeness of results), and although the very latest methodological concerns have never yet been applied to an Islamic site, it is essentially the two methods of a large-scale operation with many specialists and of surveys as well as restorations that will affect the field. The best and almost unique example of the large-scale operation is the excavation of Sirâf in Iran. It is not yet possible to judge the results of the excavation from our present point of view in defining an approach, but two general points are worthy of note. One is that the preliminary publications of archaeological work tend to be overly technical and to avoid elements of hypothesis, speculation, or interpretation which are most likely to be of use to an area specialist. The other one is that the abstractly conceived technical process of recovery and recording occasionally hampers the elucidation of a particular site in its entirety, for only too often it is not or cannot be modified easily enough in order to adapt to the content being discovered. This need not be a criticism, if funds and time available for any excavation were unlimited. Since this is usually not the case, an archaeologist is compelled to make choices, and some of us feel that choices should be made on the basis of the site's likely significance for the culture investigated rather than for purely technical archaeological needs. It should be emphasized, however, that this is a point of view likely to be challenged by many, often with very valid arguments. Yet it seems altogether true that an archaeological approach of this sort tends to provide minute information which is difficult to relate to broader issues of the visually perceptible world of Islam. It may be added that this most objectively scientific method of gathering and analyzing documents is the only one pertaining to the arts that can be used only once, for it destroys the conditions in which it was used. None of the control devices that exist can entirely replace an assumption of honesty on the part of the investigator.

One by-product of this archaeology bears more immediate fruits. It consists in technical analyses of soils, mortars, glazes, metals, woods, and so forth. Although only in their infancy, analyses of minerals in ceramics, of gold residue in silver (a technique issued from other concerns but typologically relatable), and of sources for colors are likely to have significant results in such areas as identification of workshops, schools, and international trade.

Surveys have been done in a systematic way only in parts of Iraq, but the

results have been quite fascinating, for it was possible, in R. M. Adams' work primarily, to provide fairly rapidly and at lesser cost than excavations an adequate picture of the use of land through history with all sorts of subsequent social and economic consequences. The main problem with surveys, of course, is that there is an accidental character to some of their evidence, especially in places of high occupational density, and therefore they must generally be followed by a coherent program of soundings and excavations. However, as preliminary hypotheses and as general indicators of an area's characteristics and likelihood for more precise work, they are essential.

Restorations appear at first glance to be no more than technical jobs of repair and consolidation. Their quality varies enormously from the admittedly superb work carried out in the Alhambra to controversial ones, like the restoration of the Dome of the Rock in Jerusalem, to sheer atrocities, both esthetic and historical, as in the mosaics of the mosque of Damascus. The problem is that most of the time these are generally expensive operations carried out either because of immediate necessity or for tourism. In most cases, the job is done under very loose scientific supervision and the results are rarely published. Thus there is a lack most of the time of adequate documentation of "before" and "after," and the justification for any one reconstruction is all too frequently unavailable. The objects or other remains found during the work are almost always unrecorded or thrown away. As a result, although restorations could become major archaeological sources, they have often become disasters, because for practical purposes they are irreversible. The remarkable exception of the work done by several Italian teams in Iran shows how invaluable restoration can be. Then, since all monuments cannot be preserved, there always remains the nagging question of which to preserve. Most commentators feel that all of them should be protected and old towns and new settlements should be developed in some meaningful relationship to older monuments; others argue that there are few monuments of demonstrable uniqueness and that they should not become frozen memorials to one time's esthetic choice.

To sum up the archaeological approach, one could say that it is a priori the most likely way to acquire a complete visual picture of a time or a place and that it has developed an important series of subfields of potential use. Moreover, because it is more abstract in its methodology, it can be adapted to many settings, and it is noteworthy that excellent archaeological work has been done by all Near Eastern countries, although few have dealt with the Muslim world except Iraq. The approach is weakened, however, by its cost, by the great variety of talents it requires, by an uncertainty about the degree of generality that can be derived from any one instance, and by a very spotty and unsatisfactory record of publications.

There is yet one aspect of archaeology that deserves mention, for even

though it is a practical problem, it affects very strongly archaeology's actual and potential effectiveness. Although most other approaches can be used by a single scholar working alone, archaeology is an expensive activity usually funded by the country of the chief excavator (occasionally by international bodies like UNESCO for restorations) which depends for its success on the host country. The point is that there are enormous variations at this time in the kinds of facilities, help, and expectations to be found in the countries of the Middle East. These variations are not merely pleasant or unpleasant peculiarities of any one country but reflections of archaeological work still done predominantly by foreigners. Three questions appear to be constantly raised: what is the real use of excavations by foreigners, to develop a true national heritage or to provide documents for an alien concern? Should a host country control the choice of sites to be excavated as rigidly as most countries control the artifacts that are found? If so, how? Finally, is an excavation a privilege granted to a foreign investigator, whereby the purposes may be dictated in part by local needs, such as urban or agricultural planning (e.g., several instances of so-called "salvage" archaeology), or by touristic value, with implication of restoration and total uncovering of all remains usually not within the budget or competence of an archaeological team?

There are as yet no coherent answers to these questions, nor am I aware of meetings and consultations which could lead to answers valid for the whole area. It is rather unlikely in fact that this will happen, but the very fact that the questions are raised suggests that there will be an unsettled period in successful archaeological work. This is especially true for Islamic archaeology, which appeals enormously because it deals with the past of the area's present culture but which, precisely for the same reason, touches on practical problems and susceptibilities that rarely occur with more ancient archaeology. Although it is difficult to evaluate with certainty what will actually happen, it seems to many of us that a sort of official partnership with local institutions, with a built-in training program for young archaeologists, will become the rule for any kind of long-range and large-scale excavation and that the foreign mission will be required to undertake various tasks of preservation and restoration. On the other hand, surveys are likely to be more easily continued, although their requirement for detailed maps and aerial photographs often involves sensitive areas of national security. In general, it seems rather vain and foolish to consider an archaeological approach to the study of Islamic art from the exclusively theoretical point of view of a discipline, for its very nature as a team operating for many years as an employer of labor in investigating the past of a living culture compels its practical and intellectual integration in the concerns of each country.

5. The approach of the Islamist, i.e., of the scholar trained primarily in languages and in disciplines using language as the main source of data, to the

study of the arts and of archaeology has occurred in several different ways. At times, as in the case of J. Schacht, observations incidental to a scholar's main interest led to a group of archaeological studies. In a slightly more systematic way, the interests of L. Massignon and of A. Schimmel in mysticism and of Massignon in the setting of Muslim culture led them to esthetic or archaeological considerations. Most of the time these contributions illuminate a specific aspect of a monument but they never exhaust them, nor is their point of departure so much a visual one as a textual one. It is also fascinating to observe that quite frequently students of texts fail to notice the archaeological importance of their own discoveries; for example, this has frequently occurred in the publication and study of *waqf* documents which frequently refer to and can be made clearer by existing monuments.

