

# Reports

## An Interview with Walter Dostal<sup>1</sup>

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AG/SMH: Let's start with trying to familiarize our readers from a theoretical and methodological angle with some of the major dimensions of your research. It seems evident that your work is close to those of a group of scholars such as Robert Carneiro [2003], Marshall Sahlins [1972], Elmer Service [1964, 1975], Eric Wolf [1966, 1982], and Eleanor Leacock [1981, 1982] in the U.S.A., who more or less belong to one generation. A substantial part of your own research has followed concerns similar to theirs, such as the interdependencies between the environment and society, between the economy and politics, and between local history and the regional evolution of societies. In your opinion, would it be wrong to characterize your work as the German-language version of an ecologically and materialistically oriented "neo-evolutionism"?

WD: I don't think it would be wrong; in fact, I agree with your assumption and would emphasize that I was certainly influenced by American writers but developed my own approaches. As an example, I would cite the research on complex civilizations (what used to be called *Hochkulturforschung*) on which I focused initially [1968]. It led me to several interesting conclusions, for example, on the problem of rulership and geographic space. I understand this as the establishment of a complex civilization by a state or an empire. However, in the course of time, this process of expansion may be constrained—a local region, for example, is integrated into such a civilization but then experiences an interruption that causes it to be different from another society.

Within the context of space, there is of course the problem of eco-cultural interaction. Starting with Steward's "cultural core" [1955], I tried to tackle this problem of the environment and environmental changes [1984a; Dostal and Reisinger 1981]. This was very interesting to me because I was working in the arid area all this time. Each field trip meant a new challenge for me. In that respect, Ras al-Khayma in the Omani Mountains was particularly revealing to me. There I was able to explore the difficult circumstances people were living in, for example, at elevations between approximately 1,000 and 1,800 m. Their



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supply of water is very limited; they derive it from cisterns in which they collect rainwater. When this water supply runs out, they have to leave the area. How do they breed goats and plant grains? They first have to carry all the humus soil up the mountains, an incredibly difficult task. In the valley, they live from date-palm cultivation and fishing. Only the combination of these sub-existential qualities secures their survival. I believe that this is a very interesting case [1983c].

AG/SMH: You published research on the interpretation of early Islamic history [1992a] as well as presenting a model of regional evolution [1984c]. How do you understand the relationship between an anthropological interpretation of history, on the one hand, and the theory of evolution, on the other?

WD: The theory of evolution can only offer models; beyond that, it has nothing to do with real history. As a Marxist, I am interested in questions such as "What about class societies?" From my field research I was able to conclude, for example [1985], for regional evolution that a class society preceded the traditional state society. The difficulty that most Marxist researchers face is having to collect all of their ethnographic data on their own, since the material published by the average anthropologist is usually not enough to answer their questions. I don't want to say anything against ethnologists, but certain insights into the economy would be basic and yet are absent from many research publications.

AG/SMH: After characterizing these mainly theoretical approaches, let's turn to some of your empirical insights. We have already seen that the primary focus of both your empirical work and your historical studies is the Middle East,

mainly the Arabian peninsula [1958, 1992a, 1992b, 1997]. You will probably not contradict us if we say that you were the first academically trained anthropologist to do serious fieldwork in Arabia. After your first work in Kuwait [1956], you quickly gained international recognition for your studies of the Bedouins of southern Arabia [1967]. Looking back, what interdisciplinary consequences did your early studies on the Bedouins have for geographers, historians, Orientalists, and scholars of Islam?

WD: At first there was surprise when I demonstrated that the Bedouins had their own history. This sounds awfully simple today, but that's how it was. The reaction was very positive, and it changed the way the Bedouins were regarded. Indeed, this perspective was a clear contrast to the views of the one-party-systems of the contemporary Middle East, for example, the Iraqi Baath Party until recently. The Baath Party condemns the Bedouins—condemns them to sedentarization because they are supposedly a national disgrace, symbolizing backwardness. We must consider this aspect and its impact. Thus I was perhaps able to break some ground here.

