FROM THEORY TO FACT IN ANTHROPOLOGY: THE CASE OF MEXICAN ETHNOGRAPHY

Toomas Gross[1]

University of Cambridge

> It is useless going into the field blind.
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> Evans-Pritchard (1976:242)

Abstract. The present article looks upon the relationship between theory and method in the the discipline of anthropology in general, and in the case of Mexican ethnography in particular. After a brief discussion of these issues in anthropology in general, a short overview of the development of Mexican anthropology into an applied social science is presented and various examples from the history of Mexican ethnography are used to illustrate that anthropologists in their placement into the field and in their methodological preferences have often been influenced by the theoretical schools to which they belonged, as well as their very personal intersts and backgrounds.

The present article looks upon the relationship between theory and method by examining the ways the anthropologists in their approaches have been influenced by particular theoretical schools to which they belonged. The discussion is illustrated by the examples from Mexican ethnography and its purpose is not to present an all-inclusive history of Mexican anthropology, but to demonstrate how the "anthropo-logical gaze" and the anthropologists' methodological preferences have been affected by certain paradigmatic constraints and the anthropologists' personal interests.

The existence and use of "method" or set of methods are the main criteria of "science", including social science.[2] "Anthropological method" and "anthropo-

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[1] Doctoral student of Cambridge University. Currently doing fieldwork in Oaxaca (as a visting researcher of the local *Centro de Investigaciones y Estudios Superiores de Antropoloía Social* [*CIESAS*]).

[2] Lastrucci (1963:6), for instance, defines science as "an objective, logical, and systematic *method* of analysis of phenomena, devised to permit the accumulation of reliable knowledge". (Italics added.)

logical theory" are complex issues and their discussion often problematic for various reasons. As Bernard (1995:1) claims, the word "method" has at least three different meanings in anthropology. At the most general level, it means *epistemology* or the study of how we know things. At a less general level it is about strategic choices, for instance, whether to do participant observation, library research or an experiment. At the most specific level it is, for instance, about what kind of sample to select, whether to do a face-to-face interview, whether to use an interpreter *etc*. Theory, in turn, comes in two basic sizes in anthropology and one can distinguish between elemental or *ideographic* theory, and *nomothetic* theory. An ideographic theory accounts for the facts in a single case. A nomothetic theory accounts for the facts in many cases. The more cases a theory accounts for, the more nomothetic it is. (*ibid.*: 110).

When it comes to epistemology, the key question in anthropology in most blunt and general terms is whether one subscribes to *rationalism* or *empiricism*, or, alternatively, whether one subscribes to *positivism*, or *humanism* (or what one might also call *interpretivism*). Rationalism in the context of anthropological discourse refers to the idea that human beings achieve knowledge because of their capacity to reason. That means that "out there" there exist *a priori* truths, and if we prepare our minds adequately, those truths will become evident to us. For an empiricist, on the other hand, the only knowledge that human beings can possibly acquire, comes from the sensory experience (Bernard 1995:2).

The intellectual clash between empiricism and rationalism creates a major dilemma for the anthropologists. Rationalist or empiricist stance also determines the methodological preferences in anthropology and the social sciences as a whole.

Beyond the fact

Neither the attention on social scientific methods in general, nor the concern with the relationship between theory and method are particularly novel. As is well known, the nature of social scientific method was already explored by Durkheim in his *Les Règles de la Méthode Sociologique* in 1895 and the relationship between theory and method found its most vigorous treatment in Kuhn's *The Structure of Scientific Revolutions* in 1962. Kuhn's controversial statement that scientific disciplines are governed by paradigms – certain more or less logically organised and mutually articulated sets of ideas and methods of inquiry – puts social sciences, including anthropology, into an awkward position of *proto-sciences* in which no single *paradigm* has been prevalent at any moment.

The discussion about theory and method in anthropology is as old as anthropology itself. The particular and most common anthropological method initiated by Bronislaw Malinowski and to a certain extent also by Franz Boas – the *participant observation* – is the defining method of anthropological research,

and in its essence means an extended stay in the context of the studied culture or social group, the collection of first-hand knowledge and the annotation of observable facts. Most of anthropology is thus phenomenological, phenomenology meaning a philosophy of knowledge that emphasises direct observation of phenomena. Unlike positivists, phenomenologists, starting from Edmund Husserl and followed most notably by Alfred Schutz, seek to sense reality and to describe it in words, rather than numbers – words that reflect consciousness and perception.[3] Social sciences are different from the physical ones in that respect. As Schutz (1962:59) puts it, when you study molecules, you do not have to worry about what the world "means" to the molecules. But when you try to understand the reality of a human being, it is an entirely different matter. The only way to understand social reality, Schutz argues, is through the meanings that people give to that reality.

In a phenomenological study, the researcher tries to see reality through the eyes of the informants.[4] Phenomenologists try to produce convincing descriptions of what they experience rather than provide explanations and causes. A good

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[3] This does not mean, however, that positivism and quantification do not go with anthropology at all, although some have called for this. The most outstanding project of the comparison of different cultures and codification/ quantification of different cultural elements in anthropology has been the compilation of Human Relations Area Files started by Murdoch. The most articulate spokesman against the idea that anthropology could ever be a quantified science was Paul Radin. In his *The Method and Theory of Ethnology* (1966) he attacked his professor Franz Boas, as well as his major contemporaries Clark Wissler, Alfred Kroeber, Edward Sapir, Robert Lowie and Margaret Mead, whom he accused of abandoning humanistic, historical study of culture and trying to make ethnology a comparative, ultimately quantitative science. For Radin, the scientific approach was a tragedy because quantitative studies focused on aggregates rather than on individuals.

[4] In the end this is, of course, impossible. The outsider´s experience and understanding is always different from the insider's one. The concepts of the observers and the observed about the same things are and remain distinct. Realising this, the cognitive anthropologists, especially the proponents of the so-called "ethnoscience", introduced the concepts of *emic* and *etic* into anthropological discourse in the 1960s, marking with them the insider´s and outsider´s understanding of a particular cultural phenomenon.

The questions of context and understanding were explored more extensively by hermeneutic philosophers, from Dilthey and Heidegger onwards, best articulated by Gadamer in *Truth and Method* (1975), and taken to the extreme by Feyerabend in *Against Method* (1975). Although applying the almost anarchistic theory of knowledge proposed by hermeneutic philosophers to anthropology would be self-destructive, as it would render the whole anthropological enterprise virtually meaningless, taking into consideration some of their ideas, for instance Gadamer's notion of *engagement* – our rootedness in the social world, is useful when reflecting upon the anthropologists' approaches and choices of method.

