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U N I V E R S I T Y

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Cher M. Sigaut,

Chapeau!!! (si vous permettez.)

Votre sens de l'anglais est exceptionnel. Vos corrections sont les bienvenus. Je n'osais pas trop m'éloigner de vos phrases et de votre style, mais vous pouviez et vous avez vous-même rendu le texte plus lisible.

La plupart des corrections, je les ai intégrées dans le texte. Certaines m'étaient problématiques, j'ai proposé d'autres formulations possibles, qui sont noircies.

En relisant le texte, j'ai également trouvé d'autres idées pour l'améliorer encore, que j'ai également noircies.

Je n'avais pas vérifié l'orthographe, je m'en excuse. Elle a été vérifiée automatiquement cette fois.

Vous m'avez envoyé la bibliographie. A votre sens, vaut-il que je la retape en anglais? Si c'est le cas, je le ferais après mon retour de Mexique (je pars demain, pour dix jours seulement).

J'envoie aujourd'hui même une disquette Macintosh avec ce texte dessus, à Madame Kouchner, à la Fondation Fyssen. Je vous prie de lui envoyer le texte présent, après avoir révisé ces dernières suggestions de ma part, et après avoir complété les (**Figure** ___).

Si jamais il reste des questions, j'accepte volontiers de les traiter. Le plus facile serait par E-mail, si vous en avez. Mon adresse est la suivante :

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Cordialement, et toujours a votre service,

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Organs and Instruments: the Place of the Human Body in Technical Action Schemas

François Sigaut

Anecdotes and invisible facts

"The only part of History I like is the anecdotes," declared Merimée at the beginning of his *Chronique du Règne de Charles IX* (1829). He immediately added, "I admit it, to my shame." I also admit, to my own shame, a very pronounced taste for anecdotes, that is, for events or facts which we find surprising or amusing without knowing why exactly. Anecdotes, strictly speaking, deal with events, but there are also what could be called anecdotal facts, which concern customs and mores. Such, for example, are the practical indications found in most tourist guidebooks on business hours, road traffic, food customs, etc. of foreign countries. The goal is to warn the visitor of the particularities which might surprise or embarrass him or her in the country to be visited.

It is precisely particularities and differences of this kind that establish ethnology as a comparative science of societies (Lévi-Strauss 1958: 19, 358). And there are good reasons to associate ethnology and tourism in the intellectual history of modern Europe. Both anthropologists (or ethnologists) and tourists go abroad in search of a sort of *dépaysement*. The difference lies in what they do with their discoveries once they return home. They both relate what they have seen, but to different kinds of public with different expectations. The travel book and the tourist guide are true literary genres, with their own rules and structure, or, if one prefers, their own paradigms, which belong to the art of traveling of each era.[1] Similarly, anthropology has its paradigms, which are the theories ending in "-ism" (functionalism, structuralism, postmodernism, etc.) that appear in the discipline's textbooks and field manuals. But one rarely finds in such books the particular reasons for the popularity or decline of the various paradigms, nor discussions of the reality and the limits of their influence, nor, above all, evaluations of their usefulness. Rather than true theories, in the scientific sense of the word, one wonders whether we are dealing with fashions, whose principal attraction is novelty (real or apparent); fashions are thus destined to fall out of fashion as soon as their novelty has worn off. It must be acknowledged, that, whether theories or fashions, these paradigms, which have successively occupied the center stage of ethnology over the last

[1] On the art of traveling (*ars apodemica*), see the works of J. Stagl (1995). I owe this reference to B. Rupp-Eisenreich.

century, have never taken into account anything other than a rather restricted part of the discipline. The rest of the field is usually relegated to the category of anecdote.

It was against the **dismissal** of facts as anecdotal that Mauss rebelled when, in 1934, in his presentation of "Body techniques" (*Les techniques du corps*) to the Society of Psychology, he spoke of the large number of odd facts which our ignorance forces us to class under the heading of "Miscellaneous". It is precisely to this point, however, that S. Levinson, in this volume, addresses his main criticism, when he criticizes Mauss for merely proposing a "butterfly collection". In this Levinson is not wrong: Mauss does indeed proceed much like a butterfly collector. However, why this method should be deemed illegitimate is not clear. Whether or not a collection of facts be sufficient, it is still necessary. In biology, for example, there would be no taxonomy and therefore no evolutionary theory without collections of plants and animals (including insects and butterflies). How, in any field, can one take into account facts for which no appropriate theory is available, unless it be by collecting? What is troubling is not that Mauss proceeded in this way more than sixty years ago, but that so little progress has been made since. For, contrary to Levinson's affirmations, technical facts (and not just body techniques) are not included in the "business as usual" of the various disciplines that study human behavior. The work of D. Efron (1941) on gestures in conversation opened a path of study that has been much followed ever since. Several contributions in this volume follow along this line, including that of Levinson himself. But we must be aware that this path does not lead to techniques; effective actions in tillage, harvesting, basketweaving, pottery, blacksmithing, hunting, fishing, warfare, etc. are almost never taken into account. This *de facto* exclusion radically contradicts the abstract principle according to which technical actions can in no way be distinguished from other forms of human actions. If this principle is valid, which I am entirely ready to accept, it is betrayed by the very people who pretend to defend it.

In order to make my purpose clear, I shall add two butterflies to Mauss' collection: two ways of using a knife which, while rather surprising to ordinary Europeans, are quite common in two broad regions of the world-- where they have gone practically unnoticed by anthropologists.

In Niger, butchers who sell cooked meat at markets use what can be called a "narrow knife" because of its shape. The blade is long (23 cm.) and extremely narrow (16 mm. wide) and has two cutting edges and a rounded tip. The ferrule and handle (20 cm. long) contribute to the impression of length. This knife is used to cut meat into pieces, but in a quite extraordinary way. The meat is held at the fingertips and is sliced by **the blade being drawn** between all the fingers successively --between thumb and index, then between index and middle, etc. (Figure 1). As far as I am aware, this way of using a knife, though quite common, has never been recorded in ethnographic literature.

In India, women use a sort of "knife" that they hold securely to the ground with one foot (figure 2). The instrument can be a sickle, which it is ordinarily called. But in most cases, the sickle's handle is replaced by a flat piece of iron with four supports on which the foot is placed and to which the blade is attached in such a way that it can be folded back when not in use. When opened, the cutting edge faces towards the user. She holds a food item with both hands and slices it on the knife's immobilized blade. To my knowledge, this utensil has attracted the attention of only two people. The first was the German mechanic Franz Reuleaux (1900: 674) who named it *Standmesser* ("standing knife"), and the second was the ethnologist Marie-Claude Mahias (1985: 180). And yet this way of cutting is common throughout India and beyond, and is thus shared by hundreds of millions of people.[2]

One could certainly find, throughout the world, other methods no less original for using knives. At the end of the last century, Otis T. Mason (1892, 1899) was interested in a few of them. But I am left with the impression that his research did not continue after him, even though the number of ethnological fieldworkers has increased tremendously, and most of them, when spoken to individually, admitted having been able to observe facts such as these in great detail. But the idea of publishing these observations never occurs to them, as though this kind of data were not worthy of scientific **documentation**. There lies the problem. I myself do not know what interest science might have in the two examples of knife use I have presented. But I am sure that if they are not studied, we shall never know. Such is the perverse effect of the imputation of being anecdotal. By ruling out the search for arguments by which it could be refuted, the imputation itself produces the conditions **for** its own justification. Facts **looked upon** as anecdotal become scientifically illegitimate, which is to say invisible. They no longer have even the value of simple "empirical" facts.[3]

However, it would not be particularly difficult to find significance in these supposedly anecdotal facts. In the study of body postures, for example, Gordon W. Hewes proposed five "levels of relevance" in an article which appeared twenty years after that of Mauss (Hewes 1955):

-industrial applications (taking into account postural habits in designing furniture and machinery);

-psychological and psychiatric implications;

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[2] The use of the standing knife has been observed in various locations in Indonesia (H. Conklin, personal communication) and in Burma (F. Robinne 1985). Regarding Reuleaux's travels in India, see B. Rupp-Eisenreich (1989). Reuleaux made extensive use of his observations from Asia in his final book on kinematic theory, published in 1900. In that work, he groups together three fixed tools specific to southern Asia: the standing knife, the **spike** used to open up coconuts (*épieu à écorcer*), and the coconut grater (Reuleaux 1900: 673-676).