AG/SMH: After your research on the Bedouins of southern Arabia, you increasingly turned to the question of rural economies in the Middle East. If we remember correctly, you first turned to the former North Yemen and Anatolia [1971] and then increasingly also to Oman (1972a), the Arab Emirates [1986], and eventually southwestern Saudi Arabia [1979b, 1983b, 2006]. How would you characterize the most important eco-ethnological insights and findings based on this research among rural Arab regional societies?

WD: Let me start very generally. Regarding the Arabian Peninsula, we have to distinguish clearly between West and East. The West was already inhabited in archaeological times, while this happened in the East only to a limited degree. Although Oman, for example, supplied the natural resources for copper until around AD 1200 (i.e., for a relatively long period of time), nothing extraordinary occurred there in the sense of a civilizing development of the state—completely contrary to the situation in the West, where we see early encounters between the Sabeans and the indigenous people. Here a sophisticated irrigation agriculture was gradually developed, with technical achievements such as the Marib Dam. This, for example, is one decisive difference.

Secondly, the geographical conditions are of course crucial. There is mountainous terrain in the West as well as in Oman in the East, but the Oman area is very sparsely populated because it is barren while Yemen is relatively densely populated and cultivated. Then there are the plateaus, which gradually become rather barren regions where all that grows is grain. But the population is concentrated on these plateaus. This is actually a congested area. Here huge irrigation systems could be built. Nothing like that exists in Oman. Oman is favored, however, with the monsoon rain.

Regarding technical equipment, the West had all the tools that were then distributed over almost all of southern Arabia, such as the *sharim* knife and the sowing tube (1983b). All these tools were achievements that reached the south, were further developed there, and were further spread by the people of the south. The sowing ard, for instance, is an invention that has to be operated by two people and can therefore facilitate precise work.

Finally, there are differences in the kinds of seeds in the historic interdependencies of all these factors. In Yemen, starting in the fifteenth century AD, we find commercial products such as coffee and qat besides wheat, emmer, and barley. Eventually, qat became the cancer of agriculture, so to speak, because it is easy to cultivate and drives out the grain. Therefore, Yemen has to import grain. A few decades ago, when communist-led South Yemen existed, qat was forbidden there. Now its consumption is spreading in qualitative terms, but it will probably not increase any further per consuming person.

AG/SMH: You also made some discoveries about the relationship between agricultural producers and manufacturers of crafts [1979a, 1984b]. For instance, you identified centers of craft production in rural settings and outlined the typically hierarchical relationships between many of these skilled specialists and groups of peasants and farmers.

WD: Yes, exactly. This is of course also a characteristic of many agricultural communities in the Middle East. This was exactly what we discovered in Asir. We have concentrations of craftsmen who are responsible for a relative homogeneity of the material culture as an interesting dimension in these contexts: social devaluation of many specialist groups, however, is not limited to Arabia.

AG/SMH: If we shift now to the topic of tribal organization, we are in the midst of an ongoing debate. The concept of the tribe is often rejected by anthropologists in other countries, especially those who have a regional focus other than the Middle East. In part this can be understood as a result of the critique of colonialism, which upgraded tribal relationships to some extent for its own advantage in practice and simultaneously devalued them ideologically as a symbol of backwardness. Yet, from a comparative perspective, this term certainly has a totally different meaning in the Middle East, especially in native forms such as *qabila*. There it stands for freedom and autonomy rather than for backwardness. Still, even for Middle Eastern contexts some colleagues are skeptical of the concept of the tribe. What is your opinion on this topic? Moreover, doesn't the term remain relevant for the historical Middle East at any rate?