Two other "Islamist" approaches have been far more fruitful and coherent. Although originating in many ways with Max van Berchem, the first one is best exemplified by the work of Sauvaget, who sought to find in the arts and in what he called archaeology (more a type of evidence than a methodological procedure) that "silent web of Islamic history," which Sauvaget defined in his inaugural lecture at the Collège de France. In this tradition visually perceptible and material remains are seen on a par with texts as necessary for an understanding of history; they are not an end in themselves, and Sauvaget was quite caustic about art historians, for he avoided and distrusted esthetic and value judgments as historical documents. Though he was the best exponent of this approach, it is one that has continued in the work of many younger scholars. J. and D. Sourdel's *Civilisation classique de l'Islam* is the first general book in which textual and visual information are closely bound together. A related approach, never expressed in theoretical form, appears in the work of L. Mayer, where texts are used to help the understanding of monuments but the two are not considered as totally parallel data to be sifted with the same philological or archaeological rigor, and usually the monument's analysis is more complete. However, it is worthwhile to point out that a large group of younger art historians began their careers as philologists or text-centered historians.

The other "Islamist" approach has dealt with those areas where writing and visual observation are intimately connected, epigraphy and numismatics. Although the latter is a field in itself that is not peculiar to the Islamic world but that has been strikingly advanced by the immense labor of G. Miles and of his school, the monumental writing studied by epigraphists has acquired in the Muslim world an unusually spectacular development. The reasons for this development are not important to our purposes, but it is important to point out that its investigation, which can illuminate the most varied aspects of Islamic culture, requires a knowledge of languages as well as an ability to define visually perceptible stylistic characteristics.

The key strength of an Islamist's approach to the visual world has been much more than the development of the two ancillary disciplines of numismatics and epigraphy. It involves, next to the sacrosanct text, a host of other documents, often more precise and more real in their own time than a chronicle. It can therefore be of much value to the historian, but it is an approach that is also essential to the art historian or to the archaeologist, for it compels him to put a monument in precisely defined historical, social, and human contexts. Seen theoretically, the weakness of the approach has been—but need not be—that it avoids the esthetic judgments which are almost necessary in the use of visual evidence and which were certainly an integral part of any cultural moment. Perhaps its practical weaknesses have been more important, however. One of them is that it has not really succeeded in developing the kind of partnership between text-centered and monument-centered scholarship which is necessary for its ultimate success. The best practitioners always knew both languages and at least rudiments of visual analysis (often technical rather than esthetic), but this combination, though useful, is not necessary a priori, and one can easily imagine a social historian or a historian of literature working out a number of problems together with an art historian or an archaeologist. However, such teams have simply never materialized, and at this time the double personality of the Islam-centered investigator is still the rule. The Islamic approach has been more successful with architecture and iconography than with other techniques or other art historical analyses. The reasons are easy to see, for architecture is most intimately tied to social life and subject matter is often dictated by literary references. Stylistic or modal analyses have been less frequently accepted by text-centered scholars, and art historians and archaeologists dealing with the *madrasa* have not quite been able to cope with the early literary evidence on the subject.

6. A sixth factor is a particularly delicate and difficult one to define properly, for it involves an important aspect of Islamic civilization. We may call it a presumed reluctance to the visual expression of esthetic feeling. The word "presumed" is used purposely, for it has not been established whether such a reluctance really existed and whether it was a continuous feature of the culture or only affected some segments in its history. The reasons for the existence of this feeling are several. One is the demonstrable fact of an absence of religious art with the representation of living beings. What is curious is that this absence and the existence of a number of iconoclastic texts led to the assumption that figures were absent in all arts, and as a result any example that did appear seemed to be an exception. It is interesting to recall how the discovery in the latter part of the nineteenth century of the paintings of Qusayr Amrah led to significant or ludicrous articles by almost every Orientalist of the time. It should also be recalled that many of the first academic Islamists

were Semitists who often sought in Islamic thought and practices an illustration of Semitic and, more specifically, Jewish ideas and modes of life. Another reason is that the Western and Hindu artistic traditions emphasized so much the representation of the human form that a tradition in which it was weaker seemed to be one without true artistic concepts. Then there appears to be a lack in Muslim writing of esthetic doctrines applicable to the visual arts; it is true that, as it became expressed in its eighteenth- and nineteenth-century forms, official Islam did not acknowledge visual beauty, representation, or symbols as particularly significant to its values, except in the area of calligraphy; the contrast with China or Japan is quite striking on this point. Finally, except in areas of artisanal creation such as carpets or brass objects, there had occurred a loss of immediate artistic creativity in the Muslim world by the time scientific enquiries began and a preconceived view of this creativity developed at a time when most mosques were closed to non-Muslims and the treasuries of palaces were inaccessible.

There may be other reasons as well, although in retrospect it is striking how much it is a West-centered view of what the arts ought to be that has colored the appreciation of Islamic art. In a manual of the history of architecture written in the early part of the century and still regularly reedited (Sir Bannister Fletcher), Islamic architecture is relegated to the "nonhistorical" styles, i.e., those without evolution or relationship to the grand Western tradition.

The consequence of all these factors was twofold. In the Muslim world itself it created for many decades an almost total lack of interest in Islamic monuments. Exceptions were few and usually limited to highly educated or Westernized individuals, often more concerned with Western art than with their own. But the most important result was that, as education developed at all levels, the visual world played almost no part in it. History books lack pictures, visits to museums or monuments are infrequent, and the history of art is almost nonexistent. Until the early 1960s Western art was taught at the universities of Istanbul and Ankara by Turks, but Islamic art was taught by Germans. Today Western art is taught by Iranians in Tehran, but not Islamic art; until very recently Westerners alone taught Islamic art in Beirut, and only in Cairo are Egyptians and Westerners involved. The most important consequence of this phenomenon appears as the situation in the Muslim world is compared with that of China or Japan, for in the latter many of the theoretical formulations for the study of the arts came out of the living culture. No such formulations or concepts came out of the Muslim world, not even at the artisanal level, where a whole vocabulary exists for forms; however, this has never been systematically collected.

Another consequence of this prevalent opinion of Islamic artistic creativity was that it affected the interest in it of Western scholarship. Seen almost exclusively in terms of surface ornament or of artisanal technique, Islamic art tended to be relegated to a secondary position in great artistic currents and considered interesting only as a source of possible influences or as an exoticism. Turqueries were known, but not the striking achievements of Ottoman architecture in Istanbul and Edirne, to use examples of cities which were accessible and fairly frequently visited in the nineteenth century. However, aside from the lack of concern with Islamic art, which is obvious in manuals, university curricula, and catalogs of book publishers, the subtler effect of this opinion has been that the world from Spain to India and from the seventh to the seventeenth centuries is thought of as one single world of esthetic sameness and that almost anyone can write about it. Although some changes in these feelings are beginning to appear for a variety of reasons, they are still easy to illustrate both in university appointments and in recent books.

7. The next two factors to be discussed are of an entirely different kind, for they really are methods of work rather than disciplinary approaches or historical and cultural limitations. In many ways they transcend most of the previous categories, but, partly because of the small number of practitioners in the field, they have very much affected the character of its scholarship. In many ways the most common methodological approach is *monographic* and consists in starting with a single object, painting, or monument of architecture and expanding from its description into as many questions as possible. The effectiveness of the method obviously depends on the quality and breadth of knowledge of the investigator, but it is an approach that is particularly characteristic of the art historian as a humanist, for its ultimate objective is the total understanding of an already existing entity; at its extreme it is applicable by the archaeologist who seeks to understand a particular site, but its greatest exponents have been the connoisseurs who identify and date the making of a work of art and the iconographers who explain its subject matter, its position in time, its impact on later times, and occasionally its belonging to a series.