[3] I usually refuse to talk about "empirical facts" in the current use of the term, for I feel this usage is misleading. Facts become facts only when they are observed and described, which presupposes two initial conditions. First, **knowing** how to observe and describe them, and, second, the motives to do so. These two conditions, one methodological, one problematical, are exactly what all theories are defined by. In other words, there is no such thing as a fact without a theory, that is, an empirical fact in the strict sense. As the examples presented here show, such facts are invisible: they are not really facts because no one considers them to be so.

-relations between posture and other social facts;

-history (changes in postural habits over time);

-phylogenesis (the role of postural changes in the evolution of human primates).

This list is not only valid for postures; it could easily be extended to all body techniques. And it would be possible **even now** to **put together** a short bibliography **for** each of these four headings. I am thinking in particular of works such as those by André G. Haudricourt and John Whiting.[4] But it must be acknowledged that these works have had relatively little **repercussion**. No more that those by Hewes and Mauss, they have not allowed the theme of body techniques to rid itself of its marginal status. The imputation of anecdotism weighs more heavily than ever, as Levinson's contribution in this volume clearly shows.

The situation could thus be summarized in the following manner:
(1) Body techniques are facts like any other, observable like any other (though, of course, they have their own special difficulties, none of which are insurmountable, however).
(2) Nevertheless, these facts are practically invisible, scientifically speaking; they do not belong to the "business as usual" of human sciences, because they are considered to be anecdotal.
(3) The imputation of anecdotism is circular. In order to ascertain whether any given fact is significant or not, it has to be first taken seriously. This, however, is precisely what the imputation of anecdotism renders impossible from the start.

The question thus becomes one of finding ways and means which would allow us to break out of this stalemate.

From classification to identification.

A first move out of this stalemate would be to look to classifications for a solution. So tried Mauss, and a few others after him. All in all, these attempts hardly produced convincing results. They yielded either simple inventories, which are useful in their own way but do not really permit any ordering of the facts according to a logic which would bring out new meanings. Or they involved more ambitious efforts, but the logic selected could eliminate neither double uses nor **gaps**, which shows that it was more or less arbitrary from the start.[5]

This failure does not mean that attempts at classification are without interest, or even that they are not necessary. **It is possible** that they stem from a fundamental need of the human mind

[4] See, for example, "Relations entre gestes habituels, forme des vêtements et manière de porter les charges" reprinted in A.-G. Haudricourt (1987, pp. 171-182), and "Environmental constraints on infant care practices", reprinted in E. Hollenberg-Chasdi (1994, pp. 107-134.)
[5] The essays of J. L. Pelosse (1956) and of J. Vignet-Zunz (1983) have the merit of bringing to light most of the *apories* which serve as obstacles in this direction, even though that was not the goal of the authors.

to distinguish and to identify as precisely as possible the objects to be conceptualized. But there seems to be here a confusion between identification and classification. For, even if the two operations are closely related, they are not the same. Identifying an object is being able to recognize it among other more or less similar objects, which presupposes the ability to recognize meaningful traits, whether by their presence or absence. Knowing what an object is, is also, and perhaps first of all, knowing what it is not. Identification thus necessarily implies comparisons. But comparison does not mean true classification: there is no need to be a master of insect taxonomy to be able to recognize an ant, a weevil, or a grasshopper. On the contrary, it is only after learning to identify a rather large number of such objects, both similar and different, that one feels the need to classify them. Moreover, elaborate classifications are as varied as the motives which determine them, and it is not certain that they can always be used as means of identification, especially when that is not their goal. Even scientific classifications are secondary elaborations, which suppose previous identifications. It often happens that progress in building taxonomies leads to revising the identification of certain species which had been poorly or only partially described. But once a species has been properly identified, it keeps its individuality throughout any further rearrangements of the taxonomical system. Whatever the successive avatars of bird classification, a duck will always be a duck, and a magpie a magpie. Which is perhaps this reason why, besides classifications strictly speaking, practical keys for species identification have been worked out for the use of non-specialists.

These remarks are extremely schematic. An extensive discussion of this subject would obviously go beyond the scope of this paper.[6] The suggestion I would like to make here is that if the way of classification has so far led nowhere, it is because those who have followed it have, in a way, put the cart before the horse. It is only possible to classify what has first been identified, and, for body techniques (to say nothing of techniques in general), identification remains a problem. Most of the examples cited by Mauss confirm this, though this was most likely involuntary on his part. Whether concerned with the gait of nurses in New York, or the way the English and the French used shovels, Mauss took the first step, which was to notice that there are indeed differences. But he could go no further, for he had no means to characterize those differences. For him, they were located in the gray area proposed by Vigarello in this volume with regard to sports practices. This means that the differences can be felt or perceived more or less clearly, according to what could be called the practiced or expert attention of each person. But the means to describe them with **any kind of precision** are still lacking. To use an analogy which is not without danger, I would say that in the area of body techniques, we are at the same place as we were in the study of language before the development of phonetics. It has always been known that the sounds

[6] The literature on classifications, especially in biology, is so abundant that I will refrain from citing any titles at all. I am rather surprised, however, that there is practically nothing regarding identification in the sense I have used here. Perhaps I have not looked in the right places. Otherwise, I am at a loss to explain it.

of any one language differ from the sounds of another. But since there was no standard phonetic reference, there was no way to determine the relevance of such differences. The very concept of phoneme did not exist, which, by the way, is the reason why the study of languages was limited to comparative and historical grammar.

The problem is obviously knowing to what one can apply the linguistics analogy. Could it be applied to postures, for example? Indeed, one can easily imagine using Hewes' inventory (1955), improved thanks to modern biomechanics, to develop a table of reference for all possible postures compatible with human anatomy. Confronted with the practices of a given social group, the table would allow us to identify the postural system of the group, which would be the equivalent of the phonological system of the group's language. A few recent studies have gone in this direction (Barbier-Le Deroff 1996, Kawada *et al.* 1994, Kawada, this volume) and have already led to the identification, if not of complete postural systems, at least of some elements of the systems.

The example I would like to develop here, since it is related to my own research on techniques used in harvesting grain, is that of hand movements. There is no available inventory for hand movements comparable to that of Hewes for postures. But there have been several attempts at classification (Napier 1993 [1980], Elliott and Connolly 1984, Marzke and Shackley 1986) which, in spite of inherent problems, provide us with a sound base to start from.