In addition, hermeneutics, originally a study of biblical texts, has during the recent years entered the discipline of anthropology as a particular approach in the form of close and careful study of free-floating native texts, such as myths or other stories. The hermeneutic approach stresses that myths contain some underlying meaning, at least for the people who tell the myths, and it is the job of an anthropologist to discover that meaning. By extension, the term *hermeneutics* is now also used to cover the study of free-floating acts of people, construing those acts as if those were texts whose internal meaning can be discovered by proper exegesis. (Bernard 1995:14).

ethnography – a narrative that describes culture or a part of a culture – is usually a good phenomenology, and there is still no substitute for a good story, well told – especially if you are trying to make the reader understand how the people you have studied think and feel about their lives. (Bernard 1995:15).

It is thus obvious that in anthropology, fieldwork and personal experience lead to a better understanding of the studied social or cultural group, or phenomenon than reading about these from secondary sources, or just guessing. On the other hand, however, the method of participant observation is a pretentious project that has its serious epistemological problems, concerned primarily with the nature of the relationship between theory and the observable facts. Many anthropologists have underlined this. As Agar (1980:41) puts it:

> *Ethnography is really quite an arrogant enterprise, an ethnographer carries more baggage than a tape recorder and a toothbrush, having grown up in a particular culture, acquiring many of its sometimes implicit assumptions about the nature of reality.*

These implicit assumptions and cultural baggage that the anthropologists carry with them and cannot get rid of in the field, often become explicit in their personal diaries. We have the privilege to have an access to the personal diaries of Malinowski and Boas which reveal not only epistemological clash between the anthropologist´s and the informant´s worlds but often a direct and straightforward confrontation on just simple mundane matters. In reality, anthropologists are often far from being cultural relativists and on the very personal level the cultural tolerance that anthropology and anthropologists claim to cultivate, is just a myth. Extracts from both of these diaries reveal it eloquently.

Boas spent 15 months on Baffin Island. Missing German society and his fiancée Marie Krackowizer sorely, he wrote down about 500 pages to her although he was not able to send them. (Bernard 1995:183). Here is an extract from his writings:

> *December 16, north of Pangnirtung.*
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> *My dear sweetheart ... Do you know how I pass these long evenings? I have a copy of Kant with me, which I am studying, so that I shall not be so completely uneducated when I return. Life here really makes one dull and stupid.... I have to blush when I remember that during our meal tonight I thought how good a pudding with plum sauce would taste. But you have no idea what an effect privations and hunger, real hunger, have on a person. Maybe Mr. Kant is a good antidote! The contrast is almost unbelievable when I remember that a year ago I was in a society and observed all the rules of good taste, and tonight I sit in this snow hut with Wilhelm and an Eskimo eating a piece of raw, frozen seal meat which had first to be hacked up with an axe, and greedily gulping my coffee. Is that not as great a contradiction as one can think of?* (Cole 1983:29).

An extract from the diary by Malinowski who spent almost four years on the Trobriand Islands in the Southern Pacific, expresses similar boredom and drowsiness in his diary:

Tuesday, 4.24.

... Last night and this morning looked in vain for fellows for my boat. Tis drives
me to a state of white rage and hatred for bronze-colored skin, combined with
depression, a desire to "sit down and cry", and a furious longing "to get out of
this". For all that, I decide to resist and work today – "business as usual",
despite everything. (Malinowski 1967:261)

Malinowski's and Boas' personal epistemological struggles were similar, but when it comes to theory, they were, of course, as different as could be. Malinowski was the forerunner of functionalism, and thus loaded with heavy theory. Boas, in turn, was a cultural particularist, a historicist, whose approach was almost anti-intellectual in its reluctance towards any kind of theory, including comparison.[5] Boas somewhat extraordinarily believed that what is needed in anthropology, is just fact-collection, and that theory would eventually "drop" out from between the facts by itself.

Evans-Pritchard suggested already in the 1930s, quite exceptionally for the first decades of the history of anthropology when epistemological self-critique had not much space in the minds of anthropologists, that in science, as in life, one finds only what one seeks (see Evans-Pritchard 1976:240). Aya (1990:66) similarly argues that anthropologists often tend to see and report only the kinds of facts that their theories train them to see.[6] This is obviously a serious attack against the scientific validity of the whole discipline of anthropology. All that kind of argumentation is not new, of course, dating back at least to Heracleitus who already in the 5th century B.C. claimed that "he who does not expect the unexpected will not detect it" (cf. Popper 1972:153).

Although many anthropologists (*e.g.* Pelto and Pelto 1978; Agar 1980) in recent years have called for increasing the role of methodological training in anthropology, any kind of method, any form of observation will eventually remain partial and this partiality is practically unavoidable. This has been one of the main reasons for an increasing self-criticism and crisis within the discipline of anthropology during the

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[5] His (non-) theoretical stances are best exemplified in the essay *The Limitation of Comparative Method of Anthropology*, published in 1896.

[6] Take difusionism and cultural materialism, for instance. Difusionism was the theory that claimed that cultural elements were "borrowed" by one culture from the other, or by one sociocultural region from the other, thus difusing from the centre to the periphery and forming "cultural circles" (*e.g* the British monocentrism, representatives of which were Elliot Smith, Perry and Rivers, the German-Austrian *Kulturkreis* theory represented by Ratzel, Graebner and Schmidt, or the American culture centre theory represented by Wissler and Kroeber). The existence of similar artefacts and mentifacts in different cultures could according to difusionists be explained exclusively by borrowing and direct or indirect contact between these cultures. This was what they *looked for* and *saw*, closing their eyes and minds to the possibility of an independent emergence of similar elements in different areas and times.

Cultural materialists (*e.g.* White, Harris *etc.*), in turn, tried to explain cultural and social similarities and differences exclusively by making reference to material conditions and the nutritional conditions of the diet. The nature of many cultural and social phenomena could in their view be explained by the so-called "protein myth", for instance.

last decades. On the other hand, the acknowledgement of the partiality of any kind of anthropological method is valuable in itself and definitely a step forward. As Brenner *et al* (1978:3), focusing on the problems inherent in the traditional methodology of the social sciences in general, conclude, detecting the impact of theory on method helps us to understand what goes on in the method. In order to recognise the social nature of an inquiry, the social contexts of the method should be actively investigated.