The advantages of this method, which can be illustrated by several works of R. Ettinghausen on objects and miniatures and by L. Golombek's monograph on Gazur Gah, are obvious: a monument becomes as well delineated as information makes possible at any one time, and the only limitation lies in the author's abilities or knowledge. The weakness of the method is that, unless it is pursued over large numbers of examples or preceded by a theoretical explanation for any given choice, it runs the risk of making one building or object overly significant; hence a whole period may be misunderstood or wrongly focused.

8. The second methodological approach is problem-oriented or synthetic. Its purpose is generally to answer some broad question about the visual expression of a time or about characteristics that require partial information from many sources. An example of the first type would be O. Grabar's attempt to explain the formation of Islamic art; of the second type would be

Mayer's study of Mamluk costume. Although equally affected by the quality of the investigator, the major advantages of this method are that it responds to questions reflecting scholarly or other needs and that its answers tend either to become permanently valid as documents (for instance, Mamluk costume need no longer be investigated unless new evidence appears) or to be fruitful general statements or hypotheses for nonspecialists or students. The weaknesses of this approach are that it can easily lead to superficiality and that its necessary selectivity of information can also be misleading. In many ways its effectiveness depends either on intuition or on a lot of monographs.

9. It is important to point out that Islamic art is an area that is affected by the existence of an art market, although it is perhaps unfair to consider this a factor for the study of the whole of Islamic art. The acquisition of new objects or the transfer of older ones is a major activity which occupies much of the time and energy of the world's major museums and of a number of scholars and collectors. There are many ways of evaluating the impact of an active art market on any given field of study, especially after the recent controversies on the subject and the positions taken by archaeological and anthropological organizations. Without taking sides on these issues with ramifications that are not pertinent here, we could formulate the impact of the art market in the following way. The advantage is that collections can grow all the time if they wish to do so and are rich enough to afford it. As a result it is possible for any place in the world to develop a good collection, and the impact of and interest in Islamic art grow accordingly. There is little doubt that the sale and spread of objects is one of the most successful ways for a field to become better known, and this in turn generates, or at least can generate, the kind of enthusiasm which is useful for the growth of the field. A more limited sideline of an active market has been the development of all sorts of techniques for the detection of forgeries and thus technical study of metals has been much advanced. Even aside from this particular development, the context of a museum and the visual associations that can be made between different works of Islamic art and other traditions are intellectually and educationally fruitful, as the success of so many exhibitions clearly demonstrates.

There are, however, dangers in the existence of an art market. One such danger is that richer areas deplete poorer ones and it is striking that only the cities of Cairo and of Istanbul possess collections comparable in quality to the Islamic ones in the West. For science per se, this is not a significant defect. More serious is the fact that the energies and concerns of collectors or of museums are often unduly channeled toward the acquisition of a new piece rather than the study of an already existing one. But the most serious problem is that of occasionally limiting the availability of objects. Certain miniatures of extraordinary historical importance and esthetic merit have not been seen for almost 40 years, and some objects disappear for years without a trace. The secretiveness that surrounds some of the transactions concerning works of art is certainly not in a scholarly tradition and has on occasion been harmful to research.

It is not really possible to sum up in a simple series of statements the way in which the activities of the archaeologist and of the art historian seen in a broad and general way are affected by a variety of more uniquely Islamic factors or by actual methods of work. It can perhaps be suggested that, in the study of Islamic art, the expected disciplinary spectrum which ranges from cataloging to the elaboration of general theories has been and is still influenced by an unusual and uneasily matched variety of intellectual or practical interests, by a lack of intellectual direction from the culture itself, by the novelty of the field as a scientific enterprise, and by the paucity of its practitioners. As we see below its achievements are all the more remarkable, even if insufficient.

SUBFIELDS

Even if deans, departmental chairmen, center directors, and the general public tend to see a visual tradition of more than 1000 years from Spain to India as a proper field in which to exercise one's talents, in reality a breakdown into subfields has occurred, at least insofar as scholarly concerns and research are involved. Although acknowledging this fact and its necessary occurrence, several commentators have pointed out that some of the exciting aspects of the field are that it has not become overly specialized and that it is still possible, indeed necessary, to roam over vast spaces and chunks of time and to avoid the overspecialization of other areas. While agreeing with the excitement of this traditionally Orientalist view, one may question its value for an understanding of the past. To seek to know everything often means to end up knowing nothing, and a tendency to superficiality does occur unless generalization is based on a hierarchy of reasonably secure and digested information. Furthermore, if the categories of understanding are too broad, they fail to explain a given monument in the concreteness of its time of creation.

However, though the practitioner may expect pan-Islamic knowledge, scholarship and research have tended to divide into four clear types of subfields, and two others more difficult to define, which are unique to Islamic studies.

The clear fields are regional, technical, iconographic or stylistic cross-sections of areas and times, and period studies. Whether for linguistic, national, or other reasons, a great deal of the published work tends to be limited to certain areas: Spain, North Africa, Egypt, the Fertile Crescent (if not actually Syria as separate from Iraq), Turkey and the Ottoman empire, Iran, Soviet Central Asia, Soviet Azerbaijan, and India. Most of the strengths

and weaknesses of what we have defined as the "local" approach apply to work done in this manner. Purely national approaches have been supplemented by broadly regional ones in a few cases, most successfully in western Islam. The second clear subfield is technical: the study of architecture, ceramics, glass, carpets, and so forth. Occasionally, especially with architecture, the two subfields become merged into one, most voluminously in recent years in the instances of Turkish and Central Asian architecture. Comparative studies of stylistic or iconographic motifs (e.g., the unicorn or the calyx as ornament) across techniques and regions are characteristic art historical concerns, requiring usually a particularly broad factual knowledge. Attempts to establish clearly the nature of a period's visual concerns are also typical endeavors, whether the periodization itself is based on historical definition (e.g., the art of a dynasty) or on a valid grouping of monuments.

The murkier but more uniquely Islamic subfields are the relationship between Islamic art and neighboring or earlier traditions and the question of a Muslim iconoclasm. Little has been written on the latter subject in very recent years; it is probable that neither new documents nor new ideas have occurred to anyone, and in all likelihood the traditional setting up of the problem in terms of a search for a coherent ideological position developed at a concrete time seems to have exhausted itself. Whether new ways of dealing with the matter are possible and desirable is very much an open question.

The relationship of Islamic art to earlier or neighboring arts has been the subject of much contemporary concern. The impetus for such studies came at times from other fields such as Byzantine studies or Western Christendom, and at times from the Near Eastern specialist who sought either to demonstrate certain external impacts and their nature or, perhaps more fruitfully, to seek the ways in which earlier artistic themes or ideas were adopted, rejected, or adapted by the new culture. The interesting feature of these investigations is that, when they are not simple catalogs or exercises in self-glory, they raise very important questions about the use and value of visual forms. For to borrow or to utilize a form created by another cultural setting implies a far greater (or at least different) consciousness of one's activity than to continue whatever is prevalent in one's own culture. Working independently Grabar and Ettinghausen have recently provided quite different examples of analyses of influences and impacts and the subfield is likely to be particularly fruitful, both methodologically and for its information.