According to Napier, the seemingly unlimited number of hand movements is due more to the multiplicity of objects handled than to the form of the movements themselves. Considered alone, the movements can be reduced to two classes, prehensile and non-prehensile. With regard to non-prehensile movements, Napier merely cites a few examples, such as pushing, hitting, lifting, and striking, without exploring them further. As for prehensile movements, there are, according to him, four patterns. The two main ones are the precision grip and the power grip, and the two secondary ones are the hook grip and the scissor grip. This can be summarized in a very simple chart:

Classification of hand movements, based on J. Napier (1956)

Non-prehensile movements	*Prehensile movements (grips)*
for memory:	2 main patterns:
pushing, hitting,	-precision grip
lifting, striking, etc.	-power grip
	2 accessory patterns:
	-hook grip
	-scissor grip

Simplicity is a great advantage, and that is likely the reason why Napier's classification was so successful. Over the last forty years, it has been and remains a frequently cited reference. It should be stated that the grips included in the classification are quite familiar to us, and thus are easy to remember. The precision grip is carried out with the fingers, the power grip with the whole hand (when, of course, the size of the object allows it). The hook grip is the one we use to carry a heavy suitcase (the curved fingers bear the entire load, without the intervention of the palm). The scissor grip is used to hold a cigarette or a pencil between two fingers. But this extreme simplicity also has its drawbacks. It is difficult to be persuaded that these four patterns are sufficient to account for the multiplicity of real grips. Furthermore, why are the first two called "main patterns", and the second two only "accessory"? Why are the first two grips characterized according to function (precision and power) and the other two according to form (hook and scissor)? Finally, why were the non-prehensile movements deliberately neglected?[7]

Indeed, criticism was not long in coming. In 1962, Landsmeer (cited by Elliot and Connolly 1984) noted that the notion of prehension lacked consistency. He proposed a distinction between *gripping*, the action of firmly holding an object, and *handling*, the action of moving an object within the hand. In gripping, as Elliot would later state (1979), the movements of the hand-object unit are extrinsic; that is, they are controlled by the wrist or the arm, the hand making no movements on its own. In handling, the movements of the hand are intrinsic, in that the hand itself manipulates the object. Elliot and Connolly (1984) devoted their study to intrinsic movements, excluding all others.

Limiting the field of study in this way is not without certain advantages. Elliot and Connolly's study was the first to bring clearly to light the great diversity in handling-type movements, and in this showed definite progress compared to Napier's initial work. However, it also has a few problems of its own, two of which will be touched upon here.

The first is that the distinction between gripping and handling is not as absolute as it is said to be. For to handle an object, one must first hold it. Handling thus becomes either the result of a series of different grips--the "sequential movements" of Elliot--or, more commonly, the result of a kind of partial grip. To thread a needle, one pinches the thread between the thumb and the index finger (**Figure** __), and the thread held in this way has a certain degree of mobility in relation to the rest of the hand. But the thread is immobilized between the distal phalanxes of the thumb and the index finger, exactly as the handle of a bill-hook is immobilized in the entire hand. The difference is in the number and the positions of the joints involved. In the case of complete gripping, all the joints of the hand are immobilized around the object, and the only remaining

[7] After insisting that "attempts at classifying grasp have always been inadequate", P. Rabischong still found it useful to define five basic grasps: full hand, pinching, lateral (between the thumb and the side of a finger), hook, and scissor (Rabischong 1993: 72).

possibility for movement is from the wrist. In the case of "partial gripping" (for lack of a better term), only some of the joints of the hand firmly hold the object, leaving the possibility for moving it within the hand. But, for all that, the object is still being gripped. It thus appears that handling and gripping are not located at the same level of analysis. They are not two equivalent and exclusive "either-or" classes into which all prehensile movements would fit. Rather, gripping is the realization of the hand-object coupling, a coupling which can take several forms but is always static, by definition. Handling, then, consists of either a series of different grips or a partial grip; but it always involves some kind of gripping. In other words, handling is made up of grips, in the same way (to return to the comparison with language) that the word is made up of phonemes.

The second problem with the study of Elliot and Connolly concerns the corpus they used, or rather the lack of one. For, like Napier before them, they worked only with hand movements that were familiar to them, believing that this corpus was extensive enough to represent all "natural" human hand movements. This, of course, was not the case, as was also the conclusion of Mary W. Marzke and Steven Shackley (1987) when they were confronted with the hand movements of a small group of archeologists (F. Bordes, D. Crabtree, J. Tixier and S. Shackley himself) who were trying to reconstruct experimentally the prehistoric techniques used in the manufacture of stone tools. Marzke and Shackley found Elliot and Connolly's classification useless for their purposes. Furthermore, they did not believe that a single general classification is possible, whatever its theoretical advantages. According to them, it is preferable to use different types of classification for different research objectives.

Though the classification of Marzke and Shackley may not be safe from criticism, it is, in my opinion, by far the best available today. Its validity goes far beyond the study of the manufacture of prehistoric tools for which it was designed. And if this is so, it is precisely because, by rejecting the higher objective of a general classification and focusing instead on a limited corpus of observed movements, the authors remained closer to reality. Without trying to be overly categorical, I would venture to say that they moved from a logic of classification to one of identification.

At this point, three lessons emerge, which can be stated as follows:

1) The goal is indeed identification; classifications, at best, are only means (or, perhaps, secondary goals).

2) An essential condition is to distinguish clearly the level of analysis at which a given study is situated (*cf.* the example of gripping and handling).

3) Studies must be based on corpuses of observed movements under specific conditions, and not simply on familiar movements or those gathered randomly.

Levels of Analysis

The problem of levels of analysis is probably one of the most general ones in biology and the human sciences. It is in fact a problem of levels of organization. The fundamental concepts were proposed twenty years ago by Jacques Paillard (1976, 1986). The starting point is the banal observation that "living organizations are composed of a hierarchized series of interlocking sub-structures organized in systemic units." In other words, each unit (system) necessarily belongs to a specific level of organization, but is composed of units of a lower level (sub-systems), and each unit is in turn part of higher-level units (super-systems). These three levels correspond to three descriptive points of view which can be defined by three concepts: structure, functioning [*fonctionnement*], and function.

-The concept of structure answers the question "what is it?" or "what is it made of?" It leads to a description of components (organs, parts, elements, etc.), of the arrangement of parts (assemblages, articulations, etc.), and of the exterior form resulting from the arrangement.

-The concept of functioning answers the question "how does it work?" It leads to a description in terms of processes or mechanisms, of relations or internal exchanges between the components of a given unit.

-The concept of function, finally, answers the question, "what good is it?" or "what is it used for?" It leads to a description in terms of results or production, of finality, or, more generally, of external exchanges. The function of a unit is to participate in the functioning of the higher-level unit to which it belongs.

The concept of structure is simple, practically intuitive, and thus needs no special explanation. But that is not quite the case for the concepts of functioning and function, not because they are especially complicated, but because we all have a strong, spontaneous inclination to confuse them. This is a point I have already tried to make through the example of the knife (Sigaut 1991). A knife is not used for cutting, in the sense that the cutting action in general belongs neither to function nor to functioning. For it to be a function, it would be necessary to state exactly who cuts what, in what circumstances, to do what, and for whom. As for functioning, there are a thousand and one completely different ways to cut something, and, moreover, a knife is used for several other things besides cutting. The cutting action is merely an empirical, abstract category, which, of course, is useful in everyday language, but is of little use for the needs of a rigorous analysis.

It is more or less the same for the action of grasping. It has been shown how the distinction introduced by Napier between prehensile and non-prehensile hand movements was criticized by his successors, even though the latter could not do without it. It should now be obvious why the

distinction is not valid. Like cutting, the action of grasping in general is only an empirical category. With regard to functioning, the relevant distinction, at least **in the first analysis**, is the one already made between gripping and handling. With regard to function, things are more difficult because we run into the problem of the irreducible diversity of cultural behaviors. Nevertheless, and still **in the first analysis**, it seems possible to propose a categorization of hand functions a bit less abstract than those used up to now. The proposed categorization is as follows:

-*locomotion*: the hand rests on or hooks onto objects susceptible of helping the body move or support itself (for example, climbing, gymnastics, acrobatics, etc.);

-manipulation: the hand displaces objects or detaches them from their support (for example, picking up, gathering, moving everyday objects, etc.)