Acknowledgement of the limits of theory is valuable in a similar vain. As Fortes (1969:92) argues, in anthropology we often find that what we discriminate as a discrete state of affairs relative to its external environment by one set of criteria, dissolves into constituent parts if we follow a different procedure or ask different questions. However, the predicament disappears if we realise that what we are concerned with, is a rule of procedure and a policy in the use of our conceptual tools, not a set of mechanically applicable tests.

Theory, or rather the pre-existence of theory, thus always makes observation partial. But the need for theory, or let us say "good theory", is itself also inevitable. As Parsons (1968:1) puts it, theory "tells us what we want to know". Aya (1990:2–3) concludes that even if Kuhn is right that "normal science" takes a "paradigm" theory for granted and replaces it only when failure to solve explanatory "puzzles" creates a "crisis" and a rival "candidate for paradigm" turns inexplicable "anomalies" into predictions, the point remains: empirical research depends on theoretical ideas. Better that ideas be explicit, clear, and critically examined than implicit, cloudy and uncritically assumed, Aya (*ibid.*) warns us.

Aya (1990:2) goes even as far as to claim that if philosophers of science agree on anything, it is that research cannot start with fact-collection. Nor can it end there, I would add. Many in anthropology and sociology have understood that. Malinowski (1922:517) claimed that what matters really is not the detail, not the fact, but the scientific use we make of it. Fortes (1970:129) argued that ethnographic facts, unless they are examined in the light of theory, are meaningless. Edmund Leach's oft-quoted passage also followed the same path:

> When I read a book by one of my anthropological colleagues, I am, I must confess, frequently bored by the facts... I read... not from an interest in the facts but so as to learn something about the principles behind the facts. (Leach 1954: 227)

As Evans-Pritchard (1976:241) stressed, one should not go into the field blind. And, without theory to guide it, fact-gathering "produces a morass", warned Kuhn (1962:16).

The birth of Mexican anthropology: major landmarks

Before turning to the examining of the impact of theory on method in Mexican ethnography, it is appropriate here to make a most general introduction to the formative decades of Mexican anthropology and its major preoccupations.

There is no clear consensus about when and where Mexican anthropology starts. Some see its origins in the Indian Laws introduced by the Spanish Crown in the 16th century, others in Bernardino Sahagún's *La Historia General de las Cosas de Nueva España*, or in the activities of Bartolomé de las Casas, Vasco de Quiroga, Victor María Flores and other Spanish missionaries (Comas 1964:9). Others start much later with Francisco Pimentel, who in 1864 published his *Memoria sobre las causas que han originado la situación actual de la raza indígena de México y los medios para remediarla* – a study of the problems of indigenous peoples of Mexico, and his solutions to those problems.

It is probably more correct to start from the second half of the last century if we look for the systematic and at least in their objective social scientific studies of Mexican cultural and ethnic landscape. The missionaries' writings, although valuable in many respects, did not fulfil these criteria. But neither was the anthropological research done in Mexico in the 19th century truly "Mexican", being carried out by foreigners whose presence in Mexico was enabled by the particular trends and periods in international relations and international macro-politics. The French military intervention to Mexico in the 1860s was soon followed by the anthropological one. This is yet another proof of evidence for those who claim that early anthropology was nothing more than a shadow following colonial powers and served their interests. In 1862 three French anthropologists – Gosse, Auburtin and Le Bret – prepared the so-called "Ethnological Instructions" to be used in the study of indigenous peoples of Mexico (Comas 1964:10). The presence of the French in Mexico also enabled the foundation of the *Comission scientifique du Mexique* in 1864. This was subdivided into various committees which aimed at the investigation of multiple aspects of the various Mexican regions, including the "study of the diverse races". This Commission, the members of which included some of the distinguished social scientists of the time like Quatrefages, Milne-Edwards, Longpérier, Brasseur de Bourgbourg, Coindet, Biart, Jourdanet, Orozco y Berra, Garcia Icazbalceta, and others, published its own "Instructions", which prescribed how to approach certain problems of the Mexican indigenous peoples "anthropologically". Some of the members of the Commission, notably Coindet and Jourdanet, also published the results of their somatophysiological studies of some indigenous groups of Mexico, which were among the first studies in Mexico in the field of physical anthropology. (Comas 1964:10–11).

Early Mexican anthropology in a proper sense, emerging in the 1920s immediately after the Mexican Revolution, was (and it still is) very distinct from that of early British anthropology, for instance. If one looks at the regional and theoretical preferences of early anthropological schools of various countries, one can easily discover, that the anthropological scope and lens were often congruent with the political and demographic dimensions of the particular country. Thus, early British anthropology, starting with Sir Henry Maine, and developed in its early phases by Frazer, Haddon, Rivers, Malinowski, Radcliffe-Brown, Evans-

Pritchrard and many others, was mostly concerned with its former or at that time current overseas colonies in Africa and Asia. The British anthropological research was from the very beginning orientated so that it would in the end lead to more effective exploitation of both natural and human resources in the (ex-)colonies (Comas 1964:6). In many of these cases the role of the anthropologist was limited to that of an advisor to colonial administration, although ideological conflicts between anthropologists and administrators of the respective colonies were also quite common as Evans-Pritchard (1951:109–129) has pointed out. The North American School of anthropology was distinct from the British one, as it was not so much in service of the exploitation of people and resources of the *overseas* colonies, but the acculturation and incorporation of the *internal* ones – its indigenous minorities living in the "reservations".

In Mexico, the great ethnic, linguistic and cultural diversity, united together by the social and political doctrine of the Mexican Revolution, also required an entirely different approach compared to that of the early British anthropology, as well as North American anthropology. As Aguirre Beltrán (1957:199) puts it, the regional administrator in Mexico was subordinated to the anthropologist because the aim of both was the integration and development of the concrete region, its resources and inhabitants, and it was supposed that the specialist in social sciences was the one who was more competent in the task of treating the problems of human coexistence, which arose in the contact of different cultures.

The internal cultural and ethnic diversity of Mexico and the task of Mexican anthropology to solve the country's own major internal problems and not those of the overseas colonies or the country's periphery, has made Mexican anthropology distinct in the whole world. Being mostly concerned with the integration and development of the country's indigenous population, far more substantial, than the native North-American one, the Mexican anthropology developed into an "applied" social scientific discipline at the very beginning. Already in 1917 Manuel Gamio took the initiative of using social anthropological knowledge in the improvement of the so-called "regional populations" (Comas 1964:4).[7]

Although Gamio was the real initiator of the Mexican national anthropology, its roots can be traced back already to the last decades of the 19th century. Here are some of the most important landmarks of the early Mexican anthropology that reveal its quick development into a social scientific discipline with an applied and socially relevant nature. In 1887 the first department of physical anthropology directed by Dr. Nicolás León, and in 1903 the first Chair of Anthropology were founded. The aim of anthropology at that time, much in the Boasian tradition, however, was just the collection of ethnographic and cultural data, not followed by a profound analysis. In 1906 the Mexican government under the presidency of

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[7] Applied anthropology in the Anglo-American world emerged approximately during the decade of 1925–35. More concretely, in the United States the preparation and publication of the anthropological monographs started in 1933, the aim of which was to re-orient the politics of the Bureau of Indian Affairs in Washington, then headed by John Collier (see Herskovits 1948).