WD: May I say that in my opinion, this debate about tribes is quite ridiculous? Even outside the Middle East, say, in parts of Africa, I would dare to use the term "tribes" if I included linguistic factors in the analysis. Many anthropologists have

no linguistic knowledge and therefore wander in the dark. Tribes are often a manifest reality, a social reality with relative sovereignty and sometimes even a constitution of its own [1974]. This is quite important. When one speaks of a constitution, many colleagues think that this is not possible. But they have a tribal democratic constitution, which ultimately may be fundamental. Unfortunately, this is completely ignored in current debates about democracy in the Middle East. Two years ago I met a Bedouin shaykh who had once suggested, in an Iraqi assembly, turning to elements of tribal democracy and developing their own democracy from there. But of course, this is not the case in what is going on there today. One instantly wants a Western type of democracy, and it does not work. In many Arab contexts, tribes and tribal relations will remain the authoritative social entities, even within drastic social changes and because of them. The former communist-led People's Democratic Republic in South Yemen, for example, introduced cooperatives in the late 1960s. They determined central deadlines, and everyone who was at a certain place on that day had to settle down there. So far so good. The ideology and wish behind this was detribalization, but what happened? Individual tribes persist to this day, largely live in coherent areas, and, most important, marry predominantly among themselves.

AG/SMH: One could also add a further thought to this debate: that developments like those in Somalia or Afghanistan also contain elements of retribalization.

WD: Yes, yes, of course.

AG/SMH: Now let's turn to a question of our field's history, which also refers back to our own time as students. From our personal experience of you as a teacher and lecturer, we would see you as one of the few who, after 1945, contributed significantly to introducing contemporary international theoretical and methodological approaches to German-speaking *Völkerkunde*, which was quite isolated at that time. Therefore, one can certainly speak of you as one of the major Western reformers of German-speaking sociocultural anthropology [Barth et al. 2005]. But at the same time, despite or precisely because of the major influences from Anglophone anthropology that you introduced in this regard, you have always respected the positive local impulses that you saw as based in German-speaking traditions. What were the most important of these impulses that contributed to your own development?

WD: First of all, I would mention Heine-Geldern. Heine-Geldern opened our eyes to international cultural relationships which spanned various continents. That was fascinating. I don't know how much of it is still valid today, but at that time he was an important pioneer and gave us a great deal [Heine-Geldern 1976–99]. He really enlightened us; it was an enlightenment in contrast to Koppers. I respected Koppers because he had already published an article in *Anthropos* with

the title “Die Indogermanenfrage” (The Indo-German Question) [1935] when the Nazis came to Vienna. In it he rejected the whole racist program of the Nazis. He definitely showed a lot of courage in publishing this at a time when the Nazis were already on the rise everywhere. But sometimes he was tiresome with his theological diffusionism and its ideas of pristine monotheism and a supreme god (*Hochgott*). In contrast, Heine-Geldern was a real revelation. Frobenius [1921], with his phenomenological approach, was another one who impressed me with his ideas about emotion and the essence of things. His most important student, Jensen, opposed the Nazis—this deserves special emphasis—and had to pay for it; as a consequence he was drafted into the military, where he soon contracted a serious illness. His intellectual starting point was the question of how culture is created [Jensen 1948, 1965]. In my opinion, this was interesting, too.

And last but not least, I have to mention my dear friend Joseph Henninger, who actually came from the same stream of thought as Schmidt and Koppers. Unfortunately, he never had the chance to do field research as he would have liked to; the *Societas Verbi Divini* didn't allow that. He was a good, really exemplary fatherly friend. We had a series of discussions that were very fulfilling and extremely positive in a way I wasn't used to in the circles of Viennese *Völkerkunde*.

AG/SMH: Was it Heine-Geldern who strongly inspired your intense engagement with complex civilizations and archeology?

WD: Yes, of course. In his lectures, he convinced us of it.

AG/SMH: And Henninger inspired you more in the fields of interpreting textual historical evidence?

WD: Historical texts, yes—he really did a terrific job. One becomes green with envy when one looks at his works [Henninger 1981, 1989].