-throwing (a special case of manipulation, the importance of which justifies its being mentioned separately);

-direct **fashioning**: the hand acts on an object to change its form or arrangement (for example, breaking, tearing, kneading, molding, folding, weaving, threading, knotting, etc.);

-indirect **fashioning**: the hand acts on an object (worked material) through the intermediary of a tool;

-touching: pressing, hitting, palpating, stroking, caressing, etc.

-making signs (including mime, certain dances, etc.).

This categorization is worth what it is worth. I am perfectly aware that it would be quite easy to find examples which do not fit satisfactorily, so I will not make a strong effort to defend it. But if I have decided to propose it anyway, it is as a first step out of the use of vague, abstract notions such as prehension. It is also because, if it becomes possible to build corpuses of distinctive traits of hand movements, it will be on the basis of well-identified functions, which in turn will require a veritable categorization of hand functions, whether it be the one outlined here or another, better one.

Thus ends my explanation of the role of the concepts of functioning and function in the analysis of hand movements. As for the concept of structure, I stated above that it is too simple and intuitive to need further comments. But that is only true at first look, and a few details might be useful here also.

It is necessary to distinguish two different points of view with regard to the structure of the hand: anatomy and mechanics. The anatomical point of view is centered on the hand itself, and, moreover, on the hand only; the object, **if there is one**, is merely an accessory or a constraint, allowing the hand to take such or such a position, or obliging it to move in such or such a way.

From a mechanical point of view, on the other hand, the distinctive unit is the hand-object **pair**; here, the absence of an object is merely one extreme case of no particular interest, "case zero", one could even say. In studies of the hand, the point of view of anatomy has always been most frequently represented, and by far.[8] Only Marzke and Shackley (1986) veered away from it to some degree, though neither did they adopt a clearly mechanical point of view.

There is no denying the importance of anatomy (which is inseparable from physiology) in studies of hand movements; it is obviously crucial. However, it so happens that anatomy alone will not allow us to identify hand movements, because identification requires a concept that is foreign to anatomy, relevance. From a strictly anatomical point of view, the number of possible hand positions between maximal retraction (the closed fist) and maximal extension (the hand open, with fingers stretched out to the maximum) is unlimited, as is the number of movements carried out by the hand as it changes from one position to another. These positions and movements make up an indefinite continuum; only functioning requirements--usually implying the presence of an object--cause certain forms to be individualized rather than others.

Once again, a comparison with language could be of help. In humans, the speech organs are at least as complex as the hands. Anatomically speaking, the sounds they produce are, like hand movements, unlimited in number, because they constitute an indefinite continuum of notes, tones, duration, etc. However, in this continuum, each spoken language individualizes an average of only fifty "sounds", or phonemes.[9] The logic of this individualization is purely linguistic, meaning that it depends only on the need for mutual comprehension. The phonemes must be immediately identifiable by all speakers of any given language. This implies that they must not be too numerous, and they must also form a coherent system of distinctive traits; but there must also be enough of them to allow for an unlimited number of words to be formed by combining them. These are the only determinants which intervene in generating phonemes in a language--besides, of course, **each language's history**. In this sense (and only in this sense), the selection of phonemes made by each language from the unlimited continuum of possible sounds can be said to be arbitrary. Now, this arbitrariness is precisely what is lacking in hand movements (except, to some extent, for signs). For the individualization of hand movements is not an inherent necessity, as it is for phonemes; it is only a means to mark the most efficient movements. As soon as the hand-object relation is involved in an action, the relation only becomes significant as the means **to an end** which is necessarily mechanical.

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[8] The contribution made by anatomists is all the more important in that it also concerns the hands of primates in comparison with the human hand. As for mechanical technicians, they often work towards producing prostheses which function according to principles completely different from those governing the hand itself (Rabischong 1993).

[9] The number of phonemes varies, as we know, from slightly under 30 to over 60. In some languages, tones also have phonological value.

In other words, in technical actions, the logic of the individualization of hand movements -- and of body movements in general-- functions contrariwise to the logic which governs language. Hand movements must first be effective, and it is because of their recognized efficacy that they can be individualized, for example, in order to be taught or valorized in one way or another: efficacy causes relevance. In speech, on the other hand, the phonemes of a language cannot be said to be effective in the physical sense. Phonemes are imposed by usage, the only sense in which they can be said to be effective is that their common use by speakers of a language allows those speakers to understand each other: relevance causes efficacy.

That said, we should be aware that efficacy is not the same in both cases. A proper discussion of the notion of efficacy would lead us too far away from our present subject, but the difference between physical efficacy, obtained in techniques, and the efficacy of convention obtained in language (not to mention the efficacy of belief, characteristic of rituals) is too important to pass over in silence.[10] The fact that, in technical actions, efficacy is physical--that is, perceived by the senses [*sensible*]--is obvious. The tool is held firmly in the hand or slips, woods more or less resist the saw, fire burns or goes out, dough is rolled easily or sticks to the fingers, a blow lands or misses, etc. In such examples, no social conventions or beliefs come between the action and its result. And if indeed convention or belief does intervene, it does so before the act, in the social transmission of action modes, or after the act, in the interpretation of success or failure.

In my opinion, this is what Mauss had in mind when he said that the technical act was "felt by the actor to be of a mechanical, physical, or physico-chemical order..." (Mauss 1950: 372). The phrasing is rather awkward, and has led to accusations of ethnocentrism (Levinson, this volume). It seems that there is a misunderstanding here. The true question is whether or not there is only one form of efficacy for all human actions. If we choose to answer in the affirmative, we must conclude that this efficacy has to do with social consensus, and we end up with the absolute cultural relativism that characterizes what is called post-modern anthropology (see, for example, the critical article of Carneiro 1995). As for me, the answer must be negative, for in all cultures there exist forms of action whose efficacy depends exclusively on practical [*sensible*] experience, and which are recognized as such.

By "recognized as such", I obviously do not mean the explicit formulation of the problem in Western philosophical terms, but the simple acceptance of things obvious. For example, a nail is

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[10] I have borrowed the distinction between these three types of efficacy--physical or material, of belief, and of convention--from "L'esquisse d'une théorie générale de la magie" by Hubert and Mauss (1950: 11-12) published in 1903. This distinction by no means excludes the combining of different types of efficacy in real activities. The efficacy of convention in courts of justice is frequently reinforced by oaths and sacrifices which also involve efficacy of belief. And medicine is the field *par excellence* in which physical efficacy and the efficacy of belief are almost always inextricably intertwined. But the fact that these different types of efficacy can combine with each other in no wise invalidates the principle of their distinction. They could perhaps be joined by a fourth type: psychological efficacy (seduction, intimidation, etc.), resulting directly from the manifestation of our emotions based on the behavior of others (and vice-versa).

driven by a hammer's blows, or a light turns on when the switch is flipped. Defined by the evidence of its effects, technical action thus exists as a relevant category in all societies and in all cultures, whether or not this category is conceptualized (and in general it is not). This, at least, we can admit, as long as there is no evidence to the contrary, that is, as long as we do not discover some culture in which no perceived [*sensible*] effect, however commonplace or ordinary, could be accepted as a matter of course. Such a hypothesis seems too unbelievable to be taken seriously. This is the reason why the concept of technique seems universally applicable. It can be applied even beyond the human species. For animals also have sensory skills which allow them to modulate their actions according to the effects they bring about.