Porfirio Díaz passed the first law in favour of Indians (Tarahumaras). At that time, many started to call for the acknowledgement of the cultural and sociological heterogeneity of the indigenous peoples of Mexico. Molina Enríquez (1909:293), for instance, claimed that

> *the indigenous element, composed of tribes and villages very different from each other, lacks the unity. Every tribe, and every village is a [unique] sociological individual.*

In 1909 the National Museum of Archaeology, History and Ethnology (*Museo Nacional de Arqueología, Historia y Etnología*), was founded. In 1939 it was renamed the National Museum of Anthropology (*Museo Nacional de Antropología*), and is now one of the biggest and most well-known in the world. In 1910 the Mexican Indianist Society (*La Sociedad Indianista Mexicana*) founded by Francisco Belmar, the magistrate of the Supreme Court of National Justice, and supported by the President Porfirio Díaz, started its activities with the main objective to study indigenous peoples. In 1911 the International School of American Archaeology and Ethnology (*La Escuela Internacional de Arqueología y Etnografía Americanas*), was established in as a collaborative project of the Mexican and Prussian governments, and the Universities of Columbia, Harvard and Pennsylvania, being directed among others by Franz Boas, Edward Seler, George Engerrand, Alfred M. Tozzer and finally by Manuel Gamio. In 1920 the publication of the first Mexican anthropological journal *Ethnos* started.

1920s and 30s were marked by cultural missions to various Mexican rural areas and the attempts to integrate and "mexicanise" the indigenous population. Miguel O. Mendizábal and Moisés Sáenz were especially active on that front. Mendizábal was in the avant guarde of the fight for scientific, systematic, objective and integral action in the improvement of cultural and socio-economic conditions of the indigenous peoples. In 1922 the Mexican Rural School (*La Escuela Rural Mexicana*) was started, the leading idea of which was "integrity in action" (*integridad en acción*). In the 1930s *Escuela Rural* was led by Moisés Sáenz, who was also the initiator of the first major project in applied anthropology in Mexico. Between June 1932 and January 1933 he headed the experiment of the so-called "Experimental Station of the Incorporation of the Indian" (*La Estación Experimental de Incorporación del Indio*), comprising 11 villages in the State of Michoacán. His purpose was the development of social anthropological studies to investigate the realities of the indigenous environment and the phenomena that operate in the process of the assimilation of the aboriginal population into Mexican environment (Sáenz 1936). The "Experimental Station" pretended to be an institute of ethnological and sociological studies the aim of which was to "culturise" the Indians, improve their living conditions and achieve the integration of these communities into the Mexican conglomerate (*ibid.*, 28). Sáenz also aimed at harmonising science and practice (Comas 1964:29). Although the aims of the experiment were not achieved and the project was purged half a year after it was started, the experience gained was valuable.

The 1930s were also marked by an ever-increasing presence of North-American anthropologists in Mexico. Under the auspices of the Carnegie Institute in Washington and directed by Robert Redfield, numerous projects in various regions of Mexico started in 1930. The studies in the South-Eastern part of the country, especially in Yucatan and Chiapas, developed most extensively as a collaboration between the University of Chicago, the Viking Fund, and the National Institute of Anthropology and History of Mexico. Those studies united together the forces of such distinguished anthropologists as Robert Redfield, Alfonso Villa Rojas, Sol Tax, Ricardo Pozas, Fernando Cámara, Calixta Gutieras, Arturo Monzón, Isabel Horcasitas and others.

In 1934, Beals, Tax and Redfield published a co-authored paper on the anthropological problems that had emerged in the investigation of Mexican indigenous groups and claimed that "the major opportunities in these countries lie in the fields of *community studies*[8]" (Beals, Redfield and Tax 1943:1). The North-American anthropological and academic presence in Mexico even increased in the 1940s when the Smithsonian Institution in Washington established the Institute of Social Anthropology in 1943, directed first by Julian H. Steward, and later by George M. Foster. Although the studies by that institute were carried out in many Latin American countries, the special focus was on Mexico.

In 1934, the Summer Institute of Linguistics (SIL), originally founded at Oklahoma University by William Cameron Towsend, started its activities in Mexico. Its apparent aim was the study of indigenous languages and the compilation of dictionaries, grammars *etc*. Its implicit goal, however, was to christianise the indigenous peoples by translating the Bible into indigenous languages. Some of the best linguists were working for SIL, but in the end linguistics was just a tool in the service of religious aims. It can also be argued that although organising extensive and profound studies of the local languages and cultures, in the end the activities of SIL actually lead to castellanisation and cultural homogenisation. This is also the reason why SIL in Mexico and in the whole of Latin America was severely criticised by anthropologists, and eventually (in 1979) its activities in Mexico were banned.

Ethnological and sociological studies of the indigenous peoples proliferated extensively in the 1930s, and anthropology gained an important place in Mexican academia. In 1937 the Mexican Society of Anthropology (*La Sociedad Mexicana de Antropología*), directed by Alfonso Caso, was established. Earlier, in 1930, the Institute of Social Studies (*El Instituto de Investigaciones Sociales*) had been founded at UNAM (*Universidad Nacional Autónoma Metropolitana*), which from 1939 was lead by the distinguished Lucio Mendieta y Núñez. Under the auspices of that institute, a series of ethnological studies with characteristic headings like *Los Tarascos* (1940) and *Los Zapotecos* (1949), marked by almost Herderian ideology of the unity of culture, *ethnos* and language, were published. In 1940, Carlos Basauri, the head of the Department of Indigenous Education of the

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[8]   Italics added.

Ministry of Public Education published a substantial three-volume *La Población Indígena de México*.