ACHIEVEMENTS

In order to measure achievement, we must consider three kinds of features. One is whether monuments or other appropriate documents have been adequately studied and/or published, adequacy being measured by the satisfactoriness of substituting the studies for personal and direct contact with the monument. The second feature is whether these documents, if published, are reasonably accessible, for there is little value in knowing that certain groups of Fatimid ceramics have been discussed in detail in an Arab journal unavailable in most libraries of the world or that a very rare Russian book in honor of a Georgian poet contains the only coherent discussion of a very important piece of metalwork from the late twelfth century. But a third feature is equally important. It is essentially a synthetic one and can best be formulated through a series of questions: have general or partial syntheses been done? Are there general hypotheses or conclusions from which scholarship can grow or which can be presented to students as expressing a consensus of the profession? Are these hypotheses of primary significance to other art historians or to specialists in the Muslim world? In other words, how does the field compare with the Italian Renaissance, to use the example of a highly developed field, where complete works of major (and even minor) artists are available in several languages, with a constantly shifting range of hypotheses and generalities stretching from the brilliantly innovative to the chiché-ridden potboiler? Alternatively, is an interweaving of the visual and of the literary, which occurs in the works by Huizinga or Seznec for medieval Europe, possible from the results of Islamic art historians?

Basic Publications

If we take into consideration work in active stages of development as well as work that is known to have been done but that has not been published or exists only in restricted form, the following conclusions seem appropriate.

In architecture, Spain, North Africa, Egypt (until 1350), Anatolia, Azerbaijan, and Soviet Central Asia are reasonably well-known in the sense that the visible parts of standing monuments have been adequately recorded and for the most part published. For Palestine, Syria, and Iraq, the very early centuries are available in similar fashion, but the twelfth and later centuries are not. For Iran and Afghanistan, what is available is so minimal that it is almost impossible to teach an adequate course or to do any useful research without having traveled in the two countries, although much excellent information lies in the archives of various universities and especially in the offices of local departments of antiquities. But this overall conclusion on regions is valid only insofar as basic information is concerned, that is, plans and a fair number of photographs. When we turn to measured elevations, to systems of proportions, or to such features as brick or stone measurements, repairs, contemporary texts, inscriptions, later descriptions, and interpretations, matters are far less satisfactory. In effect it is only for Cairo that the

combination of Creswell's measurements, of Max van Berchem's and Gaston Wiet's epigraphical and textual studies, and of a number of recent monographs may be considered satisfactory. Recent work in Central Asia and in Anatolia has been good for measurements but very weak on textual parallels and inscriptions. The occasional appropriate publication of the visible parts of a few Iranian monuments or of the Cordova mosque should not make us forget that for the Alhambra or for Isfahan in the seventeenth century, we still rely on publications of the middle of the nineteenth century. Finally, once again with the partial exception of Egypt and the more major one of Rempel's work on Central Asia, we have no coherent surveys of ornament, even though there is agreement on its importance, and we have only a minimal number of comparative studies or attempts at defining periods of architectural style. What there is of the latter is based exclusively on political history, not on monuments.

The situation in painting is almost worse. Preliminary surveys exist for Iran through Stchoukin's volumes, but their plates are insufficient. Only a handful of manuscripts has been published in complete form, often a long time ago and with uneven color reproductions. A few of the most famous Arabic manuscripts of the thirteenth century have been published, but not the celebrated albums from Istanbul, although a thorough search through dozens of books and articles does bring together a lot of information. Catalogs are adequate for the Chester Beatty Library, barely passable for the British Museum, the Bibliothèque Nationale, or the Leningrad public library, and practically unavailable for the richest collections in Istanbul or for most museums. Those commentators who dealt with painting have remarked with striking consistency on the inadequacy of much of the information provided even in the better catalogs; more is said about this later.

Thanks to the efforts of D. S. Rice, major works of inlaid metalwork of the twelfth to fourteenth centuries are reasonably accessible in superbly detailed form. Earlier or later objects are only intermittently available, and it is only very incidentally that the more expensive gold and silver objects or jewelry have been studied. Glass has been surveyed by Lamm and ivories are now ideally available through Kühnel's corpus. Rugs and carpets form the only area of Islamic decorative arts with a very extensive bibliography, some of which is only of commercial importance but excellent introductions and summaries do exist as well. Other textiles have been treated less systematically, although much information exists about them either in the form of Serjeant's studies of texts or in the publication of complete parts of single collections (Textile Museum, Boston), or through a number of studies devoted to individual textiles. Unfortunately there is hardly an instance where correlation could be established between a type of textile known through literary sources and a remaining artifact. If one recalls that thousands of

fragments exist and that textiles were not only a major industry but also the most common honorific gifts throughout classical Islam, this lack of correlation is rather puzzling.

A relatable problem occurs with ceramics. Almost all existing studies, including the very valuable manuals written by A. Lane, deal with *la céramique d'art*, the more beautiful or technically more elaborate "museum pieces." Some success has been achieved in identifying major series and in explaining individual objects. On the other hand, none of these works has been of much use to archaeologists faced with the problem of defining and cataloging literally hundreds of thousands of shards. It may be more appropriate to say that, whereas the main types of artistic pottery from Spain, Egypt, Syria, Anatolia, and Iran have been identified, sometimes in considerable detail, the mass of medieval Islamic ceramics, against whose background the fancier pottery could better be understood, is still very much of an unknown quantity. In many ways this is also true of glass and of certain types of bronze objects, but in these areas the problems are less crucial because the objects do not lend themselves to the same kind of precise differentiation.

Although the record of availability by individual techniques is spotty and incomplete, the same is true for individual collections containing several techniques. The excellent catalogs of the Cairo Museum or the partial one of the Louvre are 40 or more years old and no longer correspond to the collections themselves. More recently a few reasonably complete simplified catalogs of public collections in Germany and of private ones in England were made, but too many of the most important collections are simply not available. Success has been achieved only in numismatics, mostly thanks to G. Miles, and in epigraphy, through the *Répertoire d'épigraphie Arabe*. In these cases, however, as in the case of all catalogs, there is always difficulty in bringing information up to date.

Attempts have been made over the years to create independent files of complete information, usually based on one private endeavor. Such files exist in Geneva for epigraphy (Max van Berchem archives), in Washington for Iranian architecture (M. B. Smith archives in the Smithsonian Institution), and in Jerusalem for all of Islamic art (L. A. Mayer Memorial Collection). The problems of all these depositories so far have been lack of personnel, sporadic funds, and uncertain direction over long periods of time. The newest one in Jerusalem is best equipped to solve these problems.

Accessibility

Access to Islamic art by the outsider or by anyone but the users of the richest and largest libraries is the weakest part of the field. None of the general surveys is even adequate and most are downright bad. With a handful of excep-

tions even more specialized manuals, such as existing ones on ceramics or on aspects of Islamic architecture, are spotty and insufficient, whereas the few existing coffee table books contain mediocre texts and usually arbitrarily chosen illustrations. The chapters in the *Cambridge History of Iran* are limited in scope, and the chapter in the *Cambridge History of Islam* is too short. Only one recent attempt has been made at defining the art of a period. Furthermore, it is almost impossible for anyone but the narrow specialist to keep up with recent work in the field, since most disciplinary journals hardly ever pay any attention to it and one must consult dozens of publications dealing with a wide variety of topics in order to find out what is going on.