AG/SMH: After your doctoral exams and your museum post in Vienna, your first professorship was in Bern in Switzerland. From 1975 on you held the professorship in Vienna. Both in Bern and in Vienna you were one of the very few professors in this and related disciplines who, since the late 1960s, regularly taught in required lectures about National Socialism, racism, and the involvement of anthropology. At the beginning of the 1990s, your groundbreaking article “Silence in the Darkness” was published in *Social Anthropology* [1994], and it was the first article in English in which a German-speaking scholar seriously dealt with the role of *Völkerkunde* during the Nazi period. Why do you think it took so much longer to address this part of our discipline's history in sociocultural anthropology than in fields such as medical science, sociology, and physics?

WD: Actually, I cannot say; the situation is very complex. There were the real Nazis, some of them retired, and there

were followers in all sorts of shades. It is a fact that in this field there were only a very few anti-Nazis. This is very sad—particularly in so-called ethnology one would have expected researchers to adopt a more humane point of view. I think that racism and all these aberrations—maybe, I don't know—would have been at least milder if the intellectuals had stepped in energetically. I believe it is a question of courage, a question of basic beliefs, of a moral attitude—that one doesn't kill people just because they are Jewish, or Gypsies, and so on! If I may say so, my belief is that the representatives of the pre-Nazi past totally failed with regard to the facts. In one of my lectures I tried to demonstrate exactly this. Indeed, I found what I was looking for in a daily newspaper from Berlin. This paper had published a big cartoon across several columns that exposed all those horrible schemes—the gas chamber and the other atrocities—to the public already before World War I. This is outrageous! I think there is only one way out: to fight hard against these people. And they are still here today! Young people have become much more mature in this regard. One can really say that. However, there are still people from yesterday like that.

AG/SMH: Do you think it took so long to confront this issue in our field because many central scholars, in the German-speaking world at least, had also been attracted to physical anthropology and then, after 1945, were unable to rethink those temptations?

WD: Yes, that's what I think. But it is so complex and sometimes even obscure. One only needs to consider one man, a physician, Heinrich Gross [J. Gross 2000; Hörst 2000], who had hundreds of children killed and then after 1945 sat at home all too comfortably in his villa in Vienna. The justice system managed to dismiss his prosecution by claiming that he had dementia.

AG/SMH: Does your courage in your research on the discipline's history and its relationship to racism and nationalism have a distinctive biographical and familial background? You grew up in the young Czechoslovak Republic in a family that was partly affiliated with the German-speaking minority in Moravia. As a juvenile, you then had to experience Nazi rule in the Third Reich's so-called protectorate, Bohemia. And although your family had clearly displayed an antifascist orientation before 1945, you were expelled to Austria at the end of the war. How did this influence your choice of studies and your attitude in this field as a young man?

WD: I can say—as you mention—that my home was truly anti-Nazi, and I excelled at that in my juvenile improvidence. On my way to school after 1939 in Brno/Brünn, I once bought pictures of Hitler and stuck them into dog turds. On another occasion I listened to Radio London at school, which was illegal then, of course. But I took the chance whenever we

had to use the school radio to provide air-raid protection service. The headmaster caught me listening. He also was an anti-Nazi and reacted in the following way: he slapped me a couple of times in the face and said, "You are a show-off. You are going to put your family in great danger. Don't do that." Then he told me right away how to be more careful and which of the professors were Nazis. When the new authorities had taken over after 1945, we were all kept in a camp near Brno/Brünn, which was pretty bitter for me. Eventually, I had to dig my own grave and was supposed to be shot. Then a young Russian soldier came, kicked me, and said, "Run, run!" So he saved my life. A second experience was crucial: I was in the camp and a Czech sergeant approached me and said, "Are you hungry?" I replied "Yes, I am hungry." He said, "I am a simple man. I live in the countryside, and I cannot watch people getting tortured. I can't stand it any longer. I will give you all my food." Then he gave me a full bowl of goulash. That made me think. On the one side, the young Russian soldier who acted spontaneously, and on the other side a Czech sergeant who was in despair. At that point I distanced myself from the usual hatred prevailing among post-1945 camp inmates. After all, it is clear that one hates the guards in a camp. I gradually stopped hating.