In 1936, during the presidency of Lazaro Cardenas, the Autonomous Department of Indigenous Affairs (*El Departamento Autónoma de Asuntos Indígenas*) was opened, directed among others by Manuel Gamio and Julio de la Fuente. Its activities were very similar to the ones of the later-founded National Institute of Indigenous [Peoples] (*Instituto Nacional Indigenista* or just *INI*). The department paid special attention to the question of indigenous languages and organised the First Assembly of Philologists and Linguists in 1939 which in turn elected the Council of Indigenous Languages. The issue of indigenous languages and alphabetisation were in the forefront also in the 1940s. In 1946 the Ministry of Public Education established the Institute of Alphabetisation in Indigenous Languages (*Instituto de Alfabetización en Lenguas Indígenas*), later called the Institute of Alphabetisation for Monolingual Indians (*Instituto de Alfabetización para Indígenas Monolingües*).

The increasing preoccupation for the problems of indigenous peoples lead also to the birth of the National School of Anthopology (*Escuela Nacional de Antropología*) in 1938. Preoccupation for indigenous peoples reached even greater level in the 1940s both on national and international level, and resulted in the creation of the Interamerican Institute of Indigenous People (*Instituto Indigenista Interamericano* or just *III*) in 1942, lead first by the same Manuel Gamio, and the already menitoned *Insitituto Nacional Indigenista* (*INI*) in 1948 by Alfonso Caso. The foundation of *INI* also marks the birth of clear and systematic indigenous politics in Mexico.

From theory to fact in Mexican ethnography

More than seven decades of systematic anthropological research in Mexico offer numerous examples of how the theoretical determination of anthropologists has influenced their ways of observing social phenomena. Hewitt de Alcántara (1984) sees a clear connection between the two. Trying to look for the congruence between social setting and paradigmatic concerns, Hewitt de Alcántara (1984: 178) argues that the way anthropologists have approached Mexican countryside at any particular period of time has above all been a function of the intellectual structure of schools in which they have been trained and not of a random confrontation with life in rural Mexico. This means that anthropologists have often gone into rural areas in search of settings and situations which fit their preconceived images of adequate field sites and have done their best to see local reality in terms that are validated by a previously adopted assumption. But this, as Hewitt de Alcántara (1984:179) assures us, does not necessarily imply that anthropologists have not acted scientifically, at least if we reason in Kuhnian terms.

Let us take, for instance, the anthropologists' choice of communities. In general, as Pelto and Pelto (1978:179) suggest, there are three bases for choosing a researchable community, depending on what one wants to prove – choosing what one conceives of as the most "typical" or representative community; or, on the contrary, an "atypical" one; or, alternatively, one that has already been studied before. In the context of Mexican ethnography, the example of the first is Redfield's (1941) study of folk cultures of Yucatán. Being interested in "folk-urban continuum", he chose certain communities for study that he considered as representatives of the range of variation from "most contact with the outside world" to "most isolated". The example of the second is Stephen Fry's (1992) study of the Zapotec communities in Oaxaca, which, contrary to other groups of the region, he characterised as explicitly non-aggressive. The third choice is best exemplified by Lewis' (1963) restudy of Tepoztlán, studied earlier by Redfield (1930), or Redfield's restudy of his own earlier study together with Villa Rojas of a Mayan village Chan Kom (see Redfield and Villa Rojas 1962; Redfield 1962).

The particularist school lead by Franz Boas, already touched upon above, was primarily concerned with the study of "primitive" peoples in the static and non-historical framework, in the form that Wolf (1955) has called "closed corporate communities". The particularist concern strongly affected what kind of social settings were chosen and studied. Human settlements isolated from any contact with the surrounding modern society constituted the privileged setting for their paradigm. The Boasian approach had a great impact on early Mexican anthropology, especially through The International School of American Archaeology and Ethnography (*La Escuela Internacional de Arqueología y Etnografía Americanas*) which was founded under his guidance in 1911. Various anthropologists working in Mexico like Moreno, Weitlander, Comas, Basauri, Mendieta y Nuñez, Fabila, Villa Rojas, and Beals started the mapping of Mexican "cultural areas" in the 1920s and 30s, following Boasian methodology and producing ethnographies of "pure" cultures that had revealing names like *The Tepehuán*, *The Seri*, *The Cahita* and others (Hewitt de Alcántara 1984:19).[9]

Boasian particularism in Mexican anthropology was modified further by the so-called *indigenistas* who looked for cultural impediments to national participation in mountainous "refuge regions" (Hewitt de Alcántara 1984:178). Manuel Gamio, Boas' student at Columbia University, was the major figure who, as was stressed above, basically inaugurated the modern practice of anthropology in Mexico. The purpose of ethnographic fieldwork among the "small cultures", as he used to call them, was not so much to preserve cultural idiosyncrasies as to understand them in order to hasten their disappearance. Gamio set out to study indigenous groups thought to be representative of the seventeen regions into which the country had

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[9]   A particularist, or rather, culturalist approach to Mexican countryside was also applied later, most
      notably by the participants of the well-known Harvard Chiapas Project in the 1950s which dealt
      in depth with particular aspects of life in Zinacantan and Chamula.

been divided. In that sense he was the precursor of *comparative studies* in Mexico. In the end, due to the poltical situation in the country, only one study was completed – in Teotihuacán – and published as *La Población del Valle de Teotihuacán* (1922). Maintaining a negative attitude towards the "traditional" life-style in rural areas, Gamio looked only for those issues that proved his point of view of backwardness.[10] The same was true to a certain extent about the later studies by Oscar Lewis, who coined the term "culture of poverty" by which he meant that "poor" people develop certain cultural and social values that prevent them from improving their situation, and thus enter a vicious circle. In various ethnographies of Mexican families, Lewis presented the facts that would prove his theory (see, for instance Lewis 1959; 1979).[11]

Functionalism, too, looked at community integration within areas with strong communal tradition, and derived explanations from the study of the microcosm alone, treating it as a hypothetical isolate (Wolf 1982:14). Structural functionalism followed suit. According to Radcliffe-Brown (cf. Fortes 1970:128), the structural functionalists "look[ed] at any culture as an integrated system and studie[d] the functions of social institutions, customs and beliefs of all kinds as parts of such a system". Although Malinowski, the main proponent of functionalism as a whole, studied Oaxacan markets together with Julio de la Fuente in the early 1940s (see Malinowski and de la Fuente 1982), the functionalist approach in Mexican ethnography is best exemplified by Robert Redfield's study of "folk communities" which initiated the tradition of "community studies" in American anthropology. Reaction, especially by Oscar Lewis (1963), to Redfield's *Tepoztlán: A Mexican Village* (1930), in his restudy of the village, has probably ignited one of the hottest disputes in the history of anthropology over the nature of anthropological method and anthropologist's personal attitude towards the studied community. The basic difference between the two approaches was that Redfield portrayed the village life as harmonious and integrated, while Lewis stressed hostility, jealousy and greed.[12] Methodologically

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[10] This was not a unifying feature of all *indigenistas*, however. Contrary to Gamio, Moises Sáenz, who represented the anti-incorporationist wing of *indigenismo*, concluded from his study of a Tarascan village that rural communities could not all be regarded as similar and that in some people were satisfied with their lives. What was common to all *indigenistas*, however, was that they looked for the methods of initiating community development programmes and modernisation policies.