Accessibility is somewhat less of a problem if one is willing and able to travel. Most monuments of architecture can be visited and most museum collections are open, although the storerooms are often closed and unorganized. Many museums (Freer Gallery, Victoria and Albert, Louvre, Berlin, Damascus) have short brochures highlighting their collections. Photographs can generally be obtained, at a constantly growing cost and with a degree of amiability related to the importance of the seeker, but there are no central files from which one could order them, and all but the largest and richest institutions are often reluctant or unable to satisfy a scholar's or a student's needs. Matters are far more complex when one deals with private collections, for which personal entries are generally needed. A further pernicious aspect of the field has been claims on rights of publication of certain monuments without time limit.

Finally one must consider accessibility through people, primarily university teaching. In this area changes have been considerable over the past few years. Some 14 institutions in the United States and Canada (Harvard, New York University, Hunter, University of Massachusetts, University of Pennsylvania, McGill, Toronto, Victoria, Michigan, Colorado, San Diego State, University of California, Los Angeles, Florida State, and Texas) have committed themselves in more or less permanent fashion to teaching the field. It is rather curious, however, that these do not include some of the best-known departments of art history or centers for Middle East studies. As a result, however well-trained and competent individuals may be, they are often hampered by lack of adequate libraries and photographs. Matters are less clear abroad. In England, Germany, and the Soviet Union, museums have tended to be better staffed than universities but in none of these countries is there any formal organization of training in Islamic art as such, although much can be learned, almost always with an "Islamic" rather than "art historical" or "archaeological" point of view. Such is the case with the School of Oriental Studies in London and now one of the Paris universities, which do provide organized teaching. Istanbul and Ankara are very active but tend to limit their efforts to Turkey only. The American University in Cairo has been a

particularly strong center, perhaps more effective than Egyptian national universities. Some training is available in Iraq and in Israel, but none in Iran or in North Africa.

At the same time, it does seem that interest in the field has grown enormously over the past decade and a rather rough estimate is that some 300–400 students in American universities have been exposed to at least one semester course in Islamic art in 1971–1972; in 1960 the figure would have been around 50. No survey exists about the causes for the increase, but the experience of some of us would tend to suggest that students interested in the arts have become more involved in Islamic art than have area specialists. This seems clear from the success of Islamic exhibitions in large cities as well, as is demonstrated by the rapidity with which the Metropolitan Museum book on the Houghton Shahnameh became out of print. It is also true that there is much in contemporary art which bears at least superficial relationship to alleged values of Islamic art and that several prominent modern artists have claimed inspiration from the Near East. Finally, many of the more traditional fields and approaches of the history of art have lost some of their attractiveness, partly through overexposure and easy availability, and a new and ill-defined field seems exciting, at least initially. One of the commentators pointed out that in few other areas is it possible for a beginner to make major contributions about very well-known masterpieces.

But the means for effective teaching, be they good surveys or complete sets of photographic documents, are simply not there. The first result is that the gap between the few teachers in well-equipped centers and the many in less endowed ones tends to widen, and it is generally disheartening to read the pages written on the arts of Islam by historians or anthropologists, even though one realizes that the fault is not theirs. Another result of the enormity of the field and paucity of scholars is that too much of the learning and knowledge that exist come from privileged information. Tied at times to a secretiveness often associated with private collections, this need for personal contacts is dangerous, for it leads to cliques and to pettiness. We could give examples of both.

But in a deeper sense, we could argue that the field of Islamic art and archaeology is still at an artisanal level in which personal, almost family, contacts predominate for the dissemination of knowledge and ideas, whereas there is a reasonably demonstrable growth in interest on the part of potential art historians. There is a constant encounter with Islamic monuments on the part of all archaeologists (whereas they tended to dismiss the Muslim world in the not-so-remote past), and there seems to be an awareness on the part of the general public, whether the awareness is spurred by tourism, faddism, or simply an oversupply of information on Gothic cathedrals and baroque painting. It is more difficult to assess the changes that have taken place in the

Muslim world itself, as a highly Westernized generation is replaced by a new group of students, although not by better teachers. That interest in the arts as a whole has increased is clear enough, but it is far less clear whether this interest has carried over to an understanding of the artistic part of the culture. There are probably many reasons for this, but one is certainly the lack of accessible books. It is sufficient to see the works that have been translated into Arabic to realize the growing gap between the insufficiently productive and conscious "happy few" in half a dozen cities in the Western world and possibly four cities in the Near East (Istanbul, Cairo, Jerusalem, Shiraz) and the mass of young people seeking to know their own culture.

The final point about accessibility is linguistic. Works pertinent to Islamic art are available in 25 languages. No scholar or cultivated individual can possibly learn them all, nor is it likely that very rare and old books, which are frequently our only source of certain monuments, will be found in many libraries. The result is of course ignorance, for the arts cannot under any condition be nationalized according to contemporary or even linguistic frontiers. But it is not merely a question of unawareness of written words about the arts, for too often an unknown language has led to an automatic avoidance even of looking at pictures.

Beyond this level, however, there is the even more profound problem that it becomes increasingly difficult to control new material in whatever language it is published. A good knowledge of the 15,000 odd monuments of Islamic architecture in Turkey makes it impossible to know well the quarter of a million remaining Fatimid ceramics or some 10,000 illustrations of the Shahnameh. Even if it is true, as a commentator pointed out, that this mass of documents can be broken down into a manageable number of groups, the task of sifting the material in order to justify the groups has not been done. At this level, it is also a question of accessibility in the sense that the means are lacking to determine the originality or quality of any one monument.

One last point ought to be mentioned. One aspect of accessibility lies in an appropriate understanding of the manner in which a Muslim or, in a more general sense, a Near Easterner perceives the visual world surrounding him. Even if the patterns of today cannot necessarily be transferred to earlier times, it is still true that the intellectual or esthetic experience of contemporary man should or could have a bearing on the artistic production of the past. Some care should no doubt be exercised in using this evidence, for it is a priori foolish to think that a Parisian of today is more likely to understand a Gothic cathedral than an Italian or a Russian. Yet there are two ways in which contemporary Western esthetic habits are pertinent to an understanding of its past. One is that, in a variety of complex dialectic ways, the present is a product of the past; therefore, it is possible or could be possible to work backward from the better documented present into earlier times. However, the second

way is more important. It is that, as any culture is surrounded by the monuments of its past—often still used, even if not always in their original form—it develops almost automatically visual associations, modes of esthetic judgments, and relationships between setting and activities which may change over the centuries but whose transformations are probably relatively slow. An awareness of these contemporary associations, judgments, and relationships as well as of contemporary creativity can well be utilized to make earlier monuments more accessible. The methods by which this awareness can be acquired belong to the social sciences, especially ethnography and psychology, much more than to the traditional humanities. Yet at this time no one has tackled these problems and we know next to nothing about contemporary attitudes toward the arts, judgments of quality about contemporary or older creations, or the nature of associations between the visually perceptible world of the Muslim and his activities or beliefs. Very little work has been done on folk art except occasionally on carpets, but without qualitative analyses. The few very general books of pictures and short texts on artificially revived or preserved local techniques of ceramics or metalwork (as in several Indian or Central Asian publications) are usually of very low intellectual caliber. A little more is known about techniques of manufacture and artisanal vocabulary, especially in Iran, where Wulff's book on traditional crafts is an invaluable source of information. As to contemporary art, its enormous development in all major countries of the Near East generally has been discussed only in a periodical literature limited to each country and totally unknown in the West. It is not assured, of course, that a thorough sifting and study of all this information will lead to a better understanding of the past, and it is important to repeat that there are many intellectual and emotional difficulties in explaining the past through the present. Fake romanticism and anachronisms are dangers indeed, but aside from the fact that the art historian and the ethnographer (who replaces the archaeologist for the contemporary world) of the Middle East are responsible for interpreting the contemporary artistic scene, it may also be that this understanding can contribute to a better explanation of the past.