This was also the reason for my choice of studies. Originally, I wanted to go to medical school. In Vienna, then, I went to university and studied philosophy. I was absolutely bewildered by the things that were said there—and I actually did not understand them, anyway. It was really not satisfying. For that reason I decided to study *Völkerkunde* with the goal of working in human relations, for the poor and the suppressed, and so on. That was my entry into the field. And then I was really very lucky that with Koppers and Heine-Geldern I had two personalities who were very significant to me.

AG/SMH: At the beginning of your studies and afterwards, two other directions in the German-speaking arena had a major impact in addition to the closing stages of diffusionism, which we have already discussed [Gingrich and Dostal 1996, 263–65]. They were cultural morphology in Frankfurt, where you spent a year doing research under Adolf Jensen, and German functionalism under the Viennese Richard Thurnwald (who died in 1953) and his influential former student Wilhelm Mühlmann [1981 (1948), 1961]. What were your experiences with these two schools of thought at that time?

WD: I had pretty intense contact with Jensen's [1969] cultural morphology and only marginal contact with Mühlmann. I did not really feel comfortable about his functionalism.

AG/SMH: As a student and later as a graduate, did you think of Mühlmann as an influential person in German *Völkerkunde* or rather as one of many?

WD: I think he was merely one of many, but I believe that at the time many believed that they were the popes of German

*Völkerkunde*. One after the other, these popes were ousted. Nothing is left of them. He was one of those popes. After 1945, obviously, Mühlmann fought a lot for his reputation.

AG/SMH: However, his history of (German-speaking) anthropology (Mühlmann 1986 [1948]) was the only history book in our field in German.

WD: Yes, it was the only one. To a certain degree, it was even the only useful book, except that one could not read it for more contemporary time periods because the National Socialist influences were too strong.

AG/SMH: Returning to this part of your formative phase and to Heine-Geldern's influence, we think that some of your most interesting writings originate in this interest in archaeology. The ideas you expressed in "Continuities of Cultural Forms" [1964*b*] pervade your later work, such as your interpretation of the ancient South Arabian motif of bulls' heads and their ornamentation [1983*a*] and your analyses of genealogical and kinship-related terminology in epigraphic and dynastic testimonials from pre-Islamic times [1989, 1990, 1992*b*]. Why is the collaboration between sociocultural anthropology, archaeology, and early history, which you managed very successfully, not so popular in either international or German-speaking cultural and social anthropology? What kind of research perspectives would you recommend to young students today in support of such a collaboration?

WD: I don't know why it is the way it is, because I don't know young people's trends, but I regret it a lot. Even the archaeologists suffer from the lack of collaboration with ethnologists and social anthropologists. That would be the ideal bridge. Why don't they do it? Nowadays it is difficult to appreciate the idea of finding inspiration in ancient times, but archaeology is fascinating, in particular because it can be socially very relevant. It can make concrete predictions, particularly concerning major transformations and developments—of course, with certain qualifications. For example, I once did some research on saints in the Hadhramawt. I visited so many of them that I feared I would become a saint myself! One of them was a female saint, Saida. She lived 120 km east of Aden, and I visited her there. In the course of this study I was confronted with the question: Is she a goddess or not? "Saida" is a name for a god, and this and related ethnographic elements led to the question how to present adequate arguments for a historical interpretation. Well, the written Arabian sources kept me in the dark, although I had rummaged through all the local sources. That's why I came to the conclusion that one has to distinguish between analogies and interconnected parallels, which only say that there is no historical continuity. But continuity of course refers to homology and is borrowed from the natural sciences. Now, one can really work with these two terms in anthropology as well. In the case of the bulls' heads ornaments [1983*a*], for example,

I was able to prove that this was a matter of homology, but more examples would be needed. In short, methodological precision helps to promote archaeology's potential for grasping transformation and continuity.