[11] Such a predetermination and selective use of information has been common to many early anthropological studies. Margaret Mead, studying gender issues and ageing in Samoa, went even so far as to interview the missionaries' daughters instead of the local girls because of her lack of knowledge of the local language and then presented the results as representative of the Samoan girls (see Mead 1943).

[12] There are many noteworthy examples from the history of anthropology when different anthropologists have not agreed about the same community. Benedict's and Thompson's dispute with Goldfrank and Eggan over Pueblo Indians, Banfield's and Miller's debate over amoral familism, or Mead's clash with Fortune over the male role among the Arapesh are just some.

Lewis' restudy of Tepoztlán was provocative. The mere fact that Lewis studied the same village as Redfield was at that time considered almost like a breach of anthropological etiquette – entering the sacred territory that already "belonged" to Redfield (Agar 1980:7). The unwritten rule of avoiding restudies, probably a form of methodological defensiveness, contradicts, of course, the spirit of scientific study where replications are routinely done to check the reported results of an individual researcher. On the theoretical front, the criticism by Lewis and others also marked a shift of perspective from stability and harmony to process and disharmony in anthropology as a whole.[13] Many others contested Redfield's views. For instance, Foster (1967a), doing research in Tzintzuntzan, encountered mistrust, suspicion and fear rather than collectivism as an underlying feature of a rural community.[14]

Of a more particular interest to us here is Redfield's reply to Lewis' (1951: 428–9) criticism of his Rousseauist description of Tepoztlán:

> *I think that it is simply true that ... I looked at certain aspects of Tepoztecan life because they both interested and pleased me* (Redfield 1960:135; cf. Pelto and Pelto 1978:24).

The great role played by personal value preferences in shaping the argumentation and perspective of an anthropologist has been stressed by many (*e.g.* Bernard 1988, Pelto and Pelto 1978:26).[15] Agar (1980:43) goes even so far as

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[13] The focus on disharmony, change and conflict in anthropology, or rather its sub-field of political anthropology, started with Max Gluckman and the so-called Manchester School in the end of the 1940s, and was later developed further by numerous British (*e.g.* Leach, Bailey), and North American (*e.g.* Steward, Mintz, Leacock) anthropologists. Political anthropologists criticised functionalism and structuralism for their lethargic approaches, and their stress on harmony and *status quo*. Instead of stability and equilibrium, change and conflict became the central concepts of social reality for political anthropologists. Change, process and conflict were not to be seen as something anomalous but as intrinsic features of human societies, normal state of their being almost in a Hobbesian manner [see, for instance, studies by Bailey (1969), Barth (1969), and Leach (1977)].

The implicit assumption that humans are aggressive and that society controls and constrains them, can directly affect not only how something is *interpreted* but also what is actually *seen*. It has recently been noted by some authors (*e.g.* Howell and Willis 1989; Sponsel and Gregor 1994) that under the influence of the above-mentioned assumptions, peace and non-violence have rather mistakenly been regarded as aberrations from the norm of all-pervasive conflict and violence. Regarding conflict as an inherent feature of social structure might mean attaching preconceived meanings to the encountered social facts.

[14] On the other hand, as Hewitt de Alcántara (1984:35) argues, some of the criticism on the address of Redfield has been unfair. For instance, while Redfield had studied an Indian village, Foster's was a Mestizo one which renders their ideas rather incongruent.

[15] The scientist's personal influence is, of course, not a the feature of just anthropology or social sciences, although often naively considered as such. In "hard sciences" this interesting issue has deliberately been overlooked and avoided for obvious reasons. Feyerabend (1979) casts some light to this and so does Watson in his intriguing *The Double Helix*. Science, as Watson (1970:13) says, seldom proceeds in the straightforward logical manner imagined by outsiders. Rather, its steps forward are often very human events in which personalities and cultural traditions play major roles.

to explain differences between Lewis' and Redfield's work by making reference to their different personalities and backgrounds. Redfield, himself from the impersonal and over-urbanised Chicago, consequently tended to romanticise rural life, believing it to be closer to the "natural state" of human existence.[16]

More recent studies, carried by the ethos of Wallersteinian world system theory, neo-Marxism and post-structuralist or post-modernist trends in anthropological thought, have emphasised that setting community in opposition to the outside world might easily lead to regarding it, much in the way particularists and especially Redfield did, as a homogeneous unit, which, as the anthropologists now "see", it is not. Many have expressed their dissatisfaction with the previous studies in this respect (*e.g.* Pelto and Pelto 1978:177; Mallon 1995:11). Communities studied by cultural particularists and functionalists were often presented as typical of a given culture or subculture, without regard to the possible sub-cultural variations. Anthropologists made certain assumptions about the naturalness of communities as social units, endowed with a primordial unity and collective legitimacy. Influences from the outside world were seldom noted and described but frequently thought to be less important than the local cultural patterns and primary face-to-face social relationships that occur within the bounds of the community.

Starting from the 1950s, an increasing number of anthropologists working in Mexico stepped out of the geographical confines of the isolated rural community and the temporal confines of the functionalist present and started to study not so much what separated rural people from the wider economic system as what integrated them with it. Marxists and structuralists in the 1970s opposed themselves already completely to any kind of localism and empiricism and preferred to apply generic laws to the whole of the countryside, being particularly concerned with the impact of capitalist development upon rural society and what in 1950s became to be called "peasantry" (Hewitt de Alcántara 1984:181). The terms *centre* and *periphery* entered the vocabulary of anthropologists and influenced also their methodology. The forerunner in native Mexican anthropology in this respect was Gonzalo Aguirre Beltrán who was the first to study metropolis-satellite relations and rural-urban integration (*ibid.*: 50). His line was later followed by many Western/Northern anthropologists (*e.g.* Cancian, Wolf) and especially by the so-called *dependentistas* or dependency theorists.