Understood in this way, techniques are what correspond, in the domain of actions, to the "affordances" as introduced by Gibson (1979) in the domain of objects.[11] There is, moreover, a remarkable similarity between Gibson's "direct perception of affordances" and the technical act "felt [...] as being of a mechanical, physical, or physico-chemical order" of Mauss. For, in spite of the different **wording**, it seems that the two **phrases** express the same fundamental idea, that among the effects of our actions, some are immediately perceptible and do not depend on beliefs or social conventions. There is here a line that cultural relativism cannot cross without becoming a veritable collective solipsism.[12] On the contrary, it is on the model of physically effective action that our beliefs, if not our conventions, are built. And if they are at all solid, it is because we evaluate them according to the same criteria of efficacy that we constantly experience in our relations with the objects that surround us.

After such a lengthy digression, let us return to hand movements. Some of them are simple signs, and thus deal more with conventions and belief than with physical efficacy; they shall be passed over. But as soon as hand movements involve objects, the relevant unit is no longer the hand alone, with its unlimited freedom to carry out all movements which are anatomically possible. The unit to take into account now becomes the hand-object unit, of which the different forms must be identified. And the criteria for this identification is mechanical, because it is the mechanical efficacy that, for the agent, makes a particular manner of handling an object meaningful.

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[11] Ideas similar to those of Gibson can be found in the work of Jakob von Uexküll (1864-1944). The concept of affordance is not made explicit, but it is present in the form of the twin concepts of ***Wirkmal*** and ***Merkmal*** (Uexküll [1928] 1973: 158).

[12] With respect to this idea, I have spoken on cultural alienation (Sigaut 1990). This is a situation in which a social group is so strongly attached to its beliefs and conventions that it cuts itself off from reality. This pathology is quite frequent in benign forms, but it can also take a much more serious form, with possibly tragic consequences. The error of so-called " post-modern " relativism is that it ignores these consequences in its professed refusal to judge a culture according to criteria which do not belong to that very same culture. It was in a reaction against such a form of relativism (at the time one spoke of "social perception") that Gibson developed his theory of the direct perception of affordances (Costall & Still, 1989). If, then, Mauss is accused of ethnocentrism, Gibson must also be so accused. For a critique of relativism from an epistemological perspective, see also Sperber 1982, pp. 49-85.

A corpus of grips: archery

We have already seen the need to work with corpuses of well-established facts. The only recent attempt in this direction is that of Marzke and Shackley (1986). But these authors only published their classification and not the corpus itself; it therefore remains difficult to evaluate its consistency. In any case, this one example would not be sufficient; others must be sought.

As a matter of fact, we do not have much choice, for there are very few, if any, published corpuses of hand movements. The examination of existing technical and ethnographic literature, iconography, and cinematography could provide examples, at least in domains **such as** basketry, spinning, ceramics, breadmaking, playing musical instruments, etc.[13] But this would require lengthy research with no guaranteed results.

Fortunately, there is at least one exception, that of archery. During the sequence of movements ending when the arrow is released, there is a very decisive moment, before the shot itself: bending the bow while maintaining the arrow in contact with the taut string. By last century, several solutions to this problem had been observed (Morse 1885). Today, five such solutions are recognized (**Figure ___**). The first two are easily executed, but are inadequate for powerful bows; they are used by children and novices in regions where archery does not hold much importance. The other three solutions are more professional, so to speak; they are found among populations for whom archery plays an essential role, be it for hunting, for war, or for sport (Hamilton 1982).

We have seen that in his classification of prehensile hand movements Napier distinguished two secondary grips, hook and scissor. However, these two grips are clearly used in combination in the Mediterranean and Eskimo methods of bending the bow: the bowstring is held by three fingers (index, middle and ring) curved into a hook, while at the same time the arrow is held in place, scissor-like, between the index and middle fingers (**Figure____, no. 4**).

The interest of this observation lies in the possibilities for generalization it offers. I have already stated that it is necessary to distinguish *grips* and *movements*. By definition, grips are maintained in a fixed position, for they are used to immobilize the object, even if immobilization lasts only a fraction of a second. Movements, in turn, **shift the object** from one grip to another. Now we can see that within the category of grips, we must further distinguish simple and combined grips. Archery offers typical examples of combined grips, in which two simple grips are used simultaneously. And it seems probable that by incorporating these two distinctions, between grips and handling, and between simple and combined grips, the classification of Elliot and Connolly could be both simplified and broadened.

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[13] For an example of an inventory of movements used in kneading [*pétrissage*] in a North African village, see F. Abel (1984).

We can also now fully appreciate the importance of the two grips that Napier considered accessory. It is obvious that they are not accessory at all, and that, in actual use, they are just as important as any other. It is also evident that it is less a matter of two grips as of two sets of elementary grips. Seen in this way, there are just as many scissor grips as there are possible lateral conjunctions between fingers other than the thumb (index-middle, middle-ring, ring-little). Moreover, the fingers can be straight or more or less bent (such as in the case of mode no. 4 in releasing the arrow), and the object can be held at different points between the fingers, from the base to the fingertip. There are thus a rather large number of possible positions. The generalization could be taken even farther, by considering that scissor grips are only one particular type of grip which could be defined by the existence of two **more or less diametrically opposed** support points for the object, which would suppose that the objects are of small size and relatively light, and that they will not be involved in very heavy efforts.

In the same way, hook grips are by no means limited to the single case taken into account by Napier, in which all the fingers except the thumb work together. Let us return to the example of the suitcase. If it is light, one could, just for fun, hold it with only two fingers, or even one. If, on the other hand, it is a heavy suitcase, it must be carried with all four fingers (even though the little finger, likely in a painfully squeezed position, may not be of much use). Obviously, the hook grip remains **such regardless of how many fingers are used**; there is thus an enormous number of possible grips involving each individual finger or any combination thereof. The thumb can also be used for a hook grip, as is shown in the Mongolian example of releasing an arrow (**Figure ___, no. 5**). The characteristic that all hook grips share is that the object, whatever it be, is not immobilized in the hand between two or more support points which are opposed to each other. Instead, there is only one support point (a surface, in fact), against which the object is immobilized by an exterior force (the weight of the suitcase, the tension of the bowstring, etc.).

In hook grips, in other words, the object is not contained in the hand in the strict sense, it is only retained or supported--unless, conversely, it is the body itself that is retained or supported in this way to a fixed object. To that extent, hook grips may be said to be *half grips* rather than true, full grips. At this point, the analytical concepts of F. Reuleaux (1875), can be put to use: the object and the hand are considered as a pair of elements, **and, in grips,** the kinematic chain composed of all the body parts and objects participating in the movement must be closed. Thus, true grips (two or more support points) would correspond to what Reuleaux called a kinematic closure; that is, the object is immobilized between several points which prevent its movement in **any** possible direction. Kinematic closure can be described in purely geometrical terms; it depends on no force external the hand-object pair. In half grips, on the other hand, there is enclosure by force; in other words, an outside force is necessary to maintain contact between the object and the single support point or surface provided by the hand.

On these bases, it would be relatively easy to develop a general method for identifying half grips. Two series of criteria are sufficient:

-the direction of force, which can be exercised towards the body (pulling, attracting, retaining), away from the body (pushing, repelling, leaning on), or in a direction more or less parallel to the body (lifting).

-the part of the hand(s) that serves as support point, which could be the palm, one or several solidary fingers, or the entire hand (or even both hands together).