Theory

The question here is whether elements of hypotheses, explanations, and theories peculiar to Islamic art and archaeology exist which can help to understand any newly discovered monument, which can be used by nonspecialists to demonstrate some artistic or archaeological process or to illustrate some aspect of Islamic culture at large, or which lend themselves to broader generalization either confirming some art historical paradigm and

archaeological explanatory process or, at the highest possible level, creating such a paradigm.

Some such areas have been investigated. Thus much work has been devoted to the question of Muslim attitudes to images, to the early history of Iranian architecture, and to the subject matter of the twelfth- and thirteenth-century images in all media. In all these instances the Muslim example is important to such wider questions as iconoclasm, a phenomenon that is not unique to Islam, as the ways in which a strong artistic tradition adapts its forms to new needs, and as the reasons that a sudden explosion of representations occurs where few existed before. This is not to say that any of these possibilities were exploited, except perhaps iconoclasm, but the opportunity to do so certainly exists. Then there are the problems of decoration, i.e., of certain ways to treat the surface of objects or of architectural monuments, and of abstraction, i.e., of treating whatever subject one depicts in ways that are different from those of natural visual experience. In both these areas, especially the first one, the Muslim phenomenon is unique and could be significant in developing general theories about the artistic process altogether; in addition, there is an obligation to explain why it took place within the culture. Some work has been done on ornament, but the only book on the arabesque as such (by E. Kühnel) is found in only a few libraries.

All things put together, coherent statements and ideological systems comparable to what is known in the study of Renaissance or contemporary art simply do not exist. There is nothing on the esthetic of Persian painting and hardly anything on the symbolic values of Islamic visual expression. Calligraphy, acknowledged to be the highest form of visual expression in the Muslim world, has never been adequately studied. Only very recently have archaeologists realized that Islamic sites offer a wonderful opportunity for understanding such processes as urbanization and for matching excavated or other comparable evidence with literary sources, thus making it possible to evaluate archaeological methods and information. In a broader sense, however, what has not yet been achieved is a proper explanation of the epistemological significance of the visual arts in understanding the culture and a largely concomitant system of qualitative evaluation of individual monuments. Although it may be too much to hope that the field can acquire a Burckhardt, a Huizenga, or a Coomaraswamy, what can be expected is a series of generally accepted hypotheses about a tradition's character and a sense of their validity for the arts and for archaeology.

The key questions in dealing with these matters are whether such hypotheses must come from the field itself, whether they should consist primarily in attempts to fit into it theories established elsewhere, or whether this is a sort of investigation that should come from the Muslim world itself, somewhat in the manner in which native Chinese scholarship has sought to understand

its own past and possibly in ways being developed by a group of Iranians, like N. Ardalan. On these particular issues commentators found themselves divided. Some argued that the field is much too young to lend itself to theoretical considerations, that catalogs and descriptions with occasional generalities are the maximum that can be expected. Others felt that the main solution lies in acquiring a thorough grounding in Western art before even seeking to understand the Muslim visual world, and that for better or worse the discipline is stuck with its European formulation. Still others indicated that theoretical considerations are individual preferences, secondary to the main objectives of the field. A fourth point of view was that, on the contrary, a new and ill-explored field lends itself particularly well to theoretical investigations because its very backwardness may make it possible to jump over certain stages of development demonstrable in other areas. Some objected quite violently to this thought. A strong opinion among a number of Turkish scholars, for instance, is that there is some fallacy in seeking any generally Islamic interpretation of the arts, because the possible bond between so many differentiated traditions is limited to a few practices and allegiances which are not pertinent to the technical possibilities and formal habits of the arts.

There is no clear consensus as to whether Islamic art is just an art to be studied and developed in traditional ways or whether, on the contrary, it can be an experimental area for the formation of art historical or other theories. Yet as one considers the variety of rather strong reactions which were received to an originally rather forceful assertion that by its own nature and by the nature of our knowledge of it, Islamic art lends itself particularly well to theoretical considerations, one wonders whether the variety of opinions expressed is not in itself conducive to theoretical elaboration. For one could argue that this very divergence implies an uneasy tension between the humanist's tendency to emphasize the unique and the different and a social scientist's search for patterns through which the unique can be explained but also dissolved. In part it is a tension between the history of art and archaeology. It occurs as well in the study of classical times or of the Middle Ages, and it may be comparable to the tension between linguistics and the history of literature. However, the fascinating point is that this tension occurs in a field that by common agreement is underdeveloped in information and that is part of a still living culture, even if its concentration is on the culture's earlier phases. It is therefore possible to suggest that it is an almost unique instance where traditional problems and questions can be resolved not simply in traditional ways but by the adjunction of ideas and methods from other fields, by the experience and self-analysis of the culture itself, and by the opportunity to concentrate on the subject matter with clearly defined hypotheses rather than with automatically acquired prejudices. We return in the following section to some concrete proposals for implementation, but it seems

to us that the novelty of the field and the variety of its present directions can contribute in uniquely striking fashion to an understanding of both Islam and the arts or material culture in general.

PROJECTS AND PRIORITIES

The preceding observations can be summed up in the following manner. The directions of the field as a scholarly enterprise have been reasonably well mapped out (largely because they are dictated by the paradigms of two otherwise known disciplines or methods of inquiry), but the results have not always come up to the expectations, for it is still a highly personal field in which an individual's own knowledge of monuments and of his colleagues is more important than what can be acquired through readily accessible publications in a small number of languages. The field is weak in books and photographic tools for the beginner or the interested outsider, whether general art historian or Islamist. Although the more advanced student or scholar who has mastered four or five languages can find an enormous amount of written information in the better libraries of the world, he is also hampered by the lack of ready access to photographs, slides, and other basic means of work unless he gets them himself. The field is weak in hypotheses and theories, although the areas where these are likely to emerge most fruitfully can be outlined reasonably easily.

Where does one go from here? What sorts of projects and programs can be seen as useful to the field as a whole as well as to related fields and to intellectual and pedagogical endeavors? And what sort of ranking can be assigned to them?