But not only did many authors recently writing on Mexico, like Murphy and Stepick (1991), Krantz (1991) or Mallon (1995), break away from the "isolatedness" approach – they also started to regard a community as a heterogeneous field

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[16] Another interesting attempt to explain the impact the anthropologists' personal attitudes on their research-problems, worth mentioning here, is by Aya (1990). He tries to explain why most anthropologists studying peasant rebellions in the 1960s and 70s have seen these as upheavals against capitalism, although the facts do not prove it. The reason for this, Aya (1990:113) thinks, is that in the late 1960s and eary 1970s the prevailing sentiment among academic intellectuals was anti-capitalist and they thus saw peasants also motivated by the same sentiment.

of political interests and power relationships, contested identities and internal hierarchies. This change in theoretical approach where community becomes part of the world system, consequently also brought along the change in the choice of communities to be studied, until the *community* as a privileged unit of analysis was superseded altogether by the *socio-economic region* which eventually was placed within the framework of world capitalist system by proponents of *dependentismo* like Prebisch, Furtado, Cardoso, Quijano, dos Santos, Frank and others (Hewitt de Alcántara 1984:180). This applies to the whole of Latin America. The picture of the Mexican countryside in particular now began to take on the conceptual form of the *levels of interaction*[17], in which groups of people within villages were "seen" to be linked to others through various mechanisms in both rural and urban settings and the latter to still others at the apices of national and international networks of power. In anthropology this shift of perspective on social organisation of peasantry was led most notably by Barnes who called for the study of what he defined as "personalised networks" (Barnes 1954). Theoretical perspective having changed, anthropologists started to observe the reality differently, and, consequently, "see" different things. Influenced by Barnes, Wolf (1966) started seeing "coalitions" between peasants, and Foster (1967b), "dyadic contracts". Whereas before the communities had been regarded as having collective identities, now an individual stepped on the stage and life-histories acquired special importance and fell under closer scrutiny. In Mexican context, the life-history method was used most extensively in numerous ethnographies by Oscar Lewis (1959;1979), as well as Paul Friedrich (1986) and others.[18] Quite an exceptional in that context is the study by Fromm and Maccoby (1970) of the social character in a Mexican village.

As Hewitt de Alcántara (1984:181) interestingly proposes, this change in perspective can as plausibly be attributed to the change in the basic characteristics of the rural socio-economic and cultural field which anthropologists might observe in Mexico, as to the post-war facilitation of communication between European and American scientists and the consequent adoption of European concepts. The near-subsistence cultivators of the Mexican countryside were no more "peasantry" in the 1960s than they had been in the 1860s, but new concepts reaching Mexico from Europe, through the medium of European social scientists working in American universities (*e.g.* Wolf), European political refugees resident

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[17] This term has its origins in cultural ecology, within which the shift from community to global perspective was epitomised by Julian Steward's (1963:44) concept of "levels of socio-cultural integration". Steward, by taking a critical stance towards geographical localism, concluded that rural communities were being increasingly integrated into ever-wider spheres of interaction.

[18] The life-history method, although putting an individual in the centre of the stage and breaking away from the de-individualised approach to the communities, has, however, a serious default – the question of representativeness – which arises when the anthropologists claim that their informants represent an "average" or "typical" representative of particular social groups or culture.

in Mexico (*e.g.* Palerm), the experiences of Mexican and other Latin American students in European universities (*e.g.* Stavenhagen, González Casanova, Cardoso), as well as the appearance in Mexico of previously untranslated works of European social theorists, especially Marx, permitted them to be seen as such.

Conclusion

What can be learned from this brief account on Mexican ethnography? I have tried to argue that theoretical predispositions have an impact on anthropological practice. As Hewitt de Alcántara (1984:184) concludes, anthropologists in their placement into the field and perception of the surroundings were (and are) always influenced by particular schools and paradigms. They looked for material that supported their hypotheses. All this does not mean, of course, that contact with real sociocultural situations and empirical testing of hypotheses has had no impact on the paradigmatic picture, but one has to agree with Hewitt de Alcántara that the production of new kind of information in Mexican ethnography generally seems to have followed, rather than preceded, the paradigmatic change. This conclusion is interesting because it contradicts the Kuhnian view, at least in the context of anthropology, according to which paradigmatic change is ultimately driven by an increasing pressure of data on theory, and calls for a more serious reflection on the relevance of the Kuhnian model for social sciences. Social scientific paradigms seem to change for other reasons and sometimes the changes reflect the social structure of the disciplines rather than their subject matter. This, at the same time, is not necessarily harmful if the theories exist in juxtaposition to and argument with each other. In other words, we can conclude that in social sciences there are two fundamental dialectical processes at work at the same time – one between theory and theory and the other between theory and data.

There is no other way in anthropology to purge the pre-conceived assumptions and perhaps misleading theoretical dispositions about social reality than to do fieldwork open-mindedly and reflexively. Although this conclusion might sound almost like a contradiction in terms, the unwanted impact of theory on practice can be overcome only with the combination of a "better theory" and more extensive practice.

Bibliography

Agar, Michael (1980) *The Professional Stranger: An Informal Introduction to Ethnography*. New York: Academic Press.

Aguirre Beltrán, Gonzalo (1957) *El proceso de aculturación. Problemas científicos y filosóficos*. México, D. F.: Universidad Nacional Autónoma de México.

Aya, Rod (1990) *Rethinking Revolutions and Collective Violence: Studies in Concept, Theory, and Method*. Amsterdam: Het Spinhuis.

Bailey, Frederick George (1969) *Stratagems and Spoils: A Social Anthropology of Politics*. Oxford: Blackwell.

Barnes, J. A. (1954) "Class and Committees in a Norwegian Parish Island". *Human Relations* 7, 39–58.

Barth, Fredrik (1994) [1969] *Ethnic Groups and Boundaries: The Social Organisation of Culture Difference*. Oslo: Pensumtjeneste.

Beals, Ralph, Robert Redfield and Sol Tax (1943) "Anthropological Research Problems with Reference to the Contemporary Peoples of Mexico and Guatemala". *American Anthropologist* 45, pp. 1–21.

Bernard, H. Russell (1988) *Research methods in cultural anthropology*. Newbury Park, London: Sage.

Brenner, Michael, Peter Marsh and Marylin Brenner (1978) *The social contexts of method*. London: Croom Helm.

Cole, D. (1983) "The value of a person lies in his *Herzenbildung*: Franz Boas' Baffin Island letter-diary, 1883–1884". In *Observers observed*. G. W. Stocking, Jr. ed. Madison: University of Wisconsin Press, 13–52.

Comas, Juan (1964) *La Antropología Social Aplicada en México: Trayectoria y Antología*. México, D.F.: Insitituto Indigenista Interamericano.