By crossing these two sets of criteria, a two-tiered table is obtained:

Identification Table for Half Grips (or grips with enclosure by force)			
direction of force	part of the hand producing the force		
	palm	fingers	entire hand
towards the body (traction)	?	1	?
away from the body (pushing)	2	3	4
roughly parallel to the body			5

The grips for which question marks appear are theoretically possible, but they are probably too uncomfortable to be used frequently, and I am not aware of any well-documented examples. In box 1 are found hook grips, in Napier's sense, which, as we have seen, can involve any of the fingers separately or together, and even the thumb. In box 2 are all types of pushing by the palm(s), for example, as when we lean on a windowsill. Pushing with one or several fingers would correspond to box 3; this is what we do each time we push a button or a switch or use a keyboard. There are as many different possibilities as there are fingers and parts of fingers which could be used to push something (for example, the tip, pad, back, or side). Finally, the pushing may be carried out by the entire hand (box 4), which would also include many possibilities according to which side is used, although most of the pushing which could be imagined in this manner would probably not be of much practical use.

There remain the grips of box 5, which are used to carry an object without moving it closer to or farther from the body. The characteristic example is that of the *jointée*: both hands are joined together to form a horizontal cup, to dip water, to pick up sand or loose soil, etc. The *jointée* is perhaps one of the most ancient and most "natural" measures for grain (**Gardette 19__**). There are other possible examples, such as the way waiters balance and carry on one hand a tray full of drinks. In all these cases, it seems that the entire hand is involved, and that there is no point in distinguishing grips using only the palm or grips using only the fingers.

Not only do the grips thus identified have kinematic significance (in Reuleaux's sense), but they--at least some of them--also have symbolic significance. Gestures mimicking hook grips, for example, often express greed or avarice. Examples from boxes 2 through 4 could express rejection or hostility. Finally, box 5 would include gestures symbolizing offering or prayer. It should be added that *jointée* is probably spontaneous and universal among humans, whereas it seems lacking in apes. Whether it does not exist or has not been observed is not known, which again brings up the invisibility of this type of fact.

Of all hand movements, half grips thus represent a particular case which can in fact be analyzed quite easily. Indeed, all that was needed was to complete the elements already available (from Napier for the most part) with a complete, descriptive corpus, such as that of archery, and to use the kinematic concepts of Reuleaux. Would it be just as easy to apply this approach generally to other categories of hand grips? I have no answer for the moment, but I cannot think of any other direction in which a solution may be found. For the majority of hand movements will remain invisible to us unless we learn to describe them, or, in other words, to identify them.[14]

Between organs and instruments: the prosthesis?

Archery has provided us with an interesting corpus of hand grips, but is relatively limited in the number of grips, and is quite specialized. Except when practiced by children, archery requires a lengthy learning period and constant training--not to mention the manufacture of the bow and arrow, which itself is extremely complicated. The techniques involved are undoubtedly among the most elaborate to be found among non-industrial peoples. So it would be useful to turn to activities for which the techniques are simpler.

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[14] The term *jointée* ("joined hands") cannot be found in most French dictionaries, but this is just one of many examples of the ignorance of lexicographers in the domain of popular expressions. And moreover, the term *jointée* is just one of several used on French territory, the dialects of southern France having others (see Gardette 1967, I: map 108, and 1976, V: 75). The concept of *jointée* probably exists in most languages. It exists, for example in Aymara: *p"uxtuña*, the action of carrying with both hands joined together to form a cup (following Tate 1951: 70; I am grateful to H. Meininger for this reference).

Grain harvest, especially that of cereals, presents in this respect a situation of contrasts. On one hand, it can involve very elaborate techniques. The scythe, for example, is the product of a long and complex manifacturing process, and **to use it** requires a long apprenticeship. On the other hand, there are numerous other procedures for harvesting grain, some of which seem quite elementary, in that the time needed to learn them is short, almost instantaneous (for adulta). It is possible to suggest that these are cases of simple, non-specialized skills.

Some twenty years ago, I developed a general table for identifying grain harvest techniques (Sigaut 1978), which is reproduced here in a slightly modified version (Sigaut 1991). The validity of the chart can be assumed in that, so far, it has accommodated all documented harvest methods. The chart is also exhaustive, at least potentially. For though it is not based on an exhaustive inventory of all possible practices, it allows them to be reconstructed without difficulty.

Table for Identifying Grain Harvest Techniques

physical action involved	part of plant harvested				
	whole plants	grains, *épillets*	ears *panicules*	handfuls of stalks	bunches of stalks
Pulling out (*arracher, ausreissen*)	1				
Picking up (*ramasser, auslesen*)		2			
Beating (*battre, ausschlagen*)		3			
Stripping off (*érusser, abstreifen*)		4	5		
Breaking off (*briser, abbrechen*)			6		
Cutting (by pressure)			7		
Cutting (by friction)				8	
Cutting (by friction, *outil lancé*)				8'	9

Even a summarized presentation of the contents of the chart would go beyond the scope of this paper. Nevertheless, it is necessary to say that each numbered case represents not a single technique, but a group of related techniques, which make up what could be called a series. Harvesting with a scythe, for example, belongs to series no. 9, harvesting with a sickle belongs to series no. 8, etc.

The techniques in the upper part of the table (series 1, 2, 4, and 6) can be practiced without any tools, except for the receptacles needed **for** the product of the harvest. Usually, the hand directly seizes the part of the plant to be harvested, and if the methods of grasping are different, it is simply because the objects to be seized differ in volume, form, attachment to their support, etc. In harvesting by pulling out, for example, the stalks are seized with the entire hand (no. 1). In harvesting by stripping off, the fingers pinch the stem and strip it of grain in an upward motion, while collecting the grain in the hand (no. 4). This procedure is found in rice harvesting in Southeast Asia, and in harvesting wild grasses, especially in Australia.

Conversely, the techniques at the bottom of the table (nos. 8, 8', and 9) involve no direct contact between the hand that holds the tool (scythe, sickle) and the object. If there is contact, it is with the other hand (usually the left hand). **In reaping with a sickle, for example,** the left hand holds the stalks which are then cut by the sickle held by the right hand.

From the point of view taken here, the most interesting techniques are those which fall in the middle of the chart (nos. 3, 5, and 7), for the patterns of action involved are not related to the two preceding groups. There is no longer simple manual prehension, because there is a tool. But the tool does not yet have a pattern of action of its own, as cutting tools typically do (scythes, sickles, bill-hooks, machetes, etc.). Instead, the tool imitates the hand; it assists the hand, protects it, or increases its efficiency, but it does not differentiate itself from the hand, in the sense that it has no physical effects on the object that are fundamentally different from those of the bare hand.

The seed-beater of California (series 3) and the *mesorias* of Asturia (series 5) are good examples of tools which imitate the hand's action. Unfortunately, we have no precise descriptions of how the seed-beater is handled; we only know that it was used to beat the ears of wild grasses so that the mature grain fell into a basket held by the other hand (**Figure** ___). As for the *mesorias*, they are still in use today to harvest spelt, a particular variety of wheat (*Triticum aestivum* ssp. *spelta*) characterized by the fragility of the mature ears of grain. *Mesorias* are two sticks, 50 to 60 cm long, between which several stalks are squeezed; they are then pulled upward in a way that strips the ears from the stalks. The ears, still clustered together, are then taken by the other hand and dropped into a large basket dragged along by the harvester.[15]

[15] I was able to observe the harvest of spelt at Zureda (Asturia) in September 1979, in the company of a Colombian colleague, Francesco Ortiz, who filmed the main sequences (cf. Ortiz & Sigaut 1980 and Sigaut 1993).