First of all, we may agree that most work will still be done by individuals and that their interests and abilities do not lend themselves to external pressure or sponsorship, especially since most of them work far away from their most immediate colleagues. One could recruit into the field the most capable and promising students available. However, a deeper question is whether the rather striking improvement of recent years (one person teaching in the United States in 1955 and four curatorial appointments, compared with 13 teaching and seven curatorial appointments in 1972) is likely to continue. The answer does not lie within the objectives of this report, except to suggest that both the quality of the recruitment and the opportunities for jobs may well depend on the intellectual achievement that can be expected. If the field is seen less as an individual, artisanal activity than as a creative and fruitful enterprise with a strong promise of intellectual satisfaction, there is little doubt that it will attract students and compel the opening of jobs. It may even succeed in building teams working and teaching together in one institution, obviously progress over the present system of one person covering 1000 years over 10,000 miles.

A second area of need lies in what has been described as accessibility. One could suggest a many-pronged approach:

1. Publication of a series of manuals covering the field and identifying in each instance what is known, what is hypothetical, and what ought to be done: this suggestion met with considerable agreement on the part of all correspondents and commentators, and several felt that the very general picture books with incompetent texts ought to be simply banned. These manuals, whose need is particularly great in the Near East, would not be textbooks as much as teaching and research tools which could serve as blueprints for the work of at least a generation.

2. Creation of repositories of documents: this is not an original suggestion at all, and it has been mentioned that several such files have been started, usually around one individual's personal collection or through one man's initiative. Except for the new collection recently begun in Jerusalem, which may have been better planned than its predecessors, most of these repositories failed eventually for lack of continuous staff and funds. Although all commentators agreed about the need, there was much disagreement about the means of implementation, such as one center for everything, division by countries or by technique, or creation of a pilot project. Aside from the staggering expense of a well done job, two points were made. One is that many countries are planning repositories of national monuments and it may be more worthwhile to help them complete these tasks than to start new endeavors. The other is that the key obligation of such collections is to be easily accessible to all bona fide students. Finally, some felt that it may be simpler to begin with less ambitious projects, even if they are not complete, such as the repository of Qur'anic inscriptions being prepared in Beirut or the checklist of Shahnameh illustrations done at the University of Michigan. The field has been unusually slow to explore and utilize existing devices for the rapid and simple dissemination of visual materials, from movies to photographs or to surveying through photogrammetry; these are obvious means of better accessibility.

3. A journal that would be cheap enough for students to acquire in Egypt or in Pakistan, yet complete enough to transmit at regular intervals news of all latest occurrences in the field, from bibliographies to theses, museum acquisitions, or excavations: such a journal could also become the vehicle for the exchange of ideas and for discussions on a more informal basis than more common periodicals. The success of such enterprises in the social sciences seems a reasonable guarantee of usefulness and it may replace the personal and oral source of so much present knowledge.

4. A formal program of translations into English or French of major con-

temporary works or of sources in less well-known languages: this task ought to be done rapidly and in sufficiently large numbers to be really worthwhile; some commentators have questioned, however, whether this kind of exercise is worth the time and money needed for its successful completion, when any scholarship in depth would still require a knowledge of languages and thus a chasm would be maintained between the haves and the have-nots of the field.

Of these four suggestions, the first and the third are the most feasible and, initially at least, the most useful in removing some of the loneliness of each scholar and student and in forcing the field as a whole and its various subfields to define more specifically the "state of their art" than is possible here. These are also reasonably inexpensive enterprises which, after an initial push, could almost pay for themselves. The third suggestion is potentially the most fruitful one, but it is a little difficult to imagine how it could, without enormous resources in men and in funds, be properly staffed, operate with continuity over many years, and be accessible to all who need its information. Like the fourth suggestion, it may be left for the time being to individual initiatives with limited purposes, or, perhaps like the archaeological and monumental atlases suggested by several commentators, it could become a central concern of individual countries, as has happened in fact in most European countries.

The third area is scholarship itself, i.e., new knowledge and new interpretations, with or without the theoretical implications discussed earlier. This is an area where it is quite difficult to establish an order of priorities or even to identify all possible needs. For our purposes, we could identify three orders of needs, within each of which certain priorities could be established as each topic is discussed at greater length than is possible here. If more space is given to the third order, it is not that it is more important but that it has been less frequently discussed.

1. It is unlikely that complete corpora in the manner of Kühnel's work on ivories or Creswell's volumes on Egypt can find sponsors, even on the assumption that there are people to undertake them. Yet it is equally clear that the task of cataloging, ordering, and classifying the millions of remaining monuments of Islamic art must be continued, whether it is done through catalogs of museum collections, of archaeological remains already found, of libraries with manuscripts, or of the architectural monuments of a given city. New information must be acquired as well, primarily through excavations, provided these are properly funded and their results rapidly published. Two aspects of this task of classification and of cataloging documents seem to be particularly important to investigate. One is that it should seek to use more

efficient means of making itself known and available than the traditional and expensive "book" and that the information provided should be as complete as possible. The second one applies more particularly to archaeology and to restorations and consists in the particularly careful identification of the sites to be excavated or buildings to be repaired and of the reasons why. Perhaps in this particularly expensive side of the field, which has not yet demonstrated its ability to contribute as much as its technology leads one to expect, a coherent and collective effort on one task, a city, a province, or a period, could lead to methodologically and intellectually fruitful results.

Another aspect of basic information lies in vocabulary, i.e., in the establishment of a valid terminology for forms, techniques, or ideas. The main assumption here is that, whereas there are no doubt areas where neologisms issued from Western methods have to be used and artificially translated into Near Eastern languages, most of the morphology of Islamic "things" had and still has a native terminology. If it does not, the question is automatically raised whether the morphological distinctions are not themselves erroneous. There are two approaches to this question. One pertains in part to ethnography and to descriptive linguistics and requires simply collecting contemporary data, especially from artisans. The other one is more complex and demands a lexicographic study of older texts and especially of various technical manuals (when still existing in manuscripts) in order to develop a historical vocabulary of the arts. Although very little has been done in this area, some interesting results were reached by Rempel and others in dealing with architectural ornament with a local vocabulary.

2. Some areas of particular "underdevelopment" can be identified: Iranian architecture, complete publication of key manuscripts, iconographic systems, archaeologically discovered ceramics, periodization and period styles, patronage, and dozens of others as well. All these are basically "problems," in the sense that the raw data are available in reasonably sufficient form and that what is lacking is their correct or adequate interpretation. This is an area where it is particularly difficult to establish priorities, but it is also true that these are all questions that can be solved by traditional methods and that do not require uniquely special types of support. They are, so to speak, routine scholarship and their number or quality depends primarily on the scientists involved in them.

In terms of priorities, problem-oriented, interpretative, synthetic, or thematic subjects should take precedence over complete monographs, not because the latter are not needed but because the former have not been sufficiently developed. Furthermore, monographs of varying quality will continue to be done as Ph.D. theses or as the result of restorations or of other local activities. Many of them would be immensely improved and far more useful if they could be put in properly discussed and generally accepted categories

and interpretations. Only then will it be possible to pursue the dialectic processes of understanding any monument in increasingly complex depth on the one hand and, on the other, of developing increasingly sophisticated concepts of esthetic and historical appreciation.

3. Several commentators pointed out that this report deals too much with cataloging and gathering of information and documents, at the expense of developing ideas and theories. This emphasis has several sources. In part, no doubt, it conforms to the humanist's natural feeling that ideas and theories are an individual activity which is only helped by the availability of information. Secondly, the humanist fears committing himself intellectually until he is sure of himself; he fears hypotheses, mental gambles, and discussions. Finally, there is his impression that he lacks sufficient information to make wider theories more than guesses, at best brilliant intuitions.