Durkheim, Émile (1982) [1895] *The Rules of Sociological Methods and Selected Texts on Sociology and its Method* (transl. by W. D Halls; ed. and introduction by Steven Lukes). London: MacMillan.

Evans-Pritchard, E. E. (1951) *Social Anthropology*. London: Cohen & West.

Evans-Pritchard, E. E. (1976) [1937] *Some Reminiscences and Reflections on Fieldwork*. In *Witchcraft, Oracles, and Magic among the Azande*. abr. Eva Gillies, 240–254. Oxford: Clarendon.

Feyerabend, Paul (1979) *Against Method: Outline of an Anarchistic Theory of Knowledge*. London: Verso.

Fortes, Meyer (1969) *Kinship and the Social Order: The Legacy of Lewis Henry Morgan*. Chicago: Aldine Publishing Company.

Fortes, Meyer (1970) *Time and Social Structure and Other Essays*. University of London: The Athlone Press.

Foster, George (1967a) *Tzintzuntzan: Mexican Peasants in a Changing World*. Boston [Mass.]: Little, Brown.

Foster, George (1967b) "The Dyadic Contract: A Model for the Social Structure of a Mexican Peasant Village". In *Peasant Society: A Reader*. J. M. Potter, M. N. Diaz, G. M. Foster, eds. Boston [Mass.]: Little, Brown.

Friedrich, Paul (1986) *The Princes of Naranja: An Essay in Anthrohistorical Method*. Austin: University of Texas Press.

Fromm, Erich, and M. Maccoby (1970) *Social Character in a Mexican Village*. New Jersey.

Fry, Douglas P. (1992) "Respect for the Rights of Others is Peace": Learning Aggression versus Nonaggression among the Zapotec. *American Anthropologist* 94 (3), 621–639.

Gadamer, Hans Georg (1975) *Truth and Method*. London: Sheed and Ward,

Gamio, Manuel (1922) *La población del Valle de Teotihuacán: el medio en que se ha desarrollado, su evolución étnica y social, iniciativas para procurar su mejoramiento*. Mexico: Dirección de Talleres Gráficos.

Herskovits, Melville J. (1948) *Man and His Works: The Science of Cultural Anthropology*. New York: Knopf.

Hewitt de Alcántara, Cynthia (1984) *Anthropological perspectives on rural Mexico*. London: Routledge.

Howell, Signe, and Roy Willis (eds) (1989) *Societies at peace: anthropological perspectives*. London: Routledge.

Krantz, Lasse (1991) *Peasant Differentiation and Development: The Case of a Mexican Ejido*. Stockholm: Stockholm Studies of Social Anthropology.

Kuhn, Thomas S. (1962) *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press.

Lastrucci, C. L. (1963) *The Scientific Approach*. Cambridge, MA: Schenkman.

Leach, Edmund (1977) [1954] Political Systems of Highland Burma: A Study of Kachin Social Structure. London School of Economics monographs on Social Anthropology, no. 44. London: Athlone.

Lewis, Oscar (1959) *Five Families: Mexican Case Studies in the Culture of Poverty*. New York: Basic Books.

Lewis, Oscar (1963) [1951] *Life in a Mexican Village: Tepoztlán Restudied*. Urbana: University of Illinois Press.

Lewis, Oscar (1979) *The Children of Sanchez: Autobiography of a Mexican Family*. Harmondsworth: Penguin.

Malinowski, Bronislaw (1922) *Argonauts of the Western Pacific: An Account of Native Enterprise and Adventure in the Archipelagos of Melanesian New Guinea*. London: Routledge and Kegan Paul.

Malinowski, Bronislaw (1967) A Diary in the Strict Sense of the Term. New York: Harcourt, Brace & World.

Malinowski, Bronislaw, and Julio de la Fuente (1982) *Malinowski in Mexico: The economics of a Mexican market system*. London: Routledge and Kegan Paul.

Mallon, Florencia E. (1995) *Peasant and Nation: The Making of Postcolonial Mexico and Peru*. Berkeley: University of California Press,

Mead, Margaret (1943) *Coming of Age in Samoa: A Study of Adolescence and Aex in Primitive Societies*. Harmondsworth: Penguin.

Molina Enríquez, Andrés (1909) *Los grandes problemas nacionales*. México, D.F.: A. Carranza e Hijos.

Murphy, Arthur D., and Alex Stepick (1991) *Social Inequality in Oaxaca: A History of Resistance and Change*. Philadelphia: Temple University Press.

Parsons, Talcott (1968) [1937] *The Structure of Social Action: A Study in Social Theory with Special Reference to a Group of Recent European Writers*. New York: Free Press.

Pelto, Pertti J., and Gretel H. Pelto (1978) [1970] *Anthropological Research: The Structure of Inquiry*. Cambridge: Cambridge University Press.

Pimentel, Francisco (1864) *Memoria sobre las causas que han originado la situación actual de la raza indígena de México y los medios para remediarla*. México, D.F.: Andrade y Escalante.

Popper, Karl (1972) [1963] *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge and Kegan Paul

Radin, Paul (1966) [1933] The Method and Theory of Ethnology. New York: Basic Books.

Redfield, Robert (1941) *Folk Cultures of the Yucatan*. Chicago: University of Chicago Press.

Redfield, Robert, and Alfonso Villa Rojas (1962) *Chan Kom: A Maya Village*. Chicago: Chicago University Press.

Redfield, Robert (1962) *A Village That Chose Progress: Chan Kom Revisited*. Chicago: Chicago University Press.

Redfield, Robert (1973) [1930] *Tepoztlán: A Mexican Village*. Chicago: University of Chicago Press.

Sénz, Moisés (1936) *Carapan. Bosquejo de una experiencia*. Lima.

Schutz, Alfred (1962) *Collected Papers I: The Problem of Social Reality*. The Hague: Martinus Nijhoff.

Sponsel, Leslie E., and Thomas Gregor (1994) *The anthropology of peace and non-violence*. Boulder: L. Rienner.

Steward, Julian (1963) *Theory of Culture Change*. Urbana: University of Illinois Press.

Watson, James D. (1970) [1968] *The Double Helix: A Personal Account of the Discovery of the Structure of DNA*. Harmondsworth: Penguin.

Wolf, Eric R. (1955) Types of Latin American Peasantry: A Preliminary Discussion. *American Anthropologist* 57, 452–71.

Wolf, Eric R. (1966) *Peasants*. Englewood Cliffs: Prentice Hall

Wolf, Eric R. (1982) *Europe and the People Without History*. Berkeley: University of California Press.