Like all tools with handles, *mesorias* and seed-beaters are held by the whole hand. They only differ from other tools in that they are used to imitate, or, more exactly, to reproduce, certain hand movements in a way that makes **the work** a little easier, faster, or less arduous than with the bare hand. This is the reason why they can be qualified as prostheses, even though, as we shall see, this notion is not without difficulties. The clearest example of a tool-prosthesis known to me is one that is not used for grain harvest. It is a pair of half-ladles, used in winnowing grain in ancient Egypt (**Figure __**); they were used to reproduce the same movement as the *jointée* discussed earlier. The example is not unique. A almost identical tool in Ethiopia was described a few decades ago (Mengesha and Lee 1960: 16). And up until the beginning of the 20th century in the salt marshes along the Atlantic coast of France, two small boards were used in the same manner to put harvested salt into sacks (Lemonnier 1980: 107, 117).

Another example of tool-prosthesis appears in series 7: the harvesting knife used in Indonesia called *ani-ani*, or rice knife. Tools of this type could be considered as aids in picking (*cueillette*), in that they are not held in the fist, as are the previously mentioned tools. Indeed, the same hand that holds the tool also seizes the ear to be picked, **so the tool must be held in a way that** leaves the hand enough freedom to seize the ear. Once the ear is seized, it must be broken or cut from the stalk, and this is where the tool comes in.

To make things clearer, we can first look at simple picking, which is carried out without tools (series 6). This method was typically used for harvesting corn before the invention of the corn-picker; it is remarkable that in precolombian America there were no harvesting tools comparable to those in the Old World. This is probably explained by the rather particular morphology of corn. For cereals other than corn, simple picking is possible, but is only occasionally found, for it is more difficult to separate the ears from the stalk with a simple hand movement. Something else is needed to break or cut them off.

That something could be a fingernail, as was observed by H. Conklin (**1957: __**) among the Hanunóo in the Philippines. In some regions of Indonesia, a sort of thimble is used in place of the fingernail; a small bamboo or iron cylinder with a sharp cutting edge at the tip is worn on one finger (*Erntering*, Fisher 1937). The harvesting thimble is a protective device, as is the sewing thimble which it resembles somewhat, but it is also a tool, and one which **only slightly** modifies the hand movements of the user.

As for harvesting knives, two main forms are known. The first is the "ordinary" (for us) knife, which resembles any small kitchen or pocket knife, a fact which unfortunately causes it to go unnoticed. The second is the special Indonesian harvesting knife, the *ani-ani*, whose curious (again, for us) morphology, on the contrary, attracted the attention of numerous European observers in the early twentieth century (Fischer 1937, 1939). The *ani-ani* has a very specific geography. It is found today in two regions of the world, and in those two regions only:

Southeast Asia (the Austronesian archipelago, and a few places on the mainland in the same longitudes), and the Sahel and Sudan regions of Africa, from the Nile River to the Atlantic. **However,** we know practically nothing about the geography of the ordinary knife. Fischer says only a few words about it. Takei (1995) noticed it in the southernmost of the Ryukyu Islands, and I myself observed it in France (in the Vendée) for harvesting millet (Sigaut 1993, 1995) and in southern Ivory Coast for harvesting rice. But these examples are not enough to suggest a hypothesis of general spatial distribution.

It would seem that these two forms of knives are associated with two rather different hand movements.

As far as we know, the ordinary knife, when used for grain harvesting, is held between the four fingers and the palm, the blade pressed against the radial side of the curved index, the thumb remaining free. The stalk is seized between the thumb and the blade, against which it is half broken, half cut by a small rotation of the hand.

It is more difficult to describe the method in which the harvesting knife (*ani-ani*) is used, probably not because it is more complicated, but because, **since the method is so unfamiliar to us,** available descriptions are insufficient. Let us recall that the *ani-ani* has no handle; only the Asian models have a characteristic transversal hilt . The blade is held between two fingers (the middle and ring fingers, for example), which implies that the way the stalk is grasped must be quite different than when the ordinary knife is used. Given the information available today, it is difficult to be more precise. It can nevertheless be supposed that gestures vary greatly from one region to another, if the morphological diversity of the knives themselves is taken into account (**Figure __**).

Let us summarize. This brief review of techniques for harvesting grain has allowed them to be grouped into four categories, from the point of view of the respective roles of the hand and of the tool. In the first two categories, the hand acts directly on the object; these are:

1) techniques with the bare hand, unaided by a tool: pulling out, picking up, beating, stripping, picking.

2) techniques with the assisted hand; picking, with the help of accessories which facilitate the action of the hand without fundamentally changing its movement (thimble, harvesting knife).

In the last two categories, it is no longer the hand but the tool which is in contact with the object and which acts on it. The role of the hand is limited to holding and directing the tool. Thus, it is the tool's mode of action which is relevant.

3) The mode of action of the tool reproduces that of the hand (seed-beaters, *mesorias*).

4) The mode of action of the tool is original; that is, it does not resemble any of the possible modes of action of the bare hand (sickle, scythe, most cutting instruments).

In actions involving both hands, these categories can be combined. Thus, in harvesting with a sickle, the hand that holds the instrument (usually the right hand) acts through the intermediary of the tool, according to mode no. 4, while the other hand, which seizes the stalks, acts according to mode no.1. But this hand may also be covered with a wooden protection (such as the *palamarca* used in Bulgaria, **Figure __**) which increases the hand's capacity, and thus is included in mode no. 2.

The question is now whether this categorization has a validity which goes **beyond grain harvesting**, and whether it has and value in terms of human evolution. It would be premature to go too far along this path. Nevertheless, it seems that two conclusions can be drawn.

The first is that the concept of tool is too general and too abstract to be of practical use. Indeed, this was to be expected. Even speculative philosophers no longer believe it possible to affirm anything at all with respect to tools in general. Though one can imagine categorizations other than the one proposed here (for an example, see that of Oswalt 1976), it is no longer possible to say anything about tools in general, without using examples which are **merely** implicit categories.

The second conclusion is that, contrary to an opinion that is all too widespread, tools are not some sort of outgrowth of the human body.[16] It would be tempting to think this might be true for certain tools, for example, those of categories 2 and 3, which, as stated above, resemble prostheses. But it would have to be proven that these tools are primitive, which is far from evident. There is little chance of finding archeological evidence, and what is learned from primate ethology does not lead in that direction; chimpanzee tools do not resemble prostheses (McGrew 1992). Neither the termite probe nor the hammer and anvil used to crack nuts are simple accessories in a pattern of action that could possibly be carried out with the bare hands.

Perhaps we should ask ourselves whether, rather than a "simple" prolongation of an organ or a limb, the prosthesis does not suppose the opposite idea: that the limb or organ could be perceived as though it were an instrument detached, or at least distinct, from the body. This would imply, first, that there exists a mental model of the instrument, and, second, that the model is applied to the human body. Would it be possible to think of reinforcing the thumbnail with a thimble if the nail were not first considered to be a tool? And, instead of the hammer or club being

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[16] This idea was developed in the nineteenth century, by, among others, the philosopher Ernst Kapp (1877: 29-39), to whom we owe the expression "organic projection". It is found in an almost identical form in the work of Leroi-Gourhan (1964: 132), who refers to the tool as an "artificial organ [...] a veritable secretion of the body and brain of Anthropians" (see also Leroi-Gourhan 1965: 40, 48, etc.). The idea is attractive at first, but it cannot resist more thorough examination. In the experiments of P. Mounoud (1970), for example, the child first tries to use a tool by carrying out the same general type of action (prehension, for instance) as the action he normally carries out with his own hand. This usually results in failure, and it is only after the child realizes that the tool is not merely an extension of his hand--and that it therefore functions in its own way--that he begins to use it successfully.

an "improved fist", does not the fist simulate--albeit rather inefficiently--a club or hammer?[17] In studies on learning, the term "scaffolding" is used classically to designate the various devices made available to help children and novices, and which are normally abandoned once a certain level of expertise is reached. Similar examples could undoubtedly be found throughout the history of sports. The tool-prostheses in harvesting are **associated with neither learning nor sports.** But I think that they suppose the same initial condition: the application of a tool model to the human body. And even though etymology does not constitute proof, it may be recalled that "organ" is derived from the Greek *organon*, which means "tool". It is highly unlikely that the tool began as a prosthesis. Everything indicates, on the contrary, that the prosthesis implies an already elaborated conception of the tool.