AG/SMH: Another element of continuity in your own work concerns marginalized groups. We remember your early attempts at fieldwork among Roma groups around Vienna [1955, 1957], and from there the trail leads to the mysterious Sulayb or Sulubba of the Arabian Peninsula [1956, 1960, 1964*a*]. How did one lead to the other, and where do you see the significance of studies of such "pariah groups"?

WD: Well, I have to admit that I went to the Gypsies because there was no one in Vienna who could tell us how to do fieldwork. Schebesta and the other important fieldworkers of Schmidt's so-called Vienna school always talked about "my tribe"—that was the whole statement, somewhat possessive but meaningless. That's when I took a Romani language guide, went to the twentieth district here in Vienna, and made contact with the Gypsies. I was even on a Gypsy wagon and was arrested with them. The bottom line was that I committed all the fieldwork mistakes one could make. I learned a lot from that; it was an exercise. An indiscretion by a museum official made this public (I was employed at the museum at that time). I did not want to write about it, but I was more or less forced to. At the same time I had already developed a particular interest in the Sulubba. The Sulubba were a pariah tribe—this is what one called these socially discriminated-against groups at that time—and since they were pariah they were suspected of possibly representing an older cultural substratum. This kind of reasoning was still typical "Vienna School." I managed to go to Kuwait and succeeded in convincing the shakhyh to offer me his hospitality. I was able to stay there for a very long time and go to the Sulubba. On that occasion I was also told strange stories about other researchers. There was a Mr. Dickson who was a political agent; he was an Englishman and had written a book with the title *The Arab of the Desert* [1949]. He was eager to make sure that no researcher was accepted who could be competition for him. Henry Field was a physical anthropologist, and he was interested only in conducting anthropological measurements [1971, 1977], but Dickson led him in the wrong direction. This was the kind of trick he played on people, according to Field. So I was somewhat prepared, and I was able to conduct regular ethnographic work.

Even then the question came up: If something like a (Sulubba, or other) pre-Bedouin substratum existed at all, how old must the Bedouins be? How could I find that out? That encouraged me to visit the Bedouins. There I learned how to ride a camel, and the idea occurred to me to examine the saddle of the camel. That led to the identification of different types of saddles and of riding, and this is how the theory of the Bedouins' having developed in Southern Arabia came into being [1967].

AG/SMH: Many associate another aspect of your research if not with action anthropology then at least with practical and social engagement for the cultural groups that are the subjects of our field research and writing. This component of your work spans from the publication of the first Barbados conference on the rights of the indigenous population of Latin America [1972*b*], which you initiated with Georg Grünberg, to the long-term leadership of the Austrian Association for the Middle East and the editorship of its journal, *Bustan*. How do you see this side of your endeavors? Do you think that civic and social engagement is part of researchers' ethical responsibility, and, if so, what difficulties and opportunities result from that?

WD: Well, in general I am for it because it gives researchers the chance to combine the lofty realms of academia with practical fields. For example, let's assume that there is an ethnic minority group in this country. Portraying it to my neighbor in such a way that he becomes interested in it and perhaps makes friends among its members—that would be it.

The first Barbados conference was a response to the students' revolt of the late 1960s. At the time there was a joint meeting of the German, Austrian, and Swiss anthropological societies in Göttingen, in 1969. This gathering was totally transformed by the rebellious students, who energetically raised the problem of genocide. Now, genocide is a serious problem that one cannot simply ignore. I can say that my colleagues then, unfortunately, behaved stupidly and not really ethically. The students, however, released a resolution. Afterwards, I happened to be standing outside of this hall (I was waiting for someone) when the students' ringleaders came out and said, "And now, what are we going to do next?" This made me mad: Genocide is on the agenda, and they are already talking about something else. So I said, "We have to take this seriously. Let's upgrade it to the academic level." I had my colleague Grünberg with me; he was assistant professor with me at that time. I asked him to write a critical bibliography on genocide among the indigeneous peoples of South America, and he did so [Grünberg 1972].