In order to check these debilitating tendencies, one can propose a number of more far-reaching topics, most of which require a series of preparatory conferences or discussions, all of which are essential to a coherent understanding of the field, demand the involvement of scholars and students with very varied technical and cultural backgrounds, and can serve the broader aims of general disciplinary theorists.

Such would be the question of the part played by the visual experience in traditional and contemporary Muslim culture. Is the latter purely verbal and is the visual secondary? If so, what is it that one learns about the culture when one studies its artifacts or its works of art? Is there something intrinsically different between a Muslim vision and a Christian or a Buddhist one? Are all Muslim experiences similar? A large number of ethnographic, social, and psychological inquiries are needed to answer these questions and others issued from them, for which to my knowledge there are no models available, although some work on the psychology of art and of perception does exist. It is the sort of inquiry which would, by necessity, compel a meeting of social scientific and humanistic minds. Its ultimate objective no doubt would tend to be that of identifying the unique component in artistic creativity, in this case the so far elusive Islamic or Middle Eastern element whose existence underlies our field but whose nature has never been studied in any detail. But the methods and procedures that would have been used in the process are likely to be of far broader significance.

One could also raise the question of archaeological information versus art historical judgment. How significant are the miserable remains of an early mosque in Iran for an understanding of the *Masjid-i Shah* in Isfahan? Or how does one distinguish between good and bad paintings in the fifteenth century? Although frequently formalized by intellectual habit or prejudices rather than by genuine intellectual effort, the more developed areas of the history of art tend to possess a number of techniques for qualitative judgment. It can be

argued, however, that the very novelty of the field of Islamic art makes it particularly amenable to the elaboration of new and possibly more fruitful methods of qualitative and quantitative analysis and therefore that its investigation can progress faster rather than permanently lag behind other fields. It can serve for the development of models usable in other areas precisely because it is not burdened with established methods and theories. One example may suffice. By utilizing systematically the new techniques and concepts of communication theory or of transformational grammar, it may be possible to describe the arts of a period or of a technique of Islamic art through such terms as their redundant features (relatively meaningless in themselves but required to carry information) versus unique ones; stylistic change may be explained as transformations in the surface expression of "semantic structures"; the historical problem then becomes less that of describing changes and distinctions than of explaining them, for it is probably more sensible to explain changes of attitudes than changes of forms. There are other ways and other techniques, but it is striking that the immense and imaginative work done in recent years on language, on oral traditions, and on poetics has not been carried into visual communication. An underdeveloped field can be an excellent area for the investigation of such theories and methods.

As a final example one could give that of functional recognition and abstract design and thereby lead to questions that are central to an understanding of contemporary arts everywhere. Does one know from a facade whether a building is a mosque or a bath? Is it immaterial whether a certain ornament occurs on a ceramic or on a minaret? The initial point of departure of this example lies in a peculiarity of Islamic art that a small number of themes are used for a wide variety of purposes and in many media. The underlying assumption is that the nature of recognition and of visual pleasure may have occurred through means other than those of observable motifs. Was it color, geometry, size, or location that identified the purpose of a monument or of an object? For an answer one must once again turn to other areas, to social, intellectual, and psychological histories. Wherever one turns, however, the methods and the results are likely to be of broader importance than for the field of Islamic art alone.

Many other problems and questions can be raised for which it is obviously impossible to create an order of priorities, because in the final analysis this particular level of investigation will depend on the individuals working in the field far more than on the immediate requirements of the field itself.

Appendix

The elaboration of this report owes a great deal to the following: Richard Ettinghausen and Lisa Golombek, who attended the discussions, and Priscilla Soucek, Fay Frick, Erica Dodd, George C. Miles, Eleanor Sims, Marilyn Jenkins, John Shapley, Ed Binney, Donald Wilber, and Renata Holod, who wrote comments or made verbal remarks. All of them may recognize themselves in the "commentators" of the text.

Most bibliographical references can be found fairly easily in Creswell's bibliography for earlier works, in my own introduction in the *MESA Bulletin* (1968), in Pearson's *Index Islamicus*, or in the *Abstracta* of the *Revue des Etudes Islamiques*. What follows is a sample of such studies, mostly fairly recent, which seem best to illustrate the most fruitful and most useful recent investigations in the field and which have been quoted directly in the text.

1. For a history of the field, R. Ettinghausen, "Islamic Art and Archaeology," in *Near Eastern Culture and Society*, T. C. Young, ed. (Princeton: 1951).
2. For the archaeological history of monuments based on restorations, R. W. Hamilton, *The Structural History of the Aqsa Mosque* (Jerusalem: 1949); G. Zander, *Travaux de restauration* (Rome: 1968); and E. Galdieri, *Isfahan, Masjid-i Jumᶜa* (Rome: 1972–1974).
3. The Islamicist's approach has been best defined by Jean Sauvaget, "Comment étudier l'histoire du monde arabe," *Revue Africaine*, 1946.
4. Examples of art historical studies applied to and transformed by Islamic art are R. Ettinghausen, "The Emperor's Choice," *Essays in Honor of E. Panofsky* (New York: 1961) (iconography of painting); L. I. Rempel, *Arhitekturnyi Ornament Usbekistana* (Tashkent: 1961) (interpretation of designs); O. Grabar, "The Umayyad Dome of the Rock," *Ars Orientalis*, **3** (1959) (iconography of a building); S. C. Welch, *A King's Book of Kings* (New York: 1972) (connoisseurship of painting); R. Ettinghausen, "The Wade Cup," *Ars Orientalis*, **2** (1957) (style and iconography of an object).
5. Different methods and interpretations to reach comparable results occur in O. Grabar, *The Formation of Islamic Art* (New Haven: 1973) and R. Ettinghausen, *From Byzantium to Sasanian Iran and the Islamic World* (Leiden: 1972).
6. The best recent examples of monographs on individual architectural monuments are L. Golombek, *The Timurid Shrine at Gazur Gah* (Toronto: 1969) and S. Mustapha, *Kloster Ibn Barquq* (Blückstadt: 1968); the traditional masterpiece of the genre is K. A. C. Creswell, *Early Muslim Architecture*, 2nd ed. (Oxford: 1972), and *Muslim Architecture in Egypt* (Oxford: 1952, 1959).

7. Partial attempts at period styles occur in R. Ettinghausen, "T Flowering of Seljuq Art," *Metropolitan Museum Journal*, **3** (1970) and O. G bar, "Les Arts Mineurs de l'Orient Musulman," *Cahiers de Civilisa. Médiévale*, **11** (1968).
8. Broader attempts at interpretation occur in J. Sourdel-Thomine, ". et société dans le monde de l'Islam," *Revue de Etudes Islamiques*, **36** (1968) a E. Dodd, "The Image of the Word," *Berytus*, **18** (1969).
9. Examples of excellent "local" investigations are the numerous wo of G. A. Pugachenkova or L. Bretanitski; L. Hunarfar, *Ganjine-i Tarikh Isfahân* (Isfahan: 1344); I. Afshar, *Yadgarhâ-i Yezd* (Tehran, 1348); T. (*İstanbul Camileri* (Ankara: 1962).