Final comments

By writing his article on "Body techniques", Mauss wished to show that technical action could not be reduced to the use of tools. He was, of course, right in this. But though it remains sometimes necessary to recall this point , it is not essential. The essential point is that the body is involved in all techniques, without exception. "Even the most advanced computer still needs someone to push the button", I once read in an advertisement. It can be admitted, following Hewes (1955: 232) that "the push-button represents the ultimate attenuation of environmental control through postural adjustments". But a world in which all technical actions are reduced to pushing keys and buttons is quite unlikely, for such a world would be too similar to a prison. We have a vital need for meaningful contact with things and beings different from ourselves, probably because it is these differences that make us conscious of ourselves. And, to be meaningful, these contacts must be based on efficient action, in other words, on techniques. All the modern developments in sports and physical exploits in the most hostile environments possible (high mountains, polar regions, deserts, ocean depths, etc.) confirm this. The more society protects us from risk, the more numerous are those who seek adventure, even if only fictitious, by going far abroad looking for ordeals and risks **which allow** them to measure **themselves** against the world. There is a human nature, and the need to measure oneself against the world is part of it.[18]

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[17] The idea that the hammer or club is merely an "improved fist" is another of Leroi-Gourhan's (1965: 48). It supposes that the action of hitting with a fist is a primitive one for humans. However, nothing is less certain. Its absence in North America before European colonization has been signaled. And it seems that even in Europe the use of fists in fighting can be tied to certain ritualized forms of fighting that are not universal. A tale of Walter Scott (*The Two Drovers*, in *The Chronicle of the Canongate*, chapters 13 and 14) is the story of the tragic misunderstanding regarding such forms of fighting between two friends, one English, the other Scottish, in the seventeenth century. In other words, rather than the club being an improved fist, the fist would be a substitute weapon used in forms of combat in which weapons are prohibited by social convention.

[18] Methods of measuring onself against the world of course differ from one society to another, but the fact **that people do so** is surely universal. The individual must in fact go that route in order to acquire the skills that will

But it is not enough to say that the body is involved in techniques. It is necessary to say how, and, in this respect, things are hardly more advanced today than they were in the days of Mauss. This is due to the especially tenacious invisibility of what we could call elementary technical facts, those in which the body plays an important role while the tool's role is reduced. Indeed, it is only when facts are observed and described that they become facts, in the scientific sense of the term. Now, fieldworkers only regard what they are able to observe--a methodological condition--and what they judge to be significant--a theoretical condition. Neither of these two conditions is usually fulfilled for the "mass of facts" that Mauss designated by the expression, certainly debatable, of "body techniques", so most of them are as invisible today as they were sixty years ago. The invisibility is not in the facts themselves; nothing is easier to see than a standing-knife, found in every kitchen and hardware shop in India. The invisibility must be in the eyes, then, or, more precisely, in the minds of the observers. This is a veritable theoretical blindness, which poses serious problems concerning the validity of conceptions which accommodate such blind spots.

We will never get out of this situation by trying to catalog or classify facts which have already been described. Attempts along this line have not been useless, but they have reached their limits. In reality, they were premature, because before putting facts in **some sort of** order, they must be properly recognized and distinguished, in other words, identified. Identifying is not the same as classifying, even if there are similarities between them. Identification has to come first, for it involves learning to recognize the objects about which we speak. I have tried to see what it would be possible to do in order to better identify hand movements, a domain in which a solid base already exists, thanks to Napier and his followers (Elliott and Connolly, Marzke and Shackley). It turns out that two conditions must be met: the different levels of analysis (structure, functioning, and function) must be taken into account, and consistent corpuses **along with** well-defined methods must be used. There are few examples of such corpuses. I have presented two, as different from each other as possible: archery and manual grain harvesting.

However imperfect our knowledge of these two corpuses, examining them suffices to suggest new paths to consider. The archery corpus allows us to enrich Napier's table in light of the kinematic concepts of F. Reuleaux. And the corpus of harvesting techniques leads us to question the very notion of tool, it being far too abstract a concept. It is impossible to state anything exact about tools in general, and, especially, to state that they developed as some sort of outgrowth of the biological body. If the old philosophical theory of organic projection has any use, it is only by **turning it on its head**. There are indeed tool categories which serve only to assist or reproduce an action carried out by the hand directly on an object. These I have qualified (with some

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make him a valued member of the social groups to which he must belong in order to lead a normal social life (Sigaut 1990a).

reservation) as "tool-prostheses". But nothing proves that these tools are primitive. Does not the idea of prosthesis, however, presuppose that the body part whose action is being supplemented or completed is already being treated like a tool? Far from the tool being some sort of outgrowth which gradually came to be separated from the body (how, nobody knows), it is, on the contrary, the body which gradually came to be a part of our tool-kit. This integration perhaps began in action, through apprenticeship, and was pursued in reflections, leading to the objectivation and representation of the body as a set of organs modeled on tools.

With but few exceptions, a list of which has often been reproduced, tools used by animals are their organs (Tetry 1948). These organs can present, in their mechanical, physical, or chemical functioning, very striking resemblances with artificial devices, but that does not alter the fact that they are strictly biological. They are all determined by precise genetic programming, in equally precise environments. The characteristic of human tools, as Pitt-Rivers understood 130 years ago, is, on the contrary, that they no longer depend on biology. Tools are things which are used in ways not inscribed in any preexisting program, things which are "denatured", to use the expression of the novelist Vercors.[19] It was necessary, no doubt, for the process of re-creating the world as a source of instruments to be well advanced in order for man, coming full circle, to fit his own body into that world.

But the instrumentalization of the human body obviously did not take place all at once and once for all during some prehistoric period. It is a process that is still ongoing today. In the West, over the last two centuries, mechanical and biological know-how have probably brought about a veritable reshaping of the human body. But there is no reason to think that the contemporary Western world is an exception in this respect. Each society, each era, has necessarily participated in the process, and each has left its mark. That the hands and the body are the first and the most precious of all tools is an old commonplace in philosophy. But it is a commonplace without interest as long as it leads only to speculation. The only worthwhile question is how to proceed to more concrete ideas. In my opinion, one of the merits of Mauss is that he **raised** question.

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[19] General Pitt-Rivers first proposed the hypothesis that the first human tools were imitations of animal devices, but he soon abandoned it. Primitive human tools in no way resemble animal "tools", but must have been somehow reinvented anew (Sigaut 1990 b). As for Vercors' novel (*Les animaux dénaturés*, 1952), it is obviously not a scientific work. I only draw on the image present in the title, and turn it on its head. Humans are by no means "denatured animals", as the author suggests, but they have a way of their own to "denature" the things around them by instrumentalizing them. As Pitt-Rivers noted, there is nothing in nature which even slightly resembles a simple knife.