We also had a young woman with us who was working for the World Council of Churches. She explained the facts to the gentlemen there, and they called me and asked, "Can't we do anything?" I said, "Please, come visit me in Bern." As is generally known, a good dinner brings good ideas. I suggested organizing a conference to assemble our colleagues from Latin America in order to establish guidelines. I had only one wish: "No German ethnologists, because they will become hyperactive and lose their students, and this must not happen here." (I admit that this was unfair.) So I suggested sending Grünberg to North and South America to establish contacts with the local anthropologists and find out whether they would participate in such a conference. To their credit, the Latin American colleagues cooperated admirably. The ones who did not cooperate were influential U.S. Americans

and Canadians. I don't know why the North Americans declined. (I can imagine why, but I won't spell it out.) Then there came a sudden call from Geneva from the head of the World Council of Churches saying that they had calculated everything and in the end the expensive flights were going to cost them five years of their budget. So I told them that I didn't care where the conference took place, and they suggested Barbados. I didn't go there, because I didn't want to put a strain on the budget and I don't speak Spanish. What is worth mentioning and even deserving of some admiration is that the faculty at Bern ultimately picked up the slack. A colleague of mine approached me and said, "Mr. Dostal, you are about to lose your job because your involvement [in this project] is contrary to the economic interests of Switzerland." That was hard, indeed. Three days later we had a faculty meeting, and under the agenda item "Miscellaneous" I presented my problem. They instantly extended the meeting in order to add this issue to the agenda, and the result was a unanimous vote in favor of my initiative.

AG/SMH: That shows that support for such an engagement can be mobilized after all.

WD: This wouldn't have been possible in Vienna! It was typical for Switzerland. I am really proud of that.

AG/SMH: You have dealt your whole life with Islam. Today, the Muslim communities and societies of the world are going through a very difficult phase. This is often associated with al-Qaeda and Osama bin Laden. Can anthropologists offer a specific contribution to an understanding of this phenomenon, or is this a topic that goes beyond our competence?

WD: Well, I think that anthropologists *must* confront this problem as well and try to master it. This won't happen overnight, but Osama bin Muhammad bin Laden is only one figure within a much more complex set of contexts and problems. One dimension is that there are criminals such as—until his recent death—Abu Mus'ab Al-Zarqawi. Certain powers believe that they can act towards criminal figures the way the sorcerer and his apprentice acted towards evil spirits. Unfortunately, they have never read Goethe's "Sorcerer's Apprentice," or things wouldn't have gotten this far. At first the Muslims served as cannon fodder in the fight against the Russians, and then, all of a sudden, everything was supposed to be over. That was impossible. So at that crossroads Osama bin Laden, with the help of Ayman Al-Zawahiri, gave the movement a certain direction. Historically, the movement goes back to the incomplete confrontations of the nineteenth century. When Napoleon occupied Egypt, the doors were open, wide open. Gradually, Muhammad Ali did everything possible to bring about an Egyptian mode of modernization and Westernization. He thought, in fact, that until then Muslims had been too passive and that it was necessary for them to have a completely new direction and education regarding

the military, technology, administration, etc. At first, a few students were sent to Paris. Later, until around 1848, about 330 students followed. This was the basis for the beginning of modernization, except that the people who went to France were still good Muslims. They were very impressed by this or that in the West, but they did not yet dare to make the step, the radical step. In the course of time, the radicals emerged. Then the conservatives emerged, also with a modern touch. The prevailing opinion was crucial: Islam needed a modernizing correction but not the religious content. This was perpetuated, and that's how the Islamists emerged. They thought that only Islam could liberate humanity from the quandary of cultural change. There are all sorts of shades in Islam; it is a colorful range of many different facets. Osama bin Laden and these Muslim extremists are just one of them.

AG/SMH: Where is the specific contribution in our discipline, beyond the purely historical sequences, that would allow us to grasp this? Would you say we are dealing with a kind of radical revitalization movement?

WD: It is a simply brutal primitive movement of Islamists, simply primitive. I have interacted with Islamists, I even demonstrated once with Islamists in Sanaa. Everyone thought I was crazy. These were people like you and me, but some of them were illiterates who were only familiar with oral renderings of the Koran. I was at the demonstration for three full hours until one of the head functionaries said, "My dear friend, I think it would be good if you left us now." That's when I left. I questioned some people on the Koran: "What is the content of sura 114, etc.?" only a few of these half-educated imams could answer correctly. We even had a witty religious dispute during that time. We challenged each other with quotes from the Koran. This was pretty intense. And then I was in the Wadi Hadhramawt in a centre for Islamists. The organization was very elaborate. It was exclusively paid for by Indonesians, but there was also an American.

AG/SMH: Was this something like a *ma'had al-'ilm* (theological institute), or was it a madrasa?

WD: Not "ilm." One would have to say "*bayt al-din*" [i.e., not a theological institute but a "house of religion"]. They get money from Indonesia and proceed very carefully. They don't act so boldly with the Islamist idea but advance cautiously and prepare the students for the activities as Islamists. But this is only the preparation.

And then I also had contact with the highest judge in Yemen, with Qadi al-Hitar. Qadi al-Hitar founded a forum for al-Qaeda people who might be ready to renounce it. The Yemenis who returned to their homeland were immediately arrested—because of pressure from the United States—and were supposed to be prosecuted. Some were convicted and others not because they were simply followers. Their view was that they only wanted to fight for the cause of Islam, and this was their primary

reason, but religion cannot be held accountable. So Qadi al-Hitar founded a forum for dialogue, and with it he tries to persuade these people to change their attitude. He starts with simple things. For example, he tries to convince them wearing a beard as the Taliban do has nothing to do with Islam; it is not mentioned anywhere in the Koran, though some preachers claim that it is. He has to try to correct this more or less through conversations with the people.

AG/SMH: Considering these historical deliberations and your personal observations, do you think that it makes sense to distinguish between a heterogeneous Islamist mainstream, on the one hand, which will certainly continue to exist for some time, and the small sects and groups that are open to violence, on the other, or does this distinction not lead anywhere?

WD: I believe that the distinction serves a purpose because one needs to eliminate the truly criminal elements, such as Zarqawi, a Jordanian who was prone to violence from early on. I believe that these are moments when one needs to say, "Here are the limits." We can always talk about the mainstream and all the other issues. It would be very important, however, for an anthropologist to deal with these issues, particularly those concerning the nonviolent Islamists, those who are not terrorists. Discussion with them could be very promising. In fact, it would be the only way to prevent the terrible bloodshed.

AG/SMH: If students in the first semester came to you today, what advice would you give them concerning their studies and practice in our discipline? What should be the major focus of young students today?

WD: Well, one main focus should be the acquisition of languages. An indigenous language is an absolute must—I don't understand the geniuses who still "discover" matrilineages, female life experiences, patrilineages, or whatever, without any language skills. Secondly, one must be open to all sorts of methodological innovations and to new knowledge. One has to read a lot, especially methodology. I know this is a desideratum that one doesn't like to hear, but without knowledge of methodology one doesn't get very far. After all, one must reach a stage where one is able to derive a synthesis from various sources for one's own work. That is essential. I can only repeat: learning a language and being open to methodological concerns. And, in particular, one must also have a good basis in the history of methodology and theory, because those old guys were not stupid at all